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teman's exposition combined excellences of the most
co·d kinds. It was clear, vivid, and succinct—rich too
i abundance of illustration always at the command of
rmously wide reading and an astonishingly retentive
iory. A voice of sonorous sweetness, a grand fore-
id, and a profile of classic beauty, intensified the
pression of commanding power which an almost equally
mplete mastery over mathematical truth, and over the
ms of language in which he so attractively arrayed it,
uld not fail to make upon his auditors" (pp. 99, 100).

His pupils' affection, the memoir tells us, was not
ned by any laxity of discipline, for he was strict,
ecially as to quietness and punctuality.[1]

Such arduous labours as these would amply suffice for
generality of teachers, but the remainder of his time
s occupied with other work hardly less exhausting than
se. In May, 1828, he was elected a Fellow of the
tronomical Society, and in February, 1830, he took his
ce on the Council. In 1831 he was elected Honorary
cretary; in which position he entered with zeal, we are
l, into every question brought before the Society, and
place was not a sinecure. "It is not easy to say how
ch of the usefulness and prosperity of the Society . . .
s due to his incessant energy and effort, and to his
ady judgment at difficult junctures." Though his con-
tion with the Society lasted for some thirty years, he
uld never undertake the office of President. "I will
e for and tolerate no President but a practical astro-
ner. . . . The President must be a man of brass—a
cronometer-monger, a telescope-twiddler, a star-stringer,
lanet-poker, and a nebula-nabber."[2] He was frequently
ployed as a consulting actuary,[3] and bestowed also
t time and labour upon the subject of the decimal
ge.[4]

ssing from De Morgan's public labours we hurriedly
e at him as a writer, and here we cannot do better
quote Prof. Jevons: "From the above enumera-
of his mathematical and logical writings, "it will
pparent that the extent of De Morgan's literary and
tific labours was altogether extraordinary; nor was
cy sacrificed to quantity. On the contrary every
cation was finished with extreme care and accuracy,

tudent, who joined the class in 1859, has put at our disposal some
s wrote during the session 1859-60: "The class begins at 9 o'clock,
ever early we go the Professor is sure to be there. Only once or
ave I been early enough to see him coming. He has a large head,
the top, and with a tremendous halo of hair round the crown. He
black cloth suit and a parson's white neck-cloth. His coat is a
-tail, and his trousers, with fob pockets, scarcely reach his boots, of
he laces are often too long. As he shuffles along he seems to be
g the flagstones or rails, urged by a sort of centrifugal force to keep
side kerb, as Dr. Johnson used to do. In the lecture-room when the
has rung, he always goes through the same routine at commencing.
of all he takes out a large red silk pocket handkerchief, with which he
his spectacles; he then readjusts them with the bridge upside down,
ough he has only one eye he can see as keenly as another man with
He then turns back the cuffs of his sleeves, and, after passing his
through his hair, takes his compasses in hand and looks round the
e his class. He has been talking all the time, and by this is fairly
ed on his subject. . . . He often indulges in jokes with manifest
The other morning he was illustrating a point, when he said, 'This
s me of an anecdote told me once by my old Cambridge tutor, Prof.
k. He had been for some time striving to instil into the mind of a
obtuse student the difference between $4x$ and x^4. At last he timidly
d to remark, "I think, now, Mr. A., you clearly see the difference." "
I think I do, but between us don't you think, Professor, it is a need-
nement?" I think the part of his lectures I have most enjoyed has
treatment of the Theory of Probabilities. In this he seemed to
Then follow remarks similar to Mr. Taylor's, and he concludes:
rofessor takes more pains with his class, and all through the session
osits, in the Library, Tracts written by himself on the particular
hen in hand." We have similar testimony from other quarters.
t of the offices he held is given (p. 270).
was never connected with any office, but his advice was sought by
ards whenever there arose a "nodus vindice dignus."
ll account of his work in this direction occupies pp. 235-255 of the

and no writer can be more safely trusted in everything which he wrote. It is possible that his continual efforts to attain completeness and absolute correctness injured his literary style, which is wanting in grace; but the estimation in which his [illegible] is shown by the fact that th[illegible] ket price. Apart from his co[illegible]al and mathematical discove[illegible] hardly any man of science [illegible]re extensive, though it may o[illegible]pon the progress of exact and

His love [illegible] was intense:[1] "the most worthless book of a bygone day is a record worthy of preservation." Evidences of his minute acquaintance with all sorts of out-of-the-way works present themselves in almost all his writings, but are especially conspicuous in that wondrous repertory of wit and wisdom, the "Budget of Paradoxes." De Morgan's peculiar dislike of conventional titles, "which are not what they seem to be," led him to decline the honorary degree of LL.D. of the University of Edinburgh, and accounts for his not allowing his name to be put up for the F.R.S.[2] "Whether I could have been a Fellow, I do not know; as the gentleman said, when asked whether he could play the violin, 'I never tried.'" In fact, as he writes in the "Budget," he was a man who could not *groove*.

The last occurrence connected with science which gave him pleasure was the foundation of the London Mathematical Society.[3] The idea of having such a society occurred to his son George and Mr. A. C. Ranyard, and on their mentioning the matter to Prof. De Morgan, he at once gave in his adhesion to their proposition, and with the countenance thus extended by himself and other leading mathematicians who were got together in reply to a circular issued by the two founders, the Society started into existence. Prof. De Morgan was the first president, and delivered at the first public meeting (January 16, 1865) an interesting and characteristic address. He continued to take a warm interest in the meetings (being a vice-president for the last time in the session 1869-70) until November 26, 1868, after which date severe illness prevented his further attendance.[4] The end came on March 18, 1871, "just after midnight he breathed his last."[5]

In the Vacation of 1837 De Morgan married Sophia Elizabeth, daughter of William Frend. This gentleman was a member of the old Mathematical, and subsequently of the Astronomical Society, had been Second Wrangler, and a Fellow of Jesus College, Cambridge. He sacrificed good prospects as a clergyman to his conscientious scruples about the doctrines of the Established Church, and was at this time Actuary of the Rock Life

[1] He loved to surround himself, as far as his means allowed, with curious and rare books. He revelled in all the mysteries of watermarks, title-pages, colophons, catch-words, and the like; yet he treated bibliography as an important science.

[2] Why he did not care to "shine in the dignity of F.R.S."—*See* "Budget of Paradoxes," p. 18.

[3] "There is a new Mathematical Society, and I am, at this present writing, its first president. We are very high in the newest developments, and bid fair to take a place among the scientific establishments." Then in contrast with the old Mathematical Society, "But not a drop of liquor is seen at our meetings, except a decanter of water: all our heavy is a fermentation of symbols; and we do not draw it mild."—"Budget of Paradoxes," p. 236.

[4] In the recent Presidential Address it was announced that a "De Morgan Memorial Medal," of the value of £10 would be awarded triennially by the Society. The first award to be made in November, 1884.

[5] *Monthly Notices*, "at one o'clock in the afternoon."

Assurance Office. The marriage was a most happy one, and surrounded by a family of seven children, of whom three at least died before their father, De Morgan sought his happiness, as we have endeavoured to show, in his home, amongst his books, and in the earnest discharge of his professorial and other duties.

The Memoir is charmingly written, and abounds in graphic details, which bring clearly before the reader the picture of a simple, manly character that was unique in its idiosyncrasy. A prominent object in its production has been to tell the story of the Professor's connection with University College, and of the events which led to his leaving it. "After the lapse of sixteen years I trust that the narrative will provoke no revival of the somewhat acrimonious controversy which ensued."

Another feature of the work is the selection from De Morgan's extensive correspondence with contemporary scientific men; these letters are full of interest, and abound in utterances characteristic of the writer. Always effective and to the point, they are often very humorous: the humour, indeed, sometimes borders upon trifling.

Instead of thinking that Mrs. De Morgan has exceeded due limits in her selection, we would have welcomed a far larger number of specimens. On p. 333 De Morgan states that he had corresponded for thirty years with Sir W. Rowan Hamilton, but no specimens of the correspondence are given. In one or two cases Mrs. De Morgan has deviated from the general rule she laid down for herself, and to this deviation we are indebted for some very interesting letters from the late Sir Frederick Pollock, which enlighten one as to what was required to be read for the Senior Wranglership at the beginning of the present century. Is it too much to hope that another volume may be issued containing a further selection from the correspondence, and also a few of the more valuable of the early papers, such as those which appeared in the *Journal of Education*, and in the *Companion to the British Almanac*? We venture to give the following extract from a letter we received under date May 15, 1869, as an ordinary specimen of his writing to one who had no special claim upon the writer:—". . . I should decidedly object to the reference made to Barrow on the last leaf. 'Dr. Barrow with an orthodox dislike to give unnecessary credit to a Moslem author has misled. . . .' When was there an orthodox dislike to Mahometans being discoverers in science? And what possible reason is there for imputing any such feeling to Barrow, a man of most unimpeachable fairness, except in this, that when he had a congregation by the ears he would hold on for three hours until they prevailed on the organist to 'blow him down.' He had lived among the Turks at Smyrna and Constantinople, and was certainly not ill-inclined to a Mahometan, as such. But this much is enough: the imputation is quite new, or nearly so, and should not appear without proof in the *obiter dictum* of an historical writer who obviously makes it a theory to explain something he has found. . . . P.S.—I think that for one orthodox man who might be supposed likely to rob a Mahometan of geometry, I could find three who would have been more likely to toss it back again with the remark that such infidel stuff was only fit for Mahound and his slaves."

A list of writings is appended. In this we notice the following slips:—"Elements of Arithmetic," the d should be "1st edition, 1830; 3rd, 1835"; on p. 403 (18) for "No. 3" read § 3 of (15) supra"; p. 404, in (1 dele "1," and in (6) for "Trigonometry" read "Ge metry; p. 405, (5) should, of course, be "$P\,dx + Q\,dy + R\,dz$"; p. 406, in 1849, insert "Remarks" after "Sup plementary"; in (1), (2), we think the dates are inaccu rate; p. 407, read "Alfonsine."

We remark also that no account is taken of communi cations to the Mathematical Society. These were ten i number, not counting the Opening Address, which form the first number of the Society's *Proceedings*. The onl papers printed are "A Proof that every Function has Root" (No. vi., a mere notelet); "Remark on paper b Mr. Woolhouse 'on General Numerical Solution'" (No xiv.); and "On the Conic Octagram" (No. x. pp. 26-29). In this last paper occurs the characteristic note: "Thi presentation of the second hexagon was actually sug gested to me by observing that *Bloise Pascal* has tw hexagrams, and the jocose inference that there ought t be two hexagons in the theorem (given in the paper). M own names are both octagrams; but though I bow before the coincidence, I have no suggestion to acknowledge." There is a fairly full "Index of Names, &c.," but we d not grasp the principle upon which it is drawn up, a some names are inserted and others left out. We failed at first to identify "Prof. John Adams" with "Neptune Adams. There are also the following corrections to be be made:—Read "J. Baldwin Brown," "Arthur Cayley" (both here and in the "Budget of Paradoxes" th famous mathematician is called "George"), Hanssen (fo Haussen), Encke (for Hencke), Royal Society (insert most important reference to p. 172), Sedgwick, Rev. Simeon (not J.), insert John Taylor, p. 122, and dele 124" under Sedley Taylor. On p. 306 we presum should be x, and on p. 286, for 1866 read 1865; present writer succeeded George De Morgan as teac in the session 1865-66.

R. TUCKE

FISHES OF SWITZERLAND

Faune des Vertébrés de la Suisse. Par Victor Fatio, Phil. Vol. IV. Histoire naturelle des Poissons. partie I. Anarthropterygiens. II. Physostomes-C nidés. 8°. pp. xiv. et 786, avec 5 planches. (Gé et Bale: H. Georg, 1882.)

AFTER an interval of nearly ten years Dr. Fatio issued another volume of the series of excellen monographs, in which he gives the results of his researche into the vertebrate fauna of Switzerland. The fir volume, published in the year 1869, contained the M mals; the third (1872) the Reptiles and Batrachians; second, which will be devoted to Ornithology, being s in course of preparation. The one now published, whi is the fourth of the series, treats of a part of the Fishes, which class will be concluded in the fifth.

No one who studies this volume will be surprised at the long lapse of time which intervened between its appearance and that of the preceding. The author had not the advantage of being assisted in his work by collec tions already formed and available for the purpose, but had to collect the materials himself; a labour which even in a small country like Switzerland, takes years to

accomplish; especially as, for comparison's sake, he extended his researches to the fauna of the neighbouring countries. His descriptions are well elaborated and compiled from numerous observations; they include all the variations of age, sex, season, locality, &c., and particular notice is taken of those modifications, by which Swiss examples seem to be distinguished from those of Germany, France, Italy, &c. Thus, this work rises far above the level of a local publication, and is of as great a value to the student of European freshwater fishes, as to the Swiss naturalist.

The present volume treats of the Acanthopterygians and Cyprinoids only, 5 species of the former and 21 of the latter being admitted as permanent inhabitants of the country. Besides, the author distinguishes 3 sub-species, many varieties, and 3 hybrids; he also refers in shorter chapters to 13 other species and 6 hybrids, which are extra-limital, or may sooner or later be found straying into Switzerland.[1] This number of the freshwater fishes of Switzerland must appear small, when we consider that it comprises representatives of four of the principal river-systems of Europe, viz. the Rhine, Rhone, Po, and Danube; and there is no doubt that this comparative poverty is due to the altitude of the country, freshwater Acanthopterygians and Cyprinoids being generally more developed in the less rapid waters of warmer low-lying countries. The Rhine contributes the majority; 20 out of the 24 species[2] which inhabit the middle and lower sections of the river, ascending beyond the Falls of Schaffhausen. The Rhone is inhabited by 24 species, but, singularly, of these 11 only have been able to establish themselves above the Perte du Rhone, although the others freely enter the Saone or penetrate even into the upper Doubs, a river not included in Swiss territory. Of the 23 species found in the lower part of the Po, 15 reach the Swiss frontier; and this southern portion of the fauna is in its character so distinct from the northern, that 9 only of these 15 species are identical with Rhine fishes. Finally the fish-fauna of the Danube, which is stated to consist of 30 species, is represented in Switzerland by 3 only, the great altitude of the river Inn proving a most effectual barrier to the dispersal of the remainder. Moreover, these three species are common Central European types, and not peculiar to the Danube.

The author has taken great pains to ascertain the extreme limits of altitude, to which the several species can attain in the Alps. Two only go beyond the height of 2000 metres, viz. the minnow and miller's thumb, which are still found at respectively 2400 and 2200 m. The perch, the next in order, reaches an altitude of 2000 m., all the remainder living at, or below, 800 m. However, several have been successfully imported to altitudes varying between 1000 and 1700 m., thus the carp, tench, rudd, roach, and chub.

Of the fishes described in this volume, we wish to draw particular attention to two which, belonging to marine genera, and evidently being of marine origin, have acclimatised themselves in the fresh waters of Southern Europe, and penetrated into, or close to, the confines of Switzerland, viz. a goby (*Gobius fluviatilis* or *martensii*), which has ascended the Po, and a blenny (*Blennius cagnata*), which occurs in abundance in Lago Maggiore as well as in the lake of Bourget in Savoy.

Hybrids are comparatively scarce in Switzerland. The author justly accounts for their scarcity by the physical peculiarities of his country; snow-fed, rapid rivers are less adapted for their production, than the slower and warmer waters of low countries, where a greater variety of species and a larger number of individuals are mixed together, sometimes within very narrow limits.

The volume is illustrated by five well executed plates, three of which are devoted to osteological, dental, and dermal details.

The author of a thoroughly original work like the present, cannot fail to differ from his predecessors in questions of specific distinctions and numerous other points of detail, but it is our duty to testify to the fair and calm spirit, in which such questions are discussed and treated by him; and we hope that, before many years, we shall have the pleasure of announcing to our readers the completion of so valuable a work as Dr. Fatio's Ichthyology of Switzerland.

[1] The second part of the Ichthyology is estimated to contain about 21 species.

[2] These and the following numbers refer to the Acanthopterygians and Cyprinoids only.

OUR BOOK SHELF

China. By Robert K. Douglas. (Society for Promoting Christian Knowledge, 1882.)

IT may be said at once respecting this book that it is without exception the very best elementary work on China with which we are acquainted in any European language. The author has resided for many years in China, and is in the forefront of the Chinese scholarship of our time; his work is, therefore, not only accurate, but it places the reader abreast of the latest researches. One of the most remarkable of these is fully explained at pp. 359-60. The *Yih King*, or Book of Changes, is the work for which the greatest antiquity is claimed by the Chinese. Some writers have placed it as far back as between 300 and 400 B.C. However this may be, the key to its interpretation has been entirely lost, although the best native scholars of all ages, including Confucius himself, have attempted to explain it. M. Terrien de la Couperie (assisted, we believe, by Prof. Douglas himself, though this fact is not mentioned), has succeeded within the last few years in showing that "instead of being a mysterious depository of deep divinatory lore, it turns out to be a collection of syllabaries such as are common in Accadian literature interspersed with chapters of astrological formulæ, ephemerides, and others dealing with ethnological facts relating to the Aboriginal tribes of the country; but all taking the form of vocabularies, and therefore as impossible to be translated in the sense in which every commentator, from Confucius downwards, has attempted to translate them as 'Johnson's Dictionary' would be." Although we possess innumerable volumes on subjects connected with China, we have not until now a thoroughly trustworthy book covering the whole ground in a simple elementary manner. Some volumes recently published for popular reading on the countries of the East exhibit such lamentable ignorance, that we can only "gasp and stare" at their contents. Notwithstanding his own intimate knowledge of the subject, Prof. Douglas has consulted almost all that we have in our literature relating in any way to China, from Davis's Chinese poetry and Oppert's Susian texts, down to recent numbers of the English journals published in China. The two last chapters—that on "Language" and "Literature"—are models of clear and simple exposition of complicated subjects. Another excellence of the book is what we may call its perspective. The writer does not thrust any particular branch of his subject into undue prominence, to

the detriment of the rest. The first chapter gives a brief sketch of Chinese history, the second of the system of administration; various chapters are then devoted to popular customs, to education, medicine, music, dress and food, architecture, honours, names, superstitions, religions, &c. There is also an excellent map. To the general reader who desires some accurate information respecting a country which is coming nearer to us every day, or to the student who wants a *vade mecum*, no better volume can be recommended.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

On the Occurrence of Great Tides since the Commencement of the Geological Epoch

MR. BALL says very truly that the fundamental question is, what traces of great tides ought we to expect to find if those great tides had really existed? Mr. Darwin says, coarse-grained rocks, and different forms of vegetation calculated to resist the action of the accompanying great winds. Mr. Ball, in reply, remarks that high tides in the Avon are accompanied by fine sediment. He thinks with others that the high tides would have produced a vastly greater amount of sediment than is being formed at present. I quite agree with Mr. Ball about the fine sediment, but I am not at all clear that high tides mean great marine denudation. By far the largest portion of the work done by the sea as a denuding agent is due not to the wearing action of currents, or to the pounding of materials on a beach at low water, but to the direct action of the sea on the cliffs. This force is estimated as about a ton per square foot on the average in winter, on the west coast of Great Britain. This undermines the cliff at the sea level, and then the top part falls partly by its own weight, but still more through the effect of the air compressed in the caves and cracks, which by its elasticity spread the blow over a very large surface through the crack and joints of the rock. Now to undermine cliffs with a given force of wind and wave, it seems clear that the maximum effect would be produced where the tides are very small, for there the force is constantly applied at the same spot. With a rise and fall of 100 feet, each portion of the cliff would be subjected to the force of the waves for so short a time that in all probability caves would never be formed at all, and the height of the tide would be an actual protection to the land. As a matter of fact, those places where the tides are highest show, as far as I know, no signs of excessive denudation. I spent two days last year on the Bay of Fundy, where the tides are higher than anywhere in the world, and I was very much struck with the absence of any evidence of great denudation due to the tide. The cliffs at the Loggins are about high-water mark, with a long beach which slopes very gradually. The force of the waves, such as they are, is spent in hammering this beach and grinding it into fine sand and mud; the mud is carried about in suspension by the tide, and the sand is shifted about, but the denuding effect is exceedingly small. The consequence is that the cliffs are pounded by the waves for such a short time each tide that they suffer mainly from atmospheric denudation, the sea doing little more than keeping their base clear, and in many places not even doing that.

Similar phenomena are presented by the highest tides in Great Britain—those on the Severn. Here at Clifton on the Avon the tide rises 30 feet; there are no waves; the banks are covered with a thick coating of mud and denudation is nil. At Aust Cliff, again, on the Severn, where the soft red marls are peculiarly liable to erosion, the height of the tide is again a protection. The cliff is about high-water mark, and the force of the waves is expended on the beach. The case is the same at Watchet, and a good many other places on the Severn. I do not know of any part of the Severn remarkable for excessive denudation, owing to the high tides. There is a strong resemblance between the Bay of Fundy and the Severn: there are the cliffs at high-water mark, the same long beaches, the same shifting sands and mud in suspension; similar causes have produced similar results. In narrow inlets like the Bay of Fundy and the estuary of the Severn, these high tides mean rapid currents and small waves; but along shores freely exposed to the ocean, the highest tides might be accompanied by very feeble currents. But if, as Mr. Darwin says, the high tides were accompanied by trade winds about 3½ times as strong as the present ones, the battering power of the waves and the strength of the currents would be very greatly increased, and plains of marine denudation, it might be supposed, would be very rapidly formed. What would be likely to happen if such winds and waves began now to act on our shores? Should we have reason to expect that England would in a comparatively short time disappear beneath the waves? A very rapid destruction of our present cliffs would undoubtedly begin, though, as I pointed out before, this would be attributable mainly to the wind, and not to the tide; the cliffs would be driven back to about the ordinary high water mark, leaving a long shelving beach extending to a few feet below the low water mark. The cliffs would then for far the greater portion of each tide be entirely free from marine denudation, and their rate of wasting would depend on the power of the sea to tear up the long solid sloping beach, and restore comparatively deep water at the base of the cliffs. But this process is an exceedingly slow one, because there can be no undermining or assistance from compressed air, and I should anticipate that marine denudation would then be actually slower than it is at present. There would, of course, be abundance of very fine sediment formed during the first wearing back of the cliffs by the grinding of the materials between tide marks; but when once the cliffs had reached the high water line, the amount of sediment would depend chiefly on the amount of atmospheric denudation, supposing that the sea kept the base of the cliffs clear. We should, in fact, have a repetition of the phenomena presented to us now by the Bay of Fundy and the Severn. Thus far, then, it seems to me that no argument can be drawn from the fineness of the early sediments against the existence of high tides in the Geologic period; nor, on the other hand, does the quantity of sediment seem to me a strong argument in favour of it. But Mr Darwin's argument that the vegetation of the Carboniferous period could not possibly have held out against the violent winds which necessarily accompanied these high tides seems to me unanswerable. One has only to reflect on the effect produced by our present winds to feel convinced that if the winds and tides went together the Rev. were certainly Pre-Carboniferous, and almost equally certainly Pre-Devonian.

J. G. GRENFELL

Clifton College, December 29, 1882

Sir George Airy on the Forth Bridge

As Sir George Airy's last letter may, like his first, provoke replies from distinguished American and continental engineers, it may save your correspondents' time and your own valuable space if I add a few final words in explanation.

1. Sir George says:—"The danger of buckling in a horizontal direction with a length of 340 feet, remains undiminished unless it is counteracted by bracing unknown to me." Now Sir George evidently has forgotten that some time ago he was furnished with photographs of a large model of the bridge taken with the view of showing the said bracing, and that his attention was specially directed to the point.

2. Sir George thinks "it desirable that attention should be called to the magnitude of the forces concerned," and speaks of a wind-pressure of 75 tons, and an end-pressure of 600 tons. Now he clearly has forgotten that before he wrote his first letter a "stress diagram" was sent to him, on which it was noted that the wind-pressure provided for was 2207 tons on each span, and that the estimated end-pressure on the strut referred to was 2380 tons.

3. Sir George holds the engraver responsible for some of the alarmist statements in his first letter. I must remark, therefore, that it was pointed out that the bridge would have been perfectly safe had the details of the design been as he assumed. For evidence that a 340 feet tubular strut of 12 feet diameter would not fail in the manner stated by him, he was referred to Hodgkinson's experiments as published in the *Philosophical Transactions*, and Clark's work on the Britannia bridge; and further, he was lent the *Transactions* of the American Society of Civil Engineers for last year, containing the most recent experiments on long wrought iron columns. Any or all of these documents would have shown him that the Forth bridge

struts, even if arranged as he first conceived them to be, could not have failed by flexure during the wildest hurricane.

4. Sir George finds fault with the connection of the brackets, and "can hardly imagine that trains could be run through at speed." I should have been pleased to have explained the connection to Sir George, but he has not sought to know anything about the details of the bridge, and, I am sure, would be much puzzled to give your readers even the vaguest possible description of the connection, which he nevertheless stigmatises as "not very perfect."

In conclusion, as Sir George has not done so himself, I would warn any young student who may have read the investigation contained in the appendix to the first letter, that the methods therein proposed would lead to an over-estimate of the strength of struts of ordinary proportions by from 200 to 300 per cent. This warning is the more necessary, as the general tenour of Sir George Airy's letter might make a student imagine that he erred, if anything, in the direction of excess of caution, whereas the application of the principles laid down by him would, in the case of the Forth bridge, result in the compression members being made only one-third of the strength considered expedient by Mr. Fowler and myself. B. BAKER

2, Queen Square Place, Westminster, S.W.

P.S.—It may interest some of your readers to know that the maximum force recorded during recent storms by our wind pressure plates at the Forth has been 20 lbs. per square foot, upon the small and light plate having an area of 2 square feet, and 12½ lbs. upon the large and heavy one, with an area of 300 square feet. The same ratio holds good down to pressures of 2 lbs. per square foot, and it appears pretty certain that the higher blasts are of such momentary duration and of such unequal distribution, that even a small sized railway bridge could never experience ordinary anemometer pressures. Other reasons for a reduced pressure on a large surface have been advanced by Dr. Siemens in a recent number of the *Comptes Rendus*. Nevertheless, in this instance of the Forth bridge we have assumed that a 56 lbs. hurricane will act simultaneously over the whole width of the Forth, with a resultant lateral pressure of no less than 8000 tons upon the main spans. We have further assumed that the said hurricane might blow down one side of the Forth, whilst a dead calm prevailed on the other side, and have even provided for the twisting action upon the piers and superstructure due to a 56 lbs. hurricane blowing *up* the Forth on one side, and *down* it on the other. To ascertain what lateral pressure a 56 lbs. hurricane would cause, we tested, both in air and in water currents, a large model of the bridge, with cross-bracing complete, and ascertained its equivalent in square feet of flat surface. Under any of the conditions of wind pressure enumerated above, combined with any distribution of the rolling load, the resultant stresses upon superstructure, holding down bolts and piers will be far within the safe working limits as determined by our experiments upon the respective materials.—B. B.

Altitude and Weather

IN NATURE, vol. xxvii. p. 176, you notice the remarkable warm and dry weather September 21 last on Ben Nevis, during an anticyclone, and, as at the foot the air was relatively cold and humid, you see in the heat and dryness on the mountain an effect of descending air currents. In this you are quite right, but I do not think you are right in estimating that this air was saturated at a certain height above Ben Nevis. The fact is this: the increase of temperature from a certain height above sea-level to the latter being *de facto* much less than the dynamical increase of a stratum of air, due to compression sinking down, a downward current of air will be generally warm, and relatively dry. It does not matter if it sinks along the slopes of mountains (as the foehn), or vertically, as modern meteorology considers it to be the case in anticyclones. There is only one great difference: the air currents down a slope may be, and often are, very violent, and only when they are so, their relative heat and dryness are felt, while the downward currents in an anticyclone are so gentle that they are seldom felt or directly registered, and that mostly the thermometer and hygrometer are our only means of detecting them. On account of their slow motions, the effect these downward currents during anticyclones is little felt in valleys and plains, as (1) they are even more retarded near great level surfaces; (2) in the colder time of the year, especially when the ground is covered with snow, the radiation from the soil lowers the temperature of the lower strata. Thus during anticyclones in winter a very low temperature is generally experienced in plains and valleys, due to radiation, and a very high temperature and low humidity on isolated mountains, due to descending currents of air.

These conditions are best realised during protracted and considerable anticyclones, and it was Prof. Hann's merit to have explained this fact.[1] The exceedingly protracted anticyclone of December, 1879, in Central Europe, was especially favourable to the proof of the existence of descending currents, as the cold was great in the valleys, even in the high ones, like the Engadine and the Davos, but the air was warm and very dry on isolated mountains. An example from the best mountain observatory of that time, the Puy de Dôme, and the foot of it, will suffice, nine days, December 20-28, 1879, at 6 a.m.

	Feet.		Temp. F.		Relative humidity		Amount of Cloud
Puy de Dôme,	4813	...	38·8	...	38	...	1·3
Clermont (base)	1273	...	8·2	...	91	...	0·7

There is all reason to think that in these days there was no saturated stratum of air even considerably above, say the Puy de Dôme.

I must remark that Prof. Hann, in his last work, "Der Föhn in Bludenz," does not sustain his former opinion that great precipitations on the windward side of mountains is necessary to the appearance of a foehn on the leeward side. His opinion now is, that a considerable barometric gradient and the drawing in of air from considerable heights are alone necessary, for even if the air on the mountains is not abnormally warm, it will come down warm and relatively dry. A. WOEIKOFF

Ofizerskaja, St. Petersburg, December 15-27, 1882

The Fertilisation of the Speedwell

I FEAR that Dr. H. Müller's passage in Schenk's "Handbuch" would occupy too much space to be given here in full; but I can condense what he says into a few lines. Dr. Müller takes the *Veronica chamædrys* as representing a type of flowers in which the anthers have to be brought into a position to strike the body of the insect by the action of the insect itself. He finds the same arrangement in the *V. urticæfolia*. These flowers are visited by insects of various kinds, but their structure is, he thinks, explained only by what takes place when they are visited by *Syrphidæ*. When one of these insects visits such a flower, it hovers for some seconds before it, then settles upon the lower lobe of the corolla, without noticing the style which is coloured like the corolla, and which is now under the insect's body. It then crawls higher to reach the nectary, and in doing so bends down the stamens—which are also coloured like the corolla—until the anthers strike against the under part of the insect's body. The pollen thus obtained is carried to another flower, and brought into contact with the stigma when the insect first alights; and fresh pollen is again obtained by the attempts to reach the nectary. Dr. Müller either knows from observation or assumes that in the *V. chamædrys* anthers and stigma are mature at the same time. He attaches importance to the fact that both stamens and style are coloured like the corolla, and therefore appear to escape the observation of the insect; and the thinness of the base of the stamen is also noticed by him as one feature in the adaptation of the flower to the visits of *Syrphidæ*. He does not refer to the looseness of the corolla. Mr. Stapley's suggestion that this may play some part in the work of cross-fertilisation is an ingenious one, and calls for further research.

As to the *V. hederæfolia*, Dr. Müller mentions it as one of the plants that have a tendency to keep their flowers half-shut in cold and rainy weather, and thus to become cleistogamous.

I am sorry that I misunderstood Mr. Stapley's first letter upon any point; but he has misunderstood mine also, if he thinks I was not aware he wished to call attention "to the *adaptation* of the flower for cross-fertilisation." I wrote as briefly as I could, and naturally assumed that he would understand I was not thinking merely of the fact that Diptera drew down the stamens. ARTHUR RANSOM

Bedford, December 23, 1882

THE SACRED TREE OF KUM-BUM

THE dissipation of illusions is always a little painful, even after repeated experience of the process. I must confess, then, to some feeling of injury at learning from Mr. Keane's interesting review in NATURE, vol.

[1] *Zeitschr. für Meteorologie*, p. 129, 1876.

xxvii. p. 171, that Huc's "tree of ten thousand images" is nothing more than a common white lilac. Myths of this kind I have generally found to have some substratum of fact at the bottom. They can be rationalised, and mere explosion does not seem to be a satisfactory way of getting rid of them.

Now our knowledge of the indigenous vegetation of China is painfully limited. An immense portion of the flora is doubtless gone beyond recovery in the cultivated districts. Remnants of the primitive, wide-spreading forest remain, however, in the precincts of temples and monasteries, and these woods have always yielded novelties to botanists who have examined them. It had seemed, therefore, little short of certain that the sacred tree of Kum-bum would be something of considerable scientific interest if specimens of it could be got hold of.

The only edition of Huc at hand to refer to is Hazlitt's translation, published by Thomas Nelson and Sons in 1856. The well-known account of the tree will be found on pp. 324-6. According to Huc, the name Kum-bum, or as he spells it, Koun-boum, consists of "two Thibetan words signifying ten thousand images, and having allusion to the tree which, according to the legend, sprang from Tsong-Kaba's hair, and bears a Thibetan character on each of its leaves." Now, according to Kreitner, as quoted by Mr. Keane, "the Abbé Huc tells us that its leaves bear the natural impress of Buddha's likeness and of the Thibetan alphabet." As a matter of fact, he does not say anything like this. What he does say is as follows:—

"There were upon each of the leaves well-formed Thibetan characters, all of a green colour, some darker, some lighter than the leaf itself. Our first impression was a suspicion of fraud on the part of the Lamas, but, after a minute examination of every detail, we could not discover the least deception. The characters all appeared to us portions of the leaf itself, equally with its veins and nerves; the position was not the same in all; in one leaf they would be at the top of the leaf, in another in the middle, in a third at the base, or at the side; the younger leaves represented the characters only in a partial state of formation. The bark of the tree and its branches, which resemble that of the plane-tree, are also covered with these characters. When you remove a piece of old bark, the young bark under it exhibits the individual outlines of characters in a germinating state, and, what is very singular, these new characters are not unfrequently different from those which they replace."

Of the tree itself as Huc saw it some forty years ago, he gives the following account:—

"The tree of the Ten Thousand Images seemed to us of great age. Its trunk, which three men could scarcely embrace with outstretched arms, is not more than eight feet high; the branches, instead of shooting up, spread out in the shape of a plume of feathers, and are extremely bushy; few of them are dead. The leaves are always green, and the wood, which is of a reddish tint, has an exquisite odour, something like that of cinnamon. The Lamas informed us that in summer, towards the eighth moon, the tree produces huge red flowers of an extremely beautiful character."

Hazlitt's translation contains two woodcuts, one (p. 325) of the tree with its canopy, the other (p. 369) of a leaf with its markings. What the history of these illustrations is, there is nothing to show; Huc's book in the original French had, I think, none. The leaf with its markings has a by no means impossible appearance; whether the markings are like Thibetan characters, I cannot say. The outline of the leaf is not unlike that of a fuchsia, but it would not pass for a lilac.

I suspect, then, that there really was in Huc's time a tree with markings on the leaves, which the imagination of the pious assimilated to Thibetan characters. Perhaps it was the last local relic of some unknown endemic tree; in Hongkong I believe many of the endemic species are represented by but a few individuals. It may well have died and been replaced by a lilac, and the genuine markings by the fudged-up image of Budha "etched with some acid on the leaves."

It is disappointing that Szechenyi's expedition seems to have done nothing for botany. As Grisebach says, "We can only guess at the richness of the Chinese flora." Every now and then some one is induced to collect a few plants, and almost invariably they contain something new to science. A more extended knowledge of Chinese plants is now essential to a right understanding of the phyto-geographical facts of the north temperate flora. Unfortunately, the numerous Europeans who visit China are occupied with political, religious, or commercial business, with little time for subsidiary pursuits. But any of them who may chance to read these lines, may rest assured that they will be really doing a useful work by collecting and drying even a few *wild* plants in their respective neighbourhoods.

Kew W. T. THISELTON DYER

NORWEGIAN GEODETICAL OPERATIONS[1]

IN 1861 an Association was formed, under the auspices of Lieut.-General von Baeyer, having for its object the measurement of arcs of meridians, and parallels, in Europe. Most of the Continental nations joined this Association, and have carried out triangulations and spirit levellings of precision to further the objects in view. It is the intention of the Association to measure an arc extending from Palermo to Levanger in Norway, which will, however, probably be extended to the North Cape. The work before us is the report of the measurement of two base lines, and of their connection with the Norwegian triangulation which is to form part of the measurement of the above-mentioned arc. It was thought in 1862 that the existing Norwegian triangulation, supplemented and verified by some new work, would meet the requirements of the Association; but it was found, on investigation, that such was not the case, and moreover that the verifications could not be carried out, because the old trigonometrical stations could not be refound with any certainty. It was therefore decided to commence a new triangulation extending in a chain from the Swedish frontier (south of Christiana), where the chain is connected with the Swedish triangulation, to Levanger, where again a connection is to be made with another portion of the Swedish triangulation. The two base lines already mentioned are situated at the extremities of this chain of triangles, one at Egeberg, near Christiana, and the other at Rindenleret, near Levanger; both were measured during the summer of 1864, and Part I. is the report of these measurements.

The base measuring apparatus used is similar to that employed by Struve for the measurement of several base lines in Russia; it belongs to the Swedish Government, and was used for the measurement of their base lines. The apparatus consists of four cast-iron tubes, each approximately 2 toises[2] in length. One end of each tube is fitted with a small highly polished steel stud, and the other end with a "contact lever." The short arm of the contact lever terminates in a steel stud, which is intended to press against the fixed stud of the adjoining tube; the long arm moves on a scale. A measuring rod capable of varying its length to a slight extent is thus obtained, and this alteration in length can be measured with great delicacy, since the long arm of the lever greatly exaggerates it. This arrangement insures that the pressure between the rods is constant. Each tube is provided with two

[1] Publications of the Norwegian Committee of the European Association for the Measurement of Degrees. Geodetical Operations. Published in Three Parts. (Christiania, 1880 and 1882.)

[2] A toise is 2·13131116 yards as determined by Col. A. R. Clarke, C.B., R.E., F.R.S., &c.

thermometers, the bulbs of which are bent nearly at right angles to the stem, and are inserted into small holes in the tubes. In order to protect the tubes as far as possible from changes of temperature they are wrapped round with several thicknesses of cloth, and are further inclosed in a wooden box, out of which the two ends of the tube just project. During the measurement of a base line each rod is supported on two trestles, at one-fourth and three-fourths of its length, provided with screw arrangements giving slow motions laterally and in elevation. The rods are not, however, accurately levelled, and a correction has to be made for dislevelment. To measure the small angle of inclination each rod is fitted with a very sensitive level. One end of the level works on trunnions, the other is connected to a micrometer screw by means of which the level can be raised or lowered. The bed of the level is attached to the top of the box, but in such a manner that it can be adjusted truly parallel to the tube. The value of each micrometer division was determined by means of the meridian circle in the observatory at Christiana. It will be seen from the above that, as the measurement of a base line proceeds, the following readings are required for each rod: (1) the contact lever; (2) the thermometers; (3) the micrometer for inclination. These readings were taken and booked independently by two observers. Both base lines were measured twice, once in each direction.

Before and after the measurement of each base line each rod was compared with a *standard* rod, the exact length of which was known, namely:

$$= 1727{\cdot}96641\,(1 + 0{\cdot}000011476\,(t - 16^{\circ}{\cdot}25)) \pm 0.00058$$

expressed in Paris lines[1] based on Bessel's toise, t being expressed in degrees Centigrade. It was found that the rods were slightly diminished in length during the measurement of a base line (on an average 0·005 lines) owing to abrasion. An allowance was made for this diminution in length. The apparatus with which these comparisons were made consists of a massive cast-iron beam, turned up at both ends, and carrying two supports fitted with rollers upon which the rod to be measured rests. One end of this beam is fitted with a fixed steel stud, against which the contact lever of the rod under comparison bears; the other end carries a sliding scale, connected with a contact lever, and read by means of a micrometer microscope. A set of readings consisted in first measuring the standard rod, then each of the four measuring rods in succession, and lastly the standard rod again; the temperature of each rod was carefully noted. For a complete comparison twelve such sets of readings were taken.

The time occupied in measuring the Egeberg base was 18 days, and the observations for each measuring rod occupied 4 minutes; the Rindenleret base was measured more rapidly, namely, 2½ minutes per rod, due to the site being more level.

A considerable portion of Part I. is taken up in considering the errors to which the measurements of these base lines are liable, in estimating the allowances to be made to correct these errors, and in computing the probable errors of the final results. These errors are due: (1) to errors of observation in the actual measurement of the base lines; (2) to the error in the adopted length of the measuring rods.

Firstly, the errors to which the actual measurement of a base line is liable are as follows:—

A slight uncertainty attaches to the micrometer readings of the levels measuring the inclination of the rods. The probable error is computed to be

Egeberg base	...	...	± 0·350 lines
Rindenleret base	...	...	± 0·183 ,,

The errors due to the contact levers are next considered. It is shown that the error caused by the small uncertainty in the value of a degree of the scale over which the long arm of the lever moves, is too small to be taken into account, but the error caused by uncertainties in reading the scale is of sensible amount, and is computed to be

Egeberg base	...	...	± 0·015 lines
Rindenleret base	...	...	± 0·014 ,,

Further, the surface of the steel studs, at the end of the rods, is a portion of a sphere whose radius is considerably less than the length of a rod. Hence an error will occur each time a contact lever does not touch at the centre of the stud, that is if it makes an eccentric contact, and although every care was taken to obtain accurate contacts, it is considered that a correction of the following amounts should be made—

Egeberg base	...	...	− 0·351 ± 0·175 lines
Rindenleret base	...	...	− 0·314 ± 0·157 ,,

The next source of error is that due to errors in alignment, these errors will always be negative, and are due to the uncertainty in placing the rods in the line given by the directing theodolite. This error is computed to amount to

Egeberg base	...	...	− 0·294 ± 0·101 lines
Rindenleret base	...	...	− 0·262 ± 0·090 ,,

The computed variation of length of the rods due to alterations in temperature is vitiated by several errors. In the first place, the coefficient of expansion of the rods, as determined by Prof. Lindhagen, is affected by the small uncertainty, 0·000000015. Further, the correction for expansion is computed on the supposition that the thermometers do actually indicate the mean temperature of the rods at the time of taking the readings; but this is an assumption, and in fact it is estimated that the temperature indicated by the thermometers is the temperature the rod had 20·0 ± 5·9 minutes before taking the reading. This estimate is arrived at as follows: It will be remembered that each base line was measured twice; the difference between the two measurements is due to the various errors under consideration, and its probable value can therefore be computed; this computed value will contain, as an unknown, the time of which an estimate is required. Hence, by equating the computed difference to the actual difference the time can be found. The total error in the allowance made for expansion is found to be

Egeberg base	...	...	± 0·085 + 0·525 Λ
Rindenleret base	...	...	± 0·071 + 0·250 Λ

where $\Lambda = 20{\cdot}0 \pm 5{\cdot}9$ minutes.

Secondly, the errors due to the uncertainty in the accepted length of the rods are considered under four heads, namely: (1) the error in the length of the standard rod; (2) the error due to the bending of the beam of the comparing apparatus (some experiments were made to obtain data for the calculation of this error); (3) the error in comparing the rods with the standard; (4) the error due to the assumption that the diminution in length of the rods by abrasion is proportional to the length of time in use. The probable error of the accepted length of a rod during the measurement of the Egeberg base is computed to be ± 0·00081 lines, and during the measurement of the Rindenleret base ± 0·00071 lines.

Finally, the base lines had to be reduced to the sea-level; data had been obtained for this purpose by means of spirit-levelling operations. The reduction in the length of the base lines due to this cause is

Egeberg base	...	...	− 33·89 lines
Rindenleret base	...	...	− 0·852 ,,

Applying all these various corrections to the measured lengths of the base lines the final results are as follows:

[1] A Paris line is defined by 1 Paris line = $\frac{1}{864}$ toise, hence 1 Paris line = 0·088811 English inch.

Egeberg base ... ... 2025·28316 toises,

with a probable error of ± 0·00129, or $\frac{1}{1,570,000}$ of its length.

Rindenleret base ... ... 1806·3177 toises,

with a probable error of ± 0·00120, or $\frac{1}{1,500,000}$ of its length.

This is a high degree of accuracy as compared with older base lines (as for instance several base lines measured in France between 1798 and 1828, of which the probable errors are $\frac{1}{250,000}$); but this accuracy has frequently been attained of late years, and even surpassed, as, for instance, the base line of Madridejos, measured by General Ibañez in 1858, with a probable error of $\frac{1}{5,865,800}$.

Part II. is the account of the connection of the Egeberg base with the side Toass-Kolsaas, and Part III. that of the connection of the Rindenleret base with the side Stokvola-Haarskallen of the principal triangulation. The observations were made during 1864-66, but owing to an error at one of the stations, due to the bisection of a wrong object, further observations were made at that station in 1877. The connection in each case is very complete, and the work is well tied in. The centres of the trigonometrical stations were very carefully defined by letting an iron bolt into the rock, or, into a large block of stone; the centre of the face of this bolt, marked by a small hole, was the trigonometrical station. The signals, to which the observations were taken, consisted of an upright beam, to which was attached one or two boards about 0·75 m. square, which were painted white or black, and occasionally a vertical stripe 0·11 m. broad was painted on the centre of the board. At several of the stations the theodolite could be placed beneath the signal, and at such stations the signal was placed over the bolt, but in several cases, owing to the nature of the ground, or other causes, the trigonometrical station had to be placed at some distance from the signal, in one case as much as 54 Norwegian feet. In such cases the corrections to be applied to the observations were obtained by measuring a short base line, one end of which was the trigonometrical station, and the direction nearly at right angles to the line joining the station and the signal. Observations were taken from the ends of this base to the various points on the signal, which were bisected from the other stations, and these, together with the observed bearings to and from the other stations, enabled the necessary corrections to be made. The greatest correction thus required was 10′ 37″·34. But even at stations where the theodolite was placed beneath the signal, corrections were required to reduce the observations to the trigonometrical station, because different points on the signal were observed from the other stations, and these points were not vertically over the bolt. In these cases the corrections were computed in the following manner:—A piece of paper, mounted on a board, was placed horizontally on the ground over the centre of the station, and this centre marked on it. Then, by means of a small theodolite, the "traces" of the vertical planes passing through the various points observed to, were marked in pencil on the paper. The theodolite was now shifted, and the corresponding traces marked as before; the intersections of these traces gave a series of points vertically beneath the points on the signal to which observations had been made. From these points, the corresponding bearings to the various stations were plotted on the paper; and, lastly, perpendiculars were dropped, from the point representing the centre of the station, on to these bearings; the length of any one of these perpendiculars divided by the approximate distance to the corresponding station is the tangent of the correction to be applied.

Two instruments were used for measuring the angles; a 10″ universal instrument by Olsen, read by two micrometer microscopes, and a 12″ theodolite by Reichenback, read by four verniers. The errors of graduation of these instruments were investigated, and are given in a tabular form in Part II. Although, owing to the numerous observations taken to each object starting from different parts of the horizontal limb, the errors of graduation must have been eliminated to a very large extent, yet it was thought advisable to apply these corrections to the observations, in order to obtain a more accurate idea of the bearings of each station. The errors of the micrometer microscopes are also given in a table. The 10″ instrument was used at all, the 12″ theodolite appears to have only been used at two, stations. A third instrument, a 10″ universal instrument by Breithaupt and Sons, was used for the observations of 1877.

When observing, the instrument was first set at 0°, and a round of angles taken: the telescope was then reversed and the round taken again. The instrument was then set at 15° in the case of the triangulation connecting the Egeberg base, and at 20° (nearly) in the case of the Rindenleret base triangulation, and two rounds taken as before. The instrument was then again moved on 15° and 20° respectively, and so on. Thus in the first case forty-eight, and in the second thirty-six observations were taken to each station. In some few instances even a greater number were taken. The actual observations are not given in the Report, only the mean of four observations—two taken in the same position of the horizontal limb, and two in that position increased by 180°. The time occupied at each station averages four days; some stations were completed in two days.

The observations were compensated by the method enjoined by the Association for the measurement of degrees in Europe, namely, Bessel's method. The observed angles at each station are first compensated amongst themselves. A correction is then applied to each angle thus found, subject to the condition that the sum of the squares of these corrections for the whole triangulation is a minimum, and subject further to the geometrical conditions that the sum of the three angles of a triangle = 180 + spherical excess, and that the length of any side is the same by whatever *route* it is calculated. The necessary calculations are very laborious, and in the case of the Rindenleret base require the solution of simultaneous equations containing seventy-six unknowns. It is very questionable whether the result repays this labour; the method of compensation adopted for the Ordnance Survey, although perhaps not so rigid, compares favourably in this respect. The calculations for compensation are given very fully in the Report.

The Report is accompanied by plates showing the base measuring apparatus and the connecting triangulations.

ELEMENTS OF THE GREAT COMET OF 1882

(Communicated by Vice-Admiral Rowan, Superintendent U.S. Naval Observatory)

THE following elements were computed from three observations made at the U.S. Naval Observatory; the first and last being made with the Transit Circle, and the middle one compared with a known star which was afterwards observed on the Transit Circle:—

Wash. M.T.	App. α. h. m. s.	App. δ. ° ′ ″
Sept. 19·9697877 ...	11 14 18·94 ...	− 0 34 29·7
Oct. 8·7204363 ...	10 28 6·63 ...	− 10 40 22·6
Nov. 4·7009228 ...	9 6 16·22 ...	− 27 21 26·7

From these observations we deduce—

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"To the solid ground
Of Nature trusts the mind which builds for aye."—WORDSWORTH

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Perihelion Time = Sept. 17·2228200 Greenwich Mean Time.

$$\left.\begin{aligned} \Omega &= 346\ 1\ 7''\cdot91 \\ \varpi - \Omega &= 69\ 36\ 12\cdot79 \\ i &= 141\ 59\ 52\cdot16 \\ \phi &= 89\ 7\ 42\cdot70 \\ \log a &= 1\cdot9331366 \\ \log q &= 7\cdot8904739 \end{aligned}\right\} 1882\cdot0$$

period = 793·689 years

$\delta\lambda \cos\beta = -0''\cdot06 \quad \delta\beta = +0''\cdot01$

$x = r\,[9\cdot9951411] \sin(170\ 42\ 12''\cdot72 + v)$
$y = r\,[9\cdot9877234] \sin(262\ 46\ 57\cdot39 + v)$
$z = r\,[9\cdot4435130] \sin(\ 49\ 20\ 25\cdot11 + v)$

The observations as given were afterwards corrected for parallax by means of elements previously computed. These elements bear a considerable resemblance to Comet I., B.C. 371; and it may possibly be its third return, a very brilliant comet having been seen in full daylight A.D. 363.

E. FRISBY,
Washington, Dec. 19, 1882 Prof. Math., U.S.N.

THE DUMAS MEDAL

WE recently (vol. xxvii. p. 174) gave the addresses at the Paris Academy of Sciences in connection with the presentation to M. Dumas of a medal in commemoration of the fiftieth anniversary of his election to the Academy. We are now able, by the courtesy of our French contemporary, *La Nature*, to reproduce an illustration of this medal, which was presented by M. Jamin in words both eloquent and touching, as a token of the "love and gratitude" of the distinguished chemists' *confrères*, pupils, and friends. The medal is the work of M. Alphée Dubois.



PROFESSOR VON GRAFF'S MONOGRAPH ON THE TURBELLARIANS[1]

THIS splendid folio monograph consists of two volumes, the one comprising the text of over 600 pages illustrated by woodcuts, the other twenty as beautifully executed partially coloured plates as have ever been turned out, all from the author's own original drawings. The publication of the work has been assisted by a grant from the Berlin Royal Academy of Sciences.

Ludwig von Graff is Professor of Zoology at the College of Forestry at Aschaffenburg, in Bavaria. His first memoir on Turbellarians was published in 1873, at which time he first made up his mind to work out from his own observation a revision of the Turbellarians. The present monograph is, as he tells us in the preface, the result of almost incessant work during the last five years. He has made numerous journeys to the Naples and Triest stations, and has also visited many other parts of the European coasts north and south, and the fresh waters in all directions, in order to pursue his investigations on living Turbellarians. He has thus been able himself to examine 70 out of the 168 species of Rhabdocœlida which are known with certainty. The work being thus founded on so wide a personal acquaintance with the forms of which it deals, is of especial weight and value; it constitutes a systematic monograph of the Rhabdocœlida, founded on a sound basis of anatomical structure, and embracing all species hitherto described by other observers, together with those discovered by the author himself (thirty new species).

It is doubtful whether the present work will be followed by a second part embracing in a similar manner all the known Dendrocœlida. The matter depends on the amount of ground which may be covered by Dr. A. Lang's forthcoming monograph on Turbellarians, in the "Fauna and Flora of the Gulf of Naples." If this monograph proves to be so comprehensive that a further one would be superfluous, then Prof. Graff will publish a quantity of material collected by him concerning the Dendrocælida, in three smaller memoirs on the Polyclada, the Triclada, and embryology respectively. The present work is appropriately dedicated to the memory of O. F. Müller and Sir John Dalyell. It is pleasing to find the great merits of the latter thus recognised by a foreign naturalist.

The author does not admit Sidonia = *Rhodope varanii*, which, in opposition to Dr. R. Bergh, he considers to be a nudibranch, or Dinophilus, which has lately been shown to lie near the Archiannelids amongst the Turbellarians; and in the definition he gives of the group excepts the Microstomida, which differ from all other Turbellaria in having a complete pericœsophageal nerve ring, in being diœcious, and in multiplying asexually by budding.

Separating, as is now so usual, the Nemertines altogether from the Turbellarians, he divides the group into the Rhabdocælida and Dendrocælida. In the definition given of the two sub-orders, an interesting point of difference is brought out, namely, that in the former the yelk glands are always present in the form of a pair of compact glands, whereas in the latter they are always divided up into numerous separate follicles.

The Rhabdocælida are divided by the author into three groups: I. Acœla; II. Rhabdocœla; III. Alloiocœla, which are thus defined:—

[1] "Monographie der Turbellarien." 1. Rhabdocœlida. Dr. Ludwig von Graff. (Leipzig: W. Engelmann, 1882.)

1. Accela. With digestive internal substance; without differentiation of a digestive tract and parenchym tissue. Without nervous system or excretory organs. All forms as yet known provided with an otolith.

2. Rhabdocœla. Digestive tract and parenchym tissue differentiated; a roomy body cavity usually present in which the regularly-shaped intestine is suspended by a small amount of parenchym tissue. With nervous system and excretory organ. Generative organs hermaphrodite (except in Microstoma and Stenostoma). Testes, as a rule, two compact glands. The female glands present as ovaries only, ovario-vitelligenous glands, or separate ovaries and yelk glands. Genital glands separated from the body parenchym by a special tunica propria. Pharynx always present, and very variously constructed. Otolith absent in most cases.

3. Alloiocœla. Digestive tract and parenchym tissue differentiated, but the body cavity much reduced by the abundant development of the latter. With nerve system and excretory organ. Generative organs hermaphrodite, with follicular testes and paired female glands, either ovaries only, or ovario-vitelligenous glands, or separate ovaries and yelk glands. Yelk glands irregularly lobular, rarely partially branched. Genital glands almost always without any tunica propria, lodged in the spaces in the body parenchym. Penis very uniform, and either without chilinous copulatory organs, or with these very little developed. Pharynx a pharynx variabilis or plicatus. Digestive tract lobular, or irregularly broadened out. All marine except one, or possibly two species.

Under the Alloiocœla come the genera—Plagiostoma, Vorticeros, Monotus, and others.

The work commences with a complete list of the literature on Turbellarians from the time of Trembley, who, in 1744, figured a black fresh-water Planarian to that of the publication of the last of Dr. Arnold Lang's important memoirs last year. The list is followed by a general treatise on the anatomy and physiology of the Rhabdocælida. The account of the nematocysts of some forms is very interesting; their exact resemblance to those of Cœlenterata is fully borne out. *Microstonum lineare* appears to be the only species which, like Hydra and Cordylophora, possesses two kinds of nematocysts. The author thinks he has been able to detect on the surface of the cuticle, trigger hairs in connection with the nematocysts, like those in Hydroids. He considers the rhabdites or rod-bodies homologous with nematocysts, and refers, in connection with this question, to the nematocysts devoid of any thread which occur in many Cœlenterates, intermingled with fully developed ones. The structure of the pharynx is carefully gone into, and its different forms being of much use in classification, receive various names, such as *Pharynx bulbosus*, *P. plicatilis*, &c.

The water vascular system has been studied by von Graff with considerable success. It may consist of a single median main canal with a single posterior opening (Stenostoma) or a pair of laterally-placed canals with a similar single opening or two separate lateral canals with each a posterior opening (Derostoma), or there may be a pair of openings or a single one somewhat anteriorly placed. Ciliated funnel cells or flame cells such as exist in Cestodes, Trematodes, and Triclad Dendrocœles, have been discovered by von Graff also in the Rhabdocœlida. They do not, however, occur in connection with the tips of the ramifications of the water vascular canals, but almost entirely on the larger canals forming the networks. It is impossible here to follow the work further, through the interesting sections devoted to the development of Microstoma by budding, the habits of life and geographical distribution of the Rhabdocælida. In connection with the discussion on classification, a table of the pedigree of Turbellaria is given, with Proporus as the ancestral starting-point. In this family tree the Dendrocœles are shown as derived from Acmostoma, a new genus of Alloiocœla, characterised by having a distinctly marked narrow ambulacral sole, the Polyclada directly, and the Triclada through Plagiostoma. The ascertained facts as to the structure of Turbellarians seem to point even more closely to their connection with the Cœlenterata. The presence of two kinds of nematocysts in one of the Rhabdocœla and possible occurrence in members of that group of trigger hairs, is a remarkable fact. Dr. Lang, believing that a part of the nervous system in Dendrocœles is truly mesenchymatous as in Ctenophora, and from other grounds concludes with Kowalewsky that the Polyclada are "creeping Cœlenterates which have many points of structure in common with the Ctenophora, some with the Medusæ. Such being the case, naturalists await with great impatience Kowalewsky's promised further information as to his extraordinary Cœloplana, supposed intermediate between Ctenophora and Dendrocœlida. The peculiar azygos character of the otolith in so many Dendrocœlida may perhaps be explained by the similar condition of the sense organ in Cœloplana. Prof. von Graff is much to be congratulated on the completion of this most important and admirable work.

H. N. MOSELEY

NOTES

WE greatly regret to announce the death of Mr. Charles V. Walker, F.R.S., at his residence at Tunbridge Wells, on the morning of December 24, 1882, in the seventy-first year of his age. Mr. Walker had been Telegraph Engineer to the South-Eastern Railway, since 1845. He had been a most zealous worker in the science of electricity, as the many works he leaves behind will testify. Indeed, he was one of the oldest telegraph engineers in the country, was the inventor of several useful appliances in connection with telegraphy, including the instruments by which the block system on railways is worked. His name is especially associated with the origin of the distribution of time by telegraph. On May 10, 1849, Mr. Glaisher wrote to Mr. Walker that he wished to talk with the latter about the laying down of a wire from the Observatory to the Lewisham Station, and on May 23 following, the Astronomer-Royal gave Mr. Walker a brief sketch of the use to be made of the wire referred to, his scheme, as he stated, being "the transmission of time by galvanic signal to every part of the kingdom in which there is a galvanic telegraph from London." It was proposed to lay four wires underground from the Royal Observatory to the railway station at Lewisham, and to extend them to London Bridge. The South-Eastern Railway Company gave every facility. On September 16, 1852, an electric clock at London Bridge Station was erected, and connected by wire with an electric clock at the Royal Observatory, Greenwich. The first time-signal sent from the Royal Observatory was received at London Bridge Station at 4 p.m. on August 5, 1852; and on August 9, 1852, Dover received a time-signal for the first time from the Royal Observatory direct, and it was made visible at certain first-class stations between London and Dover. After that the system rapidly spread, its success depending greatly on the scientific skill and enthusiasm of Mr. Walker. For some account of the subsequent development of the system, the reader may refer to the articles in NATURE, vol. xiv. pp. 50 and 110. Mr. Walker was treasurer of the Royal Astronomical Club for several years, and at the time of his death was president of the Society of Telegraph Engineers.

THE death is announced of Prof. Listing of Königsberg.

THE honour of Companion of the order of the Indian Empire has been conferred upon Surgeon-Major George Bidie, Superintendent of the Central Museum at Madras.

AT the last sitting of the year 1882, the Paris Academy of Sciences elected M. Bunsen, of Heidelberg, a Foreign Associate. M. Bunsen was already a Correspondent in the Section of Chemistry, and he will fill the place vacated by the death of M. Wöhler. It should be remembered that, contrary to the rule for members, who must be French citizens, and Associates, who must be foreigners, the correspondents of the Academy can be elected without any qualification of nationality; but none of them, either French subjects or foreigners, may live in Paris previous to their nomination. This rule is so strict that it is stated that an eminent man of science, wishing to become a candidate, removed his home from Paris to Versailles; and having been successful, returned to Paris, where he now lives.

THE Duc d'Aumale has been elected President of the Académie Française. M. Blanchard, the naturalist, will be President of the Academy of Sciences for 1883; he was vice-president during the past year. The vice-president for 1883, and future president for 1884 would be elected on Tuesday from the Mathematical Section. Before leaving the chair M. Jamin will, according to precedent, read a list of losses experienced by the Academy in 1882, and of the nominations made during the same period; he will also give a *résumé* of the progress of the several publications of the Academy.

ON Tuesday January 2, there was a gathering of people interested in educational progress, at No. 1, Byng Place, Gordon Square, to inspect the College Hall of Residence for Women Students which has lately been established there. Complete as this hall is in itself, we understand that it is only provisional until sufficient approval and support have been obtained to justify the opening of a building capable of accommodating a larger number of ladies. We may, however, regard it as embodying the idea of its founders, and as supplying in miniature a model of that comfortable and well-adapted academic residence which it is their object to provide for female undergraduates and art-students in London. The advantages to the members of this rapidly-increasing class of entering such a hall instead of taking separate lodgings or rooms in a boarding-house, or even living at home (in many cases) are not far to seek. Students in lodgings often suffer from neglect of health and under-feeding, while those who work at home are subject to interruptions and the strain of conflicting claims; and although they might avoid both these drawbacks in a good boarding house, they would still find that their residence was not adapted to the needs of student life. Whichever plan is adopted, girls generally lack opportunities of free intercourse with minds whose training has been about equal to their own, such as of late years they have been able to obtain at Oxford and Cambridge, and which is specially needed in London, the seat of the only English University that as yet admits them formally to degrees. Hence the three greatest benefits of the new hall will be: first, to bring the women students of London into social and intellectual fellowship, and thus to improve the quality of their work by encouraging conference on the subjects of study, without which it is hardly possible to acquire and test accuracy of thought; secondly, to diminish the causes of failure of health by care and good housekeeping; and thirdly, to increase the time at the disposal of students; thus, on the one hand, affording to the zealous worker opportunities of relaxation, which in different surroundings would be absorbed in housekeeping worries or other occupations, and, on the other hand, enabling the less enthusiastic to add to the quantity of their acquirements without increase of conscious effort. The Hall has been established chiefly in the interests of the students of University College (including the Slade School), but its usefulness is much enhanced by proximity to the London School of Medicine for Women, and the British Museum; for on this account we may fairly hope that it will contain numbers of students in the various departments of Literature, Science, and Medicine, and the Fine Arts. Liberality of thought and breadth of sympathy can hardly fail to be promoted, where subjects of interest are so varied amongst companions united by the common principle of serious study. Although the Hall was only opened last term, we notice with pleasure that all the rooms are already taken; hence there is reasonable ground for hope that the larger scheme of the Committee will before long be realised. That the interests of students of science will be well looked after may be gathered from the fact that the presidents of the Royal Society and of the British Association, Prof. Huxley, Dr. Gladstone, Mr. Samuelson, M.P., Prof. Carey Foster, and others are aiding the scheme. Sir John Lubbock is the treasurer of the Building Fund.

DR. VON HOCHSTETTER, for many years president of the Vienna Geographical Society has resigned this post and has been nominated honorary president for life. In his stead Count Hans Wilczek was elected president.

THE death is announced of Karl Winter, the well-known electrician. He died at Vienna on December 7 last.

PROF. W. GRYLLS ADAMS, F.R.S., will deliver a course of lectures on voltaic and dynamic electricity and magnetism, and their applications to cable-testing, electric lighting, &c., at King's College, London, during the ensuing session. A course of practical work in electrical testing and measurement with especial reference to electrical engineering will also be carried on under his direction in the Wheatstone Laboratory. The lectures will be given once a week on Mondays at 2 p.m., and the laboratory will be open daily (Saturday excepted) from 1 to 4. The work will begin on Monday, January 15.

THE French Senate has diminished by a million of francs (40,000*l.*), the Budget of Public Instruction for 1883. It is regarded as a warning given to the Lower House, not to spend with too free a hand the public funds for educational purposes.

THE continual rains are creating serious apprehensions in Paris, and the Seine has again reached the level of disastrous inundations. A similar calamity is befalling other cities in France, amongst which the foremost is Lyons. The calamity having been foreseen by the Hydrological Service, all measures have been taken to diminish as much as possible the extent of the disaster. *Akhbar* states that heavy rains have been experienced in Algiers, and even at Laghouat, where it has been received with a real exultation. The newspapers are full of the disastrous floods caused by the rise of nearly all the great rivers in the Central European plain.

WITH reference to a recent note to the effect that snow fell on November 11 in Madrid to the depth of 1 foot, Mr. Gillman writes that snow began to descend early that morning, but had ceased at midday. He nowhere found it deeper than 6 inches, but this was uniform in the streets and open country. In the night of 11th-12th, the minimum thermometer marked -11° cent.; barometer on Sunday stood at 688 millims.

THE appearance within the last two years of two comets has been regarded as a most menacing portent by Chinese politicians. Their resemblance to flaming swords is regarded as emblematical of the vengeance of heaven on an unworthy nation. It is stated that in consequence of the last comet, an urgent decree has been promulgated in the name of the youthful monarch, stating that it is a clear indication that the officials are lax in making proper reports to the Throne, and have been keeping the Emperor in the dark as to pestilences and other calamities among the people. His Majesty has reason to believe that improper officials have been appointed; he has, moreover, subjected his Imperial hear

to a rigorous examination in the seclusion of his palace, and he is much disquieted at the result. The people, he finds, are poverty-stricken, and await relief, and the present is a time of great anxiety and embarrassment. The crisis must be met with prompt measures and a reverent heart; the ministers are accordingly enjoined to exhibit loyalty and justice, and to strenuously guard themselves against the thraldom of official routine. They are to discover the real state of the country, and to make such dispositions as may give rise to all possible advantage, and eradicate all possible evil. If all this be done, we have the Imperial assurance that the people will live in peace and quietness, till heaven be in harmony with earth, and all harmful influences allayed. If decrees were always obeyed, the comet will have exercised a beneficent influence on the condition of the Chinese people.

ALL interested in photography will find much that is useful and curious in Mr. Baden Pritchard's Year-Book of Photography for 1883.

MR. E. ROBERTS has sent us his handy and useful Tide Table for 1883, containing the times of high water at London Bridge, and showing the possible overflows; to all Londoners interested in any way in their river, this table will prove serviceable. We have also from Mr. Roberts Tide Tables for the Indian ports, and Tide Tables for the port of Hongkong, in handy little volumes, containing many carefully compiled tables calculated to be of great service.

THE total number of visitors to the Royal Gardens, Kew, for the year 1882, was 1,244,167. This is 407,491 in excess of the numbers for 1881, which in its turn was greater by 111,254 than the number of visitors in any previous year. As in 1881 the Sunday visitors (606,935) were about equal in number to those on all the other days of the week put together (637,232).

A NEW natural history magazine in the Flemish language is announced. It is published at Ghent, and the title is *Natura Maandschrift voor Natuurwetenschaffen uitgegeven door het Natuurwetenschappelijk Genootschap von Ghent*. The editors are J. MacLeod, Ed. Remonchamps, and L. Baeklandt. The natural sequence is that another Belgian magazine, in Wallon, will appear. The "gift of tongues" is daily becoming more and more a necessity for a working naturalist, and De Candolle's assertion that English is destined to become the language of science seems gradually more remote in realisation.

THE December number of the *Agricultural Students' Gazette*, Royal Agricultural College, Cirencester, contains an article by Sir J. B. Lawes on the future of agricultural field experiments, in which he points out that the time when isolated field experiments were of value has passed, and that now the questions to be solved in this way are such as can only be answered by carefully conducted experiments lasting over many years. Miss Ormerod contributes a paper on the Gooseberry Caterpillar, the larva of *Nematus Ribesii*, in which she suggests the best mode of preventing its ravages. A readable summary of the recent work of Leuckart and Thomas on the life-history of the Fluke is given by Mr. Ozame. The other papers in the number are on Contagious Diseases, by Prof. Garside; on the Harvest of 1882, by Prof. Little; on Butter-making, by Mr. Weber; besides much matter of more purely College interest. We notice that the College has commenced a series of field experiments on corn crops, in conducting which doubtless the advice of Sir J. B. Lawes will be followed. This *Gazette* in its new form promises to become of permanent value, and is exceedingly creditable to its editors, students of the Royal Agricultural College.

THE third expedition fitted out by the Milan Society for the commercial exploration of Africa, will leave early this month for Massana. The leader of the expedition is Signor Bianchi, who knows Abyssinia thoroughly. Count Salimboni accompanies him as engineer, and Prof. Licata as naturalist.

PROF. DOMENICO LOVISATO and Lieut. Bove, who jointly undertook the last Italian Antarctic expedition, are about to undertake another Antarctic journey for scientific purposes.

NEWS has been received from the German traveller, Robert Flegel, who was sent out to explore the Niger-Binue district. It appears that on April 10 last the traveller crossed the Binue River to the southern shore, and reached the large town of Wukari on April 13. By way of Bantadchi he proceeded, in four days' journey, to the decaying government city of Bakundi, in one and a half days more to Beli, and thence he reached Kontcha in the Adamnua district on May 26. From Kontcha to Jola is only a seven days' route. Flegel, whose health has much improved, strongly advises the establishment of a German station in that healthy and fertile country.

WE have on our table the following books:—Sydney Observatory, Double-Star Results, 1871-81 (Sydney); Der Electricität und der Magnetism, vol. i., Clerk Maxwell (Springer, Berlin); Cutting Tools, R. H. Smith (Cassell, Petter, and Galpin); A New Theory of Nature, D. Dewar (W. Reeves); Transactions of the Sanitary Institute, vol. iii. (Stanford); The Great Pyramid, R. A. Proctor (Chatto and Windus); Microbes in Fermentation, Putrefaction, and Disease, Ch. Cameron (Baillière, Tindall, and Co.); The Nebulæ, a Fragment of Astronomical History, A. E. Garrod (Parker); Relative Mortality of Large and Small Hospitals, H. C. Burdett (Churchills), To the Gold Coast for Gold, Burton and Cameron (Chatto and Windus); Physical Optics, R. T. Glazebrook (Longman); Essays in Philosophical Criticism, Seth and Haldane (Longmans); Year-Book of Photography, 1883, H. B. Pritchard (Piper and Carter); Report on the Oban Pennatulida (A. M. Marshall and W. P. Marshall); Catalogue of *Batrachia gradientia*, G. A. Boulenger (British Museum); The Brewer, Distiller, and Wine Manufacturer (Churchills); The Churchman's Almanak for Eight Centuries, W. A. Whitworth (Wells, Gardner, and Co.); Celtic Britain, Prof. J. Rhys (S.P.C.K.); Zoological Record, vol. xviii. 1881 (Van Voorst); Rankine's Useful Rules and Tables, sixth edition (Griffin); Madeira Spectroscopic, C. Piazzi Smyth (W. and A. K. Johnston); Ragnarok, the Age of Fire and Gravel, Ig. Donnelly (Sampson Low and Co.); The Electric Lighting Act, 1882, Clement Higgins and E. W. W. Edwards (W. Clowes).

THE additions to the Zoological Society's Gardens during the past week include a Black-eared Marmoset (*Hapale penicillata* ♂) from South-East Brazil, presented by Miss Tilleard; a Grey Ichneumon (*Herpestes griseus*) from India, presented by Mr. W. L. Brodie; a Rose Hill Parrakeet (*Platycercus eximius*) from Australia, presented by Mr. Geo. Lawson, F.Z.S.; a Black Tortoise (*Testudo carbonaria*) from St. Thomas', West Indies, presented by Viscount Tarbat, F.Z.S.; an Indian Cobra (*Naia tripudians*) from India, presented by Capt. Braddick; two Common Curlews (*Numenius arquata*), a Common Lapwing (*Vanellus cristatus*), a Golden Plover (*Charadrius pluvialis*), British, purchased.

BIOLOGICAL NOTES

ON A NEW GENUS OF CRYPTOPHYCEÆ.—It would appear that the interesting fresh-water genus of Algæ described by Bornet and Grunow as Mazæa (*vide* NATURE, vol. xxvi. p. 557) is without doubt the same as Nostochopsis of Wood. This genus of Wood was first briefly described in the *Proc. Amer. Philos. Soc.*, 1869, and more fully, and with good figures, in the "Fresh-water Algæ of the United States," 1872. The Philadelphia species, *N. lobatus*, Wood, is referred by its discoverer to the Rivulaceæ, and is apparently a different species from that described by Bornet and Grunow from Brazil.

FEMALE FLOWERS IN CONIFERÆ.—Quite recently Celakovsky has published a very elaborate criticism (on the structure of the female flowers in Coniferæ, as detailed in Eichler's well-known treatise). To this ("Zur Kritik der Ansichten von der Fruchtschuppe der Abietineen," &c. Prag, 1882), Eichler has replied in a paper read before the Gesellschaft der Nat. Freunde zu Berlin, in which he re-states the chief points of his proof and answers *seriatim* the objections brought against it. Dr. Peters sums these up as follows:—1. In all the vegetative buds of the pine, the two front leaves (Vorblätter) converge forwards towards the bract; it is hence improbable that in the fruit scale they should be turned backwards. Celakovsky, from the fact that in weak buds the former arrangement is somewhat modified, concludes that on the complete falling away of the bud from between the front leaves, these latter are enabled to push themselves backwards and cohere: an opinion not proved. 2. While in the vegetative bud, the leaf immediately following the front leaf falls backwards in abnormal fruit-scales, the portion interpreted as the next leaf falls forwards. To the representation of Celakovsky's, that owing to the fact that the front side being, in the course of development, preferentially assisted, the leaf of the assisted front side first reaches its development, Eichler opposes the statement that in the ordinary buds there is not a trace of such a preferential furtherance. 3. The part that is regarded as the third leaf of the bud cannot be a leaf, because it has its xylem on the dorsal, and its phloëm on its ventral surface. Celakovsky takes a twist of 180° for granted. This Eichler denounces as an evasion which would bring all serious scientific discussion to an end. 4. If the fruit-scale were formed by the growing together of two front leaves upon the hinder end of their axis, the latter if it developed further, would come to stand on the front side of the fruit-scale, but *de facto* it under such conditions stands behind. As Celakovsky however thinks that the middle piece of the front scale is half turned round, and is a leaf on the front side of the bud, to which both front leaves on the front side of the bud have adhered, by which means the axis comes to be posterior: therefore this opinion stands irreconcilably contradicted by his own supposition of the simultaneous pushing back of the front leaves. 5. The simplest explanation of the bud-arrangement, and of the bud itself, is got by supposing that the bract and the fruit-scale form together a single leaf which has produced an axillary bud. Here Eichler considers himself compelled to deny the charge of having set out with pre-formed notions. The change in his former opinions was brought about by a more intimate knowledge of the facts. 6. By pressure and excitation (Reiz) the axillary bud causes further changes in the fruit-scale, the formation of the keel and wings, while the central piece which is bounded by them, can separate itself from the side portions and assume the appearance of a special leaf. To Celakovsky's objection, that through the pressure of the bud-axis, only a circumscribed depression, and not a long furrow would be formed, there is this reply, that such a furrow must be produced by the growth of the scale past the early developed bud, and that this furrow can become wider as the scale becomes broader. 7. These keels (midribs) of the fruit-scale press past the bud on both sides, and hinder the development of the first lateral bud-leaves, so that the first bud-leaf now arises upon the hinder side. This explanation, characterised by Celakovsky as a forced hypothesis, is supported by the fact that the leaves could not become formed in a place where there is no room, and because on the other hand the two lateral bud-leaves show themselves if the mid-ribs are wanting or remain feebly developed (*Botan. Zeitung*, December 8).

THE TRACHEÆ IN LAMPYRIDÆ.—Heinrich Ritter v. Wielowiejski publishes in the November number of the *Zeitschrift für wissenschaftliche Zoologie* a very detailed account of the light-producing organs in *Lampyris splendidula* and *L. noctiluca*. His investigations were carried on at Jena, in Prof. Oscar Hertwig's laboratory. He sums up the most important results as follows:—1. The tracheal-terminal-cells of M. Schultze, which become black under osmic acid, are by no means—as their name would imply—the terminations of the respiratory tubes; for these branch out further on into brush-like masses of much finer capillaries, which are without the chitine spiral; they are very attenuated, and, making their way in the peritoneal layer (peritonealhaut), are numerously distributed to phosphorescent tissue. 2. The tracheal capillaries very rarely end abruptly (blind) in the phosphorescent organs, but most frequently anastomose with one another, forming an irregular meshwork. 3. The capillaries do not seem to enter into the structure of the parenchymatous cells, but rather course along their surface, often irregularly winding around and enveloping these. 4. The tracheal-terminal-cells are nothing more than the outer elements of the peritoneal layer at the base of the tracheal capillaries, which radiate in a brush-like fashion from a chitine-spiral-trachea. Their peripheral processes represent the extension of the latter upon the capillaries, and this relationship is homologous with certain embryonic stages of the tracheal system. 5. The tracheal-terminal-cells are not the seat or point of departure of the light-development. If this appears first in their vicinity, it is only a consequence of the fact that these structures have, owing to their affinity for oxygen, stored up in themselves a supply of this gas, and give it off in greater quantity to the neighbouring tissues. 6. The light-producing function is peculiar to the parenchyma-cells of the light-producing organs. It results from a slow oxidation of a substance formed by them under the control of the nervous system. 7. The ventral light-organ was found to consist of two layers, the parenchyma-cells of which are quite similar to one another in their morphological characters, but they differ from one another in the chemical nature of their contents. 8. The parenchyma-cells (is this the case with all?) seem connected with fine nerve-endings. 9. The light-organs are the morphological equivalents of the fatty-bodies.

THE STONES OF SAREPTA (ASIATIC RUSSIA).—The remarkable masses of stone found in the white sand of the Ergent Mountains at Sarepta have often caused people to inquire how they were formed. Some of them are found of the size of a hazel or walnut, and even larger; others are cylindrical, of the thickness of a half to one werschok (16 werschok = 28 inches), and a quarter to a half arschin (28 inches) long; others again target-shaped are more than a half arschin long, and one to four werschok thick. All the cylindrical ones, which are often also forked and root-shaped, exhibit, when they are broken across, a brown kernel with a white spot in its centre. Their surface is rough, and resembles a number of drops heaped one upon and beside another. When Alexander v. Humboldt visited Sarepta, the then director, Zwick, showed him these stone formations, Humboldt, while declaring that they were worthless recent things, was unable to say how they arose. Zwick, on the other hand, regarded them as very old and very problematical. Göbel also, who was afterwards shown these stones by Zwick, was unable to explain how they were formed. When Auerbach, the secretary of the Moscow Natural History Society, paid Alex Becker a visit twenty-eight years ago, he was brought to the place where these stone deposits were. He looked for an explanation of the formation of these stones and the reason of each stone containing a brown kernel. He was told that the stones were formed by roots. Auerbach said that these would form hydrochloric acid by decomposition. Becker now believes that he can with certainty assert that these formations arise round the roots of several plants that contain *milky* juice. *Tragopon ruthenicus*, *Scorzonera ensifolia*, and *Euphorbia gerardiana* grow plentifully in the white sand. Their long roots are inhabited and seamed by insects, and when their surface is once lacerated, their milky juice keeps perpetually flowing, and as it is sticky, the chalk-containing sand (the sand's colour is due only to the presence of chalk) settles firmly around the root. The root gradually dies, disappears, and there remains in its place a white, often hollow, kernel, together with the brown colour of the root-cortex. As the root is white under its cortex, the kernel also appears white, surrounded with the brown layer of the root-cortex. The round and target-shaped ones may originate from the milky juice running away into the sand, and therefore hardly any of them exhibit a brown kernel. Their guttiform surface can be explained by the drops of the milky juice. The cylindrical, forked, and root-shaped stones show clearly the form of the roots. *Euphorbia gerardiana*, to which these stone formations are chiefly ascribed, has very long roots, root-branches and root-fibres (Alec Becker, *Bull. de la Soc. Imp. des Natur. de Moscou*, 1882, No. i. p. 48).

AMERICAN RESEARCHES ON WATER ANALYSIS[1]

THE chemical results as to animal in contrast with vegetable organic matter in water, support, in general, the conclusions that have been usually drawn as to the source of organic matter, based on the more highly nitrogenous character of that from animal than that from vegetable *débris*. Still the necessity

[1] Concluded from p. 213.

for caution is shown; *e.g.* samples containing the refuse of canning tomatoes might have been erroneously thought contaminated with animal matter; others, containing a watery infusion of human fæces, with vegetable matter, &c.

Of the biological results under the same head, the most noteworthy is the well-marked pathological effect on rabbits of the injection of waters contaminated solely by such vegetable matter as would usually be thought harmless, *e.g.* peaty water. True, in the well-marked cases, the amount of organic matter present was large, but not beyond that in water sometimes used for drinking purposes. The Dismal Swamp water is an example; it has often been chosen for ship-supply, and has been spoken of as a source of supply for the city of Norfolk. On the theory (which has much in its favour) of disease caused by drinking water being due to the presence and action of living organisms, there might possibly be safety in drinking a peaty water, or water filtered through dead forest leaves, when fresh; danger, when, after some exposure, bacteria had been developed; and safety, again, perhaps, after the growth of these had fallen off, and more or less of the available organic matter had been consumed. Ship-captains say the Dismal Swamp water, after a time, becomes remarkably good and wholesome.

As to the putrescent or non-putrescent character of organic matter in water, the chemical evidence goes to prove (in opposition to Tidy's opinion) that the proportionate consumption of oxygen from permanganate within the first hour is rather greater for those waters containing vegetable than for those containing animal matter. Dr. Smart has expressed the opinion that gradual evolution of albuminoid ammonia indicates the presence of organic matter (vegetable or animal) in a fresh or comparatively fresh condition, while rapid evolution indicates that it is putrescent. His interpretations in this respect proved to be correct in a large proportion of cases, but not always.

The biological results under this head accord, on the whole, with the general belief that putrescent organic matter is more dangerous than that in a fresh or but slowly decomposing condition.

Prof. Mallet proceeds to state some *general conclusions with a view to sanitary application as to the value, separately and collectively, of the different processes of water analysis which have been under examination.*

It is not possible to decide absolutely on the wholesomeness or unwholesomeness of a drinking water by the mere use of any of the processes examined for the estimation of *organic matter*, or its constituents. Not only must such processes be used in connection with investigation of other more general evidence, as to the source and history of a water, but this should even be deemed of secondary importance in weighing the reasons for accepting or rejecting a water not manifestly unfit for drinking on other grounds.

There are no sound grounds on which to establish such general "standards of purity" as have been proposed, looking to exact amounts of organic carbon or nitrogen, "albuminoid ammonia," oxygen of permanganate consumed, &c., as permissible or not.

Chemical examination may be quite legitimately applied, first, to the detection of *very gross pollution* (as of a well from crushing of soil pipes), and secondly, to periodical examination of a water supply, so that suspicious changes from the ascertained normal character of the water may be promptly determined and their cause investigated. In the latter connexion there seems to be no objection to the establishment of *local* "standards of purity," based on thorough examination of the supply in its normal condition.

A careful determination of the nitrites and nitrates seems very important.

If he had to watch a large city water supply, the author would use all the three processes; each gives information which the others do not. Where only simple means were practicable, the albuminoid ammonia and permanganate processes might be employed together; but in no case should one only of these methods be resorted to.

Practical Suggestions as to the Use in their present form of the Chemical Processes Studied.—In general, water samples should be examined with the least possible delay after collection. Besides examination of a water in its fresh condition, samples of it should be set aside in half-filled but closed glass-stoppered bottles for (say) ten or twelve days, and one of these examined every day or two, to trace changes undergone.

In the case of the combustion process, however skilful the analyst, duplicate or even triplicate concordant results should be insisted on. To avoid the presence of ammonia from coal gas, in the atmosphere about the water-bath, the bath should be heated by steam brought in a small closed pipe from a distant boiler (preferably in another room), and the waste steam and condensed water should be carried off to a safe distance.

As to the albuminoid ammonia process, it would be well to adopt the rule that the distillation be stopped when, and not before, the last measure of distillate collected contains less than a *certain proportion*, say 1 per cent., of the whole quantity of ammonia already collected. To diminish the loss of amines or other volatile forms of nitrogenous matter, a separate distillation should be made with alkaline permanganate added *at once*, besides the usual course of treatment prescribed by Wanklyn, and the results of the two distillations compared. The details of the evolution of ammonia should always be given.

The Tidy form of the permanganate process is rather to be recommended than that of Kubel, if but one be used. The time during which the permanganate is allowed to act in the Tidy process should be increased to at least 12, better to 24 hours, *several* determinations, on different samples set aside at the same time, being made at (say) 1, 3, 6, 9, and 12 hours, to trace the progress of the oxidation.

Suggestions as to possible Improvements on the Processes examined deserving further Investigation—Combustion Process.—The author proposes to evaporate the water in a closed vessel immersed in a water bath, and connected with a good (water jet) air pump, a condensing worm being provided for the aqueous vapour, the feed to be managed through a nearly capillary tube with a glass stop-cock. The evaporation would thus be effected within a moderate time at a fixed temperature much lower than the boiling point. The loss of organic matter by simple volatilisation or oxidation would be greatly reduced; much less sulphurous acid would be required; the tendency to formation of sulphuric acid would be reduced to a minimum, and absorption of ammonia from the atmosphere about the dish quite prevented. In testing this last effect, two bulb tubes containing pure sulphuric acid might be interposed between the vacuum chamber and the pump.

For certain reasons it might be well to evaporate at first with the addition of a small excess of magnesia (as recommended by Lechartier), thus removing all ammonia, and then, the water having been brought down to a small volume, add a moderate excess only of sulphurous acid with a drop of a solution of ferrous salt (as directed by Frankland), and complete the evaporation to dryness—the whole in a jet pump vacuum, as suggested.

Further experiments as to the Williams method (copper-zinc couple) for removal of nitrates, are desirable.

From preliminary experiments, the author thinks nitrates and nitrites may be completely reduced by evaporating to a small bulk with no great excess of phosphorous or hypo-phosphorous acid, guarding against evolution of phosphuretted hydrogen by use of a low temperature, then adding magnesia in small excess, and completing the evaporation. The plan deserves to be carefully tested.

Albuminoid-ammonia Process, including Determination of free Ammonia.—To prevent (or at least largely reduce and make uniform) the loss of ammonia from imperfect condensation, the author would use a retort in a saline solution kept heated by steam (at say 102° or 105° C.), and condense in a glass worm surrounded by ice water, till the distillate should be brought to a uniform temperature not over (say) 5° C. It might be still better to distil in a completely closed apparatus, with a fixed difference of temperature between the retort and the far end of the condensing tube, with glass stopcock to draw off the distillate in successive measured portions, and a small safety-valve near the cold end.

In determination of free ammonia, it might be well to try a closed distilling apparatus connected with a (water-jet) air-pump, so as to maintain a partial vacuum within, keeping the retort at a fixed temperature much below 100° C., and collecting all the ammonia in a flask and one or two bulb tubes, with weak mineral acid placed between condenser and pump. There would be the disadvantage, however, that the progress of evolution of ammonia could not be easily traced by its collection in separate successive measures of the distillate; and it would be necessary to ascertain whether the application of the Nessler test would be at all interfered with by the sodium salts formed from the acid used.

To overcome, if possible, the most serious difficulty in the

way of correct determination of free ammonia, viz. the ready breaking up of urea (and other amides) when present, on heating with sodium carbonate, it would be well to ascertain if Schlœsing's method for determination of ammonia admits of being applied to such excessively minute amounts of it as the water analyst is concerned with.

In the conduction of the albuminoid-ammonia process proper, *i.e.* the distillation with alkaline permanganate, the author would keep the original volume of liquid in the retort constant, by admitting ammonia-free distilled water through a capillary tube, with a glass stop-cock. When there is so much organic matter as to reduce, wholly or greatly, the usual charge of alkaline permanganate, he would first determine at about what a rate the reagent is used up, then progressively supply its solution, so as to keep the original strength as nearly as possible unaltered.

Permanganate Process.—The principle involved in the last paragraph applies also to this process. There should be a constant excess of permanganate all through the process. The process should be carried on at a pretty nearly fixed temperature (say 20° C. if the Tidy method be followed).

In conclusion, the author expresses a wish that more extended biological experiments should be made as to the effects of water variously polluted on the lower animals (other animals as well as rabbits), and that the action of water introduced into the stomach as well as hypodermically injected, should be tested. It would be well to have chemical examinations, on uniform plan, from time to time made of the water supply of the largest cities at periods when the general assent of medical men indicates unusual prevalence of, or exemption from, the classes of disease most probably capable of origination from the organic pollution of drinking-water. The author would especially suggest a combined chemical and biological inquiry as to the possible effects upon living animals of the ferment or ferments of nitrification in different stages of that process. Some minor questions connected with development of nitrites and nitrates from decomposing organic matter also deserve further examination.

LOCKYER'S DISSOCIATION-THEORY[1]

IN February, 1880, I took occasion, on the ground of my observations to the spectrum of chemically pure hydrogen, to take objection, to Lockyer's view, that calcium, at a very high temperature, is dissociated.[2] From the fact, *inter alia*, that of the two calcium lines, H′ and H″, only the first is present in the spectra of so-called white stars photographed by Huggins, Lockyer proceeded to lay down the theory that calcium at a high temperature is decomposed into two substances, X and Y, of which the first gives the line H′, the other the line H″, and that in the stars referred to, only the first is met with. Against this I urged that hydrogen, besides the four known and easily visible lines, has a remarkable line of very intense photographic power, which nearly coincides with Fraunhofer's H′, and that one is the more warranted in regarding the supposed calcium-line observed by Huggins as a fifth hydrogen line, that the hydrogen lines in the spectra of those stars are developed in a striking manner, and also the ultra-violet star lines observed by Huggins, agree with the ultra-violet hydrogen lines photographically fixed by me.[3]

Lockyer, however, has not given up his idea of dissociation, but sought new proofs of it by the spectroscopic method.

He calls attention to the fact, *inter alia*, that in the spectrum of sun-spots, certain iron-lines appear broadened, and others not; that, moreover, many of them, as λ 4918 and λ 4919·7 do not occur in the spectrum of protuberances, which show other iron-lines, but do in the spectrum of spots; that in the latter again, the iron lines are occasionally absent, which the former contain, and he proceeds to say: "there is, accordingly, no iron in the sun, but only its constituents."[4]

This argumentation Liveing and Dewar[5] have already opposed, having proved that certain spectral lines of a substance, *e.g.* λ 5210 magnesium, and various calcium-lines, are only visible when certain foreign matters are present; in this case hydrogen on the one hand, and iron on the other; that accordingly the absence of certain iron lines in the spectra of the spots or protuberances may not be attributed to a dissociation, but to the absence of foreign matters which occasion the appearance of these lines in force.

Lockyer now takes his stand, however, on another fact, which is not explained by Liveing and Dewar's experiments, and which certainly seems to afford a firmer basis for his theory of dissociation than the facts referred to above. He says:[1]

"The last series of observations relates to the degree of motion of vapours in the sun-spots, which it is known, is indicated by changes in the refrangibility of lines. If all lines of iron in a spot were produced by iron vapour, which moves with a velocity of 40 km. in a second, this velocity would be indicated by a change of the refrangibility of *all* lines. But we find that that is *not* the case. We find not only different motions, which are indicated by different lines, but observe in the degree of motion the same inversions as in the breadth of the lines. This fact is easily explained, if we suppose dissociation, and *I know no more simple way of explaining it.*"

Lockyer cites as an example that in the spots of December 24, 1880, and January 1 and 6, 1881, a certain number of iron lines appeared bent, while others remained straight.

Now I believe it is possible to explain these facts on the basis of numerous observations in spectral analysis of absorption without needing to have recourse to the hypothesis of dissociation.

It is known that the position of the absorption-band of a substance depends very essentially on the dispersion of the medium in which it is dissolved or incorporated. One often observes that in strongly dispersive media the absorption-bands of a substance are displaced towards the red.[2] Now, the remarkable case often here occurs that certain absorption-bands are displaced with the increase of dispersion of the solvent, while others are not. Thus Hagenbach observed that, *e.g.*, the chlorophyll bands I. III. and IV. lie more towards red in alcoholic than in etheric solution, while the band II. in both solutions shows exactly the same position. I observed similar cases with uranian protoxide salts[3] and with cobalt compounds.[4]

Now Kundt has already called attention to the fact, that for absorption-spectra of gases the same rule holds good as for the absorption-spectra of liquid substances. He adds, indeed: "It is only questionable whether, if, *e.g.* hyponitrate gas be mixed with various other transparent gases, the displacements of the absorption-bands are so considerable, that they can be perceived." This doubt, however, does not affect the rule supposed, but merely its experimental verification.[5] The supposition, then, is permissible that, in the same way as with liquids, added media also affect the position of absorption-bands in the case of gases, and that in this case, as in the other, displacements of certain bands occur, while the position of others remains unaltered.

When, therefore, in sun-spots, certain iron lines suffer a displacement, and others in the same place do not, the cause is not motion, but the admixture of a foreign, strongly dispersive gas, which acts on the displaced lines and not on the others. It follows from this, further, that curvatures of absorption lines of the sun-spots need not by any means be always explained as due to motion of the absorbing gases in the direction of the line of observation, but only where all lines of a matter participate in the curvature.

That bright lines of a luminous gas, also, in like circumstances, "by admixture of another non-luminous vapour, or one giving a continuous spectrum," may suffer a displacement, Kundt has already shown.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

MARCUS M. HARTOG, D.Sc., M.A., F.L.S., has been appointed to the Professorship of Natural History at Queen's College, Cork, vacant by the death of Prof. Leith Adams.

[1] A paper by Herr Hermann W. Vogel, read to the Berlin Academy on November 2, 1882. Communicated by the author.

[2] *Proc. Roy. Soc.*, xxviii. 157.

[3] *Monatsb. der Berliner Acad. der Wiss.*, 1880, p. 192.

[4] *Comptes Rendus*, t. xcii. 504.

[5] *Proc. Roy. Soc.*, 30, 93. *Wied. Beibl.*, iv. 366.

[1] Herr Vogel quotes a translation in *Naturforscher*.

[2] Kundt, *Jubelband Pogg. Ann.*, *p.* 600

[3] Vogel, "Pract. Spectralanalyse." Nördlingen bei Beck. P. 248.

[4] *Monatsb. der Akad. der Wiss.* of May 20, 1878.

[5] Kundt formerly doubted also the possibility of proof of an anomalous dispersion in gases and glowing vapours. Recently, however, he has succeeded in getting such proof in the case of sodium vapours (*Wied. Ann.* 10, p. 321).

SOCIETIES AND ACADEMIES

LONDON

Royal Society, December 14, 1882.—On the genus *Meliola*, by H. Marshall Ward, B.A., Fellow of Owens College, Manchester. The author has examined the life-history and structure of several species of these epiphytic fungi. The fungus consists of a much-branched mycelium, on which appendages and fruit-bodies occur. The *hyphæ* constituting the *mycelium*, consist of cylindrical cells, with hardened, brown cell-walls and protoplasmic contents; these are attached to the epidermis of the leaves, &c., of tropical plants by rudimentary *haustoria*, which do not pierce the cell-walls of the host, but are firmly adherent to the cuticle. The appendages consist of simple or branched setaceous outgrowths, which spring from the mycelium at various points, and are especially developed around the fruit-bodies from masses of hyphæ, which Bornet considered as forming a special part of the fungus, under the name of "receptacle"; these setæ cannot be considered as subserving any special function, however, and are certainly not tubes for the outlet of spores, as earlier observers have surmised. Other appendages occur in the form of small ovoid or flask-shaped lateral branchlets; some of these become free and subserve vegetative reproduction as *conidia*. The fruit-body, or *perithecium*, arises by continuous development of one of the pyriform lateral branchlets, and the author has studied its development very particularly. The short, ovoid, unicellular branchlet, after becoming separated from the parent hypha by a septum, suffers division into two cells by a septum running obliquely across it; of these two cells one produces the outer walls of the *perithecium*, by continuous cell-multiplication, whilst the other contributes the central portion, or *ascogonium*, by slower division of its contents.

The former cell, dividing up more rapidly, produces a layer of cells which envelope the latter by a process of "epiboly," and the outer cell-walls become hard, thick, and dark-coloured. The latter—ascogenous cell—divides up more slowly into a "core" of thin-walled cells, very rich in protoplasm. After complete envelopment, the cells of the "core" are recognised in vertical sections; certain of them become elongated, and form the earliest *asci*, while others become absorbed—together with inner cells of the enveloping layer—to provide nutritive material for the developing *asci* and their progeny.

The *asci* are produced successively, and are delicate clavate sacs, containing two to eight *spores*, each spore being divided by one, two, or three cross septa. The germination of the *spore* is also described and figured; it throws out an irregular germinal tube, which soon forms rudimentary *haustoria*, and grows forth as a mycelium, similar to that from which the *perithecium* was produced.

The author examines and criticises the views held by Bornet and Fries as to the systematic position of *Meliolas*; especially the opinion that they are to be considered as tropical representatives of the European *Erysiphæ*. He shows that the original cell from which the *perithecium* arises must be regarded as containing in itself the undifferentiated elements of an *Archecarpium* and *Antheridium*-branch (in the sense of De Bary and others), and that after the primary division into two cells, we must look upon one of these—the one which becomes more rapidly divided up—as the homologue of the *antheridium*-branch and enveloping tissues of the *Erysiphæ*; the more slowly divided cell—which produces the *ascogenous* core—being the equivalent of the *ascogonium*, &c., of those fungi. The details of successive phases of development are amply illustrated by figures and many peculiarities acquired by the group are carefully examined and described.

The author concludes that the *Meliolas* must be looked upon as a group developed along similar lines to those of the *Erysiphæ*, *Eurotium*, &c., but in which the sexual process has suffered still greater reduction or withdrawal, leading to those forms in which it is entirely suppressed.

With respect to the injurious action of these fungi on their hosts, the author decides that no direct parasitic action on the cell-contents takes place, but that injury results indirectly on account of the dense black *mycelium*, when strongly developed, depriving the leaves of air, light, &c.

Chemical Society, December 21, 1882.—Dr. Gilbert, president, in the chair.—The following papers were read:—On the condensation products of oenanthol (part ii.), by W. H. Perkin, jun. The author has studied the action of nascent hydrogen upon oenanthol; when this substance is dissolved in acetic acid and acted upon by sodium amalgam, heptylic alcohol is produced, also an aldehyde, $C_{14}H_{26}O$, and an alcohol, $C_{14}H_{28}O$; if the oenanthol is dissolved in ether, heptylic alcohol, a solid aldehyde melting at 29°·5 ($C_{14}H_{28}O$), and a second substance, $C_{21}H_{40}O$, are formed. By oxidising the aldehyde $C_{14}H_{28}O$ with silver-oxide, a small quantity of an acid, $C_{14}H_{28}O_2$, boiling at 300°–310°, was obtained. The author has also studied the action of nascent hydrogen upon the aldehyde $C_{14}H_{28}O$, and discusses the constitution of these new bodies.—On the behaviour of the nitrogen of coal during destructive distillation; with some observations on the estimation of nitrogen in coal and coke, by W. Foster. It is usually stated in text-books that coal contains about 2 per cent. of nitrogen, which, during destructive distillation of the coal, comes off as ammonia. The author finds that this statement is not true. A Durham coal was used, containing 1·73 per cent. of nitrogen, and giving 74·5 per cent. of coke. If the total quantity of nitrogen in the coal be taken as 100, that evolved as ammonia is only 14·5 per cent.; as cyanogen, 1·56 per cent; as nitrogen in the coal-gas, 35·26 per cent.; left behind in the coke, 48·68 per cent.—On the absorption of weak reagents by cotton, silk, and wool, by E. J. Mills and J. Takamine. The reagents are sulphuric and hydrochloric acids, and caustic soda. This paper chiefly contains tables, with results calculated to five places of decimals.—On brucine, by W. A. Shenstone. Various observers have stated that brucine, when treated with dilute nitric acid, yields either methyl or ethyl, nitrate or nitrite. The author has studied the action of hydrochloric acid upon brucine quantitatively, and has proved that more than one molecule of methyl chloride is evolved from one molecule of brucine; he concludes that brucine is strychnia, in which two atoms of hydrogen are replaced by two methoxyl groups, and its formula may be written, $C_{21}H_{20}(CH_3O)_2N_2O_2$.—Researches on the induline group, by O. N. Witt and E. G. P. Thomas. "Induline" is a term applied to all coloured compounds formed by the action of amidoazo compounds on the hydrochlorides of aromatic amines with elimination of ammonia. The authors have studied in the present paper the formation of amidoazobenzene, and its action on aromatic hydrochlorides, and especially on anilin hydrochloride.—Preliminary note on some diazo derivatives of nitrobenzylcyanide, by W. H. Perkin.

Meteorological Society, December 20, 1882.—Mr. J. K. Laughton, M.A., F.R.A.S., president, in the chair.—Three new Fellows were elected, and Capt. J. de Brito Capello and Mr. W. Ferrel, M.A., were elected honorary members.—The following papers were read:—Popular weather prognostics, by the Hon. R. Abercomby, F.M.S., and Mr. W. Marriott, F.M.S. The authors explain over 100 prognostics, by showing that they make their appearance in definite positions relative to the areas of high and low atmospheric pressure shown in synoptic charts. The method adopted not only explains many which have not hitherto been accounted for, but enables the failure, as well as the success, of any prognostic to be traced by following the history of the weather of the day on a synoptic chart. The forms discussed are:—cyclones, anticyclones, wedge-shaped and straight isobars. The weather in the last two is now described for the first time. They also point out (1) that prognostics will never be superseded for use at sea, and other solitary situations; and (2) that prognostics can be usefully combined with charts in synoptic forecasting, especially in certain classes of showers and thunderstorms which do not affect the reading of the barometer.—Report on the phenological observations for the year 1882, by the Rev. T. A. Preston, M.A., F.M.S. The most important feature of the phenological year was the mild winter. The effect of this upon vegetation was decidedly favourable; and had it not been for the gales—especially that of April 28—the foliage would have been luxuriant, and therefore free from insect attacks, but the contrary effect has been produced on insect life, for the scarcity of insects, especially butterflies and moths, has been the general remark of entomologists.—Mr. J. S. Dyason, F.R.G.S., exhibited a series of typical clouds in monochrone, and also a series of sketches of clouds in colour, made in June, July, and August, 1882.

MANCHESTER

Literary and Philosophical Society—Microscopical and Natural History Section, December 12.—Prof. Roscoe in the chair.—Mr. James Heelis made some remarks upon the causes of the movement of the old Rhone Glacier with special reference to the power of gravity to produce such movement when

considered in connection with the gradient down which the glacier has passed.—Prof. Osborne Reynolds, F.R.S., communicated and explained an elementary solution of the dynamical problem of isochronous vibration.—Mr. John Boyd exhibited a fine living specimen of *Argulus foliaceus*, a parasite of the carp.—Mr. Charles Bailey, F.L.S., made some remarks on the occurrence of *Selinum carvifolia* in Lincolnshire, and of *Potamogeton zizii* in Lancashire and Westmoreland, and mentioned the localities where he had met with them respectively.—Mr. R. D. Darbishire, F.G.S., gave an account of dredgings made by him in company with Dr. A. M. Marshall and Mr. Archer at Oban in September last, and exhibited specimens of a considerable variety of animals taken.—Prof. A. M. Marshall, M.A., gave a detailed description of three forms of Pennatulida met with during the dredging, and suggested the desirability of the section undertaking or taking part in similar excursions in future years.

DUBLIN

Royal Society, November 20, 1882.—Sections I. and III. Physical and Experimental Science, and Applied Science.—Rev. Gerald Molloy, D.D., in the chair.—The following communications were received:—Rev. H. M. Close, M.A., on the definition of force as the cause of motion, with some of the inconveniences connected therewith.—G. Johnston Stoney, D.Sc., F.R.S., and G. Gerald Stoney, on the energy expended in propelling a bycicle, parts 2 and 3.—Prof. W. F. Barrett, F.R.S.E., physical apparatus for class-teaching.—A. H. Curtis, LL.D., improved apparatus for exhibiting double reflection in the interior of a crystal.—Prof. G. F. Fitzgerald, F.T.C.D., recent advances in physical science, an account of Prof. Rowland's curved gratings for spectrum analysis.—Prof. Fitzgerald exhibited photographs of the solar spectrum taken by Prof. Rowland.

Section II. Natural Science.—Rev. A. H. Close, M.A., in the chair.—The following communications were received:—Prof. V. Ball, M.A., F.R.S., on some effects produced by landslips and movements of the soil cap, and their resemblance to phenomena which are generally attributed to other agencies.—Prof. A. C. Haddon, M.A., exhibition of marine invertebrates, belonging to the Natural History Museum, prepared at the Zoological Station, Naples, with remarks upon the various methods for the preparation of zoological specimens.—G. A. Kinahan, on the geology of Bray Head.

December 18, 1882.—Sections I. and III. Physical and Experimental Science, and Applied Science.—A. H. Curtis, LL.D.; in the chair.—The following communications were received:—G. F. Fitzgerald, F.T.C.D., on Dr. Eddy's hypothesis that radiant heat is an exception to the second law of thermodynamics.—Communicated by Howard Grubb, M.E., F.R.A.S.: (*a*) Notes on the transit of Venus, as observed at Armagh Observatory by Dr. Dreyer; (*b*) Notes on the transit of Venus, as observed at Cork Observatory by Prof. England; (*c*) Notes of the transit of Venus, as observed at Rathowen, Co. Westmeath, by Mr. W. E. Wilson.—G. Johnstone Stoney, D.Sc., F.R.S., on means of neutralising echoes in rooms.—G. Johnstone Stoney and G. Gerald Stoney, on geared bicycles and tricycles.—Dr. Otto Bœddicker, on the influence of magnetism on the rate of a chronometer (communicated by the Right Hon. the Earl of Rosse, F.R.S.)—Mr. Grubb informed the Society that Dr. Huggins had authorised him to announce that he had succeeded in photographing the corona of the uneclipsed sun by employing absorbing media.

PARIS

Academy of Sciences, December 18.—M. Jamin in the chair.—The following papers were read:—On a recent memoir, by M. Wolf, of Zurich, on the periodicity of sun-spots, by M. Faye. From further careful study (by a method described) of data for the last 120 years, M. Wolf concludes (1) that there is a period of 10 years; also (2) a period of 11 years, 4 months; and (3) that there is not a period of 12 years, imputable to the action of Jupiter. Spite of the great difference of the two periods, the interval between a minimum and the next maximum is the same in both, viz. 4½ years. After 170 years the phenomena recur in the same order, and with the same numerical values. M. Faye added some remarks by way of theory.—Statistics of preventive vaccination against *charbon* relating to 85,000 animals, by M. Pasteur. The figures (for Eure-et-Loire, where the ravages have been worst) show a marked reduction of the mortality from *charbon*; thus, of the 80,000 sheep vaccinated, only 0·65 per cent. died, whereas the average mortality of the past 10 years is 9·01 per cent.—Contribution to the study of rabies, by M. Bert. He gives results published a few years ago, but little known. *Inter alia*, inoculation with mucus from the respiratory passages of a mad dog caused rabies, but that with the salivary liquids did not. Reciprocal transfusion of blood between a healthy and a mad dog did not cause rabies in the former. The slaver of a mad dog, after filtering through plaster, was harmless, but the portion caught on the plaster caused rabies (which is thus probably due to a microbe).—On the functions of seven letters, by M. Brioschi.—Experiments with a new arrangement of the automotor elevating apparatus with oscillating tube, by M. de Caligny.—M. Faye presented the second and last volume of his "Cours d'Astronomie."—M. de Quatrefages announced the formation of a committee, headed by M. Milne-Edwards (who is now convalescent) for a monument to Darwin, as proposed in England.—On maize at different periods of its vegetation (continued), by M. Leplay.—M. Ladureau (in a memoir) stated that he has found, on an average, 1·80 cc. of sulphurous acid (free and combined) per cubic metre of air in the atmosphere of Lille.—The Secretary called attention to a new work on Galileo, by Signor Favaro, asked to be informed of any documents relative to Fermat (whose works are to be published by the Minister of Public Instruction), and read some telegrams on the transit of Venus.—Observations of the transit of Venus at Algiers Observatory, by M. Trépied. Bad weather marred the work. The spectrum, and photographs taken in the green, blue, and violet, showed no absorption by an atmosphere round Venus.—On the transit as observed at Rome, by M. Millosevich. He thinks the spectroscopic method the only one capable of giving good results, which admit of being tested, for the first contact.—On the great southern comet, as observed at the Imperial Observatory of Rio de Janeiro, by M. Cruls. On October 15 there were two nuclei, and he thinks the appearance of the tail due to two tails (corresponding to the nuclei).—On solar photometry, by M. Crova. By a method described, and by Bougner's method, he arrives at about 60,000 carcels for the intensity of the solar light on a clear day (at Montpellier), an estimate ten times those of Bougner and Wollaston.—Reply to M. Ledieu, &c., by M. Decharme.—On the sensation of white and complementary colours, by M. Rosenstiehl. The introduction of a coloured object in an illumination apparently homogeneous and colourless, at once shows the real lack of homogeneity in the combination of lights. There is often confusion between mixture of lights and mixture of sensations.—Researches on the duration of solidification of surfused substances, by M. Gernez. He worked with U tubes holding phosphorus. The course of the phenomenon is uniform. Previous heating of the phosphorus to different temperatures did not sensibly affect the velocity of solidification. M. Gernez studies the curve for velocity of solidification at different temperatures (43°·8 to 24°·9).—On the measurement of pressures developed in a closed vessel by explosive gaseous mixtures, by M. Vieille. The method (described) was to register the law of displacement of a piston of known section and mass; (results shortly).—On the crystallisation of hydrate of chlorine, by M. Ditte.—On chloride of pyrosulphuryl, by M. Konovaloff.—On the products of distillation of colophanry, by M. Renard.—Production of surgical anæsthesia, by combined action of protoxide of nitrogen and chloroform, by M. de Saint Martin. With protoxide of nitrogen (85 vol.), and oxygen (15 vol.) M. Bert got anæsthesia by operating under pressure. If 6 or 7 gr. chloroform be added per hectolitre, the effect is had quickly at ordinary pressure.—Passage of the bacterium of *charbon* from mother to fœtus, by MM. Strauss and Chamberland.—Physiological properties of oxethylquinoleine-ammonia, by M. Bochefontaine. Like curare, it prevents passage of excitation from nerve to muscle, but, unlike curare, it makes the heart beat more slowly.—Experimental researches on spontaneous contractions of the uterus in certain mammalia, by M. Dembo.—On the formation of embryonal layers in the trout, by M. Hennegny.—Remarks on M. Lichtenstein's paper on pucerons, by M. Balbani.—Orographic note on the region of the Jura between Geneva and Poligny, by M. Bourgeat.—On a phenomenon of molecular mechanics, by M. Trève. He covers the tops of ivory balls, hung in a row, with metallic powder; when one end ball (say the left) is drawn back and let fall on its neighbour, the powder on the right half of the balls is thrown in the direction of the shock; but that on the last ball is thrown from the side opposite to the direction o the shock.

December 26.—M. Jamin in the chair.—The following papers were read:—Observations of the transit of Venus at the Naval Observatory of Toulon, by M. Mouchez. M. Rozet observed the black drop at second contact.—On two objections of Prof. Young of New Jersey, to the cyclonic theory of sunspots, by M. Faye. These are, the absence of visible traces of rotation in most spots, and the small difference of angular velocity in successive zones of the photosphere. M. Faye holds the unequal velocity sufficient to cause vortical movements of any calibre; and the general absence of agitation at the border of spots he attributes to the slowness of gyration there (our cyclones seen from above would show the same). He cites a number of positive indices of gyration.—Theory of the resistance of woven materials to extension, by M. Tresca. Such stuffs suffer elongations which increase less rapidly than the weights; and with equal weight, they show much greater elongation than those of the warp-threads composing them. The mode of interlacing of the threads explains these differences.—On the necessity of introducing certain modifications into the teaching of mechanics, and of banishing certain problems; *e.g.* the motion of the solid body of geometers, by M. Villarceau.—Considerations on the general theory of units, by M. Ledieu.—Separation of gallium (continued), by M. Lecoq-de-Boisbaudran.—Herr Bunsen was elected Foreign Associate in room of Wöhler, deceased.—Chemical studies on maize, &c. (continued), by M. Leplay.—Evolution of microscopic organisms in the living being, and in the dead body and morbid products, by M. Colin. Microbes are nowhere absent in the respiratory and digestive apparatus, and at many points they are prodigiously numerous. In normal conditions the liquids holding them are harmless, but they become dangerous after putrid alteration.—The first number of a new mathematical journal, *Acta Mathematica*, published at Stockholm (M. Mittag-Leffler, editor), was presented.—A telegram from Montevideo announced success of the transit observations at Santa Cruz.—Observation of the transit of Venus at Nice Observatory, by M. Mouchez. Five photographs were had, under difficult conditions.—Observation of the transit at Avila (Spain), by M. Thollon. They sought to observe Venus's atmosphere spectroscopically at a height of 1200 m., but bad weather prevented their getting any satisfactory results.—Photographs of the great comet of 1882 taken at the Observatory of the Cape of Good Hope, by Mr. Gill. Spite of long exposure (140 minutes for the sixth and last photograph), the stars at the centre of the image are remarkably distinct. More than fifty stars are seen through the tail. Mr. Gill does not doubt that stellar maps might be produced by direct photography of the heavens.—On the formula recently communicated to the Academy regarding prime numbers, by M. de Jonquières.—On the same, by M. Lipschitz.—Reply to a recent note by M. Lalanne on the verification and use of magnetic maps, by M. de Tillo.—Electrodynamic method for determination of the ohm; experimental measurement of the constant of a long coil, by M. Lippmann.—Measurement of the photometric intensity of spectral lines of hydrogen, by M. Lagarde. The curves from the values obtained show the inequality of intensity of the three lines, inequality variable with the induced discharge. With diminution of pressure, the curve straightens; at 6·5 mm., that for red is a straight line.—On the instantaneous pressure produced during combustion of gaseous mixtures, by MM. Mallard and Le Chatelier. With mixtures of H and O, the interior pressure exceeded by more than 2 atm. that corresponding really to the temperature of combustion; and this occurred in less than a ten-thousandth of a second. An explanation is offered.—On bisulphhydrate of ammonia, by M. Isambert.—On a case of physical isomerism of monochlorinated camphor, by M. Cazeneuve.—Biological researches on beet, by M. Corenwinder.—On the reduction of sulphates by sulphuraria, and on the formation of natural metallic sulphates, by M. Plauchud.—On the transformation of nitrates into nitrites, by MM. Gayon and Dupetit. They have isolated four distinct microbes capable of the action; one can live in chicken broth even when this is saturated with nitrate of potash, and decompose 10 gr. of the nitrate per litre daily. The microbes of chicken cholera, the bacterium of charbon, and the septic vibrion, effect denitrification much less easily.—On the poisonous principles of edible fungi, by M. Dupetit. Injecting, subcutaneously, the fresh juice of several such fungi into rabbits, &c., he observed symptoms of poisoning, followed by death. The chemical properties of the active principle recall those of soluble ferments, rather than of known alkaloids. A temperature of 100° renders the juice harmless.—Researches on the production of a general anæsthesia or a specially unilateral anæsthesia by a simple peripheric excitation, by M. Brown-Séquard. Irritation of the laryngeal mucous membrane with a current of carbonic acid will produce anæsthesia in all parts of the body, without passage of this gas into the blood.—On the physiological action of coffee, by M. Guimaraeo. The experiments (made on dogs at Rio) prove that coffee is at once a stimulant and a repairer. By permitting a greater expenditure and consumption of argotised substances, it evidently increases the power of work.—On the structure of cells of the mucous bodies of Malpighi, by M. Ranvier.—On the fœtal envelopes of Chiroptera of the family of Phyllostomides, by M. Robin.—On an usteria from great depths of the Atlantic, provided with a dorsal peduncle, by M. Perrier. This is a "find" of the *Travailleur* cruise, and is named *caulaster pedunculus*.—On the suctocitiates of M. de Merejkovsky, by M. Maupas. The type described, he says, has been long known.—Mineralogical analysis of the rock impasted in the meteorite of Atacama, by M. Meunier.

BERLIN

Physical Society, December 15, 1882.—Prof. Helmholtz in the chair.—Prof. Christiani demonstrated some acoustic experiments which he had incidentally made. In renovation of the Kœnig tuning-forks injured by the fire in the Physiological Institute, and which had to be freed from their coating of rust, and mounted on new resonance cases, one fork of the series, the fork mi_3, showed after tuning and sounding, when one side of it was turned towards the closed end of the case, a maximum of tone; it did not matter in which direction (right or left) the fork was turned round into the position referred to. Another fork mi_3 of the physical Institute in unison with the first, did not present the phenomenon, and when the forks and cases were exchanged, it appeared that the effect was connected with the new case. It was not explained. A second experiment, made by Prof. Christiani, was named by him "total absorption of tone." A singing flame was tuned approximately to the tone mi_3, and the resonance case bearing the tuning-fork mi_3 was held with its open end horizontal near the upper end of the chemical harmonica. The tone was unaffected. When, however, the same case, without tuning-fork, was brought to the same position relatively to the sounding chemical harmonica, the sound immediately ceased, and the flame burned quietly in the tube. Each time the tone of the flame ceased, when the mouth of a resonator adapted to the pitch was brought to the upper end of the tube, whereas the flame sounded again when the resonator was tuned to a different tone, or was loaded with a tuning-fork. Prof. Christiani means to investigate the phenomenon further.

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THURSDAY, JANUARY 11, 1883

GEIKIE'S GEOLOGY

Geological Sketches at Home and Abroad. By Archibald Geikie, LL.D., F.R.S. With Illustrations. (London and New York: Macmillan and Co., 1882.)

Text-Book of Geology. By Archibald Geikie, LL.D., F.R.S. With Illustrations. (London: Macmillan and Co., 1882.)

THESE two works, by the same author, are presented to the public at nearly the same time, but there is no other reason why they should be described together. The first is a collection of short papers, each presenting some matter of personal observation or some contribution to geological philosophy. The second exhibits the science of geology in a systematic way, and of necessity deals chiefly with the results of the work of others. The first is addressed to the general reader, and in part to the geologist; the second is addressed specifically to the student.

The sketches of the first volume are not new, but are here collected for the first time. Several of them received their first publication as magazine articles, others have been presented to scientific societies, and a few have taken the form of lectures. They constitute but a small portion of the author's voluminous contributions to scientific literature, and have evidently been selected because of their popular interest. A few are addressed to the popular audience only, and merely present some of the elements of stratigraphical and dynamical geology, with familiar Scottish scenes as texts; but the majority embody original contributions to knowledge, couched in so simple language that the layman reads them without being fully aware that they belong to the frontier of geological thought. Prof. Geikie possesses the happy faculty of addressing himself simultaneously to a professional and an unprofessional audience in such way that the former do not find his science too dilute nor the latter too condensed.

One of the sketches describes a journey to central France, undertaken for the purpose of studying the extinct volcanoes of that region as an aid to the imagination in restoring the condition of Scotland during the Carboniferous period; and another describes a journey to Norway with the parallel purpose of rendering vivid the mental restoration of Scotland in Glacial times. These two are perhaps the most instructive of the collection, for besides making definite additions to the geological history of Scotland, they present admirable illustrations of one of the most valuable methods of scientific investigation. The principles which distinguish modern scientific research are not easily communicated by precept, and it is by no means certain that they have yet been correctly formulated. However it may be in the future it is certain that in the past they have been imparted, and for the present they must be imparted, from master to pupil chiefly by example; and whoever in publishing the result of a scientific inquiry sets forth at the same time the process by which it was attained, contributes doubly to the cause of science.

Two chapters are devoted to a journey in the United States; a journey undertaken, like the others, for a definite purpose—that of enabling the author to see with his own eyes the monuments of erosion for which the Rocky Mountain region is so illustrious. His account deals also with a variety of geological topics, as well as with the peculiar aspects of American frontier life. He describes the geysers of the Yellowstone country, some of the extinct glaciers of the head-waters of the Missouri, the parallel shore-lines of the great extinct lake of Utah, and the great lava field of the Snake River Plain. In another chapter he appears as the apostle of massive eruptions, first recognised by Richthofen, and afterwards by many American geologists, but so foreign to European experience, that the accounts of them had seemed to many English geologists to border on the marvellous, and had even thrown discredit upon American science.

Perhaps the most important paper of all is that upon geographical evolution. It was originally read to the Royal Geographical Society, and has received in various ways so wide a publication, that it is probably accessible already to the majority of the readers of NATURE. The lecture on the weathering of rocks, as illustrated by tombstones, is also included, and a lecture on the geological influences which have affected the course of British history.

In the whole collection there is nothing polemic, nor anything that could even be called controversial. Attention is never directed to an error, except as the merest incident to pointing out that which is true. No words are given to the censure of others, but many to their praise, and one of the chapters has for its theme a eulogy on the work of the early Scottish school of geology.

The style is peculiarly genial and entertaining—a merit unfortunately rare in the writings of modern geologists; but accuracy of statement is not sacrificed to vivacity. As in all his writings, there is nothing sensational, either in description or in speculation. His inductions are not expanded into brilliant, universal theories, but are modestly advanced with all those limitations which impress themselves on the mind of one who constantly questions nature.

Turning now to the text-book, we come to consider a work of greater importance, and one especially deserving of careful criticism by reason of its relation to education. The text-books of this generation must furnish to the geologists of the next their fundamental principles, so that those who prepare them and those who commend them are responsible, not merely to the youth of to-day, but to the science of the future.

There are four features in regard to which a work designed for geological instruction should be scrutinised: Its scope, the arrangement of its matter, the quality of its matter, the manner of presentation.

In the scope of geological text-books, on the range of subjects considered and the relative space allotted to each, there has been a progressive development, parallel with and dependent upon the evolution of geology and cognate sciences. Our knowledge of the earth's history is so dependent upon and interwoven with other departments of knowledge, that a clear presentation of it cannot be made without either reciting the elements of other sciences or assuming them to be known. In the early history of the subject, when the volume of geological material was small, and when the elements of zoology,

botany, and chemistry were not so widely diffused as now, it appeared to most writers necessary to devote some space, either in a prefatory or in an incidental way, to these sciences. Mineralogy and palæontology, growing with the growth of geology, were likewise treated with it. But owing to the rapid development of geology, its own subject matter has now become so voluminous that it can only with difficulty be outlined in the compass of a text-book, and step by step it has displaced everything of which a sufficient knowledge could be assumed.

While mineralogy and palæontology have by their growth become more and more differentiated from geology, astronomy has been affiliated in a degree that was not anticipated. Previous to the revelations of the spectroscope, our earth was regarded indeed in origin, composition, and career as analogous to other planets, but only in a hypothetic and speculative way; but now that there is a large body of evidence pointing to identity of composition throughout the solar system, there is no longer any question of a common history, and every advance in celestial physics is now regarded as a contribution to the early history of the earth. A department of astronomical geology has thus arisen.

In the work under consideration no space whatever is permitted to zoology and botany; chemistry is barely mentioned; mineralogy (chiefly descriptive) is accorded only 25 pages; palæontology proper is omitted, but 28 pages are devoted to the principles of palæontological geology—a department of science clearly distinguishable if not distinct from palæontology, and inseparable from stratigraphy; mythological cosmogony is not even mentioned, but the space it has too often occupied is given to physiographical geology—a discussion of the origin of the physical features of the land. Astronomical geology is accorded 23 pages. The bulk of the volume—570 pages out of 910—is devoted to geognosy, and dynamical and structural geology that is, to rocks and rock structures, and to the physical changes whereby rocks originate. Stratigraphy, which until very recently has arrogated the lion's share of space, is here reduced to less than one-third of the total.

The distribution of space thus outlined is eminently judicious, and it may be doubted whether any could be better adapted to the present status of the science and the present demands of instruction. If it has a fault it is in the amount it concedes to the demands of the geologist in the matter of stratigraphy. The student's text-book has not yet been clearly differentiated from the geologist's handbook, and there is certainly an open field to-day for a manual specially adapted to the use of the working geologist, and not primarily arranged for instruction. All of the larger text-books have been partially adjusted to this need, and Prof. Geikie's is not an exception; but in his work the adjustment appears only in the stratigraphical chapter, which embodies a mass of detail that can serve only to bewilder if the student undertakes to master it. If the 275 pages of descriptive stratigraphy were reduced to 50, and a portion of the space thus saved were devoted to a rapid review of the salient points of the geological history of some limited region, as Great Britain, for example, I am prone to believe that the student would be afforded a better insight into the aims and results of geological inquiry.

In the classification and arrangement of the subject-matter of geological text-books, there has been as marked a development as in the scope. The number of different manners in which a congeries of allied topics can be grouped is practically limitless, for the bases of possible groupings are as numerous as the relations sustained by the topics; but not all classifications are of equal utility, and at each stage in the progress of a science there is usually some one which commends itself as of superior advantage. As, in the progress of knowledge, new relations are discovered, and the importance of relations previously known comes to be differently estimated, new classifications are adopted, in comparison with which the old appear crude. Geology is so young a science, that a single generation has witnessed a complete revolution in this regard. The primary classifications of the modern text-books have nothing in common with the earlier editions of Lyell's manual. In the division and arrangement adopted by Geikie, only a single feature is original, but the order of presentation as a whole is new.

The theme of geology is the history of the earth. In its study there are two lines of inquiry, which are so nearly independent that they form co-ordinate branches of the general theme: the one is cosmic, the other terrestrial.

Cosmically considered, the earth is one of a group of worlds believed to have a common origin, and to be pursuing parallel courses of development, in which they have reached various different stages. Assuming this to be true, the less developed worlds present phases, through which the earth has already passed, and by studying them we may learn something of the youth of our planet.

The terrestrial branch of inquiry is concerned chiefly with rocks. The changes of the crust have led to the formation of rocks, and have given to them great variety of composition and structure. It is known, moreover, that rock formation is still in progress, and that agencies whose operations can be witnessed are now forming many varieties of rocks, and are initiating many peculiarities of rock structure. It is possible, therefore, to associate certain rocks and rock structures with certain processes of change, and by this means to derive from a study of the rocks of the crust a history of the changes which led to their formation. This inquiry is greatly facilitated by the fact that rocks have been partly formed from animal and vegetable remains, and by the additional fact that there has been a progressive development of life; so that, the key once obtained, the chronological order of rocks can be deduced from their organic contents.

In presenting the second line of inquiry as to the earth's history it is therefore proper to treat: of the composition of rocks and other materials of the earth's crust (geognosy); of the forms in which rocks are aggregated, or the structure of rock masses (structural geology); of the agencies which in modern times are observed to produce changes of the earth's crust (dynamical geology); of the relation of organic remains to geological formations (palæontological geology); and finally, of the actual order in which the various kinds and groups of rocks succeed each other, and the deduced series of changes the earth has undergone (stratigraphical or historical geology). The first and last of these categories claim their respective positions without question: geognosy constitutes the alphabet of the sub-

ject, and must precede all else, while stratigraphical geology depends upon all the other divisions, and must follow them. Palæontological geology is in some sense co-ordinate with dynamical and structural geology taken together, but holds its place after them because its use cannot be explained before their principles are known. Whether dynamical geology should precede or follow structural, is a question admitting of discussion. They are to a large extent correlatives, and either is more intelligible if preceded by the other. To give precedence to structural geology is to describe phenomena in advance of their explanation. If dynamical geology precedes, a variety of natural agents are described which have no apparent connection with the general subject. The majority of writers have selected the former alternative; but a few have preferred the latter, and among them our author. All things considered, he appears to have chosen the lesser evil.

The single new departure of the volume consists in the elevation of physiographical geology to the rank of a major division. The same title it is true has been placed by Dana at the head of a primary division of the subject, but it was used by him in a different sense. With Dana it is a synonym for physical geography; with Geikie it is that "branch of geological inquiry which deals with the evolution of the existing contours of the dry land." So far as the subject has had place in earlier treatises it has been regarded as a subdivision of dynamical geology, and the classification which placed it there was certainly logical. In dynamical geology, as formulated by Geikie, the changes which have their origin beneath the surface of the earth (volcanic action, upheaval, and metamorphism), and the changes which belong exclusively to the surface (denudation and deposition) are separately treated. In physiographical geology the conjoint action of these factors of change is considered with reference to its topographical results. Starting from geological agencies as data we may proceed in one direction to the development of geological history, or in another direction to the explanation of terrestrial scenery and topography, and if the development of the earth's history is the peculiar theme of geology, it follows that the explanation of topography, or physiographical geology, is of the nature of an incidental result—a sort of corollary to dynamical geology. The systematic rank assigned to it by Geikie is an explicit recognition of what has long been implicitly admitted: that geology is concerned quite as really with the explanation of the existing features of the earth as with its past history. The separation initiated by our author is an indication of the growing importance of the subject, and it is safe to predict that in the future it will not merely retain its new position, but will even demand a larger share of space.

The following scheme exhibits the general plan of the volume:—

Book 1.—Cosmical aspects of geology.

Book 2.—Geognosy: an investigation of the materials of the earth's substance.

Book 3.—Dynamical geology.

Book 4.—Geotectonic geology; or the architecture of the earth's crust. (*Geotectonic* is a new term proposed as a substitute for *structural*).

Book 5.—Palæontological geology.

Book 6.—Stratigraphical geology.

Book 7.—Physiographical geology.

Comparing this classification with that of other authors, and viewing it with reference to the present condition of the science, we may say without hesitation that it has no superior, and that it is well adapted to existing needs.

G. K. GILBERT,
U.S. Geological Survey

(*To be continued.*)

OUR BOOK SHELF

Uniplanar Kinematics of Solids and Fluids; with Applications to the Distribution and Flow of Electricity. By George M. Minchin, M.A. Pp. viii. + 266. (Oxford: Clarendon Press, 1882.)

IN subject-matter this book is almost unique among our mathematical manuals. The only fellow to it is Clifford's "Kinematic." It consists of six chapters, the first dealing with Displacement and Velocity, the second with Acceleration, the third with Epicycloidal Motion, the fourth with the Mass-Kinematics of Solids, the fifth with the Analysis of Small Strains, and the sixth almost as long as the others put together, with the Kinematics of Fluids. The subdivisions of the last chapter are headed —General Properties: Multiply Connected Spaces; Motions due to Sources and Vortices, Electrical Flow; Conjugate Functions. There is also a short appendix, with notes on such subjects as Vectors and their Derivatives, Current-Power, and Routh's Use of Conjugate Functions.

It is impossible, without occupying considerable space, to give an adequate idea of the freshness and originality which mark Prof. Minchin's work. These are notable in the exceedingly valuable sixth chapter, but even on such well-worn subjects as velocity and acceleration, he treats us to many pleasant little surprises. Nor is this accomplished at the expense of the student; the clearness, fulness, and good arrangement specially requisite in a college text-book are all of them conspicuous; and valuable collections of exercises, worked and unworked, and given at intervals. The book is altogether one for which success may be cordially wished, not merely as a reward to the author, but in order that the science of which he treats may go on as steadily and rapidly advancing as it has of recent years been doing.

Die Käfer Westfalens. Zusammengestelt von F. Westhoff. Abtheilung ii. (Supplement zu den Verhandlungen des naturhistorischen Vereins der preussischen Rheinlande und Westfalens, Jahrgang 38, pp. 141-323.) (Bonn, 1882.)

WE have already noticed the first part of this work in NATURE. The second and concluding portion is now before us. It forms one of the most useful local Beetle catalogues that we have seen, nicely printed (the names being in bold black type), with copious local and other information. The district comprises about 450 square (German) miles, and is varied in its physical conditions. In all, 3221 species are enumerated, in 59 families. The *Staphylinidæ* comprise 667 species, *Curculionidæ* 471, *Carabidæ* 321, *Chrysomelidæ* 265, and *Dytiscidæ* 115. All the other families have each less than 100 representatives, and 10 of them less than 5. The nomenclature followed is that of the newest "Stein-Weise" German list, which, as is well known, has introduced a great multitude of changes and innovations; but other generally received names are indicated in brackets, thus avoiding confusion. Westhoff describes no new species in Part ii., but indicates and names a good many new (chiefly colour) varieties. Probably the rage for naming colour-varieties, so wide-spread at the present day, should be deprecated. For instance, in this catalogue we find a list of 27 named

varieties following the indication of *Coccinella* 10-*punctata*, L., and 6 or 8 analogous varieties are appended to many other species of Ladybirds. Taking it as a whole, this excellent catalogue may serve as a model for compilers of lists of the Beetle (or other entomological) fauna of other districts.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Equal Temperament of the Scale

In your number of November 8, 1877, p. 34, Mr. Chappell, F.S.A., has intimated that mathematicians who propose to divide the octave into twelve equal semitones instead of "equally tempered semitones," are deficient in musical ear. I have not noticed that any mathematician has replied to him.

Representing (with Mr. Chappell) the number of vibrations in the C of my piano by 1, and the octave c therefore by 2, and dividing the octave into 12 equal intervals, I obtain for the vibration-numbers—

C = 1·	G = 1·4983 = $2^{\frac{7}{12}}$
C♯ = 1·0594 = $2^{\frac{1}{12}}$	G♯ = 1·5874 = $2^{\frac{8}{12}}$
D = 1·1224 = $2^{\frac{2}{12}}$	A = 1·6818 = $2^{\frac{9}{12}}$
D♯ = 1·1892 = $2^{\frac{3}{12}}$	B♭ = 1·7818 = $2^{\frac{10}{12}}$
E = 1·2599 = $2^{\frac{4}{12}}$	B = 1·8877 = $2^{\frac{11}{12}}$
F = 1·3348 = $2^{\frac{5}{12}}$	c = 2·
F♯ = 1·4142 = $2^{\frac{6}{12}}$	

In these equal semitones each is equidistant from the preceding and following: as F is to F♯, so is F♯ to G, &c. Hence in whatever key I play a passage on my piano, the divergence from harmonic intervals will be alike at every point; the keys on my piano will have no distinctive character, the key of 3 sharps will not be more "brilliant" or less "plaintive" than that of 4 flats.

In the key of C, the harmonic third, fifth, and seventh will be, according to the above notation, 1·25, 1·5, and 1·75 respectively. As regards the fifth G it is a remarkable numerical coincidence that $2^{\frac{7}{12}}$ only differs from 1·5 by $\frac{1}{70}$, *i.e.* the equal temperament G only differs from the harmonic by its $\frac{1}{70}$ part, a difference so slight that it may be neglected. We tune fiddles by fifths therefore. This coincidence is the fundamental fact which enables us to modulate into various keys on a piano, and it is the reason why the scale must be divided into 12 (and not any other number of) semitones; for it will be found that, until you get to the unmanageably high number of 53, no other equal division of the scale has any note so near the harmonic G.

The crucial point of tempering arises on the third. The E of my piano is $2^{\frac{4}{12}} = \frac{125}{100}$, whereas the harmonic E is $= \frac{125}{100}$; my E is therefore by its $\frac{1}{70}$ part too sharp, *in the key of C*, a perceptible degree of error, unpleasant to many musicians. In ordinary pianoforte tuning, the E (by the plan in Hamilton's pianoforte tuner or some similar compromise) is tuned somewhere between $\frac{125}{100}$ and $\frac{126}{100}$, say $\frac{125\frac{1}{2}}{100}$, and the wolf between this E and the upper c is distributed.

This is all very simple so long as we remain in the key of C; indeed if we remain there, we want no tempering. But G♯ is the third to E, and c is the third to A♭; on the piano G♯ and A♭ are one. On my equal-semitone piano I have

$$c = 1;\ \mathrm{E} = 2^{\frac{4}{12}}\ (= \tfrac{125}{100}\ \text{nearly});$$

$$\mathrm{G}\sharp = \mathrm{A}\flat = 2^{\frac{8}{12}}\ (= \tfrac{158}{100}\ \text{nearly});\ c = 2.$$

I now ask the champion of "equally tempered semitones" what is the numerical value of his E and what of his G♯. If he gives them any other values than $2^{\frac{4}{12}}$ and $2^{\frac{8}{12}}$ respectively, it is clear that a *greater* error will be introduced in one part of the scale than is saved in another. Instead of algebraic proof I take an instance—suppose that Mr. Chappell tunes his E at $\frac{125\frac{1}{2}}{100}$; if he equally tempers his G♯ in the scale of E, it will be $\left(\frac{125\frac{1}{2}}{100}\right)^2$ at 5e very nearly. Then when he puts down the common chord the key of A♭, his third the c will be by its $\frac{1}{70}$ part too sharp, whereas on my equal temperament piano it would only be by its $\frac{1}{70}$ part too sharp. In other words, though the keys of C and E may be somewhat better on Mr. Chappell's piano than on mine, the key of A♭ will be *very much* worse. This is pretty nearly what occurs in practice. The point of my argument is that Mr. Chappell cannot move his E ever so little from the value $2^{\frac{4}{12}}$ without introducing a greater error somewhere else. The term "equally tempered semitone" is inaccurate; the semitones on my piano are all equal; and no one of them can be altered by a disciple of the "equally-tempered semitone" without making them unequal. The "equally-tempered semitones" are *not* equally tempered. Moreover if you "temper" *at all* you lose the effect of the harmonics; by moving E from $\frac{125}{100}$ to $\frac{125\frac{1}{2}}{100}$ you sacrifice harmonic coincidence.

The simple reason that unequal tempering is practised is because all keys are not used equally often. A piano is unequally tempered so that the keys C, G, A, F are fair, E, B♭, E♭ tolerable, the other keys being very much worse than on my equal-semitone piano. On most church organs, being unequally tempered, if you modulate even transiently into 4 or 5 flats, the effect is unendurable.

The crucial question in tuning is the question, if your E is not $2^{\frac{4}{12}}$ and your G♯ $2^{\frac{8}{12}}$, what values do you put them at? The question of the seventh is more complex; I may observe that though my equal-semitone seventh (1·7818) appears far away from the harmonic seventh (1·75), yet that the B♭ of tuners on the "equally-tempered semitone" system is not much nearer it. Their B♭ is $\frac{9}{5}$ or thereabout, or in other words, the sub-sub-dominant of C. Therefore, on the piano, you have not got the "harmonic-seventh" at all; the note which replaces it is one that suggests overpoweringly the key of F. This is the secret which underlies several of our rules in harmony. It is also the reason why valve-horn players play B♭ (though an open note) with valve *n.*2, or if they play without a key "lip it up" very carefully.

It is often supposed that the "wolf" has been introduced into music by that most useful though imperfect instrument the piano, and that the noble violin or human voice knows it not, except in so far as our natural good ear for harmonic intervals has been debauched by continually hearing tempered intervals. This is not so; the "wolf" is not only in the piano but in the scale. It is true that a violin can play in harmonic tune so long as the melody runs in one key, or if it modulates into a closely allied key, and *back again the same way*. But suppose my violin begins by rising from C to E harmonically, *i.e.* to $\frac{125}{100}$; then after playing awhile there proceeds to G♯ $(\frac{125}{100})^2$ harmonically, being then in 8 sharps; and then, after playing awhile in 8 sharps, proceeds to c; the c of the fiddle will then be $(\frac{125}{100})^3$ instead of 2, *i.e.* it will be $\frac{3}{70}$ out of tune. In this simple case the fiddle is supposed to play alone, unfettered by any harmonics but its own; in the case of a string-band, the agreeableness of many modulations actually depends upon some chords being harmonically out of tune, the note in the chord which performs the duty of G♯ to its preceding chord, performing the duty of A♭ to its succeeding chord.

The practical conclusion is that the best plan of tuning a piano for vulgar music and vulgar players is that now ordinarily practised by the tuners, and recommended by Mr. Chappell; but if the piano is to be used equally in all keys (or even frequently in 4 or 5 flats, 5 or 6 sharps) the best plan is to tune it in 12 mathematically equal semitones.

C. B. CLARKE

Animal Intelligence

In an excellent paper on "Animal Intelligence" (NATURE, vol. xxvi. p. 523), Mr. C. Lloyd Morgan says that "The brute has to be contented with the experience he inherits or individually acquires. Man, through language spoken or written, profits by the experience of his fellows. Even the most savage tribe has traditions extending back to the father's father. May there not be, in social animals also, traditions from generation

to generation, certain habits prevailing in certain communities in consequence neither of inherited instincts nor of individual experience, but simply because the young ones imitate what they see in their elder fellows?

As is well known, the stingless honey-bees (*Melipona* and *Trigona*) build horizontal combs consisting of a single layer of cells, which, if there is plenty of space, are of rather regular shape, the peripheral cells being all at about the same distance from the first built central one. Now, on February 4, 1874, I met with a nest of a small Trigona ("Abelha preguicosa") in a very narrow hole of an old canella-tree, where, from want of space they were obliged to give to their combs a very irregular shape, corresponding to the transversal section of the hole. These bees lived with me, in a spacious box, about a year (till February 10, 1875), when perhaps not a single bee survived of those which had come from the canella-tree; but notwithstanding they yet continued to build irregular combs, while quite regular ones were built by several other communities of the same species, which I have had.

The following case is still more striking. In the construction of the combs for the raising of the young, as well as of the large cells for guarding honey and pollen, our *Meliponæ* and *Trigonæ* do not use pure wax, but mix it with various resinous and other substances, which give to the wax a peculiar colour and smell. Now I had brought home from two different and distant localities two communities of our most common *Melipona* (allied to *M. marginata*), of which one had dark reddish-brown, and the other pale yellowish-brown wax, they evidently employing resin from different trees. They lived with me for many years, and either community continued, in their new home, to gather the same resins as before, though now, when they stood close together, any tree was equally accessible to the bees of either community. This can hardly be attributed to inherited instinct, as both belonged to the same species, nor to individual experience about the usefulness of the several resins (which seemed to serve equally well), but only, as far as I can judge, to tradition, each subsequent generation of young bees following the habits of their elder sisters.

FRITZ MUELLER

Blumenau, St. Catharina, Brazil, November 14, 1882

The Inventor of the Incandescent Electric Light

IN the "Notes" of NATURE, vol. xxvii. p. 209, M. de Chagny is described as "the first electrician who attempted to manufacture incandescent lamps *in vacuo* about twenty years ago." This invention and its successful practical application (irrespective of cost) was made by a young American, Mr. Starr, and patented by King in 1845. A short stick of gas-retort carbon was used, and the vacuum obtained by connecting one end of this with a wire sealed through the top of a barometer tube blown out at the upper part, and the other end with a wire dipping into the mercury. The tube was about thirty-six inches long, and thus the enlarged upper portion became a torrecellian vacuum when the tube was filled and inverted. I had a share of one-eighth in the venture, assisted in making the apparatus and some of the experiments, and after the death of Mr. Starr all the apparatus was assigned to me. I showed this light (in the original lamp) publicly many times at the Midland Institute, Birmingham, and on two occasions in the Town Hall, all of them more than twenty years ago. The light was far more brilliant, and the carbon-stick more durable, than the flimsy threads of the incandescent lamps now in use. It was abandoned solely on account of the cost of supplying the power. As a steady, reliable, and beautiful light, its success was complete. In "A Contribution to the History of Electric Lighting," published in the *Journal of Science*, November 5, 1879, and reprinted lately in my "Science in Short Chapters," may be found further particulars concerning this invention and its inventor.

W. MATTIEU WILLIAMS

Stonebridge Park, N.W.

The Reversion of Sunflowers at Night

WHILE the fact that sunflowers turn their faces toward the sun in its course during the day is as old as our knowledge of the plant, I am not aware that any record has been made as to the time of night that they turn to the east again after their obeisance to the setting sun.

One evening during a short stay at a village in Colorado, in the summer of 1881, I took a walk along the banks of a large irrigating ditch just as the sun was setting. The wild variety of *Helianthus annuus*, Lin. (= *H. lenticularis*, Douglass) grew abundantly there, and I observed that the broad faces of all the flowers were, as is usual in the clear sunset, turned to the west. Returning by the same path less than an hour afterwards, and immediately after the daylight was gone, I found, to my surprise, that much the greater part of those flowers had already turned their faces full to the east in an icipation, as it were, of the sun's rising.

They had in that short time retraced the semi-circle, in the traversing of which with the sun they had spent the whole day. Both the day and night were cloudless, and apparently no unusual conditions existed that might have exceptionally affected the movements of the flowers.

I doubt not that many persons like myself have supposed that sunflowers remain all night with their faces to the west, as they are when the sunlight leaves them, and until they are constrained by the light of the rising sun, to turn to the east again. It is not my purpose to offer any explanation of the cause of the phenomenon here recorded, but it seems to me improbable that it could have been an exceptional instance; and I only regret that no opportunity has since occurred to me to repeat the observation.

Washington, December 26 C. A. WHITE

Pollution of the Atmosphere

MR. H. A. PHILLIPS, in NATURE, vol. xxvii. p. 127, thinks that the effect of the increasing quantity of hydrocarbons in the air from the combustion of coal will be to make climates more extreme. It seems to me the effect will be the direct contrary. Gaseous and vaporous hydrocarbons absorb heat much more powerfully than air, and whatever makes the atmosphere absorb and retain more solar heat than at present will tend to equalise temperatures between day and night, and also between different latitudes. I think, however, that any possible effect of hydrocarbons will be quite insignificant in comparison with the effect of the watery vapour of the atmosphere, which, as Tyndall has shown, moderates climates by its power of absorbing solar heat.

JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim, December 28, 1882

A "Natural" Experiment in Complementary Colours

ON page 79 of vol. i. of the "Life, Letters and Journal of Sir Charles Lyell," his visit to the Fall of the Rhine at Schaffhausen is described, and he notes that "as the sun shone on the foam it took very much the rose-coloured tint so remarkable on the snow in the Alps."

His experience as regards the colour being observed in the full sunlight seems to differ from that of Mr. Chas. T. Whitmell, which you published in NATURE, vol. xxvi. p. 573.

E. J. BLES

Moor End, Kersal, near Manchester, January 8

BAIRDS' HARE AND ITS HABITS

SEVERAL instances have been recorded in which individual male mammals have produced milk from their mammary glands for the nutriment of their young. But that the young of a mammal should be ordinarily suckled by the male parent is such an extraordinary anomaly that it is very hard to believe it. Yet that such is the case in an American species of hare (*Lepus bairdii*) would seem to be highly probable from observations made by Dr. Hayden and his party during one of their expeditions in the Yellowstone Mountains. In the last number of the *American Naturalist*, Mr. Lockwood gives the following details on this curious subject:—

"In the months of May and June, 1860, Prof. F. V. Hayden and his party of United States explorers found themselves up in the Alpine snows of the Wind River Mountains, where they were detained several days in an attempt to feel their way to the Yellowstone. On May 31 Dr. Hayden declared that a new species of hare was around, as he had observed unusually large hare-tracks in the snow. As the Doctor expressed himself to us:—The tracks were very large, the feet being wide-spread, and the hair thick between the toes, thus really furnishing

the animal with snow shoes." In June, one was captured, and the Doctor named the species *Lepus bairdii*. The animal seemed limited to that small Alpine territory. But one specimen was secured, and no more was heard of this hare until 1872, when Dr. Hayden and party were in that region in the months of August and September. At this time five specimens of Baird's hare were obtained by Mr. C. Hart Merriam, the naturalist to the Hayden Survey. Of these four were adult males, and all had large teats and udders full of milk. The hair round the nipples was wet, and stuck to them, showing that they had just been suckling their young. To make all certain, resort was had to dissection, when the sex was demonstrated. Not only did Mr. Merriam make dissection, but also Dr. Josiah Curtis, a naturalist of the United States Geological Survey, with the same result. In the face of such testimony disbelief would seem discourtesy."

NOTES FROM THE LETTERS OF CAPTAIN DAWSON, R.A., IN COMMAND OF THE BRITISH CIRCUMPOLAR EXPEDITION[1]

July 30, *Fort Chipewyan, on Lake Athabasca*

AFTER practically incessant travel since leaving England, at last I find myself condemned to a week's rest, as there are no boats going to Port Rae until the Mackenzie River boats return. But here we are in the lap of luxury; we get bread, butter, and milk, which we have not tasted for ages, to say nothing of the novel experience of sleeping under a roof and on a bed. I have had a most delightful journey, but it all seems like a dream to look back to: my memory is a kaleidoscope of pine trees, rapids, lakes, and golden sunrises and sunsets. Down stream we travel day and night. At sunset the boats are lashed together, and then the crew go to sleep. It is very nice drifting down in the silence amongst the pines, but bed-time comes at last. I then roll myself in a blanket, lie down, and look at the stars till I fall asleep. At sunrise I wake to find the crew on the shore, boiling their kettle, and a cup of tea is very refreshing. My blanket and my hair, too, I find dripping wet with dew when I wake.

At noon or so we land, and cook more tea, and make breakfast usually off pemmican, which is composed of buffalo flesh dried and pounded, and put in a leather bag with grease poured over it. It is not nice, but it supports life. When we have such a luxury as flour, it is baked into cakes in a frying-pan. We get into the boat again, and eat our breakfast whilst drifting down stream. Bye-and-bye the current becomes more rapid, and at last we see the river disappearing in a cloud of spray. Here is a Portage, so the boats pull to shore and the cargo is landed. The crew then return. I take my place in the boat, and after each man has laid aside his pipe, settled himself in his seat, and got a good grip of his oar, we shove off and dash into the rapid as fast as twelve oars can take us, with shouts of "Hurrah! boys!" (the only English words the Indians know) and "ekwa," a Cree word, meaning "Come on." The guide or steersman stands on a seat in the stern steering the boat with a long oar—a picturesque figure, with his long black hair waving behind him. In a moment we are among the rapids, and seem to sink into a mass of foam, from which we emerge sideways, and are carried towards a projecting rock. Wild exclamations in French from the guide! the bow oarsman seizes a pole, and sends the boat off, and then we spin down the tail of the rapids, not without one or two bumps that make the whole frame of the boat quiver. The whole distance, a mile or two, is done in two or three minutes, and it is not bad fun. After the boats have run the rapids it is dinner-time, and then the crew set to work to carry the cargo over the portage—a work of two or three hours. In some places the boats themselves have to be hauled across on rollers, which is pretty hard work. We continue our way down stream, stopping about 4 o'clock for tea, and at sunset reach another Portage. Here we camp, and in a very short time the tents are pitched, a tree felled to make a camp-fire, and kettles singing thereon. Supper and bed-time make the day. Such is a fair specimen of a day's river travelling. With a fair wind we sail, especially on the lakes.

The crews are Chipewyans; their language is chiefly made up of clicks and gurglings in the throat, and differs altogether from Sioux, Cree, and the other languages spoken further south.

A Roman Catholic priest here showed me a Chipewyan grammar and dictionary that they have composed. There are over sixty sounds in the language, so they have to invent additional letters. There is something Asiatic in the appearance of these Indians, with their small moustache and tufts of hair on their chin, quite unlike the Indian of the plain. They are Roman Catholic.

After leaving Portage la Loche, on July 24, the first day's journey took us down to the Terre Blanche falls; here we had to haul the boats over a small hill, as the river is a succession of falls and rapids for about half a mile; a very pretty place, the river runs between limestone cliffs, crowned with pine trees, and all stained bright orange colour with lichen.

On the 28th we reached the Athabasca, a splendid river, usually half a mile in width, sometimes more. Its course is pretty straight to the north, so we often had a view of some fifteen miles or so down the valley.

On the 29th, having a fair wind, we made a hundred miles. We met two lots of Indians; from the first we got some moose, the first fresh meat we had tasted for a long time, and from the others we got some raspberries and asketoon berries, which were very refreshing.

As we drifted down the river, the pines began to give place to poplar, the poplar to willow, and the willow to reeds, till at last we saw Lake Athabasca before us, a rocky coast to the north, and to the east water as far as the eye could reach.

A fresh breeze took us across the lake in two hours, and we received a hospitable welcome at this place, together with all sorts of luxuries that had become quite strange to us.

This is quite a large place; there are about a dozen houses, two churches, two bishops, a sisterhood, and some missionaries. The country is rocky, and most desolate. To the south and west the great lake stretches away to the horizon, and the land view is composed of hills of reddish granite, no soil, plants growing here and there out of occasional crevices, and a few stunted firs scattered about. There are woods in the valleys, but the trees are of no size. No sound breaks the stillness but the weird cry of the loon, a sort of maniacal laugh that is almost a wail; and the solitude is heightened by the reflection, that for 1000 miles north, south, east, and west all is wilderness.

Towards the lake the view is pretty, as there are many islands covered with pines.

The weather is cooler than it has been, I am glad to say. For days we had the thermometer at 85° and 86°, and even higher; but though hot, the summers are short, and I think that of this year is over. The mosquitos, at any rate, are beginning to disappear, and now the climate is nearly perfect, like the best English summer weather.

August 5.—There was a fine aurora last night: a curtain of flame seemed to descend from the sky nearly overhead and right across the sky, and after waving about for a few moments, died away again. Yesterday I went to see the Roman Catholic Mission; they have quite a pretty church, which has been built some thirty years. I was also taken to see the sisters, of whom there are six. They all seemed very flourishing, and have a very nice house.

[1] Continued from p. 105.

August 6, *Sunday*.—I was at the English church is morning. It is a nice little church, and there was a ngregation composed of the Hudson's Bay people, venty or thirty. Most of the Hudson's Bay people are cotch, many coming from the Orkneys. The Bishop mpas is very pleasant, he is a great traveller, and has ved amongst the Esquimaux at the mouth of the Mackenzie River, and he works very hard.

August 9.—The weather has been stifling hot, 89° indoors, for the last three days, quite like the West Indies. Yesterday I went over to see a performance at the Roman Catholic Mission of the school children, got up by the sisters in our honour. They sang, and acted, and danced remarkably well. They have very good memories I am told.

It is curious living together without money, as one does in this country. Everything is done by barter, the unit of value being a skin; the average value of a beaver skin is said to be worth twenty ducks, or forty white fish, or twenty plugs of tobacco, so that for a plug of tobacco (about an ½ oz.) one can get a duck or two white fish, a large fish about two feet long, and very good eating. This place, like all other habitations in the north-west, swarms with large wolf-like dogs. These are used in winter for drawing carrioles, and a team of four dogs will draw 500 lbs. or more. The Indians use them too, in summer, as pack animals.

The boats have just made their appearance, four black specks on the horizon to the north, so we shall be off in a few hours.

THE SWEDISH EXPEDITION TO SPITZBERGEN, 1882

THE results of the researches of the expedition despatched to Spitzbergen last summer by the Swedish Academy of Sciences, under the eminent *savants* Baron G. de Geer and Dr. Nathorst, for the study of the geological and geographical features of the island, are very interesting. In the first instance, these gentlemen have drawn two maps, showing the exact geographical features of the island, as compared with those prepared by two previous expeditions. Of these, one shows the outlines of the fjords and valleys in the southern part of the island, with the boundary of the inland ice, and the other the relative depth of the seas around Spitzbergen and Scandinavia. From the latter it appears, that these two land-formations are really elevated ridges on a comparatively level plateau, which sinks abruptly in the ocean west of Spitzbergen. In the second instance, the expedition has ascertained that the deep fjords and narrow valleys of the island have not been formed by upheaval of the terrestrial crust or by strong water-courses, but are due to the action of glaciers during the Glacial period, while from the marks on the rocks of the Beeren Island, it may be assumed that the Spitzbergen glaciers extended even so far.

At the close of the Glacial period a sudden subsidence, followed by a still greater rising of the shores, both of Spitzbergen and Scandinavia, most probably took place, which is demonstrated by the discovery, in Scandinavia as well as Spitzbergen of old gravel beaches and the shells of salt-water mussels far inland. The existence in Spitzbergen of some of the most characteristic species of the Scandinavian flora and fauna, may perhaps be explicable by migration from Scandinavia, at a period when the plateau between the two ridges was above the level of the sea, we may assume, shortly after the close of the Glacial period. It seems impossible to explain otherwise how, for instance, birds, particularly those living on land, could have found their way to this island, some 700 miles distant from the Scandinavian peninsula.

At the same period, the common Scandinavian "Blaamusling," *Mytilus edulis*, and a few other species have, no doubt, also migrated into the island. This species is now, however, extinct, but the large quantities of shells found on the shores indicate that at one time it must have been common enough. The latter circumstance seems to prove that the climate of Spitzbergen at an earlier period was much milder than at present, and corroborates also the theory of a connection having existed between Spitzbergen and Scandinavia about the Glacial period, as such a land-barrier would have caused the eastern arm of the Gulf Stream, which now flows by the North Cape, to have taken a more northerly direction, and thus carried the softening elements of a southern clime to the now desolate rocks in the Arctic Ocean. C. S.

THE INCREASE IN THE VELOCITY OF THE WIND WITH THE ALTITUDE

THE fact that the upper strata of the atmosphere as a rule move more rapidly than those near the earth's surface, has long been inferred on theoretical grounds, though little direct evidence beyond the marvellous and often unexpected voyages of aëronauts, or casual observation of the clouds, has hitherto been furnished in its favour. The practical value of this fact is beginning to be felt by engineers since the investigations undertaken by Mr. T. Stevenson in 1876, and more recently (see *Journal* of Scottish Meteorological Society, vol. v. pp. 103 and 348), showed that even for moderate heights the old notion of assuming the wind to be of uniform velocity at all altitudes was seriously in error, and that to rely upon it in the case of lofty structures might entail disastrous consequences.

While Mr. Stevenson's experiments have shown that the wind's velocity increases very considerably, especially near the surface, they do not touch the question of the increase noticed at great heights, nor can the formulæ or conclusions derived from them be said to throw any light on a matter which evidently contains the germs of many important truths for the meteorologist.

Where the engineer ends in fact the meteorologist may be said to begin; but in this case the engineer ends a little too soon, since Mr. Stevenson's latest experiments terminate at the top of a pole only 50 feet high, where he leaves us with a formula "believed to be sufficiently accurate for practical purposes," and which is said to give the velocity for "*great heights* above sea-level." Whence Mr. Stevenson obtains this formula, or on what data he believes it to be approximately correct, we are not told, and here the question is left in a state of uncertainty for greater heights, in which we trust neither engineers nor meteorologists will allow it long to remain. It might even be advantageous to the former, if instead of trusting to a few empirical formulæ, they would ask the meteorologists what they knew about the matter, and joined with them in endeavouring to discover a rational formula which would yield satisfactory results at all elevations.

Theoretically the main factor at small elevations in determining the increase of velocity, would appear to be the diminution of friction as we rise above the surface, and as this must occur most decidedly near the surface, so the velocity must increase in the first few feet "per saltum." Mr. Stevenson's experiments and curves show this very clearly. Indeed up to a height of 15 feet the increase is so sudden, so irregular, and so clearly dependent on the nature of the surface, that no attempt has been made to include this space within a formula.

There is, however, another factor which acts *positively* in the same direction, and which, while operating for the most part at great heights, where its influence ultimately predominates to the exclusion of the friction factor, must be felt to some extent at comparatively moderate elevations.

I allude to the general increase in the barometric

gradient with the height above the earth's surface, due to the general temperature gradient between the equator and the poles, in conjunction with the earth's rotation. This fact has been thoroughly investigated by Mr. Ferrel of the U.S. Coast Survey, and the results given in his "Meteorological Researches," vol. i. In this work he has given on p. 45 the mean west-easterly component of the velocity at the surface due to the causes just mentioned, and the term by which this increases with the height (in metres) for every fifth degree of latitude on the mean of all longitudes, for the months of January and July, and for mean annual temperatures, calculated from the observed barometric pressures and temperatures in every part of the world.

For latitude 50° the eastward velocities at the surface and increment terms for the elevation are as follows in two different measures.

	Mean temperatures.	January.	July.
Miles per hour	$3{\cdot}35+8{\cdot}6h$	$3{\cdot}97+12{\cdot}1h$	$2{\cdot}73+5{\cdot}1h$
Feet per second	$4{\cdot}91+{\cdot}0024h$	$5{\cdot}82+{\cdot}0033h$	$4{\cdot}00+{\cdot}0014h$

where h represents the height in miles and feet respectively.[1]

Owing to *this cause alone* therefore the eastward (and therefore in our latitudes the prevailing) motion of the atmosphere will be increased on the mean of the year by 8½ miles per hour at a height of 5280 feet, or by 2⅝ miles per hour at a height of about 1300 feet.

The increase in the horizontal velocity which results from the joint action of these two factors, is thus probably very different from that which would arise from a mere diminution of friction alone, since at great heights this would theoretically become almost insensible.

For a thoroughly satisfactory solution of the matter, nothing will avail except anemometrical observations made at every possible elevation (preferably, as I lately suggested in a paper read before the Meteorological Society, with instruments attached to kite-strings), but in the absence of these at present, it may be worth while to use some excellent observations on the velocity of different cloud-layers recently communicated to the Austrian *Zeitschrift für Meteorologie*, by Dr. Vettin,[2] for the purpose of showing the complete breakdown of Mr. Stevenson's formula when applied to "great heights above sea-level."

The following table, which is taken from Dr. Vettin's paper, gives the mean velocity of the clouds from all directions, at five altitudes to which they respectively belong, and which, together with their velocities, have been measured by methods described in detail in the paper from which it is extracted :—

TABLE I.

Name of station.	Barometric pressure.[3]	Height in feet.	Number of observations.	Mean velocity in feet per second
	in.			
Upper cirrus	11·968	23,000	879	59·5
Under cirrus	17·953	12,800	1047	51·8
Cloudlets[4]	22·441	7200	1588	35·0
Cloud	25·733	3800	1871	30·4
Under cloud	28·127	1600	1292	37·4
Wind (sea-level)	29·922	0	4168	19·8

It will be seen from this table that while there is a rapid increase in the velocity of the wind through the first 1600 feet, an abrupt diminution occurs between this height and 3800 feet, after which the motion again increases at a more moderate rate.

Now Mr. Stevenson's formula for heights above 50 feet is $\frac{V}{v} = \frac{H}{h}$, where V, v, H, h, are the velocities and heights at the upper and lower stations respectively. If we apply this formula to the preceding table and calculate the heights at the higher levels from those at the lower ones, we get for the most favourable cases the following values :—

Observed		Calculated	
Velocity.	Height.	Velocity.	Height.
37·4	1,600	259·2	12,800
51·8	12,800	113·1	23,680

which are so absurdly in excess of those observed at the same levels, and so far beyond what we might reasonably expect as to render it doubtful whether this formula is true for any height above the first 100 feet. Even the formula which is supposed by Mr. Stevenson to fail above 50 feet gives better results than this one at the higher levels.

This formula is $\frac{V}{v} = \sqrt{\frac{H+72}{h+72}}$, and from the same observed values as those used above gives the following calculated values :—

Height.	Velocity.
12,800	103·7
23,000	69·3

but even these are far in excess of those observed. Both formulæ moreover fail lamentably up to 1600 feet, for even if we assume that 19·8 represents the velocity, not at sea-level, but at an elevation of 100 feet above it (an exceedingly favourable assumption since the velocity at this height would considerably exceed that at sea-level), the first formula would make the velocity at 1600 feet *four* times, and the second more than *three* times that actually observed by Dr. Vettin.

It is plain, therefore, that both formulæ must fail considerably below 1600 feet, and until further evidence is furnished it would seem probable that neither of them give correct results much above 100 feet or so.

For practical engineering purposes no doubt they would succeed only too well, since they would probably give a maximum velocity far in excess of the truth, and this, judging from examples such as the Tay Bridge, would be no disadvantage when the force of the wind enters into engineering calculations.

It would surely be better, however, if we could arrive at a somewhat closer approximation to the truth, and better still if we could arrive at the truth itself. This, as I have already pointed out, will be only accomplished for the lower strata by further experiment in the same direction as that already followed by Mr. Stevenson, modified by attaching anemometers to kite strings, and for the upper strata, which chiefly concern the meteorologist by observations of the clouds similar to those made by Dr. Vettin.

Meanwhile, however, I have found a formula which gives very much more satisfactory values at the higher levels than those furnished by Mr. Stevenson. This formula is—

$$\frac{V}{v} = \sqrt[4]{\frac{H}{h}}$$

And although I do not expect it will be found to hold *very near* the surface, it certainly accords, omitting the anomalous case at 3800 feet, from 1600 feet to 23,000 feet, or through a range of 21,400 feet, very closely with the values observed by Vettin.

The figures observed and those calculated from this formula are as follows :—

TABLE II.

Height of lower stratum.	Height of upper stratum.	Velocity Observed.	Velocity Calculated.
3,800	7,200	35	35·6
1,600 } 7,200 }	12,800	51·8	51·5[1]
12,800	23,000	59·9	59·5
23,000	41,000	67[2]	68·7

[1] The mean of the two. [2] Calculated by Vettin.

[1] It must be noted that the surface velocities given in this table are somewhat in excess of the truth, owing to the neglect of surface friction, but this does not affect the increment terms to any large extent.

[2] *Zeitschrift für Meteorologie*, Band xvii., July and September Heft; "Die Luftströmungen über Berlin."

[3] Reduced from the original figures in millimetres. [4] Wölkchen.

Moreover, if we assume that 19·8 is the velocity at 100 feet, the velocity at 1,600 feet calculated from it by this formula would be almost exactly *equal* to that observed, instead of *four* times as much as it was when calculated from $\frac{V}{v} = \frac{H}{h}$. The empirical formula $v = 28{\cdot}1 + 0{\cdot}2\sqrt{h}$, where v is the velocity in feet per second, and h is the corresponding height in feet, gives for the four elevations above, results in close agreement with those observed, but it would probably fail below 1,600 feet.

Mr. Stevenson in his first paper uses a formula $\frac{F}{f} = \sqrt{\frac{H}{h}}$ where Ff are the forces ("pressures" I suppose is meant by this objectionable word) at the two levels corresponding to H and h.

In his second paper he says he prefers the formula $\frac{V}{v} = \frac{H}{h} = \frac{P}{p}$, where P and p are the pressures at the two levels to the formula $\frac{V}{v} = \frac{\sqrt{H}}{\sqrt{h}}$, from which and some other remarks it would appear that pressure and velocity are considered to vary directly with each other. This is a notion which is certainly at variance not only with the hitherto generally accepted empirical formulæ, but is distinctly contrary to the results lately deduced by Mr. Ferrel from the hydrodynamical theory (see Van Nostrand's *Engineering Magazine*, vol. xxvii. p. 141). The formula usually found in the text-books is $p = {\cdot}00492\, v^2$, and the one deduced by Ferrel is

$$p = \frac{{\cdot}0027}{1 + {\cdot}003665\, t}\,\frac{P}{P'}\,v^2,$$

where $P\,P'$ are the barometric pressures at the level under consideration and at sea-level respectively, and t is the temperature in degrees Centigrade; and though from the latter formula it is evident that the pressure at the higher levels will be less for the same velocity than below (at the height of Mont Blanc, for example, it would be reduced by about one-half) there is nothing to lead us to infer that $\frac{V}{v} = \frac{P}{p}$.

If in the formula $\frac{F}{f} = \sqrt{\frac{H}{h}}$ which was discarded by Mr. Stevenson, we make the ordinary assumption that $\frac{F}{f} = \frac{V^2}{v^2}$ we get the formula which I have already shown gives results which agree closely with the values observed by Vettin above 1,600 feet, and which even below this height is much nearer the truth so far as can be inferred from the slender data employed, than the formula preferred by Mr. Stevenson.

If we take the heights as abscissæ, the curve traced out by the velocity-ordinates will be much flatter than the ordinary conical parabola, and at great heights will approximate very nearly to a straight line parallel to the axis. Ferrel's increment-term makes it a straight line all through, but his formula assumes that the temperature gradient between the pole and the equator is the same above as at the earth's surface, it leaves out friction altogether, and also supposes the velocity for the same gradient to be the same at all heights, whereas according to theory it should increase with the height in the ratio $\frac{\cos i}{P}$, where i is the angle the wind makes with the isobar, and P is the barometric pressure at the level under consideration. Notwithstanding these omitted factors, of which the first and last probably tend to destroy each other, it will be found that the addition of the increment corresponding to each altitude to the velocity at the surface observed by Vettin gives us the following fair approximation to the values actually observed, though the calculated values are too small at 1600 feet and too large at 23,000 feet by just about the same amount:—

Height.	Velocity observed.	Calculated from Ferrel's Formula.
23,000	59·5	75·0
12,800	51·8	50·5
7,200	35	37·0
3 800	30·4	28·9
1,600	37·4	23·6
0	19·8	

In conclusion, it is evident that, quite apart from the meteorological side of the question, more investigations like those undertaken by Mr. Stevenson, are urgently required to determine the actual rate of increase of the velocity at moderate heights, from which a formula like the one I have recommended may be deduced, which will yield values more within the range of probability than those furnished by the one which is apparently supposed to suffice for the rest of the atmosphere after we have reached the top of the fifty-foot pole.

E. Douglas Archibald

KRAO, THE "HUMAN MONKEY"

Through the courtesy of Mr. Farini, I have had a private interview with this curious little waif, which he is now exhibiting at the Royal Aquarium, Westminster, and for which he claims the distinction of being the long-sought-for "missing link" between man and the Anthropoid apes. Krao certainly presents some abnormal peculiarities, but they are scarcely of a sufficiently pronounced type to justify the claim. She is, in fact, a distinctly human child, apparently about seven years old, endowed with an average share of intelligence, and possessing the faculty of articulate speech. Since her arrival about ten weeks ago in London, she has acquired several English words, which she uses intelligently, and not merely parrot-fashion, as has been stated. Thus, on my suddenly producing my watch at the interview, she was attracted by the glitter, and cried out *c'ock, c'ock*, that is, *clock, clock!* This showed considerable powers of generalisation, accompanied by a somewhat defective articulation, and it appears that her phonetic system does not yet embrace the liquids *l* and *r*. But in this and other respects her education is progressing favourably, and she has already so far adapted herself to civilised ways, that the mere threat to be sent back to her own people is always sufficient to suppress any symptoms of unruly conduct.

Physically Krao presents several peculiar features. The head and low forehead are covered down to the bushy eyebrows with the deep black, lank, and lustreless hair, characteristic of the Mongoloid races. The whole body is also overgrown with a far less dense coating of soft, black hair about a quarter of an inch long, but nowhere close enough to conceal the colour of the skin, which may be described as of a dark olive-brown shade. The nose is extremely short and low, with excessively broad nostrils, merging in the full, pouched cheeks, into which she appears to have the habit of stuffing her food, monkey-fashion. Like those of the anthropoids her feet are also prehensile, and the hands so flexible that they bend quite back over the wrists. The thumb also doubles completely back, and of the four fingers, all the top joints bend at pleasure independently inwards. Prognathism seems to be very slightly developed, and the beautiful round black eyes are very large and perfectly horizontal. Hence the expression is on the whole far from unpleasing, and not nearly so ape-like as that of many Negritos, and especially of the Javanese "Ardi," figured by me in NATURE, vol. xxiii. p. 200. But it should be mentioned that when in a pet, Krao's lips are said to protrude so far as to give her "quite a chimpanzee look."

Apart from her history one might feel disposed to

regard this specimen merely as a "sport" or *lusus naturæ*, possessed rather of a pathological than of a strictly anthropological interest. Certainly isolated cases of hairy persons, and even of hairy families, are not unknown to science. Several were figured in a recent number of the Berlin *Zeitschrift für Ethnologie*, and, if I remember, both Crawfurd ("Journal of an Embassy to Ava") and Col. Yule ("Mission to the Court of Ava") speak of a hairy family resident for two or three generations at the Burmese capital. This family is reported to have come originally from the interior of the Lao country, and in the same region we are now told that little Krao and her parents, also hairy people, were found last year by the well-known eastern explorer, Mr. Carl Bock. Soon after their capture, the father appears to have died of cholera, while the mother was detained at Bangkok by the Siamese Government, so that Krao alone could be brought to England. But before his death a photograph of the father was taken by Mr. Bock, who describes him as "completely covered with a thick hairy coat, exactly like that of the anthropoid apes. On his face not only had he a heavy, bushy beard and whiskers, similar in every respect to the hairy family at the court of the King of Burmah, who also came from the same region as that in which Krao and her father were found; but every part was thoroughly enveloped in hair. The long arms and the rounded stomach also proclaimed his close alliance to the monkey-form, while his power of speech and his intelligence were so far developed that before his death he was able to utter a few words in Malay."

Assuming the accuracy of these statements, and of this description, little Krao, of course, at once acquires exceptional scientific importance. She would at all events be a living proof of the presence of a hairy race in Further India, a region at present mainly occupied by almost hairless Mongoloid peoples. From these races the large straight eyes would also detach the Krao type, and point to a possible connection with the hairy, straight-eyed Aino tribes still surviving in Yesso and Sakhalin, and formerly widely diffused over Japan and the opposite mainland.[1]

A. H. KEANE

FIGURE OF THE NUCLEUS OF THE BRIGHT COMET OF 1882 (GOULD)[2]

ALTHOUGH this comet presented a beautiful spectacle, when seen with the naked eye, I have been disappointed at the small amount of work which I have been able to do in the way of accurate observation. I give herewith the only two good sketches which I have been able to make. The aperture employed was 15 inches, and the power was 145 diameters.

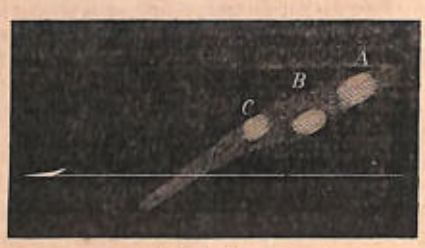


1882, October 13.

1882, *October* 13.—(See the figure.) The nucleus is curved as in the drawing. It consists of three masses. I am sure of a break at *a*; tolerably sure of the break at *b*, and I suspect a break at *c*, but I am not certain of it.

1882, *October* 14.—The night is very poor. (In general the appearances of last night are confirmed.) The nucleus, is about 1′ long.

1882, *October* 17.—(See the figure.) There are three masses, plainly separated. *B* is farther north than the line *A*—*C* by 3—4″. There is a dark division between each pair of masses. *B* and *C* are nearly in the parallel.



1882, October 17.

The brush of light from the mass *A* toward the east, comes from the south side of *A*, as it is drawn. From the W. end of *A* to the E. end of the brush of light, is about 15″.

1882, *October* 18.—The dark space between *A* and *B* is about 10″; it is as wide as *A* itself, and wider than on October 17. *C* is certainly seen as a separate mass; *A* and *B* are bright and stellar in appearance, more so than on October 17. *C* is, however, fainter than then. The dark axis of the tail extends quite up to the coma.

1882, *October* 19.—Cloudy. The nucleus is seen as before. *A* and *B* are seen, as also the dark space between them. *C* is not seen, but this is probably on account of the unsteady air.

I regret that my opportunity did not allow me to make any further sketches of value.

Washburn Observatory, University of Wisconsin,
Madison, November 3, 1882

NOTES

THE office of Director of the Geological Survey of Scotland, vacant by the promotion of Mr. Geikie to be Director-General, has been filled up by the appointment of Mr. H. H. Howell, District Surveyor of the Geological Survey of Scotland during the earlier years of its progress, but who since the separation of the Scottish branch of the establishment in 1867, has been continuously employed in England, where he has personally surveyed large tracts of the northern counties, and where for some years past he has had the direct personal supervision of the whole of the field-work in that district. He will not be able to enter fully on his duties in Scotland until the area now under his charge in the north of England has been completely surveyed. The promotion of Mr. Howell having caused a vacancy in the rank of District Surveyor, Mr. W. Whitaker has been appointed to the post. This geologist is well known for his detailed surveys of the Tertiary deposits of the London basin. He is at present engaged in the survey of Norfolk.

THE United States Transit of Venus expedition, under Prof. Newcomb, arrived at Plymouth on Sunday as passengers by the Union Steamship Company's steamer *Moor*, from Cape Town. They report that their observations were made at Wellington, fifty-eight miles from Cape Town, under extremely favourable conditions, two good observations of internal contact and 236 photographs being obtained, of which more than 200 can be measured.

THE annual general meeting of the Association for the Improvement of Geometrical Teaching will be held, through the kindness of the Council, at University College, Gower Street, on Wednesday, the 17th instant, at 11 a.m. In addition to the usual routine business, the president (R. B. Hayward, F.R.S.)

[1] See my paper on "Aino Ethnology" in NATURE, vol. xxvi. p. 524.

[2] Paper by Prof. Edward S. Holden, in the *American Journal of Science and Arts*.

will propose "that the Committee for Elementary Plane Geometry be instructed to publish Part I of the Plane Geometry, and to take such steps as they may deem advisable to secure its recognition as a basis of instruction and examination in geometry." It will be in the recollection of some of our readers that the object of the Association was extended at the last annual meeting, so as to include the effecting of improvements in the teaching of elementary mathematics and mathematical physics. This extension has met with great approval, and the novelty of this year's meeting will be the reading of three papers: (1) "The Teaching of Elementary Mechanics," by W. H. Besant, F.R.S. (2) "The Basis of Statics," by Prof. H. Lamb, of the Adelaide University. (Prof. Lamb is of the opinion that the true and proper basis of statics is to be sought for in the principles of linear and angular momentum). (3) "Notes on the Teachings of Dynamics," by Prof. Minchin. The reading of the papers will be followed by a discussion, in which it is hoped that Prof. G. Carey Foster, F.R.S., Prof. Minchin, and others will take part. The papers will be read at the afternoon meeting which begins at 2 p.m. The two present honorary secretaries will resign office; Mr. Levett, in consequence of the pressing necessity of his other duties, and Mr. Tucker, in consequence of the state of his health, which compels him to withdraw from some of his engagements; both gentlemen, however, hope to remain on the council and act as *amici curiæ* to their successors in office.

THE well-deserved honour of C.I.E. has been conferred upon Surgeon-Major James Edward Tierney Aitchison (Bengal Army), F.L.S., who did such excellent botanical work with Sir Frederick Roberts's force in the Kuram Valley during the Afghan war.

GEOLOGISTS will regret to hear that one of the most promising of the younger members of their number, Mr. E. B. Tawney, of the Woodwardian Museum, Cambridge, died suddenly at Mentone on the 30th ult.

THE death is announced, on December 22 last, of Dr. Carl Hornstein, professor of theoretical and practical astronomy, and director of the observatory in the Carl Ferdinands University, Prag, at the age of fifty-eight years.

THE thirty-sixth annual general meeting of the Institution of Mechanical Engineers will be held on Thursday, January 25, and Friday, January 26, at 25, Great George Street, Westminster. The chair will be taken by the president, Percy G. B. Westmacott, Esq., at half-past seven p.m. on each evening. The following papers will be read and discussed:—Report on the hardening of steel, by Prof. F. A. Abel, C.B., F.R.S, of Woolwich; on the molecular rigidity of tempered steel, by Prof. D. E. Hughes, F.R.S., of London; on the working of blast furnaces, with special reference to the analysis of the escaping gases, by Mr. Charles Cochrane, of Stourbridge, vice-president; on the St. Gothard tunnel, by Herr E. Wendelstein, of Lucerne; on the strength of shafting when exposed both to torsion and end-thrust, by Prof. A. G. Greenhill, of Woolwich.

THE old female Hippopotamus (*Adhela*) presented to the Zoological Society in 1853 by the then Viceroy of Egypt, died in the Gardens on the 16th ult., after having for some time past exhibited manifest signs of old age. Her mate (*Obaysch*) died in 1877, after having lived twenty-seven years in the Gardens. It is thus evident that about thirty years is the extreme limit of Hippopotamine existence, as it is not at all likely (judging from the state of the teeth and bones) that either of these animals would have been able to support existence so long in its native wilds, as under the favourable circumstances in which it lived in the Regent's Park.

DR. BLASIUS of Brunswick has recently shown that the fossil remains of a species of Souslik, found in various parts of Northern Germany, which are usually attributed to *Spermophilus altaicus*, really belong to *S. rufescens*, Keys. et Bl. It is probable that the cave-bones from the Mendip Hills, upon which Dr. Falconer established his *Sp. erythrogonoides* (Pal. Mem., ii., p. 453), are really of the same species, and that this Rodent, now driven far east into the steppes of Orenburgh (like other members of the Steppe-fauna), formerly extended all over Northern Europe, and even into the British Islands.

RECENTLY, our readers may remember, Miss Baxter of Balgavies, sister of Sir David Baxter, and aunt of the Right Hon. W. E. Baxter, and the late Dr. Baxter, Procurator-Fiscal of Dundee, gave jointly 150,000*l.* for the endowment and erection of a college in Dundee. Buildings have been acquired, professors appointed, and the work of the college will soon be begun. Miss Baxter has just given another 10,000*l.* to provide a laboratory, and the trustees of the late Dr. Baxter also 10,000*l.* to found a Chair of Law.

AS the late M. Gambetta was a member of the Society of Dissection, an autopsy of his body was made. The weight of his brain was found to be 1100 grams; M. Mathias Duval, Professor in the Faculty of Medicine, found the structure of the brain to be very fine, and the third convolution, which M. Broca associates with the speech-fying faculty, to be remarkably developed.

THE project of the United States for establishing an universal meridian has been sent to the Paris Academy of Sciences for approval. It is expected that Great Britain may object to this measure, and it has been proposed that, in consideration of the services rendered to geography by England, that the Greenwich meridian should be selected as the start-point for time and longitude.

OUR Paris Correspondent writes that the second Paris inundation is developing its ravages with peculiarities which prove that modern engineers do not pay sufficient attention to the effects of their works on the *régime* of the stream they profess to regulate. The level of the Seine is just as high at Charenton as it was in 1876, although it is 20 centimetres less elevated at Pont Royal, where it reaches only 7 metres. The reason of this difference is that an ignorant Municipal Council authorised the building of a bridge which crosses the river obliquely, and a new quay at Bercy, where in some places the dimensions of the bed of the stream have been diminished by not less than 55 metres. If the rains continue it is feared that one of the Paris bridges, the Invalides, will be carried away, which will produce real disaster.

PROF. BÖRNSTEIN, of Berlin, has brought out a small work on meteorology under the title of "Regen oder Sonnenschein." In conjunction with Prof. Landolt he has also nearly completed an important work ("Physikalisch-Chemische Tabellen") containing all the most reliable determination of constants required in chemical and physical work, some of which will be published in a collected form for the first time.

MR. J. P. MCEWEN, of Hong Kong, under date November 28, 1882, sends us some observations of the comet, taken on the morning of the 27th:—26d. 15h. 43m. 17s. mean time at place, the distance from Sirius measured by sextant was 34° 32′; 26d. 15h. 50m. 30s. mean time at place, the distance from Procyon was 39° 31′; longitude in time, 7h. 36m. 40s. A line drawn from the small brightest star in the lower part of the sword-scabbard of Orion through Sirius almost exactly passed through the nucleus of the comet; the apparent length of the tail was about twice that of Orion's belt. It was getting very indistinct, and on the 27th, owing to the bright moonlight, was more so than if the night had been dark and clear. Mr. McEwen has seen the comet several times, and when at its greatest brilliancy,

stars of the 4th or 5th magnitude could be distinctly seen through the tail. The tail pointed in a direction about midway between Sirius and Procyon. M. Dechevrens, the director of the Zi-ka-Wei Observatory (near Shanghai) has devoted a good deal of attention to this comet, the result of which will directly be published.

Amateur Mechanics is the name of a new illustrated monthly Magazine, conducted by Mr. P. N. Hasluck, and published by Trübner and Co.

We have received from the U.S. Naval Observatory the results of the observations made to determine the longitude of the observatory of the J. C. Green School of Science, Princeton, N.J. The final result is that the latter is oh. 9m. 34s.·538 east of the central dome of the observatory.

The earthquake in Panama on November 7 was followed by a violent shock on November 13 at 2.30 a.m. It was observed also at Taboga and Colon. It is remarkable that all the Central American earthquakes since August last have occurred between midnight and daybreak. Their general direction was invariably from north to south, and it is supposed that they proceeded from one and the same cause. The West Indian cable broke, at a point about thirty miles from land, during a violent shock. The centre of the disturbance seems to lie near the West Indian Isles. During the second week of December seven shocks were felt in the Spanish province of Almeria. On December 8 at 10.1 p.m. a fearful shock lasting four seconds was felt at Tecuci (Roumania). Its direction was from south-east to north-west. Another earthquake is reported from Hermagor (Carinthia). It occurred on December 10 at 2 a.m., and was preceded by a terrible thunderstorm.

An "Illustrirte Bienenzeitung," organ for the propagation of rational apiculture, will be edited by Prof. Adolphson of Zürich, beginning on the 1st inst.

In the Pelion district a moderately violent earthquake occurred on December 11, but no damage was done. Upon the island of Santorin new volcanic activity has recently been noticed; also in the subterranean volcano which formed near Missolunghi.

The additions to the Zoological Society's Gardens during the past week include a Himalayan Bear (*Ursus tibetanus*) from Burmah, presented by Capt. Connor; two Bronze Fruit Pigeons (*Carpophaga ænea*) from India, presented by Mrs. A. H. Jamrach; four Barred-shouldered Doves (*Geopelia humeralis*) from Australia, presented by Mr. Ernest L. Bentley; a Lesser Sulphur-crested Cockatoo (*Cacatua sulphurea*) from Moluccas, presented by Mr. K. Digby; a Gannet (*Sula bassana*), British, presented by Mr. Thomas Keen; a Cape Bucephalus (*Bucephalus capensis*) from South Africa, presented by Mr. H. Pillans; a White-fronted Lemur (*Lemur albifrons* ♀) from Madagascar, four Wood Thrushes (*Turdus mustelinus*), a Golden-winged Woodpecker (*Colaptes auratus*) from North America, two Cirl Buntings (*Emberiza cirlus*), two Crested Grebes (*Podiceps cristatus*), a Razorbill (*Alca torda*), a Bar-tailed Godwit (*Limosa lapponica*), a Red-throated Diver (*Colymbus septentrionalis*), British, purchased.

OUR ASTRONOMICAL COLUMN

The Total Solar Eclipse on May 6.—The right ascensions and declinations of the moon for 1883, both in the *Nautical Almanac* and the *American Ephemeris*, depend upon Hansen's Tables, with the recent corrections of Prof. Newcomb. They furnish as accurate positions as are obtainable from existing tabular data, and it will be of interest to trace their bearing upon the circumstances of the total eclipse of the sun which crosses the Pacific on May 6. On laying down the belt of totality upon the Admiralty chart of this ocean, it appears that the following islands are included within it, viz.:—Rance, Buffon, Beveridge, Flint, Caroline, and Chanel Island (in the Marquesas); the positions read off from the general chart or for Flint, Caroline, and Chanel Island, from the enlarged Admiralty charts are as follow:—

Rance Island,	Long.	176	22	West.	Lat.	24	20	South.
Buffon ,,	,,	170	0	,,	,,	20	39	,,
Beveridge,,	,,	167	50	,,	,,	20	0	,,
Flint ,,	,,	151	50	,,	,,	11	25	,,
Caroline ,,	,,	150	6	,,	,,	9	54	,,
Chanel ,,	,,	140	31	,,	,,	7	55	,,

From direct calculation for each of these points the following local mean times of beginning of totality, the duration of the same, and the sun's approximate altitude at the time, result:—

	Totality begins May 6. h. m. s.		Duration. m. s.		Sun's Altitude. °
Rance Island,	8 47 36	a.m.	3 27	...	29
Buffon ,,	9 22 18	,,	4 20	...	38
Beveridge,,	9 34 48	,,	4 1	...	41
Flint ,,	11 19 43	,,	5 26	...	61
Caroline ,,	11 33 4	,,	5 7	...	63
Chanel ,,	0 43 32	p.m.	1 47	...	63

It should be mentioned that the semi-diameter of the sun has been taken from the *Nautical Almanac*; that of the moon was obtained from her horizontal parallax, using the factor 0·2725. The duration of totality at Sohag in Egypt in the eclipse of last May was exactly given by this arrangement.

The Minor Planets.—The part of the *Berliner Astronomisches Jahrbuch* for 1885, containing ephemerides of the minor planets for 1883, has been issued to the various observatories in advance of the publication of the annual volume. It contains approximate places for every twentieth day of 224 of these bodies, the latest being No. 225, with accurately calculated opposition ephemerides of 43, each extending over about five weeks; this division of the *Jahrbuch* occupies upwards of one hundred pages.

There are six cases during the year where the planets approach the earth about opposition, within her mean distance from the sun. On June 22 *Phocea* is at a distance of 0·93, declination + 16°; on July 12 the distance of *Clio* is 0·96, declination −35½°; on August 1 that of *Isis* is 0·90, declination −28°; on October 1 that of *Polyhymnia* is 0·98, declination +8½°; on October 20 that of *Virginia* is 0·98, declination +13°, and on December 11 *Flora* in perigee is at a distance of 0·97, with declination +18°. Galle's method of determining the solar parallax, so strongly advocated and ably applied by Mr. Gill, is not likely to fail for want of opportunities of applying it. As regards the magnitude near opposition we have in the case of *Phocea* 9·0; *Clio*, 10·2; *Isis*, 8·8; *Polyhymnia*, 9·7; *Virginia*, 9·9; and *Flora*, 8·2.

During the year 1883 four of these planets descend below 14m., from coming into opposition not far from aphelion.

Comet 1882 *c*.—Mr. Gill has secured five complete observations of this comet (discovered by Mr. Barnard in September) on the meridian S.P., with the transit-circle at the Cape of Good Hope, between November 11 and 30, so that places for upwards of a fortnight after the perihelion passage will be available for calculation.

THE EDUCATION OF OUR INDUSTRIAL CLASSES[1]

It is, I believe, according to precedent, now that another year's work of the Science Classes here has been crowned by the award of prizes, that I should address you on some topic allied to the matters which have brought us together to-night. I need not search long for a subject, for the scientific education of those engaged in our national industries—upon the success or failure of which, in the struggle for existence, the welfare of our country so largely depends—is now one of the questions of the day. I propose, therefore, to lay before you some facts and figures bearing upon the education of our industrial classes, and I shall attempt to make what I have to say on that special point clearer, by touching upon some preliminary matters, which will show how it is that such a question as this has not been settled long ago; and further, that we can, if we wish, settle it now in

[1] An address delivered in presenting the prizes at the Coventry Science Classes, by J. Norman Lockyer, F.R.S.

the full light of the experience gained elsewhere, instead of wasting let us say a quarter of a century in costly experiments which may perhaps leave us in confusion more confounded. To begin, then, why is this question being discussed now? There is a great fact embodied in the most concrete fashion in the way in which our Government is now compelled to deal with our national education. Side by side of the Education Department by which our Minister controls in the main that book learning which has been given time out of mind, there has sprung up during the last thirty years another department—the Science and Art Department—by which he controls a new kind of national learning altogether. We have added to the old study of books a new study of things. This new learning was, we may say, only introduced in 1852, in which year the Queen in her speech on opening Parliament said: "The advancement of the fine arts and of practical science will be readily recognised by you as worthy the attention of a great and enlightened nation." We have since found out that they are indeed worthy the attention of a great nation, and more than this, that no nation can be called enlightened whose citizens are not skilled in both; in fact, that they are to peace what cannon and swords are to war. But for a nation to foster them is one thing, to include them in a national scheme of education is another. Ought they to be so included? Let us see. What do we mean by education? Roughly speaking, we may say that there are two distinct schools of thought on this subject, although the existence of these two schools is not so generally recognised as it should be. According to one view, the human mind is an elastic bag into which facts are to be crammed for future use. A variation of the view is that the mind is inelastic, and then the stuffing process becomes more serious, and instead of depending upon a natural expansion, a process like that in use by the manufacturers of soda-water is employed. It is not to be wondered at that the youthful mind likes neither of these methods; what ought to be a true delight becomes a real agony, and hence it is, as a Warwickshire man wrote many years ago—

"Love goes toward love
As schoolboys from their books;
But love from love
Toward school with heavy looks."

—The mind on this view resembles a store where, as our American cousins say, everything, from a frying-pan to a frigate, which shall be useful to the owner in after life, is to be found. Hence such terms as Grammar School, Trade School, Science School, Commercial Academy, and hence I am sorry to say, systems of examination which too often only serve to show what a boy can remember, and little care about either what a boy can do, or whether he can think. So much for one view. Now for the other. It is more difficult to image it, but in the absence of a better illustration, the mind may be likened to the body—a thing to be trained so that its grace, its freedom, its strength, its grasp, indeed all its powers in all directions and in all ways may be brought out by proper training. If the training is one-sided its power cannot be many-sided, but it is most useful when many-sided. Therefore, as each muscle of the body has to be properly trained to make a perfect man, so must the educational system brought into play be such as to train to its uttermost and bring out each quality of the mind. Each faculty of it when called into play becomes as a two-edged sword in the arms of a strong man. In this, or some such way, then, may we picture to ourselves the difference between instruction in its real sense, and education in its real sense. Now, which of these systems is the better one? We shall see at once that the first may give us a mind stored with facts covering a large or a small area; it may be bookkeeping, or it may be Latin, or anything else. But will the mind be able to use this store in all cases? We grant *knowledge*, but may not *wisdom* linger? Those of us who have got to Voltaire's second stage, and who have studied men, know that this too often happens, and that much knowledge does not prevent the owner from being absolutely unfitted to grapple with the problems which each rising sun brings to him for solution. The other system, on the other hand, if the training is not thoroughly all-round, may give us a man who finds that the questions presented to him on his entrance to active life are precisely those which require the application of that quality of mind, whichever it may be, which was least trained at school. He may find himself face to face with problems of the existence of which he never dreamed, and so far removed from his experience that his mind, however powerful in some directions, fails to grapple with them. We seem, then, on the horns of a dilemma. Instruction may provide us with a store of facts, which the mind does not know how to use. Education may provide us with a mind which has been trained in a world utterly different from the real one. How can we escape from this dilemma. *We must use the materials of that instruction which is most useful to us in our progress through life as a basis for the complete education of the mind.* Which instruction is the most useful to us? The poet tells us, that "the proper study of mankind is man"; but when we come to prose and read the views of those who best know the needs of modern society, and especially industrial society, we read something like this which I quote from the report on elementary and middle class instruction, published by the Royal Commission of the Netherlands: "The idea of *Industrial Society* not limited to agriculture, manufactures, and trade or commerce, but understood in its widest signification, points plainly to the acquiring of the knowledge of the present world, and to its application to economical and technical pursuits." Now, here is a subject on which a volume might be written, but I shall only point out to you the obviousness of the importance of the study, not merely of ourselves, *or* of the world around us, but of ourselves, *and* of the world around us. This lands us in the necessity of training our minds in literature or humanities, and science and art—the study of the humanities enables us to know the best thoughts, and the most stable conclusions on vital questions, arrived at by our forerunners and those who are fighting the same battles in other lands. The study of science enables us, on the other hand, to get a true idea of the beautiful universe around us, of our real work in the world, and of the best manner in which we can do that work in closest harmony with the laws of Nature. Did we study the external world alone we should not profit by the experience of those that preceded us. Did we study humanities alone we should be shorn of half our natural strength in face of many of the problems placed before us by the conditions of modern life; and, more than this, all the glories of the beautiful world on which our lot is cast, and the majesty of the universe of which that world forms part would hardly exist for us, or give rise only to dumb wonder. Here let me tell you a little story. Three years ago when travelling in America, one morning, at a little station—we were approaching the Rocky Mountains—I was astonished to see a very old and venerable French curé in his usual garb enter the car, and as he was evidently in some distress of mind, and as evidently had little command of English, I asked him in his native language if I could be of any service to him. There was a difficulty about a box which I soon settled, and then we sat down and entered into conversation. He soon found out that I was very astonished to see him there and told me so. I acknowledged it. "It is very simple," he said, "I am very old, and six months ago I was like to die and I was doing my best to prepare myself for the long journey. In my fancies I imagined myself already in the presence of *le bon Dieu*, and I fancied this question addressed to me, 'M. le curé, how did you like the beautiful world you have left'? I rose in my bed as this thought came into my head for I—I who—figure to yourself—had dared to preach of a better world for fifty years, was, oh! so ignorant of this. And I registered a vow that if *le bon Dieu* allowed me to rise from that bed of sickness I would spend the rest of my life in admiring his works—*et me voici!* I am only on my journey round the world; I am going now to stop at the Yosemite Valley a few days *en route* for San Francisco and Japan, and the box, Monsieur, which your kindness has rescued for me contains a little scientific library, now my constant companion in my delicious wanderings." Our general scheme of education, therefore, unless it is to be one-sided, must combine science with the humanities. But, so far, I have said nothing about art. Now, from the educational point of view, science and art are very closely connected, inasmuch as in the early stages of both studies the student's powers of observation are brought out and trained in the most perfect way, while in the later stages, to succeed in either, he must have learned that very important thing—how to use his hands—and at whatever age you put it that a boy or a girl should use the hand neatly and skilfully, before that age you should take care that some elementary grounding at all events, in the only training which can do this, shall have been given. No amount of Greek, or of useful or of useless geography, or even of rule of three, can prevent the fingers being all thumbs, unless some such training has been given, and for the very earliest training drawing is undoubtedly the best. But this is by no means the only advantage of the combination. Anyone who has to go over thousands of examination papers finds in nineteen

cases out of twenty that an orderly drawing or diagram is generally associated with an orderly mind. In fact, a diagram may be regarded as an index of the amount and accuracy of the knowledge possessed by the student. The text of the student who fails in the diagram is generally a more awkward jumble than the diagram itself. Hence the facts show that this training of the hand is accompanied by much good mental results. This is now so generally recognised, that in a not distant period, no professor of biology, for instance, will attempt to demonstrate practically microscopic structure to students who have had no preliminary training in drawing. This is one example out of many which might be given, for as natural science is the study of nature, and as we can only study her by phenomena, the eye, and the hand, and the mind, must work together to achieve success, and he who attempts to describe the geology of a district, the minute structure of a frog's foot, an eclipse of the sun, or the rings of Saturn, in words, and words only, has only done half his work; to complete it he must appeal to art for aid. Now, many of you may be prepared to concede, without any further insistance on my part, that an elementary acquaintance with art is of great, nay, of even essential importance, not only for its own sake, but because of its aid in natural studies. We must then add art to science and literature in order to form a complete curriculum. Here pardon me one moment's digression from the direct line of my argument. Many will agree that science is aided by art who deny that art is aided by science to the same extent. Indeed, some are prepared to urge that one who proposes to devote himself to art can derive no possible benefit from the study of science. Let us inquire into this a little. If we wish to excel in the art of figure-painting, we must know anatomy, a most important branch of science; and as a matter of fact, many artists study anatomy as minutely as many surgeons do; and in the old days, when the artist and the poet were more saturated with the knowledge of the time than they are now, we find the great Leonardo at once professor of anatomy and founder of a school of painting as yet unsurpassed. If we pass from the figure to ornamental design, or if we wish to show objects in perspective, is not every line, whether straight or curved, dominated by an appeal to geometry? Again, suppose we take landscape. Here we meet with phenomena of colour as much regulated by law as are the phenomena of form, and an anatomy of colour is fast being formulated, which to the artist of the future will be as precious as the anatomy of form has been in the past, and will ever continue to be. Let us take, for instance, an artist who wishes to paint a sunset, one of the most magnificent sights which it is given to man to witness. The sky is covered with clouds here and there, and not only do the colours of the clouds vary, almost from moment to moment, but in all cases they present the strongest contrast to the colour of the sky itself. The artist is bewildered, and finds each effect that he would seize to be so transient that at last he gives up in despair the attempt to note down the various tints. But the possession of a knowledge of the part played by the lower strata of our atmosphere in absorbing now one and now another of the components of the light of the setting sun, would change this despair into a joy almost beyond expression. For the bewildering changes of colour are then discovered to be bound together by a law as beautiful as the effects themselves. There is another point of view. One is frequently pained in seeing in an otherwise noble work of art, evidences that the artist was crassly ignorant of the phenomena he attempted to represent, and in his attempts to transcend nature had only succeeded in caricaturing her, painting, for example, a rainbow in perspective, or a moon with its dark side turned towards the setting sun. Yet these are almost trifles, and, in fact, here we have the excuse of the ignorant artist—now, I am thankful to say, the representative of a class that is fast disappearing—for his defence is, that he has nothing to do with such small matters, and that accuracy of this kind may quite properly be sacrificed to secure the balance of his picture. Now, to return to the main drift of my address, we have seen that in any complete system of education neither science nor art must be neglected by the side of the old humanities—the old more purely literary studies; and it is indeed fortunate for us that we live in an age in which the laws and the phenomena of the external world have been studied and formulated with such diligence and success that it is as easy now to teach science, in the best possible way, as it is to teach classics in the best possible way. It is half a century since the Germans found out the importance of the new studies from a national point of view. We are now finding it out for ourselves, and finding it out not a moment too soon, and it is not needful for me to tell you that the transformation which is going on is aeknowledged to be one of the highest national importance. It is no longer an abstract question of a method of education; it is a question of the life or death of many of our national industries, for, in a struggle for existence, how can a man who wins his bread by the application of national laws to some branch of industry, if he be ignorant of those laws, compete with the man who is acquainted with them? If for man we read nation, you see our present position. How far then have we got with our transformation, limiting our inquiry to primary and secondary instruction? First, as to elementary education. The idea of the education—the compulsory education, if necessary, of all the citizens in a state—dates from the time of Luther. It is a horrible thing that we should have had to wait three and a half centuries since his time for such a measure, which is an act of simple justice to each child that is brought into the world. In 1524 Luther addressed a letter to the Councils of all the towns in Germany begging them to vote money, not merely for roads, dykes, guns, and the like, but for schoolmasters, so that the poor children might be taught, on the ground that if it be the duty of a State to compel its able-bodied citizens to take up arms to defend the fatherland, it is *a fortiori* its duty to compel them to send their children to school, and to provide schools for those who, without such aid, would remain uninstructed. Thanks to our present system, now about ten years old, out of an estimated population of 8,000,000 children between the ages of two and fifteen, we had last year nearly four millions at school, and out of an estimated population of 4,700,000 between five and thirteen, we had 3,300,000 at school. Among this school population elementary science is at last to be made a class subject, and we find mechanics, mathematics, animal physiology, and botany among the specific subjects in addition to the three R's. 120,000 children received education in these specific subjects last year, and if we are justified in assuming that as many will learn science when it becomes a class subject as now already learn drawing, we may expect in a year or two to have this 120,000 swelled into three-quarters of a million. I must again insist upon the fact that practical teaching in science is the only thing that can be tolerated. Of course, with a new subject the great difficulty is the difficulty of the teacher. Any system, therefore, of economising teaching power is of the highest importance. I am glad to know that a system suggested by Col. Donnelly, which uses the utmost economy of teaching power, has been carried into admirable practical effect at Birmingham, and I believe also at Liverpool, and other large towns. So that in the most important centres we may be certain that science will be taught in the best manner. It is worth while to dwell on this system for a moment. Under it practical teaching is given to boys and girls of the fifth and higher standards, and also to the pupil teachers. The subject chosen for the boys is mechanics, that for the girls domestic economy, giving each of these subjects a wide range of meaning. There is a central laboratory in which the experiments are prepared, and from which the apparatus ready for use is conveyed in a light hand-cart to the various schools—twenty-six in number in Birmingham—belonging to the Board. In this way it is possible to give twenty lessons a week, and the circuit of the schools can be made in a fortnight. In the intervals between the visits of the demonstrator the class teachers recapitulate his lessons and give the children written examinations. About 1200 children are now being instructed in this way. To make the instruction as real as possible, children are brought out to aid in performing the experiments, objects are passed round, and questioning at the end of the lecture is encouraged. In the education, then, of our children, from the ages of five to thirteen, we may reasonably expect to find that science teaching will in the future be carefully looked after. We now come to the secondary education. Here, again, great progress has been made during the last few years. The real difficulties against its introduction have been the overcrowded state of the old curriculum, the scarcity of teachers, the want of sympathy with it, and the ignorance of its importance on the part of some headmasters. But to those headmasters who held the view that no real training could be got out of a subject which boys studied without positive pleasure, parents began to reply that whether the boy liked it or not he must get that knowledge somewhere. But where the experiment was really tried under good conditions it was soon found not only that the boys were willing to give three or four hours a week of their playtime to scientific subjects, but that the

one or two hours filched from the curriculum were more than made up for by the greater ease with which the other subjects could be learnt, in consequence of the additional training of the mind which the new subjects gave. We may hope, then, that in the course of time our secondary education may be much improved in the direction indicated. What we may expect, taking the principle of natural selection as our guide will be this. First, the head-masters will themselves be men chosen among other grounds for their knowledge of science, they will become more and more all round men. Next, the curriculum will be arranged not for the few who go to the University, but for the many who do not. We shall have more science and less Greek in the early years of the school course. We shall have laboratories, and drawing rooms, and workshops. In some schools we may find modern living languages taught in a living way replacing the dead languages altogether. Now, here our difficulties begin. We are face to face indeed with the same difficulties which the Continental nations, our precursors in educational matters, have experienced. Our secondary education is at the present moment all but absolutely separated from the primary one. Of the 4,000,000 scholars on the books of elementary schools last year there were only 44,000 over the age of fourteen, and it is to be feared that the remainder left school at that age, most of them, the best as well as the worst of them, to fight the battle of life with such an education as they had got up to that time. Germany, again, was the first to find out that this would never do, even though in that country science and art was taught in the Primary School. And for the reason that though such a meagre education might possibly do for ordinary workers in their hives of industry, it was totally insufficient for the future foremen, overseers, and the like, and special schools were established to carry their education further. Quite of late years this question has been studied in the most interesting way in the Netherlands, under the advice of a wise minister, whose example will be followed some day in our own country. Let me briefly refer to it. This work began in 1863. In that year in Holland there were no middle class or secondary schools for artisans, but there were evening schools for drawing which dated from 1827. "Burgher Schools" were established to provide the secondary instruction still felt to be needed by those who otherwise would have to content themselves with the primary instruction (although in its more extended form it contained natural philosophy, mathematics, and modern languages). In these schools—some day, some night schools (in these the lessons went on from September to May), with a course of two or three years, we find mathematics, theoretical and applied mechanics, and mechanism, physics, chemistry, natural history, either technology or agriculture, drawing, gymnastics, and other subjects among the fixed subjects, modelling and foreign languages being permissive. These burgher schools were compulsory in all parishes of 10,000 inhabitants. The evening burgher schools especially were at once seized on with avidity, chiefly by apprentices and the like. Here let me give you some statistics which will show you how these schools were working even ten years ago. They are much more flourishing now, but I have not the figures. I will show how the Dutch (of whom it cannot be said, to vary an old rhyme,

> In matters of *learning* the fault of the Dutch,
> Is giving too little and asking too much.

for the instruction is practically free), who are already learning a trade or working at one, use the evening hours for the further cultivation of their minds.

	Population.	Number of students in Burgher Schools.
Delft	23,000	171
Utrecht	64,000	283
Deventer	81,000	285
Dordrecht	26,000	146

Among the students at these schools in 1874 were 1582 carpenters and joiners, 472 smiths, &c., 236 plumbers and masons, 170 goldsmiths, engravers, &c., 320 painters, to give examples. Higher burghar schools were also established in the chief towns. In these schools still more advanced instruction was given: and here the course was for five years. In all these schools there was a considerable state endowment, and an endowment on the part of the town, so that the fees were almost nominal, and in some cases even the instruction was gratuitous. When I was inspecting these schools in Holland with an eminent man of science, whose advice had helped largely to make them such a success, and when I expressed to him my astonishment at the smallness of the fees—only a very few shillings a year—he put before me the question of State aid to schools in a way which had never struck me before. He said: "We regard it as a sort of education insurance. A small tax is paid by everybody during the whole of his life, and in this way a man who brings up children for the service of the State is helped by him who shirks that responsibility; and the payment which each citizen is called upon to make towards this instruction is spread over his whole life, and does not come upon him when he is probably most pinched in other ways. Now for one practical result of the establishment of these schools. The year 1863 found Holland full of the notion that every hour a child spent away from the desk or the bench after thirteen was time wasted; but after these burgher schools were instituted a change came over the spirit of that dream, and now no employer of labour except of the lowest and most manual kind in Holland, will look at a boy who cannot produce a certificate from his burgher school. Another very remarkable thing was soon observed, with a most important moral for us. The great difference between their burgher schools and the old gymnasia, the equivalents of our grammar schools, was a greater infusion of science into the teaching, and the introduction of three modern languages in addition to Dutch, Latin and Greek being omitted altogether from the curriculum. After four years of this training, many of the boys showed such high promise that all connected with them thought it a pity that they should not enter a university. They were therefore allowed six months as an experiment to take up Latin and Greek, and the result was that in a great number of cases they beat the gymnasia boys in their own subjects, and passed with flying colours. The Real Schul in Germany and the modern sides of our own secondary schools are almost the exact equivalents of the higher burgher schools to which I have especially called your attention. What, then, is the experience which has been gained in these gigantic educational experiments, experiments by which we may profit, as we are so late in the race, if we care to do so. One point is that if a chance is put before those who have passed through the elementary schools of further culturing their minds, they seize upon it with avidity. Another is that the employers of labour appreciate the value of the greater intelligence thus brought about. It is better to have to instruct in a trade men who have shown themselves anxious to learn, than to have to do with blockheads. Another, I think, is this: Your best secondary school is best for everybody; a secondary school with a properly mixed curriculum of literature, science, and art, is best for him who proceeds either to the University or to the workshop. A second-rate education in a second-rate school, gives us a second rate man, and we do not want our national industries to be worked entirely by second-rate men. On this point I am glad to fortify what I have said by a reference to Dr. Siemens' important address at the Midland Institute the week before last. He says: "It is a significant fact that while the thirty universities of Germany (you see they do not educate by halves in Germany; they have seven times as many universities as we have in England) continued to increase, both as regards number of students and high state of efficiency; the purely technical colleges, almost without exception, have during the last ten years been steadily receding, whereas the provincial Gewerbe Schuls have, under the progressive minister, von Falke, been modified so as to approximate curriculum to that of the gymnasium or grammar school. "As regards middle-class education, it must be borne in mind that at the age of sixteen, the lad is expected to enter upon practical life, and it has been held that under these circumstances at any rate it is best to confine the teaching to as many subjects only as can be followed up to a point of efficiency and have reference to future application. It is thus that the distinction between the German gymnasium or grammar school and the real Schule or technical school has arisen, a distinction which, though sanctioned to some extent in this country, also by the institution of the modern side, I should much like to see abolished." We see then the gradually increasing weight of opinion, and the result of the experiments both in Germany and Holland, and I may add France, point to these conclusions. Some kind of secondary education must be provided for the best students when they leave the elementary school, either before they begin work or while they are at work. Our secondary education should go practically along one line, how far soever the student goes along that line, some, of course, will go further than others; *provided always that our secondary education is the best possible, that is, having the broadest base.* Now, if this be generally conceded our problem in England, at

the present moment, is simpler than we thought it. We are face to face with the fact that it is for the good of the nation that those who have passed most successfully through the elementary education must continue that education in a secondary school, whether for two, or for three, or for six years, matters little for the argument. Are we then to build technical schools for such students? Thirty years ago the answer would have been yes. To-day we may say firmly, no. If a town has a grammar-school, let the town see that the curriculum of that school is based upon our best secondary models. If the town has no such school, then let it build one. If one school is not sufficient, then build two. That town will be the best off in the long run which gives the greatest number of free exhibitions from the elementary schools into such a school as this, and that town will be the wisest which holds out such inducements at the earliest possible moment. I have lately read with much interest a copy of resolutions and suggestions, passed at a meeting of an Association of Elementary Teachers in the north of England. From these we may gather that this question is already one of practical politics. It is agreed that the secondary education of the best boys leaving the elementary schools must also on. It is also taken for granted that the question lies between building a technical school or utilising the grammar school. One argument used in favour of the latter cause is, that the grammar school will be strengthened by drawing to itself the best boys from the elementary schools. The present proposals are that a number of free scholarships should be competed for annually, that these free scholarships shall, if need be, be supplemented by exhibitions from the fund at the disposal of the Governors (I should not accept this at once. Why should not the town pay them?), and the length of time for which these scholarships shall be tenable is not to be less than three years. You see, then, that in the north of England, at all events, it is conceded that the best children in our elementary schools should have a three years' course in a school of higher grade in which, of course, all the class subjects in the Elementary Code will be expanded, and all the linguistic studies of the grammar school taken in hand. When this system is at work, as it is bound to be in a few years, two things will happen, and it is as well we should be prepared for them. In the first place, our secondary schools—all of one model, the best model, be it understood—must so arrange its curriculum, that the students can leave after a three years' course, if need be, for the workshop or the office, or after a longer course for the University. That is the first point. The second one is this. The present system of apprenticeship will be called in question. A boy who has been educated to the age of sixteen will learn very much more in three or four years, and will be very much more valuable to his master during that time than he who was formerly bound apprentice at the age of thirteen or fourteen, with his fingers all thumbs, and no mind to speak of. It seems to me as it does to a daily increasing number, that the present mode of dealing with those matters which were formerly regarded as arts and mysteries known only to a few, and carried on on a small scale under the eye of the master, is dead against the system of apprenticeship as it has come down to us. Now the master does not teach, and the boy in nine cases out of ten has no opportunity of grasping the whole of the art or mystery at all. Many of you will begin to think that you are listening to the play of Hamlet with the part of the Prince of Denmark omitted, for so far I have said nothing whatever about technical education. I have said nothing about it for the reason that I believe the less said to a boy about technical education before he is sixteen years old the better. I now proceed to discuss this question, which is far more important, far more a national question, than you would gather from the debates in Parliament. What is technical education? It is the application of the principles of science to the industrial arts. And the rock ahead against which I am anxious to join Dr. Siemens in warning you is this: Under the influence of the present scare—for it is a scare, and a real one—there is a chance that attempts may be made to teach the applications to those who are ignorant of principles, whereas we have to fight those who study applications with a full knowledge of the principles which underlie them. We may congratulate ourselves on the fact that when we have once made up our minds as to the right place of technical instruction in our scheme of education, we have much of the necessary machinery already at our disposal; and the recent action of the City Guilds and of the Government is enormously increasing the quantity and improving the quality of this machinery. Let us first consider the classes now formed all over the country under the auspices of the Science and Art Department. Their development in the last thirty years has been something truly marvellous. When the Queen, in 1852, opened Parliament, there were already 35,000 students of art, but practically no students of science, in this country, amongst the industrial classes. That 35,000 will, if the present progress goes on, give us nearly 1,000,000 students of art at the end of this year; while the science schools have increased from 82 in 1860 to 1400 in 1880, with 69,000 students. The system which has thus developed so enormously has dealt chiefly with pure science, but for the future we shall have side by side with it, and built upon the same lines, a system of teaching the applications of this pure science to each of our national industries. He who wishes in the future to have to do in any way with the manufacture of alkali, gas, iron, paper, or glass, to take instances, or in the dyeing of a piece of silk, or the making of a watch, to take others, will find the teaching brought to his door, and obtainable almost for the asking. Here, again, we may congratulate ourselves, for while those who know most about the subject tell us that the more ambitious attempts at technical instruction in Germany and elsewhere have failed, because the teaching is not in sufficiently close contact with the works in which the processes are actually carried on, the system to which I have drawn your attention will enable the instruction to be given at night to those who have already begun practical work during the day. We have, then, come to this: that putting together what is most desirable in the abstract, and what has been practically proved to be the best, the education of our industrial classes should be, and can easily be, something like this. The boy will go to an elementary school till he is thirteen. He will then pass with an exhibition, if necessary, to a secondary school till he is sixteen. He will there go on with his science—now a class subject in the elementary school—and begin the study of languages. At sixteen he will leave school and begin the battle of life, and can still in the evening proceed further with his studies in pure science, if the secondary education has left him too ill-equipped in that direction. Having thus got the principles of pure science into his mind he will be able to take up the technical instruction in the particular industrial art to which he is devoting himself. But be the number of our future foremen and managers who who have had this extra three years of secondary instruction, large or small, if there be in Coventry let us say out of your population of 45,000, one thousand boys, or girls, or men, who are anxious not only to learn science, but its application to their particular industries, then the Government is ready to endow Coventry with a sum varying from two thousand to six thousand pounds a year, according to the results of the examinations, if two subjects of pure science are taken up, and the students pass. The City Guilds are prepared to endow the town with from 1000*l.* to 2000*l.* a year additional, provided some application of the principles of science to the industrial arts is taken up, and evidence forthcoming that the principles themselves have been studied. Now if among your 45,000 there is not 1000 who care for these things which are vital to your trades, seeing that abroad these things are cared for, how can your trades stand against foreign competition? Let such a system as this go on for twenty years, and we shall hear nothing more of the decay of our national industries. Now here I am bound to point out a distinct gap in the present system. We have classes for art, classes for pure science, classes for applied science, but where are the classes for languages? The modern languages are taught so badly in our secondary schools, that it is hopeless to expect that sufficient knowledge, either of French or German can be acquired in the three years' course to enable the student to find out what his French and German rivals are doing in the branch of industry which he takes up; and we must, moreover, consider those who may wake up to the importance of studying science and its technical applications after the chance of a secondary education is lost. Such classes then are a real want. But I will not end my address by a reference to what I regard as an unfortunate gap, but would rather conclude what I have to say by pointing out that the scheme I have sketched out need be no Utopia, so far, at all events, as a supply of well-trained teachers is concerned. This, up to the present time, has been the real difficulty. But now that the authorities at South Kensington have started summer courses of lectures to teachers, and that they actually pay the teachers for going to learn, the methods of teaching, both in the elementary and secondary schools, and evening classes, cannot fail to improve. Quite recently, too, we have seen the inauguration of a Normal

School, where Royal Exhibitioners and other free students are admitted without payment; where the teacher has the first claim, and where he can attend any single course for a nominal fee. Now every town of importance in the country should associate itself with the Government in this attempt, and should have one, at least, of its citizens always in training there, so that the scientific instruction in that town, whether primary, secondary, or tertiary, should always be at its highest level. On the other side of the road, too, at South Kensington, is rapidly rising another institution where we may hope the teachers of our technical instruction will receive an equally careful training. So that you see, to bring what I have to say to a conclusion, that though we are late in the day, though many people have not yet made up their minds as to what is best to be done—and I acknowledge that the question is hedged in with difficulties on all sides—there is an easy solution of the difficulty based on the experience of other countries, which is at the same time an act of simple justice; that this solution requires no dislocation if we adopt it, but simply a natural growth of our existing means, and that all the newest developments of our educational machinery will all fall naturally into place.

THE TRANSIT OF VENUS[1]

The Observations at the Cape

THE long looked-forward-to transit of Venus occurred yesterday afternoon, causing, we may be sure, a flutter of excitement amongst astronomers throughout the whole of the world. To some the special duty was entrusted of carefully noting everything connected with the ingress of this familiar planet, and after they had concluded their labours at the setting of the sun, it fell to astronomers in other portions of the globe to pay equally minute attention to the planet's egress. By and bye we may expect columns of thoughtfully worked-out details in connection with this peculiar and interesting astronomical event, all of which will tend to still further solve the problem of the exact distance of the sun from the earth. We need not remind our readers that herein consists the whole scientific value of the transit. When crossing the sun's disc the planet is at its nearest distance from the earth—estimated at about 25,000,000 miles—and through the peculiar facilities thus afforded of directly measuring its parallax, observers are enabled to calculate the parallax of the sun, which to astronomers is a matter of very considerable importance. The credit of the suggestion of this particular method of calculation is due to Dr. Halley, and it is still popularly held to be the best for the purpose. But accompanying the rapid strides astronomic science has taken in its development since the days of Halley, instrumental means have been invented and accepted by modern astronomers, which appear to afford methods, perhaps even more exact, of arriving at the desired result. For all this, however, the transit of Venus retains a powerful hold upon the popular mind, and, indeed, upon the minds of many astronomers, as the best method. There is, too, one specially strong argument why a particular interest should be taken in this planet's transit. No one who witnessed the phenomenon yesterday will live to see it again—unless, indeed, he fairly outrivals old Parr and other gentlemen famed for longevity. Occurring as these transits do at the unequal but regular recurring intervals of 8, 122, 8, and 105 years, no one could well expect to see more than two in a lifetime. The last took place in 1874, while the next will occur in December, 2007. It need, therefore, be no longer surprising why, both popularly and scientifically, the event is regarded as one of such special interest, and why the most eminent scientific observers are selected to note everything that takes place.

Before proceeding to refer to the observations which were taken yesterday at the Royal Observatory we may mention that, acting under the advice of the Astronomer-Royal of the Cape of Good Hope (Dr. Gill), the British Transit of Venus Committee decided upon establishing stations at Aberdeen Road and Montagu Road as auxiliary places of observation to the principal station here at the Observatory itself. And before proceeding further it may be added that Natal has come forward very pluckily in this matter, exhibiting an amount of interest in astronomic science which does great credit to that colony. Mr. Escombe himself contributed a sum of between 400*l.* and 500*l.* for the purpose of providing a proper telescope; while two merchants subscribed 50*l.* each, the Corporation of Durban giving 300*l.*, and the Natal Government voting 500*l.* towards founding an observatory for the colony, and the defraying of expenses connected with taking observations of the present transit. As a pleasant sequel to this, we are glad to learn by telegraph, that Mr. Neison, who was in charge of the party of observation there, most successfully observed the internal contact at Durban, the enterprise of Natal thus meeting with a well-merited reward. As announced by us some time since, South Africa was selected by the Americans as a station for one of their photographic transit of Venus expeditions under the charge of Prof. Newcomb, who has the reputation of being one of the most celebrated of living astronomers. On arrival here Prof. Newcomb, after consultation with the best authorities as to atmospheric conditions, &c., finally decided, with the kind consent of the trustees of the Huguenot Seminary to take his observations from the foot of the gardens of that institution at Wellington. We hope to shortly hear of the entire success of the labours of the party, and perhaps to see some specimens of their photographic skill.

At the Observatory itself it need scarcely be said that for some weeks past great preparations had been made for the event. There are few living astronomers who have more carefully studied the subject of the transit of Venus than the present Astronomer-Royal here, Dr. Gill, and few are more thoroughly posted up in all the details of the rare occurrence. In 1874 Dr. Gill was Chief Astronomer to Lord Lindsay's Transit of Venus Expedition to the Mauritius, where he not only took most valuable observations, but evinced a very intimate acquaintance with the entire subject. It was only to be expected, therefore, that in this instance no detail in connection with the arrangements for a proper observation in Cape Colony would be lost sight of by the Astronomer-Royal. The few visitors who received invitations to the Observatory yesterday found Dr. Gill courteous and affable as ever, but wholly absorbed in the important work on hand. "You may go here and go there, look through that glass and have a peep through the other one," were his remarks just before commencing operations, "but whatever you do, please don't speak to me or any of the observers until the internal contact has been made." No injunction not to speak to the "man at the wheel" could have been more respected than this, and from that moment until a couple of hours later Dr. Gill and his assistants became objects of almost reverential awe to those outside the pale of strict astronomic science.

One of the principal instruments employed was a new equatorial telescope by Grubb of Dublin, made and sent out here specially for the transit of Venus, the old wind tower in which it is now mounted having been prepared as an observatory for its reception. There was also a heliometer which had been used at the last transit by Dr. Gill at the Mauritius, and was afterwards borrowed by him from Lord Lindsay for use on the Isle of Ascension, where he made a determination of the sun's distance from the planet Mars. Subsequently this fine instrument was purchased by Dr. Gill and was brought out here as his private property on his being appointed Astronomer-Royal at the Cape. Another noticeable instrument employed yesterday was the great theodolite intended for the trigonometrical survey of India. The designs of Col. Strange, however, from which it was constructed, were so long in being carried out in manufacture that General Walker, the Director of Survey, decided not to bring it into use, especially as it was somewhat too heavy for service in the field. Upon the application of Dr. Gill, it was lent by the Indian Government, for the purpose of some special researches in which that gentleman was engaged at the time, and it was successfully employed the other day in taking observations of the great comet. The other instruments included a small equatorial telescope of 3¼ inches aperture, which was used by Mr. Stone on the occasion of the last transit of Venus; an equatorial telescope of 7 inches aperture, which has also been for some time at the Observatory, and a telescope of 2½ inches aperture belonging to Capt. Jurisch, examiner of diagrams in the Surveyor-General's department. Having mentioned the several instruments, we must go on to state by whom they were used. Dr. Gill himself observed the contact of Venus with the sun's limb, with the new 6-inch aperture equatorial, a similar observation being taken by Mr. Maclear with the 7-inch equatorial. Dr. Elkin, a scientific friend and guest of the Astronomer-Royal, took observations with the heliometer; Mr. Freeman, with the great theodolite; Mr. Pillans, with the small equatorial; and Capt. Jurisch with his own equatorial. Several important measures were also taken at the heliometer by Dr. Gill and Dr. Elkin.

[1] From the *Cape Times*, December 7, 1882.

With regard to the weather, which of course was a very important element, the sky was perfectly clear, and altogether suitable for the purpose of observation. There was a light south-east wind blowing, and this prevented the definition being so steady as might have been wished. We are "officially" assured, however, that on the whole the observations made at the Cape of Good Hope may be regarded as perfectly satisfactory, and that they will add considerably towards the solution of the problem of the sun's exact distance from the earth. We have already intimated that at the suggestion of Dr. Gill, other stations than that of the Observatory had been selected. At Aberdeen Road, Mr. Finlay (of comet fame), the first Assistant at the Cape Observatory, and Mr. Pette, third Assistant, were provided with an equatorial of six inches aperture, and the report received last evening by telegraph, was that complete success had attended their labours. Mr. Marth, the well-known astronomer, was detailed at Montagu Road in charge of one of the British Transit of Venus Expeditions, and was provided with a 6-inch aperture equatorial, his assistant, Mr. Stephen, formerly of the Observatory, and now of the Treasury Department, Cape Town, being provided with a 4½-inch equatorial. In his report last evening, Mr. Marth states that the sky was cloudless, but a heavy dust-storm prevailed during the day. He reported, however, that the important internal contact was observed satisfactorily both by himself and Mr. Stephen. A report from Capt. Skead, in conjunction with Mr. Spindler, of Port Elizabeth, states that they also obtained satisfactory observations.

We fear that the courtesy of the General Manager of Telegraphs, Mr. Sivewright, must have been sorely tested by the frequent demand upon his staff for signals for the purpose of determining longitudes, &c. The telegraphic department, we ought to state, has given the utmost facilities in connection with these operations, and thanks to the co-operation of the General Manager, everything connected with his department was accomplished without a hitch. The transit of Venus expedition will indeed be indebted to Mr. Sivewright for his energy and devotion in their interests. This additional work has necessarily fallen heavily upon the shoulders of the staff at the Observatory. Not only has the normal work of that establishment been carried on as diligently as heretofore, but there has been the additional task of taking observations of the great comet, which with other things has told severely upon the endurance of Dr. Gill and his assistants. Judging, though, from what we saw there yesterday, there is no sign of anyone breaking down under the strain of extra work.

The signals for time comparison were sent to the observers engaged in the transit about nine o'clock on Tuesday evening The night is described as having been beautifully clear, and the occultation of the bright star Spica Virginus was observed in the early morning. Signals were also sent to Mr. Eddie, Graham's Town.

We have thus far briefly sketched the manner in which the observations were taken yesterday—excepting the somewhat primitive method of smoked glass adopted by a good many of the general public, to whom the transit of Venus was not quite such a matter of exquisite nicety as to such gentlemen as those to whom we have just alluded. From a non-astronomic point of view there was even with the aid of the proper instruments, only to be seen a dark spot crossing the sun, resembling very much a Wimbledon bull's eye. Roughly speaking, the planet made its external contact at five minutes past three o'clock, when through a proper instrument it might have been seen minutely notching into the sun's edge. At twenty-five minutes past the hour—still roughly speaking, for when the calculations are worked out there might be a fractional part of a second one way or the other—the internal contact occurred.

The sun set long before the transit had been completed. It consequently fell to the lot of other astronomers to observe its egress, which of course was as eagerly watched for as had been that of the ingress. The ingress, it might be interesting to mention, was visible in North and South America; Europe, excepting the west of Russia and the north of Norway and Sweden; the whole of Africa, Madagascar, Seychelles, and the Mauritius. The egress was visible in North and South America, Australia, New Zealand, and nearly the whole of the South Pacific. This egress will have been completed by about eight o'clock this morning, and then all interested in the subject of Venus may look forward to another 122 years before the interesting occurrence again takes place.

ELECTRIC RAILWAYS[1]

WE have grown so accustomed to the regular announcement—"serious—accident on such and such a railway, several passengers injured" that we have almost come to regard railway accidents as inevitable, just as parents mistakingly think the measles and whooping cough necessary accompaniments of childhood. But speed no more means disaster than a densely crowded city means disease. The first effect of overcrowding is undoubtedly to produce fever and other complaints. If, however, the knowledge and practice of the laws of hygiene increase more rapidly than the population of a town, the death-rate, as we have seen, diminishes, instead of augmenting. And so it is with locomotion; the stage-coach journeys of our ancestors were slow enough for the most staunch conservative, and yet the percentage of the passengers injured on their journeys was far greater than even now with our harum-scarum railway travelling. The number of passengers has increased enormously, but the safety has increased in an even greater rate. If then we can devise methods introducing still greater security, a far larger number of passengers may travel at a far greater speed and with less fear of danger than at present.

Accidents constitute one charge against railway conveyance, but there is another, and that is the cost. Cheap as railway travelling now is, compared with the departed stage-coach locomotion, the price of the tickets is still far too high for railways to fulfil, even in a small degree, one of their most important functions, and that is transporting labourers from parts of the country where labour is scarce, to others where it is abundant and labourers in demand.

But how is a happier state of things to be realised? We cannot expect the railway companies to lower their fares merely to benefit humanity. If, however, we can prove to them that the present system of railways is neither the most remunerative to themselves nor the most beneficial to the community at large, we may hope to win the attention of railway directors, whose stock question is, and quite rightly, "Will it pay?"

Those of you who have read the life of Stephenson know what a protracted fight he had to carry one of his most cherished ideas, and that was the employment of a locomotive engine to draw the train, instead of a stationary engine to pull it with ropes or chains. His adversaries saw the disadvantage of adding the weight of the locomotive to the weight of the train, whereas Stephenson was especially struck with the enormous waste of power in the friction of ropes or chains passing over pulleys. [Experiments were then shown proving, *first*, that the mass of the locomotive necessitated the engine having a greater horse-power to get up the speed of the train quickly as well as a greater horse-power to keep up the speed; *secondly*, that the friction and wear and tear of ropes, such as were employed on the London and Blackwall Railway, would have been an insuperable hindrance to the development of railways.] From this was deduced that, since in Stephenson's day the only feasible mode of communicating the power of a stationary engine to a moving train was by means of ropes, his decision to adopt the locomotive was perfectly correct at the time it was made.

Attempts have been made to propel trains by blowing them through tubes, or by blowing a piston attached to the train through a tube, but such attempts at pneumatic railways have nearly all been abandoned. The employment of air compressed into a receiver on the train by fixed pumping engines stationed at various points along the line, and employed to work compressed air engines on the carriages has been effected with considerable success by Col. Beaumont, especially for tram-lines. The weight of the compressed air-engine is, however, still very considerable. Any system of pumping water through a pipe and employing the water to work a hydraulic engine on the train is hardly worth considering, seeing that the mechanical difficulties of keeping up a continuous connection between the moving train and the main through which the water is pumped seem insuperable. Gas-engines worked with ordinary coal-gas, stored perhaps under pressure, might be employed on the moving train, but the advantage arising from the absence of boiler and coal would be more than compensated for by the fact, that the weight of a gas-engine per horse-power developed is so much greater than that of a steam-engine. None of these systems, then, of dispensing with a locomotive is by any means perfect, and the success of the recent experiments on the electric transmission of

[1] Abstract of a lecture at the Royal Institution by Prof. W. E. Ayrton, F.R.S.

power has turned the attention of engineers to the consideration, whether electricity could not successfully supplant steam for the propulsion of trains and tram-cars; whether it could not, in fact, supply an efficient means of transmitting power, the absence of which caused Stephenson to abandon ropes in favour of a heavy locomotive engine.

The whole question, like every similar one, is mainly a question of expense; and what we have to consider is, whether electric transmission on the whole leads to greater economy than can possibly be obtained by the employment of any kind of locomotive. The average weight of a locomotive is about that of six carriages full of people; ten carriages compose an ordinary train, hence the presence of the mass of the locomotive adds at least 50 per cent. to the horse-power absolutely necessary to propel the carriages alone, and therefore at least 50 per cent to the amount of coal burned. But there is another most serious objection to the engines, perhaps even more important than the preceding. The heavy engine passing over every part of the line necessitates the whole line and all the bridges being made many times as strong, and therefore many times as costly, and the expense of maintenance consequently also far greater, than if there were no locomotive. And it is not possible to make the engine much lighter; for it would not have then sufficient adhesion with the rails to be able to draw the train; in fact, you cannot diminish the weight as long as the train is propelled with only one or two pair of driving wheels as at present. The employment of electricity, however, will enable a train to be driven with every pair of wheels, just as the employment of compressed air enables every pair of wheels to brake the train.

To propel a train, we must either utilise the energy of coal by burning it, or use the energy possessed by a mountain stream, or the energy stored up in chemicals, and which is given out when the chemicals are allowed to combine, or we must employ the energy of the wind. Practically we employ at present only the first store for propelling railway trains—the potential energy of coal; and that is to a great extent the store on which we shall still draw, even when we employ electric railways. For experience shows that, with the modern steam-engine and dynamo, at least one-twentieth of the energy in coal can be converted into electric energy; and that this is at least twenty times as economical as the direct conversion of the energy of zinc into electric energy by burning it in a galvanic battery.

But it may be asked, did not Faraday's discovery, in 1831, that a current could be produced by the relative motion of a magnet and a coil of wire, settle this point half a century ago? Theoretically—yes; practically, however, the problem was very far from being solved, because the dynamo machine was very unsatisfactory, and it was not until Pacinotti, in 1860, suggested the solution of the problem of obtaining a practically continuous current from a number of intermittent currents, and until Gramme, about 1870, carried out Pacinotti's suggestion in the actual construction of large working machines, that the mechanical production of currents became commercially possible. [Experiments were then shown illustrating the complete electric transmission of power, a gas-engine on the platform giving rapid motion to a magneto-electric machine, and the current thereby produced sent through an electro-motor at the other end of the room, which worked an ordinary lathe.]

In electric transmission of power there is not only waste of power from mechanical friction, but also from electric friction arising from the electric current heating the wire, through which it passes.

It was then explained and demonstrated experimentally that this latter waste could be made extremely small by placing so light a load on the electro-motor, that it ran nearly as fast as the generator or dynamo, which converted the mechanical energy into electric energy; actual experiments leading to the result that for every foot-pound of work done by the steam-engine on the generator, quite seven-tenths of a foot-pound of work can be done by the distant motor.

One reason why electric transmission of power can be effected with so little waste is because electricity has apparently no mass, and consequently no inertia; there is, therefore, no waste of power in making it go round a corner, as there is with water or with any kind of material fluid. Another reason why electro-motors are so valuable for travelling machinery is on account of the light weight of the motor. Experiment shows that one horse-power can be developed with 56 lbs. of dead weight of electro-motor, and that for large electro-motors of several horse-power the weight per horse is even much less; a result immensely more favourable than can be obtained with steam, gas, or compressed-air-engines.

In addition to the loss of power arising from the heating of the wires by the passage of the current, there is another kind of loss that may be most serious in the case of a long electric railway, viz., that arising from actual leakage of the electricity due to defective insulation. To send an electric current through a distant motor, two wires, a "going" and "return" wire must be employed, insulated from one another by silk, guttapercha, or some insulating substance; and if the motor be on a moving train, there must be some means of keeping up continuous connection between the two ends of the moving electro-motor and the going and return wire. The simplest plan is to use the two rails as the two wires, and make connection with the motor through the wheels of the train; those on one side being well insulated from those of the other, otherwise the current would pass through the axles of the wheels, instead of through the motor. It is this simple plan that is employed in Siemens' Lichterfelde Electric Railway, now running at Berlin; the insulation arising from the rails being merely laid on wooden sleepers having been found sufficient for the short length, 1½ mile. The car is similar to an ordinary tram-car, and holds twenty passengers. [Photographs were then projected on the screen of this and of the original electric railway laid by Siemens in the grounds of the Berlin Exhibition of 1879, and exhibited in 1881 at the Crystal Palace, Sydenham.] It was explained that on this latter railway, which was 900 yards long, both the ordinary rails were used as the return wire, and that the going wire was a third insulated rail rubbed by the passing train. [Photographs were then projected on the screen of Siemens' electric tram-car at Paris, used to carry fifty passengers backwards and forwards last year to the Electrical Exhibition.] In this the going and return wires were overhead and insulated, connection being maintained between them and the moving car by two light wires attached to the car, and which pulled along two little carriages running on the overhead insulated wires, and making electric contact with them. [Experiments followed, proving that although two bare wires lying on the ground could be quite efficiently employed as the going and return wire, if the wires were short and the ground dry, the leakage that occurred if the wires were long and the ground moist was so great, as to more than compensate for the absence of the locomotive.] Consequently Prof. Perry and myself have for some time past been working out practical means for overcoming these difficulties, and we have arrived at what we hope is an extremely satisfactory solution. Instead of supplying electricity to one very long, not very well insulated rail, we lay by the side of our railway line a well insulated cable, which conveys the main current. The rail, which is rubbed by the moving train, and which supplies it with electric energy, we subdivide into a number of sections, each fairly well insulated from its neighbour and from the ground; and we arrange that at any moment only that section or sections, which is in the immediate neighbourhood of the train, is connected with the main cable; the connection being of course made automatically by the moving train. As then leakage to the earth of the strong propelling electric current can only take place from that section or sections of the rail, which is in the immediate neighbourhood of the train, the loss of power by leakage is very much less than in the case of a single imperfectly insulated rail such as has been hitherto employed, and which being of great length, with its correspondingly large number of points of support, would offer endless points of escape to the motive current.

Dr. Siemens has experimentally demonstrated that an electric railway can be used for a mile or two; Prof. Perry and myself, by keeping in mind the two essentials of success, viz. attention to both the mechanical and electrical details, have, we venture to think, devised means for reducing the leakage on the longest railway to less than what it would be on the shortest.

For the purpose of automatically making connection between the main well-insulated cable and the rubbed rail in the neighbourhood of the moving train we have devised various means, one of which is seen from the following figures.

A B (Fig. 1) is a copper or other metallic rod resting on the top of and fastened to a corrugated tempered steel disc D D (of the nature of, but of course immensely stronger than the corrugated top of the vacuum box of an aneroid barometer), and which is carried by and fastened to a thick ring E E made of ebonite or other insulating material. The ebonite ring is itself screwed to the circular cast-iron box, which latter is fastened to

the ordinary railway sleepers. The auxiliary rail AB and the corrugated steel discs D D have sufficient flexibility that two or more of the latter are simultaneously depressed by an insulating collecting brush or roller carried by one or by all of the carriages. Depressing any of the corrugated steel discs brings the stud F, which is electrically connected with the rod AB, into contact with the stud G electrically connected with the well-insulated cable.

As only a short piece of the auxiliary rail AB is at any moment in connection with the main cable, the insulation of the ebonite ring E E will be sufficient even in wet weather, and the cast-iron box is sufficiently high that the flooding of the line or the deposit of snow does not affect the insulation. The insulation, however, of G, which is permanently in connection with the main cable, must be far better. For this purpose we lead the gutta-percha, or india-rubber, covered wire coming from the main cable through the centre of a specially formed telegraph insulator, and cause it to adhere to the inside of the earthenware tube forming the stalk. And as, in addition, the inside of each contact box is dry, a very perfect insulation is maintained for the lead coming from the main cable. Consequently as all leakage is eliminated except in the immediate neighbourhood of the train, this system can be employed for the very longest electric railways. Fig. 2 shows a modification of the contact box when the insulated rail L, instead of extending all along the line, is quite short and is carried by the train, and by its motion

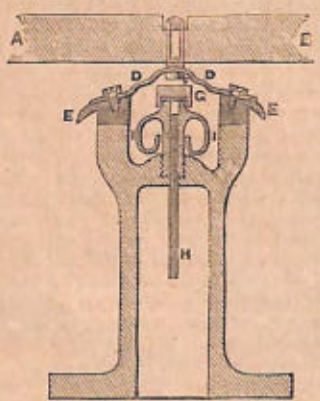


Fig. 1.

presses forwards and downwards a metallic fork on the contact-box, thus making contact between F and G. [Other diagrams were explained, illustrating modifications of the contact-boxes; in one case the well-insulated cable is carried inside the flexible rail, which then takes the form of a tube, shown in Fig. 3; in another case the cable is insulated with paraffin oil instead of with gutta-percha or india-rubber, shown in Fig. 4, &c.]

The existence of these contact-boxes at every 20 to 50 feet also enables the trains to graphically record its position at any moment on a map hanging up at the terminus, or in a signal-box or elsewhere, by a shadow which creeps along the map of the line as the train advances, stops when the train stops, and backs when the train backs. This is effected thus:—As the train

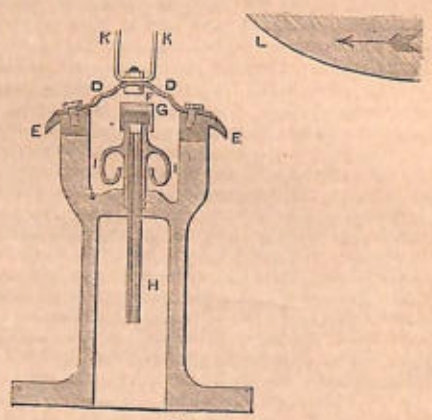


Fig. 2.

passes along, not only is the main contact between F and G automatically made, as already described, but an auxiliary contact is also completed by the depression of the lid of the contact-box, and which has the effect of putting, at each contact-box in succession, an earth fault on an insulated thin auxiliary wire running by the side of the line. And just as the position of an earth fault can be accurately determined by electrical testing at the end of the line, so we arrange that the moving position of the earth fault, that is the position of the train itself, is automatically recorded by the pointer of a galvanometer moving behind a screen or map, in which is cut out a slit representing by its shape and length the section of the line on which the train is, as shown

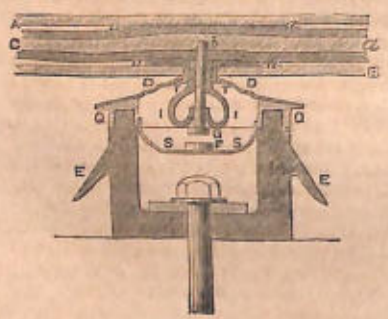


Fig. 3.

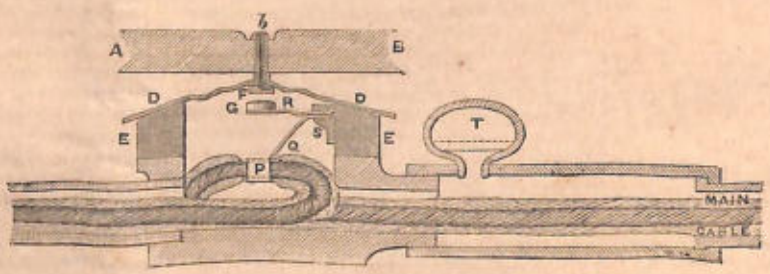


Fig. 4.

in Fig. 5. In addition, then, to the small sections of 20 feet or more into which our auxiliary rubbed rail is electrically divided, there would be certain long blocked sections one mile or several miles in length, for each of which on the map a separate galvanometer and pointer would be provided. [Experiments were shown of the system of graphically automatically recording the progress of a train.]

In the preceding systems there are several contact-boxes in each section of the insulated rubbed rail, and several sections of the insulated rail in each section of the line blocked, but in the next system the rubbed rail is simply divided electrically into long sections each of as great a length as the particular system employed to insulate the rubbed rail will allow. In this case we arrange that the electric connection between the main cable and the rubbed conductor shall be automatically made by the train as it enters a section, and automatically broken as the train leaves a section. The model before you is divided into four sections, each about 11 feet in length, and you see from the current detectors that as the train runs either way, it puts current into the section just entered, and takes off current from the section just left.

[Experiments were then shown of the ease with which an electric train could be made to back instead of going forwards, by reversing the connections between the revolving armatures and the fixed electro-magnets of the motor; also that the accidental reversal of the field magnets of the main stationary generator, although it had the effect of reversing the main current, produced no change in the direction of motion of an electric engine, the direction of motion being solely under the control of the driver.]

But more than this, not only does the train take off current from the section 1 when it is just leaving it, and entering section 2, but no following train entering section 1 can receive current or motive power until the preceding train has entered section 3. [Experiments were then shown proving that with this system a following train could not possibly run into a preceding train even if the preceding train stopped or backed.] Now why does the following train when it runs on to a blocked section pull up so quickly? The reason is because it is not only deprived of all motive power, but is powerfully braked, since when electricity is cut off from a section, the insulated and non-insulated rail of that section are automatically connected together, so that when the train runs on to a blocked section the electromotor becomes a generator short circuited on itself, producing, therefore, a powerful current which rapidly pulls up the engine. [Experiments were then shown of the speed with which an electromotor, which had been set in rapid rotation and then deprived of its motive current, pulled up when its two terminals were short-circuited.]

Whenever, then, a train, it may be even a runaway engine, enters on a blocked section, not only is all motive power withdrawn from it, but it is automatically powerfully braked, quite independently of the action of the engine-driver, guard, or signalman. No fog, nor colour-blindness, nor different codes of signals on different lines, nor mistakes arising from the exhausted nervous condition of overworked signal-men, can with this system

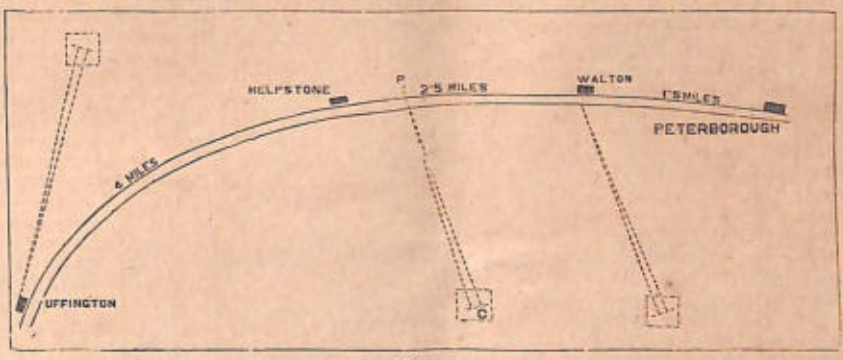


FIG. 5.

produce a collision. The English system of blocking is merely giving an order to stop a train; but whether this is understood or intelligently carried out is only settled by the happening or non-happening of a subsequent collision. Our Absolute Automatic Block acts as if the steam were automatically shut off and the brake put on whenever the train is running into danger; nay, it does more than this—it acts as if the fires were put out and all the coal taken away, since it is quite out of the power of the engine-driver to re-start his train until the one in front is at a safe distance ahead.

But all trains will undoubtedly be lighted with electricity; must, then, the train be plunged into darkness when it runs on to a blocked section to which no electric energy is being supplied? No! If some of the electric energy supplied to a train when it is on an unblocked section be stored up in Faure's accumulators, such as are at present used on the Brighton Pulman train, the lamps will continue burning even when the train has ceased to receive electric energy from the rubbed rail.

When, then, we commit the carrying of our power to that fleet messenger to which we have been accustomed to entrust the carrying of our thoughts, then shall we have railways that will combine speed, economy and safety; and last, but not least to us Londoners, we shall have the entire absence of smoke, the presence of which nearly causes the convenience of the Underground Railway to be balanced by the pernicious character of its atmosphere.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, December 21, 1882.—"On the Origin of the Hydrocarbon Flame Spectrum." By G. D. Liveing, M.A., F.R.S., Prof. of Chemistry, and J. Dewar, M.A., F.R.S., Jacksonian Prof., University of Cambridge.

In previous communications[1] to the Society we have described the spectra of what we believe to be three compound substances, viz., cyanogen, magnesium-hydrogen, and water.

In these investigations our chief aim has been to ascertain facts, and to avoid as far as possible adopting any special theory regarding the genesis of the spectra in question.

Specific spectra have been satisfactorily proved to emanate from the compound molecules of cyanogen, water, and magnesium-hydrogen, so far as we can interpret in the simplest way the many observations previously detailed. The fact that a fluted spectrum is produced under certain conditions, by a substance which does not give such a spectrum under other conditions, is of itself a proof that the body has either passed into an isomeric state or has formed some new compound; but we are not entitled to assert, without investigation, which of these two reasonable explanations of the phenomena is the true one. There is, however, a spectrum to which we have had occasion to refer in the papers on the spectra of the compounds of carbon, which closely resembles that of a compound substance, and which we, in common with some other spectroscopists, have been led to attribute to the hydrocarbon acetylene, without, however, being able to bring forward such rigid experimental proofs of its origin as we have adduced in the case of the three substances above referred to. In other words, the experimental evidence that the hydrocarbon flame spectrum is really due to a hydrocarbon was always indirect. Thus, we showed that many flames containing carbon, such as those of hydrogen mixed with bisulphide of carbon or carbonic oxide, and the flame of cyanogen in air, did not give this spectrum, and these particular flames are known, from the investigations of Berthelot, to be incapable of generating acetylene under conditions producing incomplete combustion. On the other hand, we found that a flame of hydrogen mixed with chloroform, which easily generates acetylene, gives the hydrocarbon flame spectrum in a very marked manner, and it is known that the ordinary blow-pipe flame, in which the same spectrum is well developed, contains this hydrocarbon.

These and other experiments point to the intimate relation of hydrogen and carbon in the combined form of acetylene to the production of this spectrum during combustion. In our various observations on the spectrum of the electric arc taken in different gases, the flame spectrum was always noticed, and seemed to be independent of the surrounding atmosphere. In the mode in which those experiments were conducted, it was easily shown that the carbons were never free from hydrogen, and that the gases always contained traces of aqueous vapour. Under these conditions acetylene is formed synthetically during the electric discharge, the line spectrum of hydrogen being absent; so that we were never convinced that the spectrum was not due to the former substance.

It is well to remark in passing, that our previous work on the spectrum of the carbon compounds was mainly directed to that particular spectrum which is characteristic of the flame of cyanogen, and only indirectly to the flame spectrum of hydrocarbon. We were further supported in connecting the latter

[1] "On the spectra of the Compounds of Carbon with Hydrogen and Nitrogen." I and II. *Proc. Roy. Soc.*, vol. 30, pp. 152, 494. "On the Spectrum of Carbon," *ib.*, vol. 33, p. 403. "General Observations on the Spectrum of Carbon and its Compounds," *ib.*, vol. 34, p. 123. "On the Spectrum of Water," *ib.*, vol. 30, p. 480 and vol. 33, p. 274. "Investigations on the Spectrum of Magnesium," *ib.*, vol. 32, p. 189.

spectrum with acetylene, by observing that cyanogen compounds are continously formed when the arc discharge takes place in gases containing nitrogen, and that in all probability their formation is due, as Berthelot has shown, to a reaction taking place between acetylene and nitrogen. Berthelot is positive in his assertions that cyanogen is never formed by a direct combination between carbon and nitrogen, and any such apparent combination is due to impure carbon, and the presence of an imperfectly dried gas; in other words, hydrogen is essential to the production of cyanogen under such conditions according to the views of Berthelot.

The fact that carbonic oxide, which is one of the most stable binary compounds of carbon, forms a distinct spectrum of a character similar to that of the flame spectrum, tended to support the view that the flame spectrum might originate with acetylene. The similarity in the character of the magnesium-hydrogen spectrum to that of the hydro-carbon flame spectrum induced us to believe that they were due to similarly constituted compounds, and seeing we felt sure about the accuracy of the view, which assigns the former spectrum to some compound of magnesium with hydrogen, we accepted the analogy in favour of the supposition that acetylene is the substance which produces the flame spectrum; or, at any rate, that acetylene is a necessary concomitant of the reaction taking place during its emission, and consequently might give rise to this peculiar spectrum.

Having examined this question in the way described, we adopted the view of Angström and Thalén as to the genesis of this spectrum in opposition to the views of Attfield, Morren, Watts, Lockyer, and others, who held that this spectrum was really due to the vapour of carbon. The delicate character of the experiments which were required to discover the origin of the peculiar set of flutings in the more refrangible part of the spectrum of cyanogen made it apparent that, whatever views as to the origin of the hydrocarbon flame spectrum were adopted by different workers, it could not be regarded hitherto as experimentally proved which was the correct one.

With the object of being able to exhaust this question, a special study was subsequently made of the ultra-violet line spectrum of carbon, in order to ascertain whether any of its lines could be found in the spectra of the arc or flame. We have found that the ultra-violet lines of metallic substances have as a rule the greatest emissive power, and are often present when no trace of characteristic lines in the visible part of the spectrum can be detected. If carbon resembled the metals in this respect, then we might hope to find ultra-violet lines belonging to its vapour, thus enabling us to detect the volatilisation of the substance at the relatively low temperatures of the arc and flame. The test experiments made on this hypothesis are recorded in the paper entitled "General Observations on the Spectrum of Carbon and its Compounds." It is there shown that some seven of the marked ultra-violet spark lines of carbon occur in the spectrum of the arc discharge, although one of the strongest lines, situated in the visible portion of the spectrum at wave-length 4266, could not be found. Further, it is proved that the strongest ultra-violet line of carbon does occur in the spectrum of the flame of cyanogen fed with oxygen. Thus it seems probable that the same kind of carbon molecule exists, at least in part, in the arc and flame, as is found to be produced by the most powerful electric sparks, taken between carbon poles or in carbon compounds.

Now the spark gives us the spectrum which is associated with the highest temperatures, and therefore it is assumed that this spectrum is that of the simplest kind of carbon vapour. If that be the case, we cannot avoid inferring that denser forms of carbon vapour must exist in arc and flame, emitting, like other complex bodies, a fluted, in contrast to a line, spectrum; or rather that the two distinct kinds of spectra may be superposed. Such considerations showed that a series of new experiments and observations must be made with the special object of reaching a definite conclusion regarding the origin of the flame spectrum, and the following paper contains a summary of the results of such an inquiry.

Vacuous Tubes.—We have heretofore laid little stress on observations of the spark in vacuous tubes on account of the great uncertainty as to the residual gases which may be left in them. The film of air and moisture adherent to the glass, the gases occluded in the electrodes, and minute quantities of hydrocarbons of high boiling-point introduced in sealing the glass, may easily form a sensible percentage of the residue in the exhausted tube, however pure the gas with which it was originally filled. The excessive difficulty of removing the last traces of moisture we learnt when making observations on the water spectrum, and the almost invariable presence of hydrogen in vacuous tubes is doubtless due in great measure to this cause. Wesendonck (*Proc. Roy. Soc.*, vol. 32, p. 380) has fully confirmed our observations as to this difficulty. By a method similar to that employed by him, we have, however, succeeded in so far drying tubes and the gases introduced into them that the hydrogen lines are not visible in the electric discharge. For this purpose the (Plücker) tube was sealed on one side to a tube filled for some six or eight inches of its length with phosphoric anhydride, through which the gas to be observed was passed, and on the other side to a similar tube full of phosphoric anhydride, which was in turn connected by fusion to the (Sprengel) pump. To dry the gas it is not enough to pass it through such a tube or even a much longer one full of phosphoric anhydride; it has to be left in contact with the anhydride for several hours, and to get the adhering film of moisture out of the tube it has to be heated after exhaustion while connected, as above described, with the drying tubes up to the point at which the glass begins to soften, and kept at near this temperature for some time. To get most of the gases out of the electrodes, the tube must be exhausted and sparks passed through it for some time before it is finally filled with the gas to be observed. Even when these precautions have been observed, the lines of hydrogen can often be detected in tubes filled with gases which should contain no hydrogen. The general result of our observations on the spectra observed in tubes so prepared is that the channelled spectrum of the flame of hydrocarbons is not necessarily connected with the presence of hydrogen; it does not come and go according as hydrogen is or is not present along with carbon in the way that the channelled spectrum of cyanogen comes and goes according as nitrogen is present or absent. Our observations confirm those of Wesendonck on this point. This spectra given by various tubes containing carbon compounds are described in the paper.

Tubes filled with carbonic oxide exhibit in general at different stages of exhaustion the following phenomena. When the exhaustion is commencing and the spark will just pass, the spectrum is usually that of the flame of hydrocarbons and nothing else. As the exhaustion proceeds, the spectrum of carbonic oxide makes its appearance superposed on the former, and gradually increases in brilliance until it overpowers, and at last at a somewhat high degree of exhaustion, entirely supersedes the flame spectrum. This is when no jar is used. In the earlier stages of exhaustion, the effect of the jar is to increase the relative brilliance of the flame spectrum, and diminish that of the carbonic oxide spectrum, and at the same time to bring out strongly the lines of oxygen and carbon; at a certain stage of the exhaustion, when the flame spectrum is very weak without the jar, the effect of the jar is to bring it out again, but without sensibly enfeebling the carbonic oxide spectrum, and without bringing out the carbon lines. At a still higher stage of exhaustion, when the carbonic oxide spectrum is alone seen without the jar, the flame spectrum is sometimes, not always, brought out by putting on the jar, though the carbon lines again show well. At this stage, at which the flame spectrum is not seen at all, the distance between the striæ in the wide part of the tube is considerable, and much metal is thrown off the electrodes, which are rapidly heated by the discharge. In a tube filled with carbonic oxide mixed with a little air imperfectly dried, when not too highly exhausted, the carbonic oxide spectrum, that of the flame of hydrocarbons, and that of cyanogen, may all be seen at once superposed when no jar is used. With a jar and a tolerably high exhaustion the carbonic oxide spectrum, the hydrocarbon flame spectrum, and the carbon line spectrum, may all be seen at the same time.

Spectrum of the Spark in Compounds of Carbon at Higher Pressures.—In the spark taken between poles of purified graphite in hydrogen, the spectrum of hydrocarbon flames is seen, and it increases in brilliance, as the pressure of the gas is increased up to ten atmospheres, and continues bright at still higher pressures so far as we have observed, that is, up to twenty atmospheres. The spark without condenser in carbonic oxide at atmospheric pressure, shows both the spectrum of carbonic oxide and that of the hydrocarbon flame; and as the pressure of the gas is increased, the former spectrum grows fainter, while the latter grows brighter, no jar being used. The line spectrum of carbon is also visible. At the higher pressures the flame spectrum predominates and is very strong. The observations were carried up to a pressure of twenty-two and a half atmospheres. On letting

down the pressure, the same phenomena occur in the reverse order. All the parts of the flame spectrum, as seen in a Bunsen burner, are increased in intensity as the pressure is increased. The fact that the effects of high pressure are so similar to those produced by the use of a condenser at lower pressures, seems to point to high temperature as the cause of those effects. But against this, we have the fact that at reduced pressure we get in carbonic oxide, the carbonic oxide spectrum and the line spectra of carbon and oxygen simultaneously, without that of the hydrocarbon flame. As we cannot doubt that a very high temperature is required to give the line spectrum of carbon, we must suppose that reduced pressure is unfavourable to the stability of the molecular combination, whatever it be, which gives the hydrocarbon flame spectrum. Wesendonck has remarked (*loc. cit.*) that in carbonic acid at pressures too low for the flame spectrum to be developed without a jar, it is only in the narrow part of the tube that the use of a jar brings out that spectrum. It would appear, therefore, that the constraint, due to the confined space in which the discharge occurs, has the same effect, in regard to the stability of the combination producing the spectrum in question, as increase of pressure.

Cyanogen Flame Spectrum.—Our former observations "On the Flame Spectrum of Cyanogen Burning in Air" were made on cyanogen gas, prepared from well-dried mercury cyanide, which was passed over phosphoric anhydride, and burnt from a platinum jet fused into the end of the tube. We observed what Plücker and Hittorf had noted, that the hydrocarbon bands were almost entirely absent, only the brightest green band was seen, and that faintly. When gaseous cyanogen is liquefied by the direct pressure of the gas, the researches of Gore (*Proc. Roy. Soc.*, vol. 20, p. 68) have shown that it is apt to be contaminated with a brownish, treacley liquid, which probably arises from the imperfectly purified or dried cyanide of mercury. In order to obtain pure cyanogen, we have prepared quantities of liquid cyanogen, not by compression, but by passing the already cooled gas into tubes placed in a carbonic acid and ether bath. By this method of condensation any easily liquefiable substances are isolated, and any permanently gaseous substance escaped. The samples were sealed up in glass tubes into which different reagents were inserted. After such treatment the cyanogen was used for the production of the flame in dry air or oxygen. The liquid cyanogen was left in contact with phosphoric anyhdride, Nordhausen sulphuric acid, and ordinary sulphuric acid. By means of a special arrangement of glass tubing surrounding the flame dry oxygen could be supplied, or oxygen made directly from fused chlorate of potash could, by means of a separate nozzle, be directed on to the flame, and thus perfectly dry and pure gases used for combustion. Liquid cyanogen which had remained in presence of the above reagents gave only the single green hydrocarbon flame line faintly in dry air, all the cyanogen violet sets being strong. When oxygen made directly from chlorate of potash was directed on to the flame, all the hydrocarbon flame sets appeared with marked brilliancy. The set of lines which we have formerly referred to as the three set of flutings of the cyanogen spectrum, showed marked alteration of brilliancy with variations in the oxygen supply. Thus, liquid cyanogen purified by the action of the above reagents, does yield the spectrum of hydrocarbon flames on combustion in pure oxygen. From the great precautions we have taken, we feel sure that the amount of combined hydrogen in the form of water or other impurities in the combining substances must have been exceedingly small, and that the marked increase in the intensity of the flame spectrum, when oxygen replaces air is essentially connected with the higher temperature of the flame, and is not directly related to the amount of hydrogen present. This being the case, it must be admitted that the flame spectrum requires a higher temperature for its production during the combustion of cyanogen than that which is sufficient to cause a powerful emission of the special spectra of the molecules of cyanogen. Now, the two compounds of carbon, which give the highest temperature on combustion are cyanogen and acetylene. Both of these compounds decompose with evolution of heat, in fact, they are explosive compounds, and the latent energy in the respective bodies is so great that if thrown into the separated constituents a temperature of near four thousand degrees would be reached. The flames of cyanogen and acetylene are peculiar in this respect, that the temperature of individual decomposing molecules is not dependent entirely on the temperature generated by the combustion, which is a function of the tension of dissociation of the oxidised products, carbonic acid and water. We have no means of defining with any accuracy the temperature which the particles of such a body may reach. We know, however, that the mean temperature of the flames of carbonic oxide and hydrogen lies between two and three thousand degrees, and if this be added to that which can be reached by the substance independently, then we may safely infer that the temperature of individual molecules of carbon, nitrogen, and hydrogen in the respective flames of cyanogen and acetylene may reach a temperture of from six to seven thousand degrees.

A previous estimate of the temperature of the positive pole in the electric arc made by one of us, was something like the same value.

The formation of acetylene in ordinary combustion seems to be the agent, through which a very high local temperature is produced, and this is confirmed by the observations of Gouy on the occurence of lines of the metals in the green cone of the Bunsen burner, which are generally only visible in spark spectra. On this view acetylene is a necessary agent in the production of the flame spectrum during combustion. The fact that the flame spectrum is often invisible when the arc is taken in a magnesia crucible, although the cyanogen spectrum is strong, but may be made to appear by introducing a cool gas or moisture, must be accounted for by an increased resistance in the arc producing temporarily a higher mean temperature. The experiments in course of execution, where the arc will be subject to a sudden increase of pressure, will, we trust, solve this difficulty.

Further evidence of the high temperature of the cyanogen flame is afforded by the occurrence in the spectrum of that flame, when fed with oxygen, of a series of flutings in the ultra-violet, which appear to be due to nitrogen. The series consists of four, or perhaps more, sets, each set consisting of a double series of lines overlapping one another. The lines increase in their distance apart on the more refrangible side, otherwise the flutings have a general resemblance to the B group of the solar spectrum.

The four sets commence approximately at about the wavelengths 2718, 2588, 2479, 2373 respectively. They are frequently present in the spectrum of the arc taken in a magnesia crucible, and show strongly in that of the spark taken without a condenser either in air or nitrogen. As they appear in the spectrum of the spark in nitrogen, whether the electrodes be aluminium or magnesium, and do not appear when the spark is taken in hydrogen or in carbonic acid gas, they are in all probability due to nitrogen. When a large condenser is used they disappear.

Linnean Society, December 21, 1882.—Alfred W. Bennett, M.A., in the chair.—Prof. Adolph. Ernst, of Venezuela, and Dr. W. C. Ondaatje, of Ceylon, were elected Fellows.—Prof. T. S. Cobbold exhibited specimens of Ligules from the Bream, the Minnow, and the Grebe to compare with those from man. The worm from the Bream is called *L. edulis* by Briganti, and is eaten under the name "macaroni piatti."—Mr. T. Christy called attention to experiments lately made, which show that the Kola nut possesses singular properties of clearing fermented liquors.—Mr. Thos. H. Corry read a paper on the development and mode of fertilisation of the flower in *Asclepias Cornuti*. R. Brown, 1809, J. B. Payer, 1857, and thereafter H. Schacht, have made Asclepias the subject of interesting study; Mr. Corry nevertheless has added new observations thereto. He finds that the petals and stamens, which in the early stage originate separately, become afterwards adnate; the stamens, moreover, by their broad filaments form a fleshy pentagonal ring, *i.e.* are monadelphous. The "*stigma-disk*" is not formed by the fusion of two stigmas, for the styles proper remain distinct throughout their entire extent. The greatest analogy of the flower to that of the Apocynaceæ is at this period; thereafter differences ensue. From a careful study of the different stages of the pollen in Asclepias it appears to exhibit a perfectly isolated and peculiar case of formation. The idea that self-fertilisation can take place with the parts *in situ* is shown to be impossible, and the need for insect or artificial aid rendered imperative. Cross-fertilisation is the great law in the Asclepiads.—Dr. F. Day read a paper, "Observations on the Marine Fauna of the East Coast of Scotland," founded on a recent survey by H.M.S. *Triton* off Aberdeen, Kincardine, and Forfar. As regards the herring and its migrations, they shift their locality for breeding purposes or in search of food, occasionally being driven from a spot where extensive netting or other causes disturb them. The herring seems of late years to take to deeper water off shore, but at times they appear to return to their old

habitats in the comparatively shallow water. Although it is true that some fisheries—Wick, for instance—have decreased in plenty, at the same time other places, *e.g.* Fraserburgh, have proportionately increased. The fishery records prove that from the beginning of this century onwards there has been a steady annual increase of fish taken, though desponding fishermen aver to the contrary. At Wick, herring of different ages and conditions arrive and depart thrice yearly. Dr. Day recounts the results of his dredgings, and describes the Crustaceans and Mollusks obtained.—An additional report on the Echinoderms collected by Dr. Day, was made by Prof. F. J. Bell, and of the Zoophytes and Sponges by Mr. S. O. Ridley.—Mr. J. G. Baker afterwards read his second contribution on the Flora of Madagascar. In this paper, upwards of 150 new species of monopetalous dicotyledons are characterised. They were gathered chiefly by the Rev. R. Baron, F.L.S., of the London Missionary Society. Among others described are four new genera, one nearly allied to Cinchona, a second of semi-parasitic Scrophulariaceæ, and two of Acanthaceæ; besides these, many representatives of well-known European genera occur.—Prof. T. S. Cobbold read a description of *Ligula Mansoni*, a new human Cestode. He shows it to be extremely probable that the trout's ligule is the sexually immature state of the great broad tapeworm of man. Other interesting genetic relations are established, and several important generalisations discussed.—Additions to the Lichens of the *Challenger* Expedition was a short paper by the Rev. J. M. Crombie.—Mr. J. G. Baker made a second communication, being descriptions of about thirty plants from the Fiji Islands referred to by Mr. J. Horne in his recent work on the economic resources of Fiji.

Victoria Institute, January 1.—A paper upon "Design in Nature," was read by Mr. W. P. James. It was stated that Prof. Stokes, F.R.S., would read a paper at the next meeting.

PARIS

Academy of Sciences, January 2.—M. Blanchard in the chair.—M. Rolland was elected vice-president for 1883.—The Academy has lost four Members during 1882, viz MM. Liouville, Decaisne, Bussy (Free Academician), and Wöhler (Foreign Associate); and six Correspondents, viz. MM. Plantamour, Lutke, Billet, Darwin, Cornalia, and Schwann.—Memoir on the vision of material colours, &c. (continued), by M. Chevreul.—Researches on hyponitrites; first part, chemical researches, by MM. Berthelot and Ogier. They study hyponitrite of silver, describing their analyses, and examination of the action of heat and oxidising agents, also calorimetric measurements. The formula $N_2O_3Ag_2$ agrees best with the results.—Ramifications of *Isatis tinctoria*, formation of its inflorescences, by M. Trécul.—It was announced that the U.S. Congress had invited the President of that country to convoke all nations to a conference with a view to adoption of a common initial meridian and an universal hour.—Reply to the objections presented by MM. Faye and Hirn to the theory of solar energy, by Dr. Siemens.—On a method of photographing the corona without an eclipse of the sun, by Dr. Huggins.—On geodetic circles, by M. Darboux.—On algebraic integrals of linear differential equations with rational coefficients, by M. Autonne.—On a communication of M. de Jonquières relative to prime numbers (continued), by M. Lipschitz.—Remarks on the subject of a note of M. Hugoniot, on the development of functions in series from other functions, by M. du Bois-Reymond.—Does oil act on the swell or on the breaker? by M. Van der Mensbrugghe. His theory applied only to two cases: where calm water, covered with oil, came to be acted on by wind, and where waves break. The relative calm of phosphorescent portions of tropical waters, he attributes not to increase of cohesion of the water (Admiral Bourgeois), but to the innumerable floating objects forming an obstacle to the slip of surface-layers over each other.—Decomposition of formic acid by the effluve, by M. Maquenne. The results are the same as those got by M. Berthelot in decomposing gaseous formic acid in a closed vessel, by heat alone, about 260°.—On the chloride of pyrosulphuryl, by M. Ogier.—On a vibrion observed during measles, by M. Le Bel. It is found in the urine in the early stages, and disappears with the fever; is a slightly curved, very refringent rod, moving very slowly; contains oval spores at one-third of its length, in a bag of dead protoplasm, which gradually disappears, the spore showing then a zone of mucilage around it. Another occurrence of spores on the thirty-fifth day was observed in an adult. The vibrion also may be got from the skin at the time of desquamation. M. Le Bel cultivated the vibrion, and injected it into a guinea-pig; which, on the tenth day, showed small vibrions in its urine, but did not seem incommoded. The urine in scarlatina and in diphtheria shows a microbacterium and a micrococcus, respectively, both quite different from the vibrion of measles.—Existence of zinc in the state of complete diffusion in dolomitic strata, by M. Dieulafait.—On the Marine Carboniferous of Haute-Alsace; discovery of culm in the valley of the Bruche, by MM. Bleicher and Mieg.—On the excitant property of oats, by M. Sanson. He has experimented with Du Bois Reymond's electrical apparatus on the neuromuscular excitability of horses, before and after ingestion of oats, or of an excitant substance, which he isolated from oats (from the pericarp of the fruit); this is called *avenine*, is quite unlike vanilline, is uncrystallisable, brown in mass, finely granular, and has the formula $C_{56}H_{21}NO_{18}$. All kinds of cultivated oats elaborate it, but in different quantity; as a rule the white varieties have less than the dark. The quantity seems also to depend on the place of cultivation. Crushing the grain weakens the excitant property. The total duration of the excitation (which grows to a point, then gradually disappears) seemed to be about an hour per kilogramme of oats ingested.

Errata in last week's report.—P. 236, top of second column, 7th line, for "Guimareo" read "Guimarães" 9th line, for "argotised" read "azotised"; 13th line, for "usteria" read "Asteria"; 16th line, for "*pedunculus*" read "*pedunculatus*"; 16th line, for "suctocitiates" read "suctociliates."

VIENNA

Imperial Academy of Sciences, November 9, 1882.—The following papers were read:—K. Laker, studies on the hæmatic discs (Hayem's hæmatoblasts), and on the so-called dissolution of the white blood-corpuscles in the process of the purification of the blood.—E. Ludwig, note relating to the chemical composition of the damburite from the Scopi Mountain (Graubündteng).—T. Herzig, on guaiaconic acid and guiacic acid.—On the action of nitrous acid on guiacol, by the same.—A. Grunow, preliminary communication on the Diatomaceæ collected by the Austro-Hungarian North Polar Expedition.

November 16, 1882.—The following papers were read:—N. Polejæff, on the sperm and spermatogenesis of *Sycandra rajahamus Hæckelii*.—F. v. Hauer, new contributions to the knowledge of the elder tertiary Brachiuara fauna of Vicenza and Verona (Italy).—M. Margules, note on the dynamo-electric process.—A. Tarolimek, contributions to mechanical theory of heat.—K. Zelbr, on the comet Schmidt, October 9, 1882.—A sealed paper dated from November 6, 1882, was opened and read containing a short note by Josef Popper, on the transmission of power and the realisation of unused natural powers by electricity.

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THURSDAY, JANUARY 18, 1883

GEIKIE'S GEOLOGY[1]

Geological Sketches at Home and Abroad. By Archibald Geikie, LL.D., F.R.S. With Illustrations. (London and New York: Macmillan and Co., 1882.)

Text-Book of Geology. By Archibald Geikie, LL.D., F.R.S. With Illustrations. (London: Macmillan and Co., 1882.)

II.

WE now come to consider the quality of the matter contained in the volume, the discrimination exercised in its selection, the validity of the theories presented, and the fidelity with which the science is portrayed. It is the function of a text-book to exhibit to the student an impartial and symmetric outline of the science. Its author is under obligation to present the views which are generally entertained by the great body of geologists, carefully withholding those which are peculiar to himself. From the great mass of available matter he must select that which will afford a well-balanced and comprehensive review, and he must sedulously avoid giving undue prominence to those matters which have special interest to himself by reason of his individual studies. In the work before us this has been accomplished in a manner which may truly excite admiration. Although the author is an original investigator in several departments of the science he delineates, he has permitted his own predilections to give little if any additional prominence to his special topics, and the wisdom he has displayed in the selection of material and the balancing of parts will commend itself to all professional readers.

It is useless to attempt an analysis of a work which is itself an epitome of a great science, but we may refer to the treatment of a few mooted points and to a few matters of novelty or of current interest.

The microscopical characters of rocks are treated more at length than in any other text-book. In the general account of rock characters they are accorded even more space than are the macroscopical, and they form part of the description of each specific rock. They are, moreover, illustrated by a series of cuts, showing the appearance of thin slices when highly magnified. A chapter is devoted to the subject of rock determination, and an analytical table is included therein.

The results of the *Challenger* exploration of the bottom of the ocean are given at some length, and the conclusion is drawn that the continental regions of the globe have been marked out from the earliest geological times. This is not treated as an hypothesis but as an established theory, and its logical consequences appear in numerous places.

In the taxonomic terms of stratigraphy, the convention of the Bologna Congress is not adopted. The terms *system*, *series*, and *stage* are used in the same order, but *group*, which by the congress was made more comprehensive than *system*, is by Geikie used as the equivalent of *stage*. He remarks, with propriety, that the attempt to alter the signification of a term so universally employed in English literature would produce far more confusion than can possibly arise from a failure to conform to continental usage.

One of the most conspicuous omissions of the book is with reference to the antiquity of man. The subject is treated with great brevity, because it is regarded as belonging more properly to archæology, but an account is nevertheless given of the earliest human vestiges. Mention is made of man's association with the Loess and with the inter-Glacial deposits of Europe, but the Californian claims to his pre-Glacial existence are ignored. It is true that these claims have been disputed, and it is true that the evidence in regard to each of the individual finds upon which they rest is incomplete; but since Whitney has assembled all the facts in his "Auriferous Gravels," it must be admitted that their cumulative force entitles them at least to recognition and consideration, however slow we may be to accept them as demonstrative.

In the section which treats of man as a geological agent, there are enumerated a great variety of ways in which he modifies the face of nature, but one of the principal, if not indeed the chief of all, is omitted, namely, the stimulus he gives to denudation by tilling the soil. The mat of vegetation, living and decaying, which naturally covers the soil in all humid regions, affords great protection against the erosive work of rain. Not only is the beating of the rain resisted, but the velocity of its outflow is retarded, so that from surfaces of gentle inclination it washes away very few particles. When this mat has been removed, and especially when the surface has been stirred by the plough, the conditions become exceedingly favourable to rain erosion, and the rain rills are charged with sediment. Moreover, cultivation and the cutting of forests increase the magnitude of river floods, and since rivers perform their chief work of erosion and transportation during flood-stage, the quantity of their work is thus augmented. It is safe to say that the rate of degradation of the surface by rains and rivers is increased several hundred per cent. by the removal of forests and the tillage of the soil, and it may be added that for this reason most attempts to measure the natural rate of denudation by means of the outscour of rivers have been abortive.

The unconformability between the Archæan and the Palæozoic is not mentioned in such way as to convey an impression of the profoundness of the chronological break. There is no known locality where a newer formation rests conformably upon the Archæan. There are few where the discordance of dip is not great. There are few where the superior formation is not relatively unaltered, and none where the inferior formation is not highly metamorphosed. So far as we know, the Archæan strata were both thrown in great folds and plicated in detail, were universally subjected to a metamorphism such as in later rocks seems to have been accomplished only at a depth beneath the surface, and were subsequently worn away upon a most stupendous scale before they received any sedimentary covering within the regions now accessible for examination. Compared with this, all other chronological breaks are trivial, and we may almost say that, compared with this, all other stratigraphical breaks are local.

In treating of the condition of the interior of the earth, Geikie concisely presents the prominent hypotheses, and

[1] Continued from p. 239.

then remarks that it is "highly probable that the substance of the earth's interior is at the melting-point proper for the pressure at each depth." In treating of the age of the earth, he sets forth the geological and the physical arguments with commendable brevity, but withholds all expression of individual opinion. In treating of the origin of orographical displacement he gives a brief history of opinion, and states that the contractional hypothesis is now generally accepted. A foot-note, however, refers to Fisher's "Physics of the Earth's Crust," which appeared while the text-book was passing through the press. The cause of ice motion is not discussed.

In the classification of formations there is nothing new. The Cambrian and Silurian are marked as independent and co-ordinate divisions, the latter beginning with the Arenig group in Great Britain and with the Calciferous in America, but the opinion is expressed that a subsequent revision of the subject may result in "throwing all these older Palæozoic rocks into one palæontological system." The pre-Cambrian rocks are designated by Dana's title of Archæan. The Rhætic is included with the Trias. In the table of formations the American Laramie is placed in the Tertiary; but this appears to have been done by inadvertence, for in the descriptive text which follows it is treated as Cretaceous.

In the classification of rocks the primary division is into crystalline and clastic. The crystalline are separated into stratified, foliated, and massive, and the clastic into sand rocks, clay rocks, volcanic fragmental, and organic fragmental. Of the massive crystalline rocks, the principal sub-group is indicated as *feldspar bearing*, and four small groups (the nepheline rocks, the leucite rocks, the olivine rocks, and the serpentine rocks) are indicated as co-ordinate with this.

The subject of geological climate is treated almost exclusively from the astronomical point of view, and the theory of Croll is the only one which receives more than passing mention. Its statement was prepared especially for the volume by Dr. Croll himself, and covers six pages. It is undoubtedly true that this theory has been widely accepted, that it is very generally entertained as a working hypothesis, and that it is the most probable one before the public; and it should for these reasons be given great prominence in a text-book; but I cannot help regretting that it has been presented with so little qualification. It deals with a series of physical laws and physical conditions which interact upon each other in an exceedingly complex way—in so complex a way that meteorologists, who have to deal with only a portion of them, do not claim and scarcely hope for a complete analysis of their combinations. The opportunities for arguing in a circle are most seductive, and the *a priori* probability that important considerations have been overlooked is not small. The only manner in which so comprehensive and intricate an hypothesis can be established is by stimulating inquiry which shall lead to corroborative evidence, and this is precisely what Croll's hypothesis after eight years of wide publicity has failed to do. If it is true, then epochs of cold must have occurred with considerable frequency through the entire period represented by the stratified rocks; and iceberg drift, if no other traces, should have been entombed at numerous horizons. It has not been found, however, and of the eight horizons claimed by Croll to show evidence of glacial action, the treatise under consideration mentions only two with confidence, and two others with doubt. In the two instances to which queries are not attached, the phenomena appear to indicate local and not general glaciation. If the hypothesis is true, the cold of the Glacial epoch must have been many times interrupted by intervals of exceptional warm, but little has been added to the evidence adduced by Croll for such an interruption, and in America, where there is now great activity in the investigation of glacial phenomena, the evidence of a *single* inter-glacial period is cumulative and overwhelming, while there is no indication whatever of more than one. If the hypothesis is true, submergence in polar and temperate regions should have been coincident with glacial expansion, and emergence coincident with glacial retreat, but the Quaternary history of Great Britain, as drawn in the new text-book, includes two periods of maximum ice-extension, *separated* by a period of maximum submergence. While these difficulties exist it appears to me unadvisable to convey to the student the impression that a satisfactory solution to the problem of glacial climate has been reached.

Because I have mentioned some points in which my individual judgment differs from that of Prof. Geikie, it must not be supposed for a moment that I undervalue his work, or that I regard it with anything short of enthusiastic commendation. It is broad and catholic, conscientious in detail, masterly in treatment. It is imbued especially with a spirit which for want of a better name may be called scientific modesty. The majority of our text-books, including all of our best text-books, have been written by teachers, and have been more or less affected by the peculiar mental attitude of the teacher. The investigator is under the constant necessity of holding his judgment in abeyance, and of treating every conclusion as an hypothesis, to be tested by future researches, and possibly amended or even abandoned. The teacher is under an equal necessity to formulate his knowledge so that he may communicate it in definite shape—he must not doubt, he must know; and under this compulsion he naturally and unconsciously acquires an undue confidence in results that have simply arisen from the weighing of probabilities. He is especially tempted to regard classifications as final, and not to recognise them as temporary presentations of temporary stages of knowledge. It is the especial merit of Prof. Geikie's book that it is untainted by this teacher's bias. It cautions the student against the confusion of geological synchrony with stratigraphical homotaxis; it cautions against the free use of palæontological evidence in the inference of geological climate; it cautions against deductions which may be vitiated by the imperfection of the geological record, and against negative evidence in general; it cautions against the impression that there are in nature any hard and fast lines separating epochs or formations or rock species; and, in addition, it heeds its own cautions. Its readers cannot escape the impression that the science of geology is in its youth, that it is developing at a rapid rate, that many of its results are tentative, and that its unsolved problems are as numerous and important as those it has successfully attacked.

It is only by a conscious effort that one gives attention to the literary style of Prof. Geikie's text-book. It is so

direct and plain that it serves the purpose of conveying thought without leaving an impression of the manner of conveyance. As in the matter, so in the manner, his personality is not permitted to intrude. He says one thing at a time, and therefore his sentences are short; but he does not exaggerate, and therefore he never indulges in epigram.

A noteworthy feature of the illustrations is the reproduction of a large number of De la Beche's cuts, which are derived directly from the original blocks. All of these are good, and so are the majority of the remaining illustrations, but there is also a considerable number, especially in the chapters on stratigraphy, which are not so distinct as is desirable, and which probably owe their imperfection to the employment of some photo-mechanical process. The typography is excellent, and a page of errata is not called for.

The foot-notes contain a very large number of useful references. These are not mere citations of authorities in support of statements in the text, but are indications to the student of treatises in which he may find the fullest exposition of subjects to which the text introduces him.

G. K. GILBERT,
U.S. Geological Survey

SACHS'S TEXT-BOOK OF BOTANY

Text-Book of Botany, Morphological and Physiological. By Julius Sachs, Professor of Botany in the University of Würzburg. Edited, with an Appendix, by Sidney H. Vines, M.A., D.Sc., F.L.S., Fellow and Lecturer of Christ's College, Cambridge. Second Edition. (Oxford, 1882.)

THERE are not wanting signs that the study of botany is steadily increasing in this country. An immense number of text-books or manuals have been published in English during the last thirty years on the subject, some of which have been very popular, to judge by the many editions they have passed through. Referring to these introductions to the study of botany in general terms, it was to be noted that they all, in a more or less complete manner treated of the vegetable kingdom from a morphological and classificatory point of view; but that the morphological portions were deficient in clear descriptions or conceptions of the origin or development of the members of the plant's body which they described, and the student who required instruction as to physiological, anatomical, or embryological details, had to look for such in the pages of the botanical periodical literature of the day. Most modern workers in biology will agree that the greater portion of this literature was derived from German sources, and it is scarcely to be denied that the first general compendium of note appeared in the German text-book of Sachs. This work had reached a fourth edition in 1874, but the previous editions had found their way into several of the centres of botanical teaching in Great Britain and Ireland, and had caused a considerable change in the older methods of teaching botany. Still it must have been a matter requiring some courage for the delegates of the Clarendon Press to undertake the costly work of translating, editing, and printing in English this work of Sachs', forming a large octavo volume of nearly 1000 pages, a text-book one would think far too large and expensive for most ordinary students. This work was, however, issued from the Clarendon Press in the spring of 1875, and it is not without interest to note that for the last two or three years it has been completely out of print, so that the edition must have been exhausted in the course of the first four or five years after its issue. It was most unfortunate that this edition, so ably translated by Messrs. Bennett and Thiselton Dyer, had not been based on the fourth German edition, which had been published nearly a year before the English translation made its appearance. The success of the translation may, however, be looked on as to a certain extent condoning this misfortune, and there can be doubt as to the revolution in the study of botany in these kingdoms, which has been brought about by its appearance. Instead of to an endless catalogue of under- and above-ground forms of stems, instead of a list as long as that of the ships in Homer of the forms of simple and compound leaves, the student has had his attention—at least in some schools—called to the important structures to be met with in these varied portions of a plant and to their peculiar functions and ontogeny. The subject of plant life and development seems to have become of more especial interest and to have fallen like a new story on many even old ears. It was not, under these circumstances, surprising that a new edition was called for, but it did excite some surprise that, having in a great measure made the demand, the Delegates of the Clarendon Press seemed unable for a time to supply it, and let several Long Vacations glide by without its appearance; even this new edition comes to us late in the autumn season of the year, when the year's fruits have been well garnered in. Still it is welcome as an important contribution to the study of a science that has of old and for long been fostered by the University of Oxford.

Welcome as this new edition is, it would, we firmly believe have been a much more complete text-book and have reflected more of credit on the Clarendon Press Series, if the present Editor had been given a fairer field to work on. Although the fourth German edition was in advance of the previous one, yet the half-dozen years that have elapsed since it made its appearance have been years during which botany has advanced with no tardy footsteps. Even Prof. Sachs himself could not be persuaded to face the torture of a fifth edition of the original, for he felt, as he tells us, that the expanded views of the present period would not even fit into the framework of his text-book, so that a faithful translation of the fourth edition is even more out of date in 1882 than the translation of the third edition was in 1875.

Hence it must have been distressing for the Editor of the volume before us to find, on entering on his task, that nearly the whole of Book I., which treats of the general morphology of the cell, the tissues, and the external conformation of plants had been for some time in type, and that consequently a number of recent discoveries had not been noticed in it. No one could have been better fitted than Mr. Vines to have brought this most important section up to date, and it is a pity that only 32 out of its 232 pages were reprinted, for there is a decided awkwardness in looking in an appendix for supplementary

remarks which are often found to explain away or totally alter the meaning of the text.

As it is, we would emphasise the prefatory remark of the Editor, that "the Appendix has come to be an important feature" of the book, and is to be especially recommended to the notice of the reader. The student who will judiciously introduce the new material in this appendix into the older structure in the text will be afforded a tolerably clear insight into the present standpoint of vegetable morphology.

'Book II. forms the largest third of the volume, and from a purely critical point of view, was the least satisfactory portion of the original. No doubt it was by far the most difficult portion to condense into any reasonable compass, and it bristled with unknown quantities and controverted points, and indeed it may well be doubted if the immense subject of "General Morphology and Outlines of Classification" could be fairly well mastered by any one botanist. That the editor has added a great deal of new material—no doubt assisted by some whose criticisms and suggestions he gratefully acknowledges—is, on the face of this second book, abundantly clear. That a good deal might have been still added is also, on a little examination, made apparent. Detailed criticism on this portion of the volume would be here to a large extent out of place, and serve no good end, but as a justification of these remarks we would observe that among the very first forms alluded to—the Protophyta arranged under the Cyanophyceæ—of which the Nostocaceæ form a highly interesting group—the description of the formation of the Nostœ filament, though amended, as noted within brackets, is, despite the warning of Bornet, founded on a misconception of Thuret's account. In a footnote, too, we read that Archer has described the occurrence of spores in *Nostoc paludosum*, as if this were something novel, but their appearance in many species has not only been known to but made even a factor in their classification by Bornet. It seems inexplicable to us why this distinguished author's works should be so little known to English writers, but so it is, and on turning over page 247 to see what would be said about the Rivulariaceæ—the Scytonemaceæ—we felt disappointed at not finding even a reference to show the student how much has been done by Bornet in recent years to add to our knowledge of these groups. To these remarks we will only add that the large and important groups of Palmellaceæ are dismissed in a paragraph of ten lines. In order that these remarks may not be mistaken, we may observe that we did not expect to find more than a sketch of the natural history of the forms to be found in these groups lying at the base of chlorophyllaceous life, but we did expect that what was narrated of these would be exact, and that a reference to the latest literature of the subject would be given. It would be easy, at least among the Thallophytes, to extend these criticisms. Such an excessively interesting algal form as Pithophora is nowhere alluded to, though its first birth-place seems to have been our Royal Gardens at Kew. Wittrock's paper on this form was fully as important as his on Mesocarpeæ. Nor is the student referred under Fucoideæ to the splendidly illustrated work on the group by Thuret and Bornet; but criticism is not our object, and we gladly pass from the notice of Book II. to notice Book III., from which, knowing the excellent work done by the Editor in vegetable physiology, we expected great things, nor have we been disappointed. It seems to us an excellent account of vegetable physiology, with all or most of all the modern discoveries alluded to, and we know of no compendium on the subject at all approaching to it.

Should in another four years this second English edition be sold out, let us hope that Mr. Vines will, like its author, cease trying to mend the old garment, but will of his own energy and knowledge give us an Introduction to the Study of Botany, which we doubt not would be worthy of appearing as one of the Clarendon Press Series, and which will wipe away the reproach, true to this of physiological botany as of the drama, that we are forced to fly with all too borrowed plumes.

E. P. WRIGHT

RECENT ELECTRICAL PUBLICATIONS

Electricity. By Robert M. Ferguson, Ph.D., F.R.S.E., of the Edinburgh Institute. New Edition, revised and extended by James Blyth, M.A., F.R.S.E., Professor of Math. and Nat. Phil. in Anderson's College, Glasgow. (London and Edinburgh: W. and R. Chambers, 1882.)

Electric Illumination. By Conrad Cooke, James Dredge, M. F. O'Reilly, S. P. Thompson, and H. Vivarez. Edited by James Dredge. Chiefly compiled from *Engineering*. With Abstracts of Specifications, prepared by W. Lloyd Wise. Vol. I. (London: Office of *Engineering*, 1882.)

PROFESSOR BLYTH has done good service by the judicious additions which have to a great extent revivified Dr. Ferguson's well-known little manual, and brought it up to the level of the times.

The actual progress in electrical science since the original book appeared has not been anything extraordinary, but the amount of it which the public are willing to learn has undergone a prodigious increase, and the modern text-book is therefore expected to enter into details about a number of matters which a few years ago would have been scouted as altogether too difficult. It is these semi-advanced portions which Prof. Blyth has incorporated with the old stock of the work, the stock remaining about the same. There was very little to which one could object in the original; if it erred, it erred as a rule only by omission. In the new edition, however, we have information, and though concise it is mostly good and reliable information very intelligibly expressed, concerning Sir Wm. Thomson's electrometers, mariner's compass, and thermo-electric discoveries, also concerning electrostatic and electromagnetic induction, and other matters which had been but very lightly glanced at in the original edition. It also refers to Mr. Crooke's experiments, Mr. Spottiswoode's coil, Prof. Tait's thermo-electric diagram, and Dr. Kerr's discoveries. The operation of making a text-book may be compared to the operation of skimming, and the depth to which this operation may be safely carried depends, we suppose, mainly on the taste of the public at the time. Prof. Blyth has added to Dr. Ferguson's original skim a slightly deeper and more substantial layer; and fortunately neither of the authors have

forgotten the very important preliminary operation of blowing aside the froth and scum which accumulates on standing, and which an injudicious skimmer is very apt to obtain and exhibit as his sole result.

The book appears almost contemporaneously with Prof. S. P. Thompson's little work on the same subject, which we noticed some months back, and may be taken as complementary to it. Although both are of the same scope, yet the area open to them was so wide that it seldom happens that they both contain equally full information on precisely the same subjects.

The second volume which we here notice, viz. the compilation entitled "Electric Illumination," is of very different appearance and scope. It is a handsome large octavo, well printed, and with admirable illustrations. It is not addressed to students, but to engineers and practical men, and it is a most useful summary of notices which have appeared in the pages of *Engineering*, concerning dynamo machines, electric lamps, and the other paraphernalia connected with the practical applications of electricity. It aims, of course, more at completeness than at judicious selection; and it therefore naturally includes a number of contrivances which are hardly likely to come into any notorious existence.

While it is very useful as a book of reference, therefore it is scarcely calculated for ordinary reading, the style of the descriptions being not seldom tiresome, and giving one the usual dismal feeling of "letterpress" written up to a picture. Some of the sections are very full, as for instance that relating to the manufacture of Jablochkoff candles, where the account is so complete that the usual form of the Wheatstone bridge is depicted and carefully explained as if it were a specialty of the Jablochkoff system: while some other sections are distinctly meagre. At the same time it is only natural that some kinds of information should be easier of access than others, and that all that came to hand should be utilised. At the beginning of the book we have a sketch of the early history of dynamo machines, several admirable sketches of lines of force, and a very clear elementary exposition of the principles of magnetic induction. There are also very excellent and instructive skeleton figures of the Gramme and Siemens armatures, as designed by the late Antoine Breguet, though the writer of the article rather absurdly seems to take them as embodying researches which throw a new light on the action of the machines, instead of as useful and interesting illustrations of what was perfectly clear to every physicist.

Throughout the book, in fact, one comes across various curious statements, which, if read hypercritically and pressed, would be either annoying or misleading; but still more frequently one is in the presence of a cautious vagueness which conceals the want of exact knowledge by the turning of a phrase, and one notices a laudable desire to avoid the ascription of either praise or blame and to take the odiousness out of all comparisons.

But to say that some of the writers are often only half acquainted with their subject, and that they accordingly take precautions to avoid mistakes, is only to say that the book belongs to modern periodical literature; to that kind of literature, namely, which is written and read with the tacit understanding on both sides that in a few years at most it is sure to be out of date and forgotten, and that accordingly any serious labour expended on either its production or its assimilation would be labour misspent.

Taken for what it is, however, it is difficult to imagine a more complete and handy publication of information for which at the present time there is a great demand, and the book will be welcomed by all who take an interest, professional or otherwise, in those applications of electricity which are now so evidently imminent, and which must ultimately assume such vast proportions.

O. J. L.

OUR BOOK SHELF

Introductory Treatise on Rigid Dynamics. By W. Steadman Aldis, M.A. (London: G. Bell and Sons, 1882.)

THIS little work is truly characterised by its above title. The portions of the subject selected by the author will be best indicated by the headings of the several chapters. An introductory chapter on kinematics is followed by one on D'Alembert's principle: general equations of motion of a rigid body; impulsive forces. Chapter iii. treats of moments and products of inertia; Chapter iv. of motion round a fixed axis (centres of suspension, oscillation, and of percussion); Chapter v. of motion of a body with one point fixed; and Chapter vi. of the motion of a free body. Chapter vii. discusses certain general principles, as conservation of linear momentum, of moment of momentum, and of energy. In Chapter viii. miscellaneous problems are investigated, as of moving axes, initial motions, small oscillations, and "tendency to break." As might have been expected from so accomplished a teacher, the exposition of the general principles is most clear, and these principles are fully illustrated by a capital selection of exercises, many of which are solved, and for the solution of many others hints are given at the end. We know of no better introduction to this difficult branch of study. The text is most carefully printed.

Encyklopædie der Naturwissenschaften. Herausgegeben von Prof. Dr. G. Jäger (and seven other gentlemen). Erste Abtheilung (Parts 16, 19, 20, 22, 24, 26, 27). (Breslau: E. Trewendt, 1880, 1881.)

THESE seven numbers form parts 6 to 12, *i.e.* the second volume of a "Handbuch der Mathematik," edited by Dr. Schlömilch, the several treatises being written by Dr. R. Heger, Professor at Dresden. The pagination is continuous (1-963 pp.), and there are 235 woodcuts.

The first treatise is on "Analytical Plane Geometry" (pp. 1-194). The first 164 pages are devoted to the conic sections: the mode of treatment, or rather the order of arrangement of propositions, is different from that of any English text-book with which we are acquainted, but approximates most closely to that of Dr. Salmon's classical work. It is a full, able, and interesting presentment of the properties of these curves. The remaining thirty pages are devoted to a rapid sketch of the principal known properties of curves of the third order, in which are embodied most, if not all, of the results of modern research.

The second treatise is on "Analytical Geometry of Three Dimensions" (pp. 195-380); the third is on the "Differential Calculus" (pp. 381-568); and the last is on the "Integral Calculus" (pp. 569-902).

This last work is broken up into three parts, of which the second treats of elliptic functions, the theta functions, and of elliptic integrals; the third is devoted to differential equations.

There are two smaller works, one (pp. 903-928) on the method of least squares (Ausgleichungs-rechnung), and the other (pp. 929-957) on insurances (Renten-, Lebens-, und Aussteuer Versicherung). A list of works on the different subjects treated of in the "Handbook" is given

on pp. 961-963. These works are all in German, and the only English mathematician whose works are cited is Dr. Salmon, in Teutonic dress.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Pollution of the Atmosphere

In answer to Mr. Joseph John Murphy's letter in NATURE, vol. xxvii. p. 241, stating that the radiation of marsh gas (from the incomplete combustion of coal) would be insignificant compared to that of vapour, I would like to say that their behaviour in the atmosphere is different; the moisture in the air is there, so to speak, on sufferance as long as the pressure and temperature allow it; that is, it is held in suspension by what might be called the capillary attraction of the air of a certain pressure and temperature. If you reduce the temperature you reduce the capacity of air for vapour, first by its reducing the capillary attraction, and secondly, by reducing one of the conditions that makes OH_2 a vapour. If you reduce the pressure, you enlarge the spaces between the molecules and reduce the capillary attraction, if I may apply this term to a gas. This is borne out by the balloon ascents of Mr. Glaisher. At 4 miles high the temperature was 8°, the dew-point was −15°, or a difference of 23°; at 5 miles it was 28°, and at 30,000 feet he states that there is no doubt that the dew-point is a difference of 50°; that is, the higher, the less vapour. But this will not be the case with marsh gas, as it is a permanent gas, and being of less density than even vapour, or about half the density of air, there is no reason why it should not be found in larger quantities at greater altitudes; and I think that its effect there would be that in the temperate zones and at the poles it would radiate its temperature of say from −8 at 30,000 to −30, and produce cold and rain, snow and floods as the storms on the Alps and the floods on the Continent, and in the Tropics to make the nights colder. In fact, it will have a tendency to do the reverse of vapour; vapour retains our heat and shields us from the cold of space. This radiator and absorber will tend to radiate the cold to us or to the vapour in the lower atmosphere, and produce rain and wind.

9, Bootham Terrace, York, January 13 H. A. PHILLIPS

A "Natural" Experiment in Complementary Colours

Since I wrote to NATURE last October (vol. xxvi. p. 573) on the above subject, I have been both surprised and gratified to read no less than six communications on the same matter (vol. xxvi. p. 597, vol. xxvii. pp. 8, 78, 150, 174, 241). Only to-day I have received a letter from a German friend drawing my attention to Goethe's observation at Schaffhausen, he evidently being unaware of Mr. W. R. Browne's interesting letter (vol. xxvi. p. 597). My friend goes on to point out that "Gischt" is certainly foam, for in the context Goethe describes how he saw a rainbow in the "Dunst," or mist, thus enabling us to contrast the two words.

The special point of my communication was the excellent illustration, afforded us naturally, of the advantage of toning down the brightness of the white surface, upon which the complementary tint is to be cooked, until that brightness is suitable to that of the exciting colour. In the experience related by me I was unable to see complementary tints in the foam, upon which full sunlight was falling; the glare of light was too strong.

Mr. C. R. Cross (vol. xxvii. p. 150) speaks of seeing them even in strong sunlight on the crests of waves; may not these crests have been in slight shadow, if the waves were just curling over? The example he gives of cloud *shadows* appearing purple on the ocean illustrates excellently my own observations. The letter from Mr. E. J. Bles (vol. xxvii. p. 241) gives a quotation from Sir C. Lyell, but without further detail I do not feel that much weight can be given to his observation from my special point of view. But I do not at all wish to say that complementary tints are not visible on a white surface in full sunshine; but theory and my own observations are certainly in favour of the advantage (and this is all I claimed) of a reduction of brightness to a level comparable with that of the exciting colour.

CHAS. T. WHITMELL

8, Maryland Street, Liverpool, January 15

The Comet

In my letter relating to the September comet, published in vol. xxvii. p. 108, I was guilty of carelessness in copying from my notes the difference of micrometer readings instead of their value in arc. The value of one revolution of the screw is 15″·31075, and consequently the distances given in my letter should be as follows:—

a =	6″·57	November 3
b =	16·90	
c =	8·34	
a =	7·36	November 6
b =	16·51	
c =	10·15	

W. T. SAMPSON

U.S. Naval Observatory, Washington, D.C., January 2

The Transit of Venus

Will you be kind enough to make the following correction in your published report of the times of contact of phases of the transit of Venus. The third contact should be 2h. 39m. 57s. in place of 2h. 38m. 57s. These were both inadvertencies. From a comparison of Mr. Finlay's place at the Cape of Good Hope on September 8, I find that these elliptic elements satisfy the place within 7 seconds of arc in Right Ascension and 1·5 seconds of arc in Declination. EDGAR FRISBY

U.S. Naval Observatory, Washington, D.C., January 3

Early Coltsfoot

Last year I recorded (NATURE, vol. xxv. p. 241) Coltsfoot in blossom on January 6, on the sides of the railway near here, probably an unprecedentedly early date. The mild weather lately prevailing induced me to suspect the former early blossoming might find a parallel *this* year. I saw the plant in flower *this morning*, near the same spot; one flower-stalk was fully four inches high, so it should have been observed some days previously had sunshine prevailed. Last year the winter was practically over (so far as hard frost was concerned) at the beginning of January. Will this be paralleled by the winter of 1882-83? R. MCLACHLAN

Lewisham, January 12

Baird's Hare

Some of your readers may be interested in reading the following extract in which mention is made of a fact similar to that found in NATURE vol. xxvii. p. 241, about *Baird's Hare*. The extract is from the Life of St. Francis Xavier, by H. J. Coleridge, S.J. In a letter written from Amboyna in May, 1546, Francis says:—

. . . "In the island of Amboyna I have seen what no one would believe . . . a *he-goat* giving suck to his young kids with his own milk; he had one breast which gave every day as much milk as would fill a basin. I saw it with my own eyes, for I would not believe it without seeing it. A respectable Portugese has the goat, and is taking it away, meaning to carry it to Portugal." T. MARTYR

49, High Street, Clapham, S.W., January 15

The Projection of the Nasal Bones in Man and the Ape

The form and projection of the osseous frame-work of the human nose being considered by anthropologists of considerable value in a racial point of view, a close comparison has recently been made of the profiles of the external nose of man and the nose-case of the anthropoid apes. It has resulted in the conviction (1) that the absence of projection in the nasal bones of the chimpanzee, the gorilla, and the orang constitutes a distinction of more importance than has generally been assigned to it, and not the less so seems the fact (2) that a slight nasal elevation is

observable in the skulls of some of the gibbons, and in the lower monkeys, as, for instance the baboons.

The distinction appears to me to be of the same kind as the erect position of man and the different order of the length of his toes as compared with the ape and many of the lower animals—as for instance the third toe in the lion, bear, dog, badger, and hare.

It should be remembered that the nasal bones in man form merely a bridge or back to the osseous structure of the nose, which is mainly due to the upheaval laterally of the pre-maxillary bones. These are less elevated in other animals, and there is no tilting of the nasals proper. In the chimpanzee and the orang the nasals are as flat as in the hippopotamus. On referring to Prof. Mivart's essay on the apes in the "Encyclopædia Britannica," I find he alludes to the transverse convexity of the bones of the nose, which he considers a marked character of man's skull, entirely absent in the chimpanzee. He adds: the nasals in the orang are exceedingly small and flat, "often even uniting in one bone."

In connection with the subject, it may be mentioned that in Quain's "Anatomy" the external nose is said to be due to the development of the frontal lappets in the fifth or sixth week of the human embryo. It is represented in a woodcut in Balfour's "Embryology" as well-formed and prominent so early as in the ninth week.

The existence of the nasal spine in the nostrils of man, but not in the ape or any of the lower animals, is a fact that has to be accounted for. It appears to have been overlooked, but is of some importance in connection with the development of the human nose.

J. PARK HARRISON

January 14

P.S.—The peculiarities of the human nose and the *rationale* of its formation are fully treated of in Prof. Humphry's "Human Skeleton," p. 220.

THE COMET

THE following communication from Dr. Gould of Cordoba Observatory (Argentine Republic) appears in *Astronomische Nachrichten*, No. 2481:—

On September 6 I received information that a bright comet was visible in the east before sunrise. My informant had seen it on the morning of the 5th, and described it as being as bright as Venus and with a brilliant tail. Inquiry showed that it had been seen for several days by *employés* of the railroad and other persons whose duties required them to rise before daylight.

Not only was the morning of September 7 cloudy, but the eastern sky was overcast on every morning for a whole week. On one occasion it seemed that a part of the comet's tail could be distinguished, but not even an approximate position could be obtained for the head. On the morning of the 14th the comet was first seen at the Observatory, and an approximate position obtained from the circles of the equatorial telescope by pointings with the finder. It was then only 13′ south of the equator, and moving northwardly.

The telescope was equipped with the photographic lens and apparatus, and as my series of stellar photographs was nearly completed and its continuance for a few weeks demanded constant attention, I was reluctant to change the adjustments. It has been my uniform policy in Cordoba to confine our instrumental observations to the southern half of the sky, and, in general, to such regions as are not well visible from northern observatories. And as the comet had been conspicuous for more than a week, was on the equator, and the date of equinox was close at hand, it appeared unadvisable to sacrifice important and unique observations for the sake of determinations of the comet's position which I could not doubt were making under more favourable circumstances in the north. Consequently no micrometric observations were undertaken; but rude determinations of position were repeatedly made, from that time on, by use of the finder and the graduated circles, in order to follow the comet's course and deduce approximate elements and ephemeris.

On September 16 the brightness of the head was such, that it was visible with the finding-telescope throughout the day; and I prepared to observe it on the meridian, having followed it with the equatorial until within half an hour of the time of transit. Its declination was about +0° 52′. But not more than five minutes before that moment a large cloud drifted across the meridian, making the observation impossible.

September 17 the comet was very bright and easily found in the full sunlight. At 10h. 40m. a.m. it was necessary to use a shade-glass, on account of its proximity to the sun; and at 11h. the sun and comet were in the same field of view. I again attempted to observe it upon the meridian, but was prevented by a new difficulty. The comet was hidden by the disc of the sun, and although I carefully scrutinised this and especially the preceding limb as it traversed the field of the meridian-circle, no token of the comet could be seen, nor could it be found during the afternoon. Although it must have passed in front of the sun, I then supposed it to have passed behind it and been occulted.

On Monday the 18th the comet was again on the preceding side of the sun and decreasing in declination at the rate of more than 2½′ hourly. Early in the day its brilliancy attracted popular attention throughout the country, and the "blazing star near the sun" was the one topic of remark. Telegrams came to me from all parts of the country, as well as from Chile and Uruguay, calling attention to the phenomenon. In the small telescope it presented the aspect of a brilliant nebulous mass, having at each end curved appendages like horns or wings, nearly large as the central body, and at their base quite as brilliant; the general form of the whole reminding one of the winged globes carved on ancient monuments. This appearance, unquestionably due to the outrush of glowing vapour from the nucleus, was also exhibited, although to less extent, on the two following days, during both of which the comet remained visible to the naked eye.

As soon as the elements of the orbit could be obtained, its similarity to that of the comet of 1843-1880 was manifest, and the suspicions regarding its identity and the hypotheses to which these gave rise presented themselves forcibly, as I am sure they must have done to astronomers in the northern hemisphere, where I doubt not they have long since been a theme of discussion. The perihelion-distance, although small, seems clearly larger than that of the orbits of 1843 and 1880; but how far such discordance is consistent with the hypothesis of identity must be decided by future investigation. The comparatively small amount of study which I have been able to give to the question leads me to think that the orbit deduced from observations before the perihelion may differ somewhat from that indicated by the observation since September 17; but as the Cordoba observations prior to that date were of a crude description, I have impatiently waited for tidings from other observatories. No. 2459 of the *Astr. Nachr.*, which has just reached me, leads me to fear that micrometric or meridian observations may not have been made before perihelion. In such case the rough positions obtained here with the finder and circles of the equatorial may possess a value far greater than was supposed possible at the time. Those previous to the perihelion are ten in number, and although I do not believe that their probable error can exceed a minute of arc in either coordinate, they are not represented within this limit by the elements deduced from observations made since the perihelion. Should no better positions have been obtained I will send these to you; but I cannot yet abandon the hope that some belated astronomer may have seen this brilliant object in season to secure a series of observations before the perihelion passage.

Micrometric determinations have been made here on various dates since October 17, and have now begun

systematically, as the comet is growing lower for northern observers. But as it will not pass the limit of 30½°, S. Decl. the southerly observatories of Europe and all those of the United States will probably be able to follow it as long as it remains visible, and will find comparison stars in Argelander's Zones.

It will not have escaped your notice that all the elements differ from those of 1880 in the same direction in which these differed from those of 1843.

In the earliest observations made with the large telescope there appeared to be, in the place of the nucleus, a series of bright points following the axial line. The preceding and brightest of these seemed scarcely to exceed the tenth magnitude, and all were connected by intermediate material of somewhat less brilliancy which made it difficult or impossible to count them. Mr. Thome, who has made all the micrometric measurements, thinks that there were certainly not less than five or six, and perhaps more. The appearance was as though the original nucleus had been resolved into a series of ill-defined granules. These have gradually become less and less distinct, until the place of the nucleus now appears occupied by a line of irregular definition and unequal brightness, about 45″ in length, and of an average width of about 5″. All our determinations of position were made for the preceding and brightest of these nodules while they were clearly distinct; and, since then, for the anterior extremity of the bright line, where is a point which is still somewhat brighter than the remaining portions.

Since we are at present overloaded with work, in the preparation of the Zone-Catalogue, and observations are, without doubt, still making in Europe and North America, I will reserve the micrometric determinations until the reductions can be revised.

Those made on the meridian are as follows:—

	α	δ
	h. m. s.	° ′ ″
1882, Sept. 18 ...	11 20 51·3 ...	+0 16 39·3
19 ...	11 14 31·0 ...	−0 32 38·6
21 ...	11 4 57·9 ...	−1 59 30·2

Cordoba, November 14, 1882 B. A. GOULD

DESTRUCTION OF LIFE IN INDIA BY WILD ANIMALS

IN a recent communication I called attention to the loss of human and animal life in India from snake bites; I now proceed to describe the mortality due to wild animals, which, though much less than the former, is very considerable, and forms an important item in the mortuary returns.

The statement appended shows in detail for each province the number of persons and cattle killed by wild animals, and the number of wild animals destroyed, with the rewards paid for their destruction during the year 1881, as compared with the previous year. The figures are summarised in the following tables:—

Number of Human Beings and Cattle Killed by Wild Animals

	Persons killed.		Cattle killed.	
	1880.	1881.	1880.	1881.
Madras	223 ...	238 ...	8,667 ...	8,668
Bombay	136 ...	141 ...	4,537 ...	2,398
Bengal	1,295 ...	1,367 ...	14,567 ...	8,423
North-Western Provinces and Oudh ...	561 ...	470 ...	8,140 ...	7,971
Punjab	42 ...	27 ...	7,986 ...	4,083
Central Provinces ...	289 ...	248 ...	3,711 ...	2,929
British Burma	32 ...	34 ...	978 ...	898
Coorg	Nil ...	Nil ...	219 ...	191
Assam	234 ...	211 ...	3,269 ...	2,802
Hyderabad Assigned Districts	24 ...	18 ...	3,560 ...	3,013
Ajmere-Merwara ...	4 ...	3 ...	216 ...	264
Total	2,840 ...	2,757 ...	55,850 ...	41,640

Number of Wild Animals destroyed and Amount of Rewards Paid

	Destroyed. 1880.	Rewards. 1880.		Destroyed. 1881.	Rewards. 1881.	
		Rs.	a. p.		Rs.	a. p.
Madras.....	1,284 ...	16,579	10 0 ...	1,429 ...	20,251	5 0
Bombay	1,717 ...	4,775	1 0 ...	1,367 ...	4,965	13 8
Bengal.....	4,783 ...	24,841	10 6 ...	4,213 ...	23,316	3 4
N.-W. Provinces and Oudh...	2,924 ...	7,295	4 0 ...	3,037 ...	8,434	14 0
Punjab.....	1,389 ...	4,715	0 0 ...	1,411 ...	4,856	3 0
Cent. Provinces	1,408 ...	17,887	8 0 ...	1,351 ...	15,842	0 0
British Burma.	639 ...	3,468	0 0 ...	1,059 ...	4,260	8 0
Coorg.....	26 ...	140	0 0 ...	15 ...	215	0 0
Assam.....	541 ...	7,022	10 0 ...	1,176 ...	7,552	2 0
Hyderabad Assigned districts	167 ...	1,590	0 0 ...	216 ...	2,156	0 0
Ajmere-Merwara	8 ...	13	0 0 ...	5 ...	Nil	
Total ...	14,886 ...	88,327	11 6 ...	15,279 ...	91,850	0 0

The resolution of Government, dated November 8, 1882, in dealing with this subject, gives the following details, which are so far satisfactory, as they show that organised measures are now being put in force for the destruction of wild beasts, and that already there has been diminution in the loss of human and domestic animal life. As in the case of venomous snakes, the prevention, or at all events diminution of loss of human and domestic animal life from the ravages of wild animals, is a question mainly of time, perseverance, and expenditure of money. The last consideration perhaps may have stood in the way of progress, not that expenditure of rupees either has been or would be grudged, were there certainty that it would overcome the evil, but that there may have been, perhaps is, a natural reluctance to spend public money for what seems an uncertain benefit, as some have regarded a system of rewards for destruction of snakes and wild animals. The Government of India has always evinced a desire to adopt any steps that might reasonably afford hope of relief, and many resolutions by the supreme and local Governments, and considerable expenditure of money with this object in view, proves that the authorities have been and are alive to the magnitude of the evil and to the importance of repressing it, and that they have taken measures which in some districts have been attended with a fair amount of success. But the absence of a thoroughly organised system of dealing with the evil, and the desultory and varying methods employed have prevented the attainment of the success that might fairly be expected and would be obtained under better arrangements; and it will not be until some complete organised system have been steadily and perseveringly prosecuted that the desired result will be accomplished. A few years ago (in 1878), when calling attention to this subject, I noted that the loss of life from wild animals in 1875 and 1876 had been as follows:—

Animals.	Killed in 1875. Persons.	Cattle.	Killed in 1876. Persons.	Cattle.
Elephants	61 ...	6 ...	52 ...	3
Tigers	828 ...	12,423 ...	917 ...	13,116
Leopards	187 ...	16,157 ...	156 ...	15,373
Bears	84 ...	522 ...	123 ...	410
Wolves	1,061 ...	9,407 ...	887 ...	12,448
Hyænas	68 ...	2,116 ...	49 ...	2,039
Other animals ...	1,446 ...	3,011 ...	143 ...	4,573
Total... ...	3,735 ...	43,642 ...	2,327 ...	47,962

Comparing these returns with that of 1880-81 it will be observed that the loss of life has not been materially diminished

	Persons killed.
1880	2,840
1881	2,757
1875	3,735
1876	2,327

though there is reason to hope that future yearly reports will be more favourable.

Registration is now becoming more accurate than it has been, and the returns are probably more reliable than they were, but they do not indicate any marked improvement on the whole. It is evident, however, from the terms of the resolution before referred to, that Lord Ripon is determined to deal vigorously with the evil, and, just as in the case of the poisonous snakes—only, perhaps, more surely—will the result, in time, justify the expenditure which must needs be incurred.

Of the wild animals and venomous snakes which destroy life in India, the wolf and tiger, it will be seen, are the chief offenders among the former, the cobra and bungarus (krait) among the latter. A list of the rewards that have been offered at various times and in different parts of India is appended, but I do not know the amount now offered for each animal, though it is probably much on the same scale. If these rewards be distributed regularly and systematically throughout India, they will probably suffice to insure a steady reduction in the number of noxious animals, and so will diminish a great evil.

"The figures quoted show a decrease during the year under review, as compared with the previous year, both in the number of persons and cattle killed; and, on the other hand, an increase in the number of wild animals destroyed. As was the case in the previous year, the mortality which occurred in Bengal and in the North-Western Provinces and Oudh, was far greater than in other provinces. Of the total number of deaths, 2757 were caused by wild animals, the figures for the previous year being 2840.

The number of persons killed in Bengal (747), and in the North-western Provinces, and Oudh (208) by wild animals other than those specifically named in the returns, was considerable. In future returns the animals which come under the general head "other animals," and which causes in all provinces a very large proportion of the mortality, should be specified in a foot-note, with the number of deaths caused by each kind.

The total number of cattle killed also decreased. This result is chiefly due to the exclusion from the Bengal return of sheep and goats, of which a large number were included in the figures of the year 1880. There has, however, been a marked decrease in the number of cattle killed by wild animals in the Bombay Presidency. In the Punjab, also, the number of cattle killed was considerably less than in the preceding year, but in this province, as in the case of Bengal, the decrease appears to be due to the exclusion of sheep and goats from the returns of the year 1881.

The number of wild animals destroyed was 15,279, against 14,886 in 1880. The number of tigers, leopards, bears, and wolves destroyed was 1557, 3397, 991, and 4538 respectively, as compared with 1689, 3047, 1100, and 4243 in the preceding year; and the number of human beings killed by these animals respectively, amounted to 889, 239, 75, and 256, against 872, 261, 108, and 347 in the year 1880.

Of the total amount of rewards paid during the year, Rs 91,850 were awarded for the destruction of wild animals.

In the review of the returns for the year 1880 a hope was expressed that endeavours would be made to induce men belonging to the Shikari class to devote themselves specially to the work of destruction in districts which are more than usually infested with wild animals, and Local Governments were authorised to make special arrangements for the experimental employment of such men. From the present reports it appears that the Government of Madras has decided that the employment of a paid corps of Shikaris is undesirable, as the cost of supervision would be excessive, while the employment of such a corps would discourage local Shikaris. On this point the Governor-General in Council desires to remark that where local Shikaris exist it is very desirable that every encouragement should be held out to them, and that in such cases it is preferable to trust to fixed, certain, and prompt payments according to results, as the most effective way of inducing the Shikaris to devote themselves to the work. At the same time certain tracts of country exist in which the special and temporary employment of men from outside may be very useful and expedient, and the reports show that the adoption of this plan has in some cases been followed by satisfactory results. For instance, in the Futehpore district, in the North-Western Provinces, the entertainment of a body of special Shikaris resulted in the destruction of a considerable number of wolves with which that district was infested. In Dinapore, in the Lower Provinces, also, professional hunters were engaged during the closing month of the year for the destruction of tigers.

"In the Central Provinces the ravages committed by tigers in the Balaghat and Seoni districts necessitated the offer of enhanced rewards for their destruction, and the district officer of Seoni has endeavoured to organise a special expedition of shikaris for the purpose of hunting down the animals, and has provided the shikaris with ammunition. Licenses under the Arms Act appear to have been more freely given than hitherto to persons who require arms for protecting themselves and their cattle and crops from the attack of wild animals, but the Governor-General in Council desires to take the opportunity of expressing a hope that this matter will be carefully kept in view by Local Governments and Administrations in order that every possible facility may be offered to cultivators and others for obtaining such licences in districts in which wild beasts are more than usually abundant."

Wild Animals destructive to Life in India

	CARNIVORA
	Felidæ
Felis—F. leo	Lion
F. tigris	Tiger
F. pardus	Leopard
F. jubata	Hunting Leopard
	Hyæninæ
Hyæna—H. striata	Striped Hyæna
	Canidæ
Canis—C. pallipes	Wolf
C. aureus	Jackal
	Ursidæ
Ursus—U. isabellinus	Brown Bear
U. tibetanus	Black Bear
U. labiatus	Sloth Bear.
	UNGULATA
	Elephantidæ
Elephas—E. indicus	Elephant
Rhinoceros—R. indicus	Rhinoceros
	Suidæ
Sus—S. indicus	Wild Boar
	Bovinæ
Gavæus—G. gauri	Bison, gaur
Bubalus—B. arni	Buffalo, arna
	SAURIA
	Crocodilidæ
Crocodilus—C. palustris	Crocodile
C. biporcatus	,,
C. pondicerianus	,,
Gavialis—G. gangeticus	Gharial
	PISCES
	Carcharidæ
Carcharias—C. gangeticus	Groundshark of Ganges

Poisonous Snakes of India

Those marked with an * are most deadly.
Those marked with a † are most common among the most deadly.

Poisonous Colubrine Snakes

Elapidæ

1. Naja	N. tripudians †, cobra, several varieties
2. Ophiophagus	O. elaps *, hamadryas
3. Bungarus	B. cæruleus †, krait
,,	B. fasciatus, sankni
4. Xenürelaps	X. bungaroides
5. Callophis	C. intestinalis and several other species

Hydrophidæ, or Sea Snakes (all deadly)

1. Platurus	P. scutatus, P. Fischeri
2. Hydrophis	H. cyanocincta, and several other species
3. Enhydrina	E. bengalensis
4. Pelamis	P. bicolor

Viperine Snakes

Crotalidæ, or Pit Vipers

1. Trimeresurus	T. gramineus and several other species
2. Peltopelor	P. macrolepis
3. Halys	H. himalayanus
4. Hypnale	H. nepa

Viperidæ, or true Vipers

1. Daboia	D. russellii †, Chain Viper, Tic-polonga
2. Echi	E carinata †, Phoorsa snake, Afaë, Kuppur

The following is a scale of the rewards offered in different parts of India, at different times, for wild beasts and snakes :—

Tigers

	Rupees.
Bengal	12½ to 50
Berar	10 ,, 20
Bombay	6 ,, 60
Burmah	5 ,, 20
Central Provinces	10 ,, 100
Hyderabad	20
Madras	50 to 500
Mysore	35
North-West Provinces	10
Oudh	None
Punjab	None
Rajpootana	10 to 15

Lions

The only record of which I find official mention, is 25 rupees in Kotah.

Panthers, Leopards, Cheetahs

	Rupees.
Bengal	2½ to 10
Bombay	3 ,, 12
Burmah	5 ,, 10
Hyderabad	10
Madras	25
Mysore	15
North-West Provinces	5
Rajpootana	8 to 10
Central Provinces	5 ,, 12

Wolves

	Rupees
Bengal	5 to 20
Berar	3 ,, 5
Bombay	4
Central Provinces	2 to 5
Madras	5
North-West Provinces	5
Oudh	1 to 6
Rajpootana	5

Hyænas

	Rupees.
Bengal	1 to 2
Berar	5
Central Provinces	½ to 2
Madras	3½

Bears

	Rupees.
Bengal	1¼ to 2½
Berar	5
Bombay	3 to 12
Burmah	5 ,, 12
Hyderabad	5
Madras	5
Central Provinces	2 to 5
North-West Provinces	3
Rajpootana	5

Snakes (Species not reported)

Bengal	4 annas
Berar	—
Bombay	6 pie to 4 annas
Burmah	—
Central Provinces	1 rupee
Hyderabad	8 annas to 2 rupees
Madras	1 anna
Mysore	8 annas
North-West Provinces	2 rupees
Oudh	—
Punjab	2 annas
Rajpootana	1 to 8 annas

No rewards appear officially proclaimed for elephants, buffaloes, or bisons. In cases of notorious rogue elephants rewards have been specially given. In Burmah 5 to 20 rupees offered for alligators ; in special cases, more has been given in Bengal and Madras.

The difference in the amount of the rewards appears to indicate that higher sums were offered in special cases, probably when the creature was a notorious man or cattle-slayer.

Now I cannot help thinking that if Government made it part of the duty of district officers, not only to proclaim these rewards but to encourage the destruction of wild animals and snakes, by means of an organised establishment, which should be supplied in these districts, much benefit might result. The money rewards already offered would probably suffice for wild animals, but those for venomous snakes should be increased; if, at the same time, the people were encouraged to work for the rewards, and were aided by persons acting under properly selected superiors, the result would soon show a diminution of the wild animals and snakes. But, I repeat that not until some organised establishment is formed, to be worked steadily throughout the whole country—not dependent on the will or subject to the caprice of individuals, but under local officers subject to one head—will any real or progressive amelioration of the evil be effected. Such a department under a selected officer, would, as was the case with the Thugs and Dacoits, soon make an impression on a death-rate which, so long as it continues in its present condition, must be referred to a defect in our administration.

J. Fayrer

Palæolithic Implements of North-East London

In 1855 Prof. Prestwich published in the *Quarterly Journal of the Geological Society* an account of a fossiliferous deposit in the gravel of West Hackney. The precise locality of the excavation is given, and from 1855 to now many neighbouring excavations have been made. They almost invariably exhibit the "Palæolithic Floor." In 1855 only little was known of palæolithic implements, yet it is a remarkable thing that none of these objects, so common and well-made as they usually

are at West Hackney, arrested Prof. Prestwich's attention. It is also remarkable that although a list of twenty-three land and freshwater shells is given in that paper, yet it does not include the only two of especial interest, viz. *Corbicula fluminalis*, Müll., and *Hydrobia marginata*, Mich.; the first of which is extremely common, and the latter frequent. The branches of trees, "sharply broken into short pieces," and the fossil bones, "showing no trace of wear or fracture," are frequent in all the West Hackney pits. One may be sometimes very near a curious discovery and yet miss it.

In NATURE, vol. xxvi. p. 579, I described and illustrated the West Hackney, or Stoke Newington, palæolithic gravels as understood by me, confining myself to the geological aspects of the situation. I now approach the subject of the weapons and tools contained in the drift of that place. Of the stone implements there are three distinct varieties, each belonging to a different geological time. The implements of these ages are not confined to the valley of the Thames, as they are marked with almost equal distinctness in other places as at Canterbury, Bedford, Southampton, and elsewhere.

In looking for the oldest human works, it would be unreasonable to expect symmetrical implements. The very earliest weapons and tools used by our most remote precursors must have been natural or accidentally broken stones:—naturally pointed stones and stones with a naturally suitable cutting or chopping edge; the first attempts at implement making must have been at the time when the primæval savages "quartered" a stone by smashing it, and then selected pointed and knife-like pieces of this stone for tools.

None of the following rules are without exceptions, for amongst the implements which are usually very large, a very small specimen may now and then occur; and amongst those which are usually very small, there may be at times a large example. The lustre and deep ochreous tints may at times vary a little. Notwithstanding exceptions, when all the characters are taken together, the distinctness of the three classes will hold good.

The oldest known tools are the rarest, and, according to my estimate, can be recognized by the following characters:—they are generally lingulate, or club-shaped, with a heavy butt, often rudely ovate, never acuminate, generally large and very rude, frequently with a thick, ochreous crust, and always greatly abraded, as if they had been tossed about for ages in the sea. Some of these implements are so much abraded that they have lost almost every trace of flaking. These old implements acquired their ochreous crust before they were buried in the gravel, as they occur amongst sub-angular lustrous flints and even chert gravel, where only the implements and a few stray stones exhibit the ochreous crust. I have seen no trimmed flakes or scraping tools belonging to this older age. In London, these old implements are generally found near the bottom of the twenty feet (or even thirty feet) excavations. At Canterbury they occur in thin seams of distinct ochreous material where all the contained flints have an ochreous surface. All these older tools were made at a long distance from where they are now found. Two Canterbury examples are illustrated, half actual size at Fig. 1 and 2, Nos 100 and 126 in my collection. A very important point has now to be especially noticed: when these ochreous instruments were originally tossed about and buried in the gravel some of them became chipped and even broken. Now, the chipped and broken surfaces of these older implements, as at A A, Fig. 1, are *never ochreous*, but invariably of the natural colour of the flint and lustrous. This lustre has been acquired since the gravels were laid down, and it exactly agrees with the lustre of the sub-abraded lustrous implements of medium age found from 8 feet to 10 feet above the ochreous ones. It follows, therefore, that the lustrous implements, although enormously old, can only be as old as the time when the ochreous ones were bruised and broken in the gravels where they are now found.

Another fact must be mentioned here: the men who used the oldest known tools sometimes broke them in two whilst they were at work with them; the accidentally fractured surfaces of this class are of course as old as the tools, and therefore always ochreous. Points and butt ends wholly ochreous are of common occurrence: these pieces of tools must have been shattered for long ages before the gravels of middle age were laid down.

The men who made and used the rude ochreous tools were to a great extent a "whole handed" race—they had not learned the full use of their fingers but held the weapons as one would now hold a heavy stone for smashing. It is probable that the more pointed end of the club-shaped implements was sometimes grasped in the hand and the butt used as a club or hammer. The absence of scrapers indicates that the men probably knew nothing of dressing skins, and were unclothed.

In and near London lustrous and sub-abraded tools of medium age are commonly found at a depth of 12 feet: these tools show a distinct improvement in workmanship over the older ones. Most of the examples are lingulate and acuminate, and the butt, and sometimes the umbo, shows signs of hammering, the ovate form is not uncommon, but the cutting edge all round I have not yet seen. A few chisel ended implements occur. Rude choppers and somewhat large scraping tools are common. All the artificially chipped stones of this medium age are sub-abraded and lustrous. They were not made where now found, but have been carried by the drift for a short distance. A pointed weapon and chopper of medium palæolithic age are illustrated half real size at Figs. 3 and 4, Nos. 588 and 482 in my collection. A scraper of the same age is illustrated at Fig. 5, Scraper No. 9 in my collection.

When the lustrous sub-abraded tools were made the men had by that time acquired the habit of holding their weapons in a lighter fashion,—still in the palm, but more lightly held with the thumb and two next fingers. The frequent presence of horse-shoe and side scrapers now indicates that the men had possibly learned to rudely dress skins for clothing. Sometimes unfinished implements are found; one of medium age from Lower Clapton, London, is illustrated at Fig. 6. The dotted line shows where the point would have been, if the maker had finished it. Implements roughly blocked out to form, and without any secondary trimming, are common: it would appear that the men sometimes first blocked out a number of implements rudely with a heavy hammer-stone, and afterwards finished with neater fabricating tools. An implement in a preparatory stage, of which I have many similar examples, is illustrated in Fig. 7, from Bedford. Many implements were accidentally shattered in the course of manufacture, and the shattered failures are common in all implementiferous gravels.

Long after these two classes of tools were buried by floods of water deep in the gravel and sand, there lived a third race of palæolithic men, as far removed from the men who made the lustrous sub-abraded implements as these latter men were from the makers of the ochreous and highly abraded instruments. These newer tools are found at Stoke Newington at about 8 feet above the lustrous examples, and generally about 4 feet from the present surface. In some places so much top material has been taken off for brickmaking that the stratum containing the newer implements is almost exposed on the surface. Denudation since palæolithic times has considerably altered the contours round north London, and the "Palæolithic Floor" at South Hornsey, close to Stoke Newington, is 14 feet below the surface instead of 4 feet—this 10 feet has been removed in some places by the rains of centuries, in others by modern brickmakers and nurserymen.

The newest palæolithic implements are as a rule not highly lustrous as in the last, but sub-lustrous, and often even dull; not abraded or sub-abraded, but as sharp as on the day they were made. As a rule they are very much smaller and lighter than anything belonging to the two previous periods. An example is illustrated half real size at Fig. 8, No. 403, in my collection. Other characteristic specimens are illustrated at Figs. 9 and 10. Fig. 9

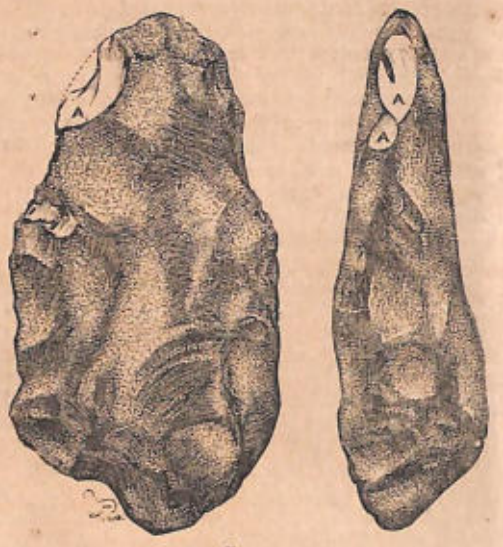


Fig. 1

is a thin and exquisitely manipulated trimmed-flake, No. 47 in my collection, weighing only $1\frac{5}{10}$ ounces. Fig. 10 is an implement worked on both sides, the natural crust of the flint being left untouched on the butt, weight only $1\frac{1}{8}$ ounces, No. 627 in my collection. Oval implements with a cutting edge all round now appear; a few examples, as in the last period, occur where the broad end (as in neolithic celts) appears to have been designed for cutting or chiselling; scrapers are common, not large

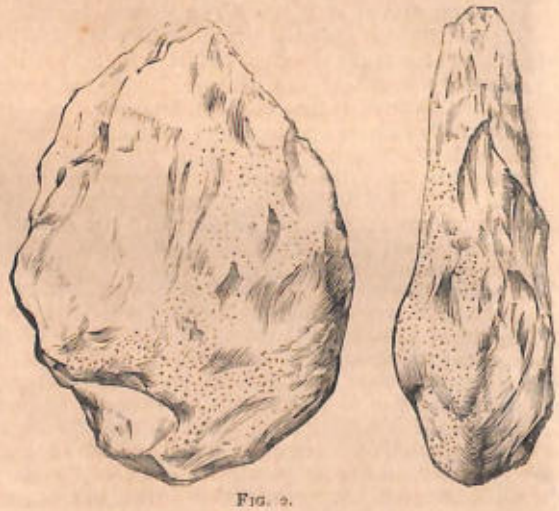


Fig. 2.

and rough, but as a rule small and extremely neat. One is illustrated at Fig. 11, half actual size, scraper No. 22 in my collection; small knives, *i.e.* flakes, with the edge or edges showing very neat secondary trimming, are common, and hardly to be distinguished from neolithic "knives." As a rule every object is now neat, small and fine.

That these later implements are of a different age from the last is proved by the curious fact that the newer implements are sometimes re-made from older ones, *i.e.* re-trimmed after the lapse of a vast period of time. I have several such examples, one a scraper belonging to the "Palæolithic Floor": it is made from an old lustrous flake of medium age, all the more recent work being dull and sharp. At Fig. 12 is illustrated, half real size (No. 452 in my collection), an implement of later palæolithic age from Bedford. It is an old implement that was "found" after a lapse of time by a newer palæolithic man and re-pointed. The finder had probably sense enough to know that the thing he found was really a human-made implement, only wanting a little fresh work to make it "as good as new." This man stands in contrast with the very few individuals (still extant) who say they can see no evidence of design in drift tools. The original form of the implement is indicated by the dotted lines, C, C, C; the natural crust of the flint is present at the base on both sides, shown by dots in the illustration. The original mid-age flaking is shown at B, B, B, B, and

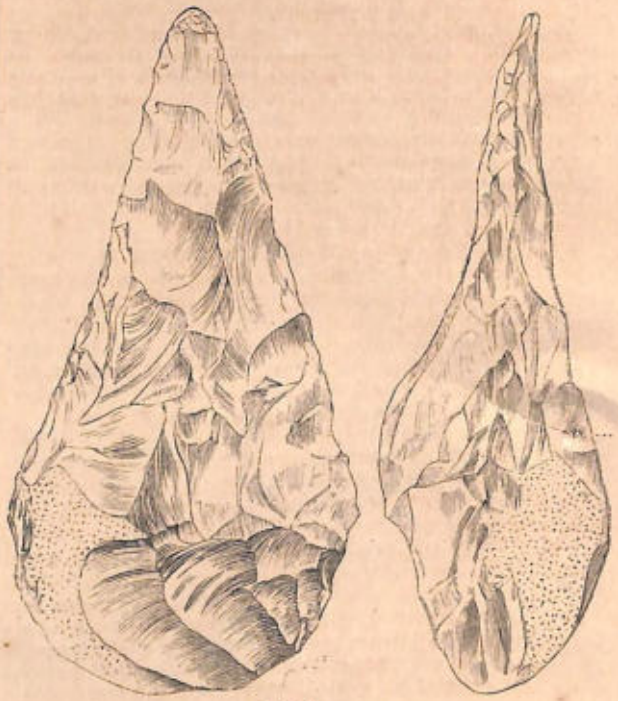


Fig. 3.

the work of the newer palæolithic man is exhibited at D, D, D. The old finder of the implement gave two new edges and a new point to the tool, and improved the shape of the butt; the newer work is creamy white and lustrous, and in distinct contrast with the older work. When this implement was thrown out of the pit by the workman, the newer point got accidentally injured, at E, F. This injury, by exposing the interior of the flint, shows that the tool was originally a greyish-black one, and that since it was last pointed, it has acquired a thick, white bark by the decomposition of the flint. Now, neolithic flints at Bedford (where the example under examination was found) remain blackish-grey to the present day; the thousands of years (say from two to ten) since they were chipped have been insufficient to cause even the thinnest conceivable while film of decomposition to appear, but this palæolithic example has acquired a white bark of a sixteenth of an inch in thickness. How much older then must this *new point* be than the neolithic flints from the same place. The new point being inconceivably old, how much older must the old butt be! The implement, how-

ever, that was "found" (as proved by the flaking of medium age) was new as compared with the older, highly-abraded examples. There is other evidence of the extreme antiquity of these things. They are *all* beneath the "trail and warp." Now the "trail" belongs to geological time, and the period of its deposition is so remote that one can only guess at its age in years. The newest palæolithic implements are every one beneath and older than the "trail," how very much older, then, must be the oldest implements. The proofs that they are really older I have given.

The tools of the later palæolithic period show a marked development of the hand in the makers, for the chippers of these later tools had learned to hold small instruments with the fingers, much as we now hold a small pen, pencil or knife. From the rude and heavy bludgeon the men had advanced to beautiful oval and ovate forms almost perfect in geometrical precision. The progress from the large and rude, to the extremely small and neat scraper, shows that the men had probably progressed in the art of dressing skins, and in every way did finer and neater things. That these men and women now wore necklaces, and possibly bracelets, seems proved by the fact of specimens of *Coscinopora globularis*, D'Orb., occurring with the natural



FIG. 4.

orifice, artificially enlarged. I have several specimens thus enlarged from a horde of more than two hundred, examples all found together near Bedford. Mr. James Wyatt, F.G.S., noticed a similar fact, as recorded by him in the *Geologist* 1862, p. 234. These later palæolithic men lived in large and probably peaceful companies, and their living places stretched in unbroken lines on the old river banks. The "Palæolithic Floors" are not little isolated patches, but places extending for many miles, how large they are it is impossible to say from paucity of excavations.



FIG. 5.

They are not confined to the valley of the Thames, but they occur in many places.

The newer implements and those of middle age are innate with, and have belonged to the gravel from the first. The older implements are distinctly "derived" like the cretaceous fossils commonly found in the gravel. We know whence the fossils have come because they are so common, the abraded ochreous implements on the other hand are very rare, and this rarity makes it difficult to say whence they have been derived, they possibly belong to none of our existing rivers. As in 1868 (*Journal of Anthropological*



FIG. 6.

Institute, Feb. 1879), I recorded my discovery of flakes and implements in the so-called middle-glacial gravel of Amwell, Ware, and Hertford, I have little doubt that the older implements found at North-East London have been derived from these positions. Whether the above-mentioned gravels are really glacial or not, I am not prepared to decide. How the implements got into the gravel I cannot say. I found them in the ballast thrown out of the pits and in the pits themselves. If the gravel is glacial could not glaciers have swept up flakes and tools from old surfaces in the same way as the "trail" has undoubtedly done?

Great caution must be exercised in the acceptance of implements as of glacial age, even if found on the surface of glacial gravels. Men of the later palæolithic age lived only seven miles south of Ware, and there is no reason why they should not have strayed over those high

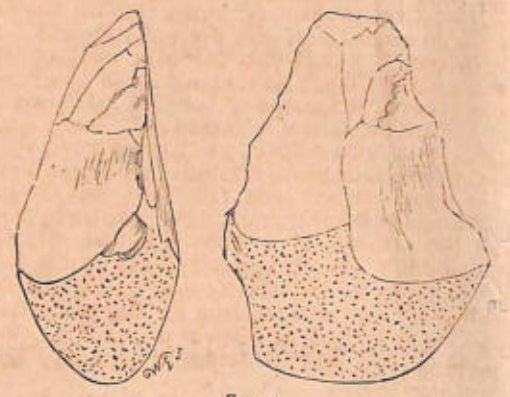


FIG. 7.

positions. Some of the later tools have glacial striæ on the original crust.

There is apparently, but perhaps not really, a gap between each of these three palæolithic periods, as there is apparently a gap between palæolithic (in its vague general sense)



FIG. 8.



FIG. 9.

and neolithic times. Each older period however, has forms which foreshadow the forms which follow in succeeding periods even down to neolithic times. No doubt the fossil bones, if a good series could be obtained, would show a succession of, or possibly different groups, of animals



FIG. 10.



FIG. 11.

in the different deposits, but the bones, antlers, and teeth met with by me, are at present insufficient to define any such groups with distinctness.

The day will come when we shall know much more of palæolithic men than we know now. At present we only know that such men once existed and made weapons and tools of stone during long periods of time. How or where they first appeared as human creatures we can only guess. When we know more we shall modify our use of such terms as "River Drift Men," "Cave Men," &c., and we shall probably be able to mark out more or less distinctly a succession of men, a succession of geological events, and a distinct succession of progressive steps in the men from the lowest savage to the barbarian. Some of our ignorance is undoubtedly caused by the undue attention that has been bestowed on the collection of ornate implements and to the employment of gravel-diggers for their collection. No greater mistake can be made than the mere getting together of the more highly-finished and perfect implements. We only learn from them that certain makers, at first few and far between, common at last,—acquired extraordinary skill in the manufacture of stone tools and weapons. For one perfect example, twenty have their points, butts, or edges injured either by peaceful or warlike work. Collectors

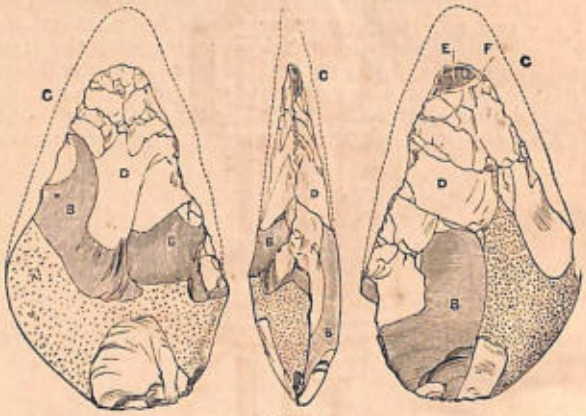


FIG. 12.

will not put the damaged examples and failures in their "cabinets;" but every damage tells some story of the use of the implement, and throws some light on the character of the being who made and used it.

Implements could not have been made without fabricating tools—without punches, hammer-stones, and anvils;—where the ordinary implements are, these latter things also are. Implements such as are seen in museums are only fit for moderately rough work; very rough work was sometimes done, but rough and massive stones artificially worked are seldom seen in collections.

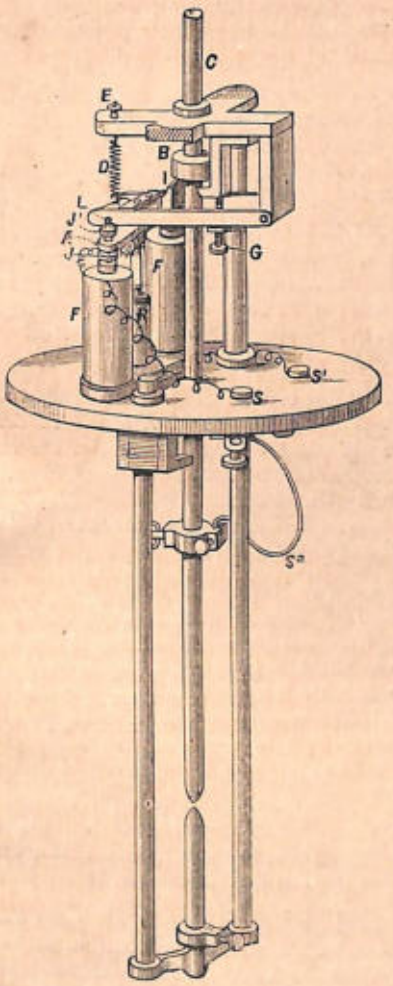
Knives, scrapers, wedges, heavy choppers, punches, anvils, cores, abraded hammer-stones, and other things have all been recovered by me from Stoke Newington, London; but as this paper has already exceeded the limits set apart for similar articles, the description and illustration of these less-known objects had better be deferred.

WORTHINGTON G. SMITH

LEVER'S ARC LAMP

SO many rival forms of lamps have recently been devised for regulating the electric arc light that even specialists in this branch of applied science have some difficulty in keeping up a knowledge of all the various systems. Amongst those, however, there is a tolerably well-defined class of lamps in which the movements of the carbon-holder are regulated by a clutch or kindred device, which grips the holder and raises it, lowers it, or releases it when required. Clutch lamps date back, indeed, to the year 1858, when a lamp of this type invented by Hart, the instrument maker, received a prize from the Royal Scottish Society of Arts. Amongst the more modern forms of clutch lamp those which have hitherto

found favour with the public are the well-known inventions of Brush and Weston. Though the clutch device is in itself simple and efficient, the difficulty which has beset the action of such lamps has been that of arranging suitable electric mechanism to work the clutch. In Hart's lamp an electro-magnet through the coils of which the main current passed on its way to the lamp, lifted the clutch, and again released it when the increasing resistance of the arc interfered with the strength of the current. In the lamps of Weston and of Brush a much more complicated arrangement was adopted, the magnets which worked the clutch being in both these patterns of lamp wound "differentially," that is to say, with a coil of fine wire connected as a shunt to the lamp, acting in opposition to another coil of thick wire through which the



main current flowed. This differential principle was originally applied in the Siemens' lamp, wherein, however, no clutch was used. In the Pilsen lamp, and in many others, combinations of shunt magnets and main-circuit magnets have been similarly applied. The lamp which we illustrate in the figure, the invention of Mr. Charles Lever, of Manchester, is a clutch lamp, but of remarkably simple, yet efficient construction. And as it possesses sundry points worthy of notice from a scientific aspect, we will briefly describe it. The upper carbon is clamped in a holder or carbon rod C, which consists of a tube of brass sliding smoothly through the upper framework of the lamp. Fitting accurately, but not tightly to it, is a brass washer, or collar, B, which is supported from below or on one side by an adjustible screw, G, and on the other by a metal piece, I, projecting from the jointed framework below. This framework is held up by a spiral spring, D, which, when the lamp is not in action, keeps the piece, I, pressed up under the washer, B, and tilts it. When thus tilted it clutches the carbon-holder, C, and raises it. Attached to the under-side of the jointed framework alluded to is an iron bar, A, bearing two broad-ended iron screws, J, below which, again, are seen the two limbs of an electro-magnet, F F, with the poles upward. This electro-magnet is wound with fine wire, and connected as a shunt to the lamp. Now, as described above, when the lamp is not in action, the carbons are held apart by a spring. When the current is turned on it must therefore pass through the shunt magnet, which immediately attracts the bar, A, lowers the piece, I, releases the clutch-washer, B. The upper carbon then falls, and the current is diverted from the shunt-magnet to the lamp itself, passing through the carbons. But when this takes place, the spring, D, being no longer opposed, draws up the framework, and picks up the clutch, thus raising the upper carbon through the space requisite for the production of the arc. A more simple or efficient mechanism would be difficult to devise; and its action is extremely regular and steady in practice.

NOTES

PROF. HUXLEY has been appointed to deliver the Rede Lecture (Cambridge) this year.

MR. G. H. DARWIN, M.A., F.R.S., has been elected to the Plumian Professorship of Astronomy and Experimental Philosophy at the University of Cambridge, vacant by the death of the Rev. James Challis. This was the first election to a professorship since the approval of the new University Statutes by Her Majesty in Council. By the new statutes the election to certain professorships is vested in a Board nominated by the Special and General Boards of Studies and by the Council of the Senate, the persons so nominated being elected by the Senate. The members of the Board appointed to elect to the Plumian Professorship are the Vice-Chancellor, Prof. H. J. S. Smith, of Oxford, Mr. W. H. M. Christie, the Astronomer-Royal, Mr. W. Spottiswoode, President of the Royal Society; Professors Adams, Stokes, Cayley; Dr. Ferrers, Master of Gonville and Caius; and Mr. Isaac Todhunter, of St. John's.

THE subscription for the Darwin Memorial has awakened so much enthusiasm in Sweden that the local committee there formed has received subscriptions from no less than 1400 persons, including "all sorts of people," writes Prof. Loven in a letter to the English Committee, "from the bishop to the seamstress," the sums varying from five pounds to twopence. The English Committee, which has its head-quarters at the Royal Society, London, has now received (inclusive of subscriptions from abroad), 4000*l.*, but the number of subscribers in the United Kingdom is only about 600. From this it would seem that an interest in science is not nearly so widely spread in Britain as it is in the more thinly peopled land of Sweden.

IN announcing the death of Mr. Darwin to the American Philosophical Society at its meeting on April 21, 1882, Dr. Le Conte stated the general bearing of Darwinism in a striking and unusual way:—"To no man more than to Darwin does the present age owe as much, for the gradual reception of the modern method of close observation over the scholastic or *a priori* formulæ, which, up to a brief period, affected all biological investigations. To him, above all men, we owe the recurrence to the old Aryan doctrine of evolution (though in those ancient times promulgated under the guise of inspiration) as preferable, by reasonable demonstration, to the Semitic views, which have prevailed to within a few years, and are still acceptable to a large number of well-minded but unthinking men. The doctrine

of evolution, in its elementary form, means nothing more than that everything that exists has been derived from something that pre-existed; that the former is related to the latter as effect is to cause. And it is most pleasing evidence of the acceptability of this doctrine, that it is now heard from many pulpits in the land, and is a strong illustration of the instructions which are thence given."

LETTERS have been received from Mr. Forbes dated from Shonga, on the Niger, at the end of October last. Shonga is a small trading-station a short distance up a creek on the right bank of the main stream some fifty miles below Rebba. Mr. Forbes had been there three weeks, and was expecting to remain about three more, when the steamer would call for him, and try to get up to Sokoto—an excursion that would occupy at least six weeks. After this Mr. Forbes would return direct to England. Having been pulled down by fever and the want of good food, Mr. Forbes had not been very successful in his collections at Shonga. His list of species of birds obtained at the date of his letter was only 105, and the difficulty in obtaining spirit had interfered with the preservation of fishes, of which many species were abundant.

IN a collection of birds and insects just received from Mr. Andrew Goldie by Messrs. Salvin and Godman are specimens of a fine new Bird of Paradise, obtained in the D'Entrecasteaux Islands, south-east of New Guinea. This species, which belongs to the restricted genus *Paradisea*, is shortly characterised by Messrs. Salvin and Godman in the last number of the *Ibis* as *Paradisea decora*, and will be fully described and figured in the next number of the same journal.

THE *Lancet* is happy to be assured that the rumours respecting the infirm state of health of Prof. Owen are unfounded. The large circle of the professor's friends will share with us in the hope that his valuable life will be prolonged many years beyond the seventy-nine which it has already reached.

A GROWING want has for some time been felt by lecturers on biological subjects, and especially by those whose lot it is to address large audiences or classes, of a good series of lantern slides, which would do for biology what has been so well done for physical science by York's series of slides. The ever increasing use of the oxyhydrogen lantern as a means of illustration, especially with popular audiences, renders this need more apparent. Arrangements have, however, now been made with Messrs. York and Son, 87, Lancaster Road, Notting Hill, London, W., to issue such a series, under the supervision of Dr. Andrew Wilson and of Mr. Wm. Lant Carpenter, to whom, at 36, Craven Park, Harlesden, London, N.W., or to Dr. Wilson, 110, Gilmore Place, Edinburgh, any communications on the subject may be addressed. It is intended that, in the first instance, the series shall comprise some of the principal types and life-histories of the lower forms of plant and animal life, and the elementary facts of animal and vegetable physiology. It is believed that the knowledge that these are in preparation, may save the construction of diagrams by some lecturers, and may lead others to make valuable suggestions as to sources of illustration, &c., to one of the above named gentlemen.

PROF. COOK, of Canterbury College, New Zealand, points out in the new number of the *N.Z. Journal of Science* that while the colony is remarkably well provided with museums, it is entirely without a public astronomical observatory. It is a fact that some years ago about 250*l.* were collected for such an observatory, but it came to nothing. We heartily endorse Prof. Cook's able advocacy for the foundation of an observatory in New Zealand, which, if perfectly equipped and directed could not fail to do good work. Out of a total of ninety-five observatories in the *Nautical Almanac* only eight are in the southern latitudes.

AT the Guildhall last week Dr. Siemens and Dr. Percy were each presented with the freedom and Livery of the Worshipful Company of Turners. The honour was conferred upon Dr. Siemens in recognition of his eminence as an engineer, his successful application of physical science to valuable practical purposes, especially electricity and metallurgy, and his personal support of technical education. The new member made a suitable reply in returning thanks for the honour conferred on him, an honour which was specially precious to him, and of which he should ever be proud. Referring to electricity, he said it was a new science, the applications of which had all to be developed, and in the development of which wonderful results had been produced. In the case of Dr. Percy, the honour was conferred in recognition of his distinguished scientific attainments, especially in connection with metallurgy, the great value of his researches, and his teaching not only to turners, but to all workers in metal.

THE German Fishery Society has petitioned the Reichstag to make a grant of 10,000 marks, chiefly to enable Germans to take part in the approaching London Fishery Exhibition. It is desired that an official delegate should represent this Empire in London in connection with the enterprise.

THE astronomical observatories of Greenwich, Kiel, Pulkova, Vienna, Milan, Paris, Utrecht, and Copenhagen have fixed on Kiel as the centre for astronomical telegrams. For an annual payment of five pounds each of the above-mentioned observatories will receive by telegraph information of every fresh astronomical discovery wherever made.

DR. SCHLIEMANN is desirous of commencing a new series of excavations in the North-West of Athens. In the neighbourhood of the old Academy was the site of the official burial-ground, and there were buried the ancient Athenians who had fallen in battle. Dr. Schliemann hopes in this spot to find the grave of Pericles. At a subsequent period it is his intention to begin fresh excavations in Crete.

IN an address on education at Birmingham on Monday, Mr. Mundella said: "They were asked if they were not over educating; he said no, and he would tell them why. Our idea of education was the lowest, certainly, on this side the Alps. Those who had the longest experience in education, those nations which had spent the most on it, were at this moment making the greatest efforts. The educational impulse throughout Europe was something they could hardly believe, and it was so because the people on the Continent had found that knowledge was power, not only military power, but industrial power. Whereas in Birmingham, last year, the expenditure on education was 2*s.* 3*d.* per head, in Paris it was 12*s.*"

THE following gentlemen have kindly promised to deliver popular lectures, with lantern illustrations, at the Royal Victoria Coffee Hall, Waterloo Road, on Friday evenings at 9 o'clock. January 19, Mr. Wm. Lant Carpenter, B.A., F.C.S., on "The Telephone and how to talk to a man 100 miles away." January 26, Mr. C. A. V. Conybeare on Pompeii. On February 2, instead of a lecture a magic lantern entertainment, entitled "Here, There, and Everywhere," will be given by Major George Verney. February 9, Mr. E. B. Knobel (Sec. R.A.S.), "The Sun and his Family, with a glance at other Suns."

ACCORDING to the *Journal* of the Russian Physico-chemical Society, the priority in photographing with the electric light belongs to the well-known St. Petersburg photographer, M. Lewitski, who obtained such photographs in the winter of 1856, on the following occasion:—To produce the electric light during the celebration of the coronation of the Czar Alexander II. at Moscow, a Bunsen battery of 800 elements had been constructed.

The following winter this battery was taken to St. Petersburg, and Prof. Lenz demonstrated its action to a distinguished auditory, formed of members of the Imperial family and generals of the army. It was during this lecture that M. Lewitski obtained a photograph of the professor. A positive of this portrait was presented by M. Lermantoff to the Russian Physical Society at the *séance* on December 14, 1880. It is by no means a poor photograph, but full of detail in the shadows and half tints.

In a recent report of the Berlin Physical Society (p. 95) we referred to some valuable observations by Dr. Kœnig with Prof. Helmholtz's new instrument, called the *leukoscope*. We observe that a detailed account (with illustration) of the instrument and of the results obtained with it, appears in *Wiedemann's Annalen*, Nos. 12 and 13 of last year.

Prof. F. W. Putnam has concluded a very successful course of lectures at the Peabody Museum, Boston, on some of the most interesting of American antiquities. The *Boston Evening Transcript* in an article on the lectures says:—"It is to be hoped that the curator will not again be retarded in his work from the want of means for its prosecution, when he has shown, as he has in this course of lectures, how much can be done at comparatively little expense under proper methods of research. As he said in his lecture, what is to be done must be done at once, and it would be a great pity to have the opportunities now open to him lost to science. The ancient city known to the present inhabitants of the Little Miami Valley, thirty-five miles east of Cincinnati, as 'Fort Ancient,' would be worth to American scholars for study as much as any of the old Greek cities that have been so thoroughly dug over by European explorers and students. Certainly American scholars should lead in American archæology and ethnology. The restoration or preservation of these wonderful remains of a comparatively enlightened prehistoric American people would be a glorious monument for any American Institution of learning and science."

Shocks of earthquake have been felt in the province of Murcia, in Spain. Seven shocks occurred at Archena on the 11th inst. Shocks have also been felt at Fortuna, Muta, Ricotel and other towns in Murcia. Eleven distinct shocks were felt on Tuesday morning at Archena, between the hours of three and six. Some lasted fifteen, and others lasted two seconds. An earthquake of a few seconds duration was experienced at Kultorp, near Kalmar, in Sweden, at 8.50 p.m. on the 12th inst. A slight shock of earthquake was felt at Monmouth at five o'clock on Tuesday evening, accompanied by a light, rushing noise. The wave seemed to pass from south-east to north-west.

A remarkable discovery of the elder *Runic* inscriptions has just been made in Ryfylke in Norway. The characters have been made on a stone, the arrival of which in Christiania is awaited with great interest by *savants*.

The French Minister of Postal Telegraphy in France has established at the central office a special course of lectures on Wheatstone's automatic apparatus, to which sixteen competent operators, from different parts of the country, have been admitted. The course of lectures and experiments has lasted two months. The pupils are now passing an examination, and a special certificate will be issued to the successful candidates, which will greatly help them in their future promotion in the postal telegraphic service.

The Parc Montceau, placed in one of the most fashionable parts of Paris is now lighted by Jablochkoff candles with success.

Admiral Mouchez has issued his invitation for the Soirées de l'Observatoire, at which as usual will be exhibited all the scientific novelties of the year.

M. Chevreul has been unanimously nominated once more President of the French Société Nationale d'Agriculture.

It is expected that the French Government will take in hand the celebration of the centenary of the discovery of balloons. The two committees which had been formed by several aëronautical societies have been amalgamated, and M. Gaston Tissandier has been appointed president. The scheme of an international exhibition for balloons and instruments used in aërial investigations has been adopted by M. Herrisson, the Minister of Public Works, and will be carried into effect by M. Armengaud Jeane, the well-known civil engineer.

In his speech on laying down his office, previous to being admitted Vice-Chancellor for the year 1883, Dr. Porter, Master of Peterhouse, Cambridge, referred to the endowments of the new Professorships of Physiology and Pathology, increased grants to the museums and lecture-rooms, and a chemical laboratory on an adequate scale, as among the more urgent claims on the new funds available to the University.

Prof. Frisby writes from the U.S. Naval Observatory, Washington, that in the circular he lately sent (Nature, vol. xxvii. p. 226), giving elliptic orbit of great comet, $\phi = 89° 7' 42''{\cdot}70$ should be $\phi = 89° 13' 42''{\cdot}70$.

The additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus radiatus* ♂) from India, presented by Mr. C. James; a Common Otter (*Lutra vulgaris*), British, presented by Mr. E. P. Squarey; a Black-necked Hare (*Lepus nigricollis* ♂) from Ceylon, presented by Mr. W. Bowden Smith; an Indian Antelope (*Antilope cervicapra*) from India, presented by Capt. R. Brooke Hunt; a Bohor Antelope (*Cervicapra bohor* ♀) from India, presented by Mr. W. J. Evelyn; a Black-backed Jackal (*Canis mesomelas*) from South Africa, presented by Mr. J. S. Crow; a Larger Hill Mynah (*Gracula intermedia*) from India, presented by Mrs. M. R. Manuel; three Passenger Pigeons (*Ectopistes migratorius*) from North America, presented by Mr. F. J. Thompson; a —— Horned Lizard (*Phrynosoma* ——) from California, presented by Mr. Martin R. de Selincourt; a Common Adder (*Vipera berus*), British, presented by Mr. J. Harris; an Indian Black Cuckoo (*Eudynamys orientalis*) from India, purchased; an Axis Deer (*Cervus axis* ♂), born in the Gardens.

APPROXIMATIVE PHOTOMETRIC MEASUREMENTS OF SUN, MOON, CLOUDY SKY, AND ELECTRIC AND OTHER ARTIFICIAL LIGHTS[1]

Sir William Thomson pointed out that the light and heat perceived in the radiations from hot bodies were but the different modes in which the energy of vibration induced by the heat was conveyed to our consciousness. A hot kettle; red-hot iron; incandescent iron, platinum, or carbon, the incandescence in the electric arc, all radiate energy in the same manner, and according as it is perceived through the sense of sight, by its organ the eye, or by the sense of heat,[2] we speak of it as light or heat. When the period of vibration is longer than one four-hundred-million-millionth of a second, the radiation can only be perceived by the sense of heat; when the period of vibration is

[1] Abstract of lecture at the Glasgow Philosophical Society, by Sir William Thomson, F.R.S.

[2] Sometimes wrongly called the sense of touch. The true list of the senses, first given, I believe, by Dr. Thos. Reid, makes two of what used to be called the sense of touch, so that, instead of the still too common wrong-reckoning of five senses, we have six, as follows:—

Sense of Force.
" Heat.
" Sound.
" Light.
" Taste.
" Smell.

shorter than one four-hundred-million-millionth of a second, and longer than one eight-hundred-million-millionth of a second, the radiation is perceived as light, by the eye.

Pouillet, from a series of experiments, deduced a value of the energy radiated by the sun, equal in British units to about 86 foot-pounds per second per square foot at the earth's surface, or about 1 horse-power to every 6½ square feet of the earth's surface. We may estimate from this the value of the solar radiation at the surface of the sun. The sun is merely an incandescent molten mass losing heat by radiation, and surrounded by an atmosphere of incandescent vapour, so that the radiant energy really comes out from any square foot or square mile of the sun's surface, as from a pit of luminous fluid which we cannot distinguish as either gaseous or liquid. Take, however, instead of the sun, an ideal radiating surface of a solid globe of 440,000 miles radius. The distance of the earth being taken as 93 million miles, the radius of the sun is equal to, say in round numbers, one two-hundredth of the earth's distance, hence the area at the earth's distance corresponding to one square foot of the sun's surface, is equal to 40,000 square feet. The radiation on this surface is (40,000 × 86, or) 3,440,000 foot-pounds, which is therefore the amount of radiation from each square foot of the sun's surface. This amounts to about 7000 horse-power, which, according to our brain-wasting British measure, we must divide by 144, if we wish to know the radiation per square inch of the sun's surface, which we thus find to be 50 horse-power.

The normal current through a Swan lamp giving a 20-candle light is equal to 1.4 amperes with a potential of 40 to 45 volts. Hence the activity of the electric working in the filament is 61·6 ampere-volts or Watts (according to Dr. Siemens' happy designation of the name of Watt, to represent the unit of activity constituted by the ampere-volt). To reduce this to horse-power we must divide by 746, and we thus find about 1-12th of a horse-power for the electric activity in a Swan lamp. The filament is 3½ inches long, and ·01 of an inch in diameter of circular section; the area of the surface is thus 1-9th of a square inch, and therefore the activity is at the rate of 3-4ths of a horse-power per square inch. Hence the activity of the sun's radiation is about sixty-seven times greater than that of a Swan lamp per equal area, when incandesced to 240 candles per horse-power.

In this country the standard light to which photometric measurements are referred is that obtained from what is known as a standard candle. Latterly, however, objections have been raised against its accuracy. It has been said that differences of as much as 14 per cent. have been found in the intensity of the light given by different standard candles, and that serious differences have been observed in the intensity of the light from different parts of the same candle in the course of its burning. The Carcel lamp, the standard in use in France, has been regarded as the only reliable standard. It is, no doubt, very reliable and accurate in its indications, but it should be remembered that its accuracy is greatly owing to the careful method and the laborious precautions taken to secure accuracy. If something akin to the precautions applied to the Carcel lamp by Regnault and Dumas were applied to the production and use of the standard candle, there is little doubt but that sufficient accuracy for most practical purposes could also be obtained with it; probably as good results as are already obtained by the use of the Carcel lamp.

At the Conference on Electrical Units which met in Paris lately, a suggestion was made to use as a standard for photometric measurements the incandescence of melting platinum, and very interesting results and methods in connection with the proposal were presented to the meeting. According to experiments by Mr. Violle, which M. Dumas reported to the Conference, a square centimetre of liquid platinum at the melting temperature gives of yellow light seven, and of violet twelve times the quantities of the same colours given by a Carcel lamp. The apparent area of the Swan filament, being one-ninth of a square inch, is ·23 of a square centimetre, and when incandesced to 20 candles must be about as bright as the melted platinum of Mr. Violle's experiment, as the 7 carcels of yellow and 12 of violet must correspond to something like 10 carcels or 85 candles, in the ordinary estimation of illumination by our eyes. The tint of Mr. Violle's glowing platinum cannot be very different from that of the ordinary Swan lamp incandesced to its "20 candles." Thus both, as to tint, and brightness, it appears that melted platinum at its freezing temperature is nearly the same as a carbon filament in vacuum incandesced to 240 candles per horse-power.

For approximative photometric measurements the most convenient method is certainly that of Rumford, by a comparison of the shadows cast by the sources of light on a white surface. The apparatus necessary are only a piece of white paper, a small cylindrical body such as a pencil, and a means of measuring distances. Ordinary healthy eyes are usually quite consistent in estimating the strength of shadows, even when the shadows examined are of different colours, and with a reasonable amount of care photometric measurements by this method may be obtained within 2 or 3 per cent. of accuracy. The difference in the colours of the shadows is of course due to each shadow being illuminated by the other light.

Arago has compared the luminous intensity of the sun with that of a candle, and estimates it as equal to about 15,000 times that of a candle-flame.

Seidel, as Sir W. Thomson had been informed by Helmholtz, estimated the luminous intensity of the moon as about equal to that of grayish basalt or sandstone. An experiment on sunlight made in Glasgow on the 8th of this month (since this paper was read), compared with an observation on moonlight, which he made at York during the meeting of the British Association there in 1881, had led him to conclude that the surface of the moon radiates something not enormously different from one-quarter of the light incident upon it. It would be exactly this if the transparency of the Glasgow noon atmosphere of December 8, 1882, had been exactly equal to that of the York midnight atmosphere of September, 1881, referred to below, for the respective altitudes of the sun and moon on the two occasions. The observation on moonlight referred to above showed the moonlight at the time and place of the observation (at York early in September, 1881, about midnight, near the time of full moon) to be equal to that of a candle at a distance of 230 centimetres. The moon's distance ($3{\cdot}8 \times 10^{10}$ cm.) is $1{\cdot}65 \times 10^{8}$ times the distance of the candle. Hence, ignoring for a moment the loss of moonlight in transmission through the earth's atmosphere, we find $(1{\cdot}65 \times 10^{8})^2$, or 27 thousand million million as the number of candles that must be spread over the moon's earthward hemisphere painted black, to send us as much light as we receive from her. Probably about one and a half times as many candles, or say forty thousand million million would be required, because the absorption by the earth's atmosphere may have stopped about one-third of the light from reaching the place where the observation was made. The moon's diameter is $3{\cdot}5 \times 10^{8}$ centimetres, and therefore half the area of her surface is 19×10^{16} square centimetres, which is nearly five times forty thousand million million. Thus it appears that if the hemisphere of the moon facing the earth were painted black and covered with candles standing packed in square order touching one another (being say one candle to every five square centimetres of surface), all burning normally, the light received at the earth would be about the same in quantity as estimated by our eyes, as it really is. It would have very much the same tint and general appearance as an ordinary theatrical moon, except that it would be brightest at the rim and continuously less bright from the rim to the centre of the circle where the brightness would be least.

The luminous intensity of a cloudy sky he found about 10 a.m. one day in York during the meeting of the British Association to be such that light from it through an aperture of one square inch area was equal to about one candle. The colour of its shadow compared with that from a candle was as deep buff yellow to azure blue, the former shadow being illuminated by the candle alone, the latter by the light coming through the inch hole in the window shutter.

The experiment on sunlight of last Friday (December 8) showed, at 1 o'clock on that day, the sunlight reaching his house in the University to be of such brilliancy that the amount of it coming through a pinhole in a piece of paper of ·09 of a centimetre diameter produced an illumination equal to that of 126 candles. This is 6·3 times the 20-candle Swan light, of which the apparent area of incandescent surface is ·23 of a square centimetre, or 3·8 times the area of the pin-hole. Hence the sun's surface as seen through the atmosphere at the time and place of observation was 24 times as bright as the Swan carbon when incandesced to 240 candles per horse-power. By cutting a piece of paper of such shape and size as just to eclipse the flame of the candle and measuring the area of the piece of paper, he found about 2·7 sq. centims. as the corresponding area of the flame. This is 420 times the area of the pin-hole, and therefore the intensity of the light from the sun's disc was equal to

(126 × 420) about 53,000 times that of a candle-flame. This is more than three times the value found by Arago for the intensity of the light from the sun's disc as compared with that from a candle-flame; so much for a Glasgow December sun!

The ·09 cm. diameter of the pin-hole, of the Glasgow observation, subtends, at 230 centimetres distance, an angle of 1/2556 of a radian; which is 23·7 times the sun's diameter (1/108 of a radian). But at 230 cm. distance the sunlight through the pin-hole amounted to 126 times the York moonlight (which was 1 candle at 230 cm. distance). Hence the Glasgow sunlight was [(23·7)² × 126 times or] 71,000 times the York moonlight. We cannot therefore be *very far* wrong in estimating the light of full moon as about one-seventy-thousandth of the sunlight, anywhere on the earth. This, however, is a comparison which, because of the probably close agreement of the tints of the two lights, can probably be made with minute accuracy: and we must therefore not be satisfied with so very rough an approximation to the ratio as this 70,000. A lime light, or magnesium light, or electric arc-light, carefully made and remade with very exactly equal brilliance, for each separate observation of sunlight and moonlight, might be used for intermediary.

THE HYPOTHESIS OF ACCELERATED DEVELOPMENT BY PRIMOGENITURE, AND ITS PLACE IN THE THEORY OF EVOLUTION[1]

IN our days the student of the biological sciences may look forward towards his life-task with sincere gratitude. Gratitude not only for what has already been achieved, and for the ends that have been attained in this domain, but more especially for all that which the future promises, since the sage whose mortal remains were lately deposited in Westminster Abbey has thrown the light of his genius over regions which hitherto were shrouded in deepest obscurity and has opened new vistas on old problems, of which man has been seeking the solution for many thousands of years.

It is to him we have to give thanks that the dawn of a new life has commenced for those sciences; to him, moreover, we owe it that the twilight has only lasted a short time, and that the full light of day has shone so soon upon an extensive field. And if by this light we perceive numerous new problems, the existence of which was not even dreamt of before, and which cover the field of our work as far as the horizon reaches, still we notice that their shapes have obtained definite outlines. In future they may serve as milestones on our way onwards, before, when we were still groping in the dark, they were as many stumbling-blocks which prevented us from advancing.

If to-day I call before your mind the image of this great reformer, it is not to give you an eulogy of Darwin, whose sudden death some months ago has filled with grief the whole civilised world. He is before my mind, because I belong to the generation whose youth coincides with that of the "Origin of Species;" a generation deeply filled with gratitude towards this great master. A gratitude bursting forth with doubled intensity in him who enters upon a career in which he will have ample opportunity to continue work in that field of science to which he has become more and more attached through the inspiring influence of Darwin.

It is not only by the contents of his work that Darwin takes hold of us, it is also his personal character which leaves such a forcible impression. The history of his life, his method of work, his amiable individuality, have excited our enthusiasm over and again, and always in an increasing measure. Similar to other grand figures in the history of the world, who by their life and their example have perhaps wrought more than by their teaching—which at the hands of less eminent adepts soon took a dogmatic, *i.e.* a degenerate shape—this reformer of biological science has left behind him a remembrance which will be kept and transmitted by his followers with quite as much care and piety as the writings he has left.

What strikes us most and all at first in everything emanating from him is his passionate honesty,[2] which has already become proverbial. Never did he pass over in silence, in the interest of his argument, a point which might eventually appear to be in favour of the opposite plea. In the enumeration and refutation of such points he was always quite as careful as in the collection of positive proofs. He was never biassed, unless biassed in the good sense of the term, *i.e.* enabled, when once he was of opinion that it was necessary to choose a decided side with respect to any dubious point, to devote to the careful consideration of this point not only hours, but if necessary months and years of his life,—months and years of daily returning observations concerning what appeared to be unimportant facts, which, however, when they were afterwards brought together, permitted him to draw highly important conclusions.

Unlimited veracity and undaunted patience, two principal requirements of the true naturalist, thus found their most perfect incarnation in Darwin, and with these two for his guides, he brought together, from far and near, building stones for the completion of the grand structure which his mind had conceived. The quarries from whence he excavated those building stones were very different from those to which the scribes in biological science habitually resorted. It must be understood that since the appearance of Cuvier's "Le Règne Animal distribué d'après son Organisation," a reaction had sprung up against descriptive zoology which in many cases went further than Cuvier himself would ever have acknowledged. The numerous volumes of his excellent "Histoire naturelle des Poissons" furnish ample proof that Cuvier had always endeavoured to combine careful description of the species and conscientious sifting of all the material concerning its life history, its geographical distribution, and its synonymy with the study of the comparative anatomy of the group to which it belonged. Several of his followers have, however, concluded that since researches upon the internal organisation of so many classes of animals allowed him to make most important deductions, it was from similar researches only that anything could be expected for the future. Their ambitious aspirations could not manage to forget that a combined investigation by Cuvier and Geoffroy St. Hilaire was once described by one of the two in the following words:—"Nous ne déjeunions jamais sans avoir fait une découverte."

And so a period was opened up in which our knowledge of the internal organisation of animals was not only increased on all sides and firmly based upon facts by zealous workers, but in which this knowledge was gradually pushed into the foreground as the pre-eminent, as the only true zoology. The careful study of the species and its life history was left with a smile and a shrug of the shoulders to dilettanti and museum zoologists. In order further, to indicate how the results of researches of these men were looked upon as popular and unimportant, this new school invented the well-sounding name of "scientific zoology."

The eminent researches of von Siebold on parthenogenesis and on the freshwater fishes of Germany; Kölliker's important monograph of the Pennatulids, &c., show that even its founders were subject to impulses which drove them back into this very field, or rather that it was not they, but their less gifted followers from whom the contemptuous meaning which that combination of words gradually attained has emanated.

Thus for a certain lapse of time the wind blew from a different quarter, and attempts have repeatedly been made to call into life classifications which were based upon certain points in the internal organisation, points which were considered to be of the more importance the less they were visible. Fortunately the great masters to whom we owe comparative anatomy, and who have made it such as we know it in the present day, have not joined in this movement. Johannes Müller's "System der Plagiostomen" stands side by side with his "Comparative Anatomy of the Myxinoids," showing that this one-sided exaggeration would never have been encouraged by himself. Gegenbaur, Huxley, &c., have similarly kept aloof from the "scientific zoologists" in the stricter sense, whose narrow-minded doctrines are still pullulating, be it in a somewhat modified form. At the present day it is not so much the internal organisation which forms the shibboleth by which entrance is obtained to the holy circle of self-styled orthodox zoologists, but now it is the history of development, embryology, that gives the pass-word. This important branch of biological science has made gigantic strides of late; it counted in its foremost ranks, among the most promising and large-minded, the man whom a cruel fate had doomed to find his death in the Alps of Switzerland, the talented Balfour. He never overvalued in a petty way the labours of the select batallion of which he was one of the leaders. In the rear of this army, however, voices are heard claiming infallibility for embryology, and the splendid generalisation: "the development of the individual is a repetition on a reduced scale of the development of the race," must often serve to hide unripe

[1] By Prof. A. A. W. Hubrecht. Inaugural Address delivered in the University of Utrecht, September, 1882.

[2] Cf. Huxley, NATURE, May, 1882.

attempts at classifications deduced from the developmental stages of eggs and larvæ of questionable origin, and applied to groups of animals of which the adventurous embryologist would certainly not be able to distinguish the different members specifically.

But enough of this distressing partiality, knowing that we find a complete reaction against it, in Darwin's word and example, which will be our strongest antidote against similar influences. We are thus carried back to our starting-point, where it was observed that the value of the sources from whence Darwin has drawn so much valuable information, was scarcely recognised up to his time. He entered into connection with cattle-rearers and bird-fanciers, and gladly availed himself of the remarks of trustworthy observers who were acquainted with animals and plants in their daily life, even if they had always remained outside the pale of science.

And what far-reaching results may be obtained by careful study of the habits and life-history of animals is shown by the last volume which we owe to Darwin's hand. Here it is apparent, upon almost every page, that from conscientious observations on the habits of an animal so common as the earthworm, conclusions follow which furnish us with new and quite unexpected views about the formation and the changes of a large area of the earth's surface.

The most striking example of Darwin's all-embracing genius is obtained when his Monograph of the Cirripedia is compared with the chapters in which he enunciates and discusses his hypothesis of pangenesis. The one, the most scrupulous study of details, the comparison of slight differences both between individuals of the same species and between specifically distinct specimens; the evaluation of these distinctive characters one against the other; in one word, pure systematic zoology with all its appurtenance of patience, scrupulousness, and nearly painful conscientiousness. The other—one of the most daring hypotheses which the human understanding has ever wrought, upon which only a very limited number of observed facts can be brought to bear. A hypothesis which boldly penetrates into the most hidden secrets of organic nature; which brings the marvellous effects of heredity on a level with the reproduction of lost parts, yea even with the healing of wounds. A hypothesis which no longer looks upon the cells as the morphological units of the living organism, but which postulates the existence of a continual flow of separate minute gemmulæ, feeding and reproducing themselves, and being derived from all the cells and all the tissues in all the consecutive periods of their existence. These gemmulæ, in the individual being we have before us, circulate along paths which remain wholly unknown to us, and finally reunite in millions in every ovum, in every spermatozoon, in every bud, and in every pollen-grain.

The laws by which these inscrutinable processes are governed, do not lose anything of their mysteriousness when we glance at the disparate and incomprehensible phenomena which they have to explain: atavism, in which heredity takes a sudden leap backwards into the grey mists of the past; the transmission to the child of the effects of an increased or decreased use of a limb by the parents; the reproduction of a lost limb or tail; the growth of an entire plant out of a severed portion of a leaf; the change which pollen and sperma may occasionally call forth not only in the ovules but also in the tissues of the mother-form; the hybridisation in the vegetable kingdom by the union of the cellular tissue of two plants independently of the organs of generation; the appearance of a complex metamorphosis in the course of the development of certain animal forms, the nearest allies of which are entirely devoid of anything like it; &c.

Nevertheless this hypothesis was put forward by the very same Darwin whom we have to thank for the monograph of the Cirripedia. It is clear that the frame of mind required for completing the one is widely different from that in which he enunciated the other. There is, however, a common link uniting the two. In the specific description of the Cirripeds we find him ever and again in collision with the opinion then generally accepted of the definite boundaries limiting the species, and thus this work cannot have remained without influence on the later development of his ideas. On the other hand, he looked upon the hypothesis of pangenesis as a necessary sequel, to a certain extent as "le couronnement de l'édifice" of his theory of evolution by means of natural selection.

We have not here to enter into a discussion concerning the hypothesis of pangenesis, nor to inquire into the different attacks to which it has already been exposed. I must, however, observe that with it Darwin has entered the domain of physiology, a field upon which all the questions into which the great problem of evolution may be subdivided, as heredity, influence of use and disuse of organs, adaptation to modified circumstances, must find their solution.

Whereas the physiology of man and the higher animals is developing and growing with rapidity, and what has been thought and wrought in Utrecht has largely influenced this development, Comparative Physiology, which has to track all the different problems just mentioned all through the animal kingdom down to their simplest form in the lowest organised beings, is only in its infancy. And yet this branch of science will shortly come abreast of morphology further to secure the basis of the theory of evolution and to contribute to its harmonious development. It was not by mere chance that the legislature specially mentions Comparative Physiology as a branch of science which will have to be cultivated and taught by him who is called to the chair I am about to occupy.

Although the greater part of this territory is still wrapt in obscurity, still it is at the University of Utrecht that the prospects for Comparative Physiology are promising in the highest degree, be it by the efforts of others than the legislature had in view. It must for certain be acknowledged that researches concerning the phenomena of life in the very smallest organisms, investigating their reaction towards light and oxygen, and even penetrating into the effects of hunger and thirst as manifested by these lowly-organised beings, eminently belong to the domain of Comparative Physiology. The vicinity of a laboratory in which such excellent results have already been obtained is a strong stimulus for us all towards further labour in this field.

Venturing to-day along that road, I may hope to claim your attention, because in so doing, I wish to make an attempt to weaken one of the chief arguments against the theory of evolution, an argument which was termed by Huxley "the stock objection."[1]

I wish to speak to you about the hypothesis of accelerated development by primogeniture and its place in the theory of evolution.

I must begin with calling to mind that provisionally it is not upon the firm basis of proved facts, but more upon the quicksands of theoretical conjecture that we shall be moving. Our track first leads us into the domain of a science which is of such an exceptional value for the theory of evolution, because this science only, the science of palæontology, can furnish us with direct evidence towards the truth of that theory.

If, indeed, living organisms form one continuous chain with those that have already become extinct; if these organisms have not been called into life in successive periods by repeated creative acts but if they are in direct blood-relationship to each other—a relation which as we penetrate further into the past must be accompanied by a simplification of organisation—then palæontology must furnish us with the evidence of this process. Then, indeed, the superposed strata which have been deposited since the cooling of the earth's crust under the combined influence of internal vulcanism and external atmospherical influences, must contain the archives in which the most trustworthy and direct proof for the validity of the theory of evolution are to be found. Moreover, the material which we find heaped in these archives must show—if we place confidence in it—that gradual increase of complication which accompanies the development of the more highly differentiated forms out of simpler types by the aid of natural selection, in a succession exactly corresponding to that of the deposition of the strata. We know how far palæontology had advanced in 1859, we understand how it was that Darwin insisted on the imperfection of the geological record in the first edition of his "Origin of Species." He diligently collected arguments to explain this incompleteness and to oppose the objection against his doctrines which it might furnish. I cannot at present enter into details concerning this refutation. Still it is quite as valid to-day. So many deposits are wholly devoid of animal remains, it is so obvious that of other animal forms, fossils can hardly ever have been formed, and lastly, only such a small portion of the earth's surface has been adequately searched, that we have indeed more reason to be astonished at the quantity of facts that have already come to our knowledge, than at the much larger quantity which yet remains hidden from our view.

This is especially present to our minds when we remember

[1] T. H. Huxley, American Addresses.

the invaluable deposits that have of late years been opened up in North America, where not only the successive periods of the tertiary epoch form extensive deposits, but where they moreover contain perfectly preserved animal specimens which have lived in these successive periods, and which indeed show in the most irrefutable way that a direct connection accompanied by an increase of differentiation undeniably exists. We here find a very remarkable page of the book thrown open upon which nature has written down for us the history of the development of the horse, and whoever has learnt to read this handwriting is brought to the inevitable conclusion: this development has started from an older form of a less specialised organisation, and has proceeded along successive steps which are entirely in accordance with the theory of evolution.

Similarly the numerous remains of the fossil group of the Ornithoscelidæ are only known since a recent date, and a gradually increasing knowledge is thus attained of those interesting animals which link together reptiles and birds, two classes of animals which were formerly looked upon as amongst the most thoroughly separated.

Together with these irrefutable proofs that evolution has indeed taken place, starting from the simpler, more generalised types, and tending towards the more complicated and more specialised forms, palæontology acquaints us with certain other facts. I allude to the persistence of the same form, of the same genus, sometimes even of the same species in all successive strata and periods. Thus, for example, among Mollusks, Chiton and Pleurotomaria have persisted from the Silurian down to the present period; Dentalium from the Devonian; Pinna and Cyprina from the Carboniferous period. Amongst the Foraminifera certain genera occur in the Carboniferous epoch, which at the same time are members of the living fauna. Amongst Brachiopods our living Lingulas, Rhynchonellas, and Terebratulas are very ancient types; representatives of the osseous fishes lived in the Cretaceous period, which cannot be generically distinguished from their living relatives, whilst certain genera of cartilaginous fishes reach even into a much farther distant past.

(*To be continued.*)

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—Owing to the early occurrence of Easter, the term has begun a week earlier than usual this year. The first University business of importance will be the constitution of the New Boards of Faculties. These will consist of the professors as *ex-officio* members, and of members elected by the College lecturers in the various faculties, the number to be so elected being first decided by vote. The first step towards the new state of things has been the appointment of Mr. Lockhart, of Hertford College, as General Secretary to the Board of Faculties.

In the department of Physics at the University Museum, Prof. Clifton continues his course on the Electricity Developed by the Contact of different Substances; Mr. Stocker lectures on Mechanics, and Mr. Heaton on Problems on Mechanics and Physics. Prof. Price continues his course on Optics, and also gives a course on Hydro-mechanics.

Prof. Pritchard is absent in Egypt completing his measurements of the magnitude of the stars. The observatory will be open on Tuesday and Thursday evenings under the charge of Mr. Plummer.

In the Chemical Department of the Museum, Prof. Odling will give a course on Elementary Facts and Doctrines. Mr. Fisher will lecture on Inorganic Chemistry, and Dr. Watts on Organic Chemistry. The laboratory will lose the services of Mr. F. D. Brown in the middle of the term, as he has been elected to the Professorship of Chemistry and Physics in the New University at Auckland, and leaves for New Zealand in March.

In the Biological Department Prof. Moseley continues his course on Comparative Anatomy (followed by practical work). Mr. Hatchett Jackson lectures on the Fundamental Principles of Embryology, Mr. Poulton on the Geographical Distribution of Animals, Mr. Lewis Morgan on the Teeth of Vertebrata and on Human Osteology, and Mr. Hickson on Histology. Mr. Barclay Thompson has been obliged to give up lecturing on account of ill health.

Prof. Prestwich gives a course of lectures on Stratigraphical Geology.

The following courses will be given in the private College laboratories:—At Christchurch Mr. Vernon Harcourt lectures and gives practical instruction in Quantitative Analysis, and Mr. Baynes on Thermo-dynamics. At Balliol Mr. Dixon lectures on Organic Chemistry, and Elementary Electricity. At Magdalen Mr. Yule gives a course of demonstrations on the Chemical and Physical Properties of the Blood, Circulation, Respiration, &c.; and Mr. Chapman gives a practical course on Elementary Vegetable Morphology.

A scholarship in Natural Science will be offered at Keble College of the value of 80*l.* per annum. The examination will be in Biology and Chemistry; a scholarship will also be offered for competition at Queen's College in Physics, Chemistry, or Biology.

CAMBRIDGE.—Mr. J. E. Marr, M.A., Fellow of St. Johns College, is the Sedgwick Prizeman this year.

Science lectures commence on the following days: Prof. Liveing, General Principles of Chemistry, January 23; Prof. Dewar, Organic Chemistry, January 23; Prof. Newton, Geographical Distribution of Vertebrate Animals, January 31; Mr. Caldwell, Morphology of Invertebrata, Advanced, February 1; Dr. Hans Gadow, Morphology of Vertebrata, Advanced, January 30.

The names of Messrs. Casey (Trin.), Harvey (King's), A. R. Johnson (St. John's), Turner (Trin.), and Welsh (Jesus) appear in alphabetical order in the First Division of the List for the Third Part of the Mathematical Tripos, to which only the Wranglers were admitted. One name is in the second division, and eight in the third.

MR. F. J. M. PAGE, B.Sc., F.C.S., of University College, London, was elected, on January 11, Lecturer on Physics at the London Hospital Medical College.

SCIENTIFIC SERIALS

Journal of the Franklin Institute, December, 1882.—An improved dynamometer, by W. P. Tatham.—The isochronal Worthington pumping engine, by J. K. Maxwell.—Explosive and dangerous dusts, by T. W. Tobin.—Economical steam power (continued), by W. B. Le Van.—The universality of vibrations, by C. C. Haskins.—Report on European sewerage systems, &c. (continued), by R. Hering.

Annalen der Physik und Chemie, No 13 (December 2, 1882).—Absolute measurements with bifilar suspension, and especially two methods for determining the horizontal intensity of terrestrial magnetism without time measurement, by F. Kohlrausch.—The reduction of the Siemens unit to absolute measure, by E. Dorn.—On electric vibrations with special regard to their phases, by A. Oberbeck.—Experimental researches on galvanic polarisation, by F. Streintz.—On M. A. Guebhard's representation of equipotential curves, by E. Mach.—The electromotive force of the Daniell element, by E. Kittler.—On amalgamation-currents, by H. Haga.—Explanation of electric shadows in free air, by P. Riess.—On the material parts in electric sparks, by F. Wächter.—On the magnetic screening action of iron, by J. Stefan.—On tone-vibrations of solid bodies in presence of liquids, by F. Auerbach.—A small alteration of the pyknometer, by E. Wiedemann.—Remark on Herr Galn's memoir on the density of the luminiferous ether, by the same.—On the true cohesion of liquids, by the same.—On the condensation of liquids on solid bodies, by the same.—The leucoscope and some observations made with it, by A. König.—Contribution to the theory of diffraction in telescope-tubes, by H. Struve.—On the elliptical polarisation of reflected diffracted light, by W. Koenig.—On the Poggendorff fall-machine, by K. L. Bauer.—Contributions to the history of natural sciences among the Arabs, viii. and ix., by E. Wiedemann.

Bulletin de l'Academie Royale des Sciences de Belgique, Nos. 9 and 10.—Notice on a peculiarity in the aurora borealis of October 2, 1882, and on the increase in intensity of scintillation of stars during auroræ, by C. Montigny.—Some theorems of elementary geometry, by E. Catalan.—On curves of the third order, by C. Le Paige.—Aspect of the great comet of 1882 (Cruls) observed at Louvain, by F. Terby.—Note on the aurora borealis of October 2, 1882, by the same.—Action of chlorine on tertiary butylic chloride, by Baron d'Otreppe de Buvette.

No. 11.—Note on some bones of the Biscay whale at the

Museum of La Rochelle, by P. J. Van Beneden.—On some uniform geometric transformations, by C. Le Paige.—Second notice on the comet, by F. Terby.—On the functions of M. Prym and M. Hermite, by A. Genocchi.—On glycogen in Mucorineæ, by L. Errera.

Journal de Physique, December, 1882.—Remarks on timbre, by M. Koenig.—Remarks on the critical state, by M. Stolatow.—Experimental study of the reflection of actinic rays; influence of specular polish, by M. de Chardonnet.—Note on the theory of the Laurent saccharimeter with white light, by M. Dufet.—Notes of science in *Il Nuovo Cimento* and the *Journal* of the Russian Physico-Chemical Society.

Rivista Scientifico-Industriale e Giornale del Naturalista, October 31, and November 15 and 30, 1882.—Inconveniences of the usual pluviometer, and a few words about the pluvio-pulverometer, an apparatus for rain, dew, and atmospheric dust, by P. Lancetta.—Non-sensitive mercury thermometer; demonstration of the principle of the telephone, by G. Govi.—A doe with hairy horns.—Review of a prize memoir by G. Poloni, on the permanent magnetism of steel at different temperatures, by A. W.—Double-action mercury air-pump, by G. Serraville.—Fundamental principle of electrostatics, by S. Mugna.—Male genital armatures of saltatory Orthoptera, by A. Tozzetti.—Brief notice of the fluoriferous volcanoes of Campania, by A. Scacchi.

Reale Istituto Lombardo di Scienze e Lettere. Rendiconti. Vol. xxv., fasc. xviii.—On a recent landslip near Belluno, by T. Taramelli.—On drunkenness in Milan, II., by A. Verga.—Jacobi's theorem regarding periodicity, and the illegitimacy of a part of the consequnces that have been deduced from it, by F. Casorati.

Nouveaux Mémoires de la Société Helvetique des Sciences Naturelles, vol. xxviii., 2nd part.—The Diluvium round Paris and its position in the Pleistocene, by A. Rothpletz.

SOCIETIES AND ACADEMIES

LONDON

Mathematical Society, January 11.—Prof. Henrici, F.R.S., president, in the chair.—Messrs. H. T. Gerrans and W. L. Mollison were elected members.—Dr. Hirst, F.R.S., spoke on the resolution of congruences into systems of quadric reguli; Mr. Glaisher, F.R.S., discussed the solution of a differential equation allied to Riccati's; and Mr. Tucker communicated a paper by Prof. Cayley, F.R.S., on the automorphic transformation of a binary cubic function.

Zoological Society, December 19, 1882.—Prof. W. H. Flower, LL.D., F.R.S., president, in the chair.—Mr. Sclater exhibited some photographs of a new Zebra, from Shoa, lately named *Equus grevyi*, by M. A. Milne-Edwards, F.M.Z.S., which had been sent to him by that gentleman, and pointed out the difference which separated this animal from the nearly allied *E. zebra*.—The Rev. H. H. Slater, F.Z.S., exhibited and made remarks on the skin of a Shrike (*Lanius* sp. inc.) which had been obtained near Spurn Point, Yorkshire.—The Secretary exhibited, on behalf of Lord Lilford, the skin of a young male *Emberiza rustica*, which had been taken at Elstree Reservoir on November 19 last. Only one other example of this bird had hitherto been recorded as having been met with in Great Britain.—Dr. Günther exhibited, on behalf of Sir Campbell Orde, Bart., a specimen of a Charr (*Salmo alpinus*), obtained in a loch in North Uist, being the first example ever obtained in this loch.—Mr. P. H. Carpenter exhibited and made remarks on some microscopical preparations of *Antedon eschrichti*, in which a nervous plexus derived from the fibrillar envelope of the chambered organ was visible at the sides of the ambulacra of the disk.—Prof. Flower exhibited a photograph (presented to the Society by Mr. James Farmer, F.Z.S.) of Seal Point, Farallone Islands, off the coast of California, showing the immense number of Seals (*Otaria gillespii*, M'Bain) frequenting that locality.—Prof. Flower read a paper on the whales of the genus *Hyperoodon*, in which he pointed out that one of the most important points in the history of these animals yet unsolved was whether the large-headed form, with great development of the maxillary crests, called by Dr. J. E. Gray *Lagenocetus latifrons*, was a distinct species, or whether, as suspected by Eschricht, it was the adult male of the common form known as *Hyperoodon rostratus*. The author had asked Capt. David Gray to avail himself of his exceptionally favourable opportunities of observing these animals in their native haunts, to solve this question, with the result shown in the next communication.—A communication was read from Capt. David Gray, ss. *Eclipse*, called "Notes on the Characters and Habits of the Bottlenose Whale (*Hyperodoon*)," in which it was stated that he had killed 203 of these animals last season, and had traced in the males every gradation of development between the two forms, and had therefore conclusively proved that *Hyperoodon* or *Lagenocetus latifrons* had no existence as a distinct species. The communication was illustrated by sketches and photographs, showing the external characters and cranium in various stages of growth.—Mr. P. H. Carpenter read a paper on the classification of the *Comatulæ*. He criticised the method of formulation recently proposed by Prof. F. J. Bell, and pointed out its disadvantages for the purposes of classification, owing to its being inapplicable to those *Comatulæ* which have irregular arm-divisions. He explained his own system of formulation and classification, and stated that he believed it to be capable of dealing with all possible variations of *Comatula* structure.—Mr. F. Day read a paper on the identity of *Arnoglossus lophotes*, Gthr., with *Pleuronectes grohmanni*, Bonap. A second paper by Mr. Day contained remarks on some hybrids between Salmon and Trout.—A paper by Messrs. Godman and Salvin was read, describing some Butterflies from New Ireland, received from the Rev. G. Brown and Mr. E. L. Layard. Among these were examples of two new species, named respectively *Prothoe layardi* and *Danaius adustus*.—Mr. Oldfield Thomas read a paper containing descriptions of two new species of Fruit-Bats of the genus *Pteropus* from the Caroline Islands. The author proposed to call them *Pteropus phæocephalus* and *Pt. breviceps*.—A communication was read from Major G. F. L. Marshall, F.Z.S., containing some notes on Asiatic Butterflies. A species of *Amecera* was mentioned as new to the Beluchistan fauna, and three species were described as new to science.—Mr. G. A. Boulenger read the description of a new species of Lizard from Dacotah, based upon some specimens lately presented to the Society's collection by Mr. S. Garman, of the Museum of Comparative Zoology, Cambridge, Mass., and proposed to name it *Sceloporus garmani*.—Mr. Arthur G. Butler read a paper in which he gave an account of a collection of Spiders made by the Rev. Deans Cowan in Madagascar. In addition to many interesting and singular forms were specimens of the curious tailed species *Arachnoura scorpionoides* from Central Madagascar. Six new species were described.—Mr. W. N. Parker read a paper on some points in the anatomy of the Indian Tapir.—Mr. Herbert Druce read a paper descriptive of new species of Moths chiefly from Western Africa and New Guinea. Fifteen new species were described, as was also a new genus of *Chalcosiidæ* from New Guinea.

Geological Society, December 20, 1882.—J. W. Hulke, F.R.S., president, in the chair.—Percival Fowler, Alfred Eley Preston, and Robert Blake White, were elected Fellows of the Society.—The following communications were read:—On generic characters in the order Sauropterygia, by Prof. Owen, C.B., F.R.S. After referring to the subdivision of De La Beche's group of Enaliosauria into the orders Ichthyopterygia and Sauropterygia, the author indicated that the latter showed differences in the proportional length of the neck and the number and form of its vertebræ bearing relation to the size of the head, together with modifications of the teeth, of the sterno-coraco-scapular frame and of the paddle-bones, leading to the formation of two genera, namely, *Plesiosaurus* and *Pliosaurus*, the latter so-called to indicate the nearer approach made by it to a generalised Saurian type. In Crocodilia the crowns of the teeth show a pair of strong enamel ridges, placed on opposite sides of the teeth, and these occur also in *Pliosaurus*; while in *Plesiosaurus* they are not present. *Pliosaurus* further approaches the fresh-water Saurians by the large size of the head and the shortness of the neck.—On the origin of valley-lakes, mainly with reference to the lakes of the Northern Alps, by the Rev. A. Irving, B.A., B.Sc., F.G.S. The author, having given reasons for considering this question, still an open one, proceeded to criticise Prof. Ramsay's theory as it was expounded by him in 1862. The author proceeded to show that the lakes of the Northern Alps are found, as a rule, just among those strata where subsidence would be most likely to occur. In this way it was shown that we are *not shut up*, by Prof. Ramsay's reasoning, to the hypothesis of *glacial excavation*. Further, other agencies than those discussed by Prof. Ramsay may have co-operated to form lakes,

such as (*a*) *Alterations in the relative levels* of different parts of a floor of a valley, connected with movements of parts of a mountain-system on a large scale. The effects of (1) lines of flexure crossing older lines of valley-erosion; (2) of lateral thrusts closing in a valley (partly), were here considered. (*b*) *Upthrust* of the more yielding strata (as in the "creeps" of coal-mines) by resolution of forces due to pressure of the mountain-masses at the side of a valley. (*c*) The *dead weight of the huge glaciers* which filled the Alpine valleys, and *crushed in the floor*, in places where extensive underground erosion had gone on in preglacial times. (*d*) The partial *damming up of valleys*, (1) by *diluvial detritus*, (2) by *moraines*, (3) by *Bergstürze* (recently investigated by Prof. Heim of Zürich. (*e*) *Faults*. (*f*) *Chemical solution*, by Alpine waters derived from the melting of the snow, which has undergone long exposure to the atmosphere. It was shown that the very situation of the great majority of the lakes of the Northern Alps is distinctly favourable to the operation of one or more of these agencies. The Königsee was mentioned as a special instance of *subsidence*; the Achensee of a lake lying in a *faulted* line of dislocation; Lake Alleghe and Lake Derborence as lakes formed by Bergstürze during the last century; the prehistoric delta of the Arve as the most conspicuous instance in the Alps of the partial damming-up of a valley by diluvial detritus; the *quondam* Lake of Reutte as an instance connected with violent inversion of strata; and the ancient lakes of the Grödner and Oetz Thals as instances of the action of moraines. The common fact of observation that lakes are more numerous in glaciated than in non-glaciated countries, the author thought, was partly explained by some of the foregoing principles, partly by the better preservation of lake-basins in glaciated countries from silting up and from becoming thus obliterated, while in some glaciated regions lakes are wanting.

Victoria (Philosophical) Institute, January 15.—Prof. Stokes, F.R.S., Lucasian Professor of Mathematics at Cambridge, read a paper on the Absence of Real Opposition between Science and Religion.

EDINBURGH

Royal Society, December 18, 1882.—Mr. Robert Gray, vice-president, in the chair.—Prof. Tait read a paper on the laws of motion, in which an attempt was made to express the fundamental principles of dynamics without introducing the idea of "force." The conservation of energy forms of course the basis. The region of space, in which a given particle is, is mapped out by its equipotential surfaces. Newton's First Law is expressed, then, by saying that the potential of the space is the same from point to point, so that the kinetic energy of a moving particle suffers no change. If the potential varies, then the kinetic energy must vary. As a simple case, consider two regions separated by a plane, the potential function being constant throughout each region. Then the velocity of a particle approaching the plane may (since motion is purely relative) be referred to a point moving parallel to the plane, so as to make the velocity of the particle wholly perpendicular to the plane. It thus appears that the component of the velocity at right angles to the plane only is altered, so that if the direction of motion is originally inclined to the plane, the direction as well as the speed is altered. This, in fact, is the well known problem of refraction according to the corpuscular theory of light; and the principle of least action thus appears under the form of the conservation of velocity at right angles to the direction of greatest potential slope. In expressing Newton's Third Law, Prof. Tait extended the second interpretation as given in the now well known Scholium to include vector as well as scalar quantities.—Mr. George Seton, in a paper on illegitimacy in Scotland, gave a careful analysis of the returns for the last decade, which showed a decrease of '9 per cent. as compared with the returns of the previous decade. The counties in which the percentage was under the average for the whole country all lay to the west of a line drawn from the north coast to Loch Ryan, down the eastern boundaries of Sutherland, Inverness, Perth, Argyle, Renfrew, and Ayr. That this difference could not be referred as altogether due to difference of race was proved by the fact that amongst the pure Scandinavian population of Orkney and Shetland the rate was much below the average. The results pointed to a low moral tone in the agricultural districts of Elgin, Banff, Aberdeen, Roxburgh, and Galloway.—Prof. Tait communicated an account of Prof. J. E. MacGregor's experiments on the absorption of low radiant heat by some gaseous and vaporous bodies. The apparatus was a gigantic form of that which has already been described in these columns (see NATURE, vol. xxvi. p. 639). Air saturated with water vapour at 12° C. behaved almost exactly like air mixed with '06 per cent. by volume of olefiant gas.—Prof. Tait, in a note on the compressibility of water, stated that water seemed to be less compressible at higher than at lower pressures, and more compressible (as compared with steel or glass) at lower than at higher temperatures. This latter result was obtained by comparison of his own laboratory experiments with the experiments carried out by Mr. Murray and Prof. Chrystal in their deep-sea sounding expedition last summer on the north-west coast of Scotland. Both series of experiments were made with Prof Tait's steel and glass gauges.—Prof. Crum Brown communicated a note by Mr. A. P. Laurie on an application of Mendeljeff's law to the heats of combination of the elements with the halogens. Laying off as abscissæ numbers representing the heats of combination of different salts of a given halogen, and measuring as ordinates the corresponding atomic weights of the other element in the compound, Mr. Laurie obtains a succession of points which show a remarkable periodic arrangement. The curves so drawn for the different halogens are strikingly similar.

PARIS

Academy of Sciences, January 8.—M. Blanchard in the chair.—The following papers were read:—Observations on the last communication of Dr. Siemens concerning solar energy, by M. Faye.—On the ice-plant (*Mesembrianthemum crystallinum*), by M. Mangon. This plant (which is covered with transparent vesicles filled with liquid, like frozen dew-drops) is formed of a weak solution of alkaline salt, kept in the solid state by a vegetable tissue, whose weight reaches less than two per cent. of the whole mass. The ashes, formed of salts of soda and potash, constitute nearly half (43 per cent.) the weight of the dried plant (recalling seaweed). M. Mangon notes the plant's elective power, suggests that its cultivation, as a potash-plant, might be useful in some cases, and in any case, it might do good service in removal of alkaline salts in excess from ground on the Mediterranean coast.—Researches on hyponitrites; second part; calorimetric measurements, by MM. Berthelot and Ogier.—On the natural formation of bioxide of manganese, and on some reactions of peroxides, by M. Berthelot.—Experiments relating to disorders of motility caused by lesions of the apparatus of hearing, by M. Vulpian. He describes a series of disorderly movements produced in rabbits by pouring a few drops of Lydrate of chloral solution into one ear or both ears. The same experiment with dogs and guinea-pigs gave much less marked effects.—On complex units, by M. Kronecker.—Examination of the analogy between electrochemical and hydrodynamical rings, and the curves $\Delta V = 0$; Best process of discussions in the experimental method, by M. Ledieu.—Experiments on the motion of current waves in various passages, contracted either in the interior or at the extremity of a canal debouching into a reservoir, by M. de Caligny.—Report on a memoir of M. de Salvert on conic umbilici.—On the precision of longitudes determined with use of the new chronometric method, by M. de Magnac. He shows by a list of chronometric longitudes obtained in the *Jean Bart*, on the coast of Brazil and Montevideo, compared with the telegraphic longitudes deduced from observations by three American officers, that the difference is remarkably small.—A case of damage to a building from lightning was reported; the effect was attributed to breaks in certain metallic parts (there was a good lightning-rod and there were trees near).—The periodicity of comets, by M. Zenger. He finds the origin of comets intimately connected with the rotation of the sun. Dividing the intervals of times of perihelion by various whole numbers, he obtains a mean value of 12·56 days, which is exactly that of a demi-rotation of the sun. Thus, between successive formations of comets there must have elapsed an even or odd number of solar demi-rotations. He supposes enormous explosions driving far out the matter of protuberances; large meteorites near the outer border of the corona may thereby be enabled to agglomerate coronal matter round them and form a comet. The general law of motions of planets applies equally to comets, but the duration of revolution of comets must be a multiple of that of a half-rotation of the sun.—Addition to a note on prime numbers, by Mr Lipschitz.—Influence of cooling on the value of maximum pressures developed in closed vessels by explosive gases, by M. Vieille.—Remarks on the expression of electric magnitudes in the electrostatic and electromagnetic systems, and on the relations deduced from it, by MM. Mercadier and Vaschy.—

Phosphorography of the infra-red region of the solar spectrum; wave-lengths of the principal lines, by M. Becquerel. He gives a determination of new lines of the solar spectrum and their wave-length, and he has observed in the infra-red spectrum, maxima and minima of extinction proper to different phosphorescent substances manifested by various luminous sources, and similar to phosphorogenic maxima and minima in the other end of the spectrum.—On solar photometry, by M. Crova. Correcting a numerical error, he obtains 8,500 carcels for the sun's luminous intensity in a clear sky, and so removes the inexplicable discordance of his former figures with those of Bouguer and Wollaston.—Manganese in dolomitic strata; origin of the nitric acid, which often exists in natural bioxides of manganese, by M. Dieulafait. There are two classes of ores of manganese; those of the first class are directly derived from action of sea-water on primordial rocks, and they are deposited but a short distance from their place of extraction. Those of the second class have been, since the origin of seas, in complete solution in their waters, and have been deposited at all epochs, where chemical conditions have been favourable.—On the existence of the genus *Todea* in Jurassic strata, by M. Renault.—On a trombe observed at sea, by M. de Tromelin.—A work by Prof. Inostranzeff, of St. Petersburg, "On the prehistoric man of the stone age of Lake Ladoga," was presented by M. Daubrée.

BERLIN

Physical Society, January 5.—Prof. Helmholtz in the chair.—Prof. Spörer, of Potsdam, first communicated the results of an investigation of the sun-spot observations in the twenty years, 1861 to 1880, with a view to settlement of the question whether movements of the spots indicated surface-currents on the sun. It appeared that such currents, towards the pole, or towards the equator, were not demonstrable. Herr Spörer further spoke at length on a quite peculiar phenomenon he had noticed on observing the transit of Venus on December 6. He premised that the phenomenon might be explained in two ways; either it might be regarded as an effect of fatigue or over-stimulation of the eye (though he had not marked other signs of such exhaustion), or it might be connected with a very cloudy atmosphere of Venus, whose presence is supposed to be indicated by the glow which some astronomers have seen to extend along the Venus-crescent over the whole planet. The phenomenon itself was as follows: the transit of Venus was observed in Potsdam with a 10-foot telescope; the sunlight was reduced to a degree of brightness bearable by the eye by means of a polarising arrangement (two pairs of parallel mirrors). The sun's limb was much agitated, and the first contact could not be observed. When the planet had made a distinct indentation on the sun, it was considerably blacker than the ground of the heavens; and the part of the planet lying outside the sun was invisible. After more than half the disc had entered the sun, it was observed that the borders of the black-planet disc was still at right angles to the sun's border, and the two sun-points were absent. Later, when Venus was further advanced, the whole disc, and even the small part lying outside, appeared brighter than the ground of the heavens, and with a dull grey light. Another minute later a small interrupted line having previously been seen outwards and upwards from the planet's disc, on the ground of the heavens, there appeared, out from the grey disc, a dark crescent-shaped segment, which, above and below, was distinctly defined, and in the middle merged indefinitely in the similarly coloured ground of the heavens. The grey disc with the dark crescent advanced on the sun, so that it was not possible to distinguish precisely the planet's disc. At about 3h. 11¼m. the outer border of the grey disc was in the connecting line of the two solar horns, and so in the first internal contact; one minute later, an alteration (not more exactly describable) had occurred in the aspect of the planet's disc, and about 3h. 13¼m., at the time of the previously-calculated first internal contact, the outer border of the dark segment had entered; one saw a fine luminous line on the sun without black drop. One minute later the sun disappeared behind a bank of clouds.—Dr. Herz communicated the results of calculations he had made with a view to answering the question, whether the tidal action of the moon is capable of producing currents of water-masses on the earth of such an order of magnitude, that the ocean-currents observed might be explained by this cause. Proceeding on the assumption of a water channel running round the equator, he found for liquids with friction, that the tide must indeed produce a current; and for a whole series of such channels reaching from the equator to the pole there appeared currents, which in their co-operation would present the form of the great ocean currents, but their order of magnitude was such, on the assumption at the outset, that the actual ocean currents cannot be due to this cause. Herr Herz then, conversely, calculated from the astronomically-proved retardation of the earth's rotation, the tangential force, which can produce such a retardation, and determined the differences of the water-levels on the east and west coast of a very narrow dividing ridge of land, which would produce such a pull; these differences of level were deducible from the tidal action of the moon.—Prof. Ostwald, from Riga, reported, on his experiments for measurement of the chemical forces of affinity. As is usual in physics, he measured these forces by mass and velocity of the reactions, or by the force with which equilibrium is maintained. Two acids were each brought into contact with a base, and the salts formed were determined; in all cases, the affinities were found proportional to the reacting mass and the square of the velocity of the reaction. The formula construteed a few years ago by Herren Guldberg and Wage for affinity, has been confirmed by the author by numerous experiments.

VIENNA

Imperial Academy of Sciences, November 30, 1882.—V. Hausmaninger, on the variability of the coefficient of diffusion between carbonic acid and air.—P. Kowalewsky, on the relation of the nucleus lentiformis to the cortex of brain in man and animals.—V. Hilber, on recent land-snails and land-snails found in the loess from China (part 1), containing a description of Helix species collected by L. v. Loczy during the Asiatic Expedition of Count Szecheniji. E. Stefan, on the experiments made by Boltzmann on sound-vibrations.

December 7, 1882.—V. v. Lang, on his capillary balance.—H. Niederriss, on trimelhene-glycol and the bases of trimelhene.

December 14, 1882.—F. Streintz, on the usefulness of the method of Fuchs.—Tg. Klemencic, on the capacity of a plate-condenser.—A Wassmuth, on the internal connection of some electro-magnetic phenomena resulting from the mechanical theory of heat.—V. Gruber, fundamental experiment on the cutaneous sight of animals.—G. Vortmann, on the separation of nickel from cobalt.—R. Canaval, on the earthquake at Gmünd (Austria) on November 5, 1881.—E. Weiss, communication on the observations of the transit of Venus in Austria.—H. Weidel and M. Russo, studies on pyridine.

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THURSDAY, JANUARY 25, 1883

THE THIRST FOR SCIENTIFIC RENOWN

FEW students of science can fail to feel at times appalled by the ever-increasing flood of literature devoted to science and the difficulty of keeping abreast of it even in one special and comparatively limited branch of inquiry. Were merely the old societies and long established journals to continue to supply their contributions, these, as they arrive from all parts of the country, and from all quarters of the globe, would be more than enough to tax the energy of even the most ardent enthusiast. But new societies, new journals, new independent works start up at every turn, till one feels inclined to abandon in despair the attempt to keep pace with the advance of science in more than one limited department.

One of the most striking and dispiriting features of this rapidly growing literature is the poverty or worthlessness of a very large part of it. The really earnest student who honestly tries to keep himself acquainted with what is being done, in at least his own branch of science, acquires by degrees a knack of distinguishing, as it were by instinct, the papers that he ought to read from those which have no claim on his attention. But how often may he be heard asking if no means can be devised for preventing the current of scientific literature from becoming swollen and turbid by the constant inpouring of what he can call by no better name than rubbish!

Some sciences seem to be specially exposed to inundation of this kind. Geology lies exposed to it in an unusual degree. Popular in its subject, and capable of ready apprehension as to its general principles, this department of science allures the outsider into its precincts, where he too frequently soon arrives at the belief that to have read a geological book or two is to become a geologist. This belief would be harmless enough, did it not speedily bear fruit in "papers" communicated to scientific journals, and stamped with all the enthusiasm and crudity of a beginner. On no account should any check be placed on the legitimate ambition of the youngest aspirant after scientific renown. But we venture to think that the common precipitate publication of his earlier efforts is not a legitimate ambition; but on the contrary is really an injury to himself and a positive hindrance to the progress of the science which he no doubt loyally desires to serve. It too frequently happens, moreover, that his first efforts are directed to the pleasant task of discovering flaws in the work of those who have preceded him. And of course the more eminent these predecessors, the greater his credit in setting them right. Let him take to heart the old maxim, *Festina lente*. The longer he delays his appearance as an author, and the wider he meanwhile extends his practical experience of nature, the more tolerant will he become of the work of others and the less overweeningly confident of his own. In no department of natural knowledge can a real acquaintance with the subject be gained save as the result of prolonged study.

These reflections have been suggested on the present occasion by the perusal of a pamphlet which exhibits in the most glaring way the tendency on which we have animadverted. It is devoted to the announcement of a brand-new theory of the origin of Fingal's Cave.[1] Curiously enough this is not the first time that the basaltic colonnades of Antrim and the Scottish Isles have furnished the text for teaching the most arrant nonsense. Nearly forty years ago a sailor, familiar with tropical bamboo jungles, started the idea that columnar beds of basalt are neither more nor less than petrified growths of bamboos. After vainly trying by occasional newspaper letters to find supporters, he seems to have given up the struggle against the blind prejudices of geologists. In 1864, however, his views were taken up by another writer yet more outrageous, who published a pamphlet of nearly 100 pages, entitled "The Giant's Causeway once Bamboos," and in supporting his dogma ran a tilt at religion, science, tradition, history, in short at everything that happened to suggest itself in the course of his incoherent and erratic pages.

The author of the pamphlet cited below, Mr. F. Cope Whitehouse, M.A., &c., is obviously a man of original genius, and is resolved that the world shall know it. In the summer of 1881 he seems to have come with the mob of tourists that annually makes a pilgrimage to the coast of Antrim. But instead of merely submitting to be led through the usual route by the inevitable and inexorable guide, he boldly separated himself from the gaping crowd, and proceeded to meditate. To his rapid mental vision it was soon apparent that the caves of that coast-line, instead of being the work of the sea, as ignorant mankind has hitherto believed, have been hollowed out by human hands. At once he could perceive the intimate relationship of Gothic doorways, ancient civilisation, mediæval castles, Irish manuscripts, and the "Kelto-Iberian, Wend or Phœnician" race. The narrow sea-worn gullies of Antrim being thus shown to have been ancient harbours, his eye looked northwards to the dim blue Scottish Isles, and his venturous imagination at once demanded whether that world's wonder, Fingal's Cave, might not after all be merely a piece of man's handiwork. To state the question was in effect to answer it affirmatively. Nevertheless that no unsympathetic geological Philistine might blaspheme, our courageous hero sailed for those far islands of the west, saw Staffa with his mortal eyes, and found how well his prophetic intuition had divined the secret of that weird place. We almost envy the thrill of satisfaction that must have vibrated within him as he proudly felt that scientific observers for a century past, from the days of Sir Joseph Banks down to our own time, had one and all missed the true meaning and history of the Caves of Staffa, and that it was reserved for him, casual visitor as he was, to lift the veil and reveal the mystery to our astonished gaze.

Knowing well the type of which Mr. Whitehouse is a fresh and most characteristic example, we hardly require his assurance that as soon as his eye lighted on Staffa his "conjecture received strong and unexpected confirmation. It was subjected to rigid examination; it was strengthened by opposition." Of course it was. Then, like all similar enthusiasts, his soul could find no rest until he had proclaimed the truth to the nations. Returning to America, he found an opportunity of enlighten-

[1] "Is Fingal's Cave Artificial?" A paper by F. Cope Whitehouse, M.A. (New York: Appleton and Co., 1882.)

ing the darkness of the American Association for the Advancement of Science, at its meeting in Montreal in August last. A few weeks later he proclaimed the great discovery to the Academy of Sciences of New York. But these were limited audiences, though composed, no doubt, mainly of scientific men on whom as yet the true light had never shone. It was absolutely necessary to appeal to a wider, and possibly more sympathetic public. Accordingly he published his views in the December number of the *Popular Science Monthly*. But he was still unsatisfied, till at last he conceived the noble idea of combining the spread of truth with promoting the erection of the Statue of Liberty enlightening the World. He hired a theatre in New York, gave an account of his astonishing observations, charging a dollar a head for admission, and stated that the proceeds of his "matinée" were "to be devoted to the pedestal of the colossal statue." Let us hope that the sum realised was worthy at once of the great truths proclaimed by the lecturer, and of the national object to which it was to be given. Future pilgrims to the colossal Statue of Liberty will piously scan the pedestal, searching for the stone that shall hand down to the far future the name of the illustrious seer who could brush away the tangled cobwebs spun by a century of scientific babblers, and pierce into the true meaning of the Cave of Fingal.

Mr. Whitehouse has published so far only an abstract of his address, but he has had it well printed with good illustrations, and seems to have generously scattered copies of it broadcast over this country. It was not of course at all necessary that he should communicate the steps of the reasoning by which he was led up to his great discovery. And he has considerately refrained from troubling the world with such unprofitable details. Still one cannot help trying to follow the mental process by which an epoch-making deduction has been reached. We get from the abstract glimpses of the way in which the received explanation of the caves of Staffa collapsed at the touch of Mr. Whitehouse's genius. He visited the scenery in calm summer weather. From Staffa he could see the great sweep of the Mull cliffs to the east and the broken rampart of islands all round the rest of the horizon. As the smooth sea mildly heaved along the base of the basaltic colonnade, he could easily persuade himself that Staffa must be a "singularly sheltered," "land-locked" island, and that "the force of the breakers is inconsiderable." How absurd then must it have appeared to him to attribute to that placid lake-like water the power of hollowing out caves in a rock so obdurately stubborn as basalt! Moreover, he could see no reason why the sea, supposing it gifted with such power of erosion, should have chosen the places where the caves actually occur. And his inability to find this reason satisfactorily disposed of any possible action of the waves. Not only so, but from his stand-point at Staffa his clear vision could take in the whole coast-line of Scotland, and he made the further important announcement, which will doubtless for ever silence our northern geologists, who believe in the geological power of the waves, that "there are very few hollows worn by the sea in the Scotch coast!" Having cleared away the fictions of so-called scientific observation, he could apply the much more reliable conjecture which his glance at the Giant's Causeway had evoked in his own mind. To his trained eye the caves of Staffa were obviously artificial. Oracularly he tells us that they are "strikingly Phœnician." "No such Gothic arch was ever formed by nature. No natural cave has an entrance higher than the interior." (!) Lastly, from the end of Fingal's Cave you can see the Hill of Iona rising against the sky, consequently the cave must have been excavated by men who lived on Iona. This final argument must be regarded as a crushing answer to those who have recklessly talked of the power of the waves in these regions. On what conceivable grounds can we suppose that the sea would make a tunnel, from the end of which the Dun of Iona would be visible?

Perhaps some unconvinced outsider may be tempted impertinently to ask for what object such stupendous excavations could have been devised by any civilisation, whether ancient or modern—excavations always swilled with the surge, often unapproachable for weeks together, and in which, even in calm weather, unless care be taken, a boat is liable to have its bottom knocked in. Of such questions Mr. Whitehouse very properly takes no notice. Again, we were in the belief that the religious community of Iona had been an eminently peaceable folk, liable to invasion by pirates from the sea or marauders from the mainland, but with little more to oppose in the way of defence than the prestige of their sanctity. But this conception also is now found to be false. We learn, on the same reliable authority, that they were a warlike race, quite able to look after themselves. It appears that Mr. Whitehouse has shown "the strategic importance of Staffa, and the probability that the wealth and refinement of Iona were due to the protection it afforded." He will have no difficulty in further proving that the traditional picture of the saintly Columba is a mere myth, and that the abbots of Iona possessed an army and navy, made war on heathen Pict and savage Scot, curbed the fury of fiery Norseman, and employed their gangs of prisoners in tunnelling the caves of Staffa!

There is a hollow among the rocky knobs that rise inland from the summit of the cliffs above Fingal's Cave. We would fain place Mr. Whitehouse there on a day when the gathering clouds have blotted out Ben More, when thick mists are driving along the opposite precipices of Gribon, when the Treshnish Isles grow fainter every moment in the western sky, when even Iona, that lies so near, is fading into the general gloom, and when the wild moan of the rising south-western gale among the crags around is answered by the hoarse clamour of the surge below. We should like to keep him there while the gale rapidly increases, breaker after breaker careering madly forwards with foaming crest from under the pall of driving rain that hides the sea, dashing into every creek and cave, rushing in sheets of green water up the face of the crags, and pouring back in hundreds of yeasty torrents into the boiling flood. We would ask him still to stay till the storm has reached its height, that he might feel the solid island shake under his feet, that he might see the sheets of water, foam, and spray thrown far up into the air, that he might hear the cannon-like thunder of the shock as each billow bursts into the Cave of Fingal. He would return a wiser (and a wetter) man, and would regret that in a rash moment he had published some childish nonsense

about man having excavated sea-worn caves, and had expressed an opinion about the power of the sea of which he would then feelingly admit that he had been profoundly ignorant.

The pamphlet here noticed did not in itself deserve consideration in these columns. We have made use of it as a type of publication painfully frequent in the literature of science. If in exposing its characteristics we deter any rash and immature aspirant for fame from at once rushing before the world with what he conceives to be his discoveries, we shall have done a service at once to him and to science.

CINCHONA PLANTING

A Handbook of Cinchona Culture. By Karel Wessel van Gorkom. Translated by Benjamin Daydon Jackson, Sec.L.S. (London: Trübner, 1883.)

Die Chinarinden in Pharmakognostischer Hinsicht dargestellt. Von F. A. Flückiger. (Berlin: R. Gaertner, 1883.)

THE rapid extension of cinchona planting in India, Ceylon, and Jamaica will make a translation of Van Gorkom's account of the methods of cultivation and harvesting pursued by him, as Director of the cinchona plantations belonging to the Dutch Government in Java, useful to many who propose to turn their attention to this profitable industry. At present intending planters in British possessions have had little beyond Dr. King's Manual of Cinchona Cultivation (1876) to serve as a guide. In Ceylon the planting community includes many men of first-rate ability, and the singularly energetic journalism of the island speedily ventilates for the common good any fresh idea or point of practice in planting procedure.[1] Indian planters share the benefit of this, while Jamaica has the advantage of possessing in Mr. Morris, a director of its botanical department, who has carried to the West Indies an intimate knowledge of all that is being done in Ceylon. It is not very probable that those who are at present occupied in cinchona enterprise in British possessions will glean much from Van Gorkom's book. Still such a manual will not be without its use for those who have everything to learn about the matter, and, as will be seen, it cannot fail to be interesting to those who watch from an independent point of view the economics of the subject.

The book is handsomely printed and got up—too handsomely, indeed, for workmanlike use, for which its size, that of a small folio, seems particularly unsuited. We must too make a serious protest as to the style of the translation, which, we think, cannot be considered tolerable, even with every allowance for "seeming inelegancies" which Mr. Jackson pleads for in his preface. Take, as a sample, the first sentence which caught our eye:—

"If we trust that this excellent opportunity for fruitful comparisons shall lead to unfettered judgment, still more do we look for, from the impressions received and the enlarged field of view, the scientific work carried on, which has so long been in hand, and most certainly with great completeness and undisputed knowledge of material, will indicate our present standpoint in the domain of quinology" (p. 264).

[1] Of T. C. Owen's Cinchona Planter's Manual, published at Colombo, we know nothing beyond the name.

Now it is quite certain that this is not English, and we have some doubts whether it really conveys any meaning at all. But at any rate we would ask what is the use of translating in this way a work the purpose of which is not literary but essentially utilitarian. There seems, in fact, to be a deep-rooted superstition about the value of so-called fidelity in translating books of mere information. In rendering a foreign language as a philological undertaking, it is often desirable to sacrifice, to some extent, style and form, in order to convey as nearly as may be, the exact force of each word and of each turn of expression. But where, as in a technical treatise, it is only the context we care about, it is exasperating to find the translator exhibiting a would-be scholarly care over the exact reproduction of the vehicle. All we want him to do is to master the meaning and give it to us in clear, straightforward English.

Having said so much by way of criticism we may indicate a few points which we think will be interesting even to some who are not colonial readers of NATURE. A hundred of the three hundred pages of which the volume consists is given up to historical matter regarding the history of *Cinchona* and the development of its culture in Java and in British possessions. All this is an oft told tale, and contains little that will not be found in Mr. Markham's Peruvian Bark (reviewed in NATURE, vol. xxiii. pp. 189-191). An exception must be made, however, as to the interesting account of the commencement of cinchona cultivation in Bolivia. The existence of this enterprise was known, but we have not met with any previous account of it. The Dutch Consul-General reported to his Government:—

"The great event in the agricultural region of Bolivia is the planting of the Bolivian cinchona forests, of which an earnest beginning was made in 1878. . . . The river Mapiri, in the province of Larecaja, department La Paz, has been the centre of the movement, and already the young trees of two years' growth, may be reckoned at from four to five hundred thousand" (p. 17).

Doubt is, however, expressed whether the planting will be maintained in the face of labour difficulties and a possible fall of prices in consequence of increasing exports from the East Indies.

Modern cinchona enterprise in Java has aimed at the production of barks rich in quinine. With the lucky purchase from Mr. Ledger in 1865 of a packet of seeds of the now well-known *Cinchona Ledgeriana*, the Dutch "cinchona culture of the future has entered upon an entirely new phase" (p. 77). About 20,000 of the seeds germinated in Java, and first and last Mr. Ledger received about 24*l.* from the Dutch Government, and "was therewith well content" (p. 91). Fortunately the greater part of the seed originally imported was purchased by a well-known Indian planter, Mr. Money, and some of it seems by private channels to have found its way to the Government plantations in Sikkim. The Dutch having got this valuable kind seem to have managed it with extraordinary intelligence and skill. Men like De Vrij, Moens, and Van Gorkom were well-trained European scientific men and competent chemists. Their object was by continuous selection, controlled by repeated analyses of bark made on the spot to obtain races of *Cinchona Ledgeriana* richer and richer in quinine, and it is a matter of general

notoriety how well they have succeeded.' It is the part of Van Gorkom's treatise dealing with this matter which cinchona planters will be grateful to Mr. Jackson for putting within their reach. Two conditions of success in harvesting good seed are insisted upon.

"For seed saving, the handsomest strongest trees are selected, and especially amongst those whose superior value has been ascertained by chemical examination. Disappointment is inevitable where the eye and botanical characters alone are made use of and trusted to; *the whole issue depends upon the certainty that varieties rich in quinine are exclusively propagated.*

"The choice being made there is something else which must not be neglected; it further behoves us to be perfectly sure that the tree is not fertilised with foreign pollen, that is to say, pollen of an inferior tree or variety" (p. 136).

The last condition cannot be insisted upon too forcibly, notwithstanding that competent botanical opinion can be quoted against it. In their home in South America the different species of *Cinchona* are localised at different points of the Andine chain. Geographical isolation keeps them uncrossed. But where they are brought together in one plantation they hybridise freely. *Cinchona robusta*, which is now widely diffused in India, undoubtedly first originated in Ceylon as a cross between *C. officinalis* and *C. succirubra*.

The aim of the Dutch Government being to produce a commercial bark of high quinine-producing quality, in which they have met with extraordinary success, Van Gorkom is somewhat disposed to criticise the different policy which has been pursued in British India:—

"The Bengal Government . . . makes its cinchona culture serviceable before all things to the wants of its population, and thus only asks itself, how the people and army may be provided with febrifuges on the most advantageous terms" (p. 229).

He sets against this the "well-known fact that not one half of the alkaloids possessed by the raw material are obtained, the greater part being lost.' Even supposing, however, that things are as bad as this, and not susceptible of improvement, it is still arguable whether, looking at the cheapness with which red bark can be grown and converted into a febrifuge—the usefulness of which is incalculable—the theoretical waste is a matter for the present of much consequence. But it is unreasonable to suppose that the Bengal methods of extraction are not susceptible of improvement, though they will probably never reach the standard practicable by more expensive methods in Europe. But the objection of wastefulness must be measured by the circumstances. The proprietor of an estate in England who, with a view of bringing a portion of his park into tillage, began by burning the timber upon it, would be considered a madman. But this is habitually done in clearing a piece of tropical forest for cultivation, and as it is not easy to see what else could be done, a complaint as to the waste would not be much to the purpose. It might have been expected that Van Gorkom's sympathies would have centered in the quinine-producing yellow barks which are for the moment most in favour. This, however, is largely due to the unreasonable importance which is attached to quinine over other cinchona alkaloids. Van Gorkom does not share this prejudice:—

"The conviction has more and more gained ground, that good cinchona barks judiciously applied, frequently do not merely rival quinine, but even surpass it in useful effect" (p. 212).

This point of view is exceedingly important with regard to red bark (*C. succirubra*), which is the easiest of all species to cultivate.

"There is no cinchona bark richer in alkaloids, and though *C. succirubra* is not suitable for the preparation of quinine, because it can only be treated with trouble and much expense, yet it has a preponderance of the secondary alkaloids. No better material for pharmaceutical purposes is known, and on that account its propagation is desirable from every point of view" (p. 100).

High class yellow barks are by no means free in their growth or particularly easy of cultivation. It has been found useful to graft them on *succirubra* stocks, and the practice has been adopted in Sikkim and Ceylon; Van Gorkom gives a useful account of the method adopted in Java.

We must refrain from pursuing many other points which these pages suggest. Two of the concluding chapters deal with the possible synthesis of quinine and the commerce of the barks. As to the former the author has little doubt of success. Two isomerous bodies, chinoline and chinoleine, are known, of which the former is obtained by the distillation of coal tar, the latter by that of quinine This is thought then to be the clue by which the construction of quinine from coal-tar products will be eventually achieved. But he takes comfort for cinchona planters from two considerations. One is that the synthesis of a vegetable substance when effected does not always result in its practical commercial replacement. The synthesis of alizarine it is found after all does not give the dyer quite what the madder plant gives him. Artificial quinine then may—if ever produced—prove only of interest to the chemist. His other consolation is based on what is said above—that pharmacy can never dispense with the total aggregate extracted products of bark, and the day may be regarded as indefinitely distant when the chemist will be able to replace these any more than such complexes as the contents of our tea- and coffee-pots.

As to commerce it is interesting to learn that London is the most important market for bark, and Paris next. We fear, however, from statistics obtained from another source, that this country has no corresponding lead in the production of the manufactured products, only about 10 per cent. of the quinine of the world being made in England. Yet Van Gorkom states emphatically that "the consumption at the present day of cinchona and its alkaloids, merely represents a paltry fraction of the quantity which will be required to satisfy the prescription of humanity in every country, and among all classes and races of men" (p. 236).

We have left ourselves but little space to notice Prof. Flückiger's handy and concise work, which, though of importance to cinchona planters, is primarily a pharmaceutical study of the subject. The bark of *Cinchona succirubra* has been recently adopted as the official bark of the German Pharmacopœia—a fact of no small importance to planters in British possessions, when it is remem-

[1] Acknowledgment must be made of the striking liberality with which the Dutch Government officials have always placed what they could spare of their selected seed at the disposal of planters in other countries.

bered how enormous is the extent of its cultivation in their hands. It is this fact which has won it its official status, as though poor in quinine its quality is tolerably uniform, and being easily grown its supply can always be depended on. Prof. Flückiger gives a figure of the plant as well as of *Cinchona Ledgeriana*—the quinine bark *par excellence*—and of *Remijia pedunculata*, one of the sources of the *Cinchona cuprea* which has of late years been poured into European markets from South America.

MARINE SURVEYING

A Treatise on Marine Surveying. Prepared for the use of younger Naval Officers, by the Rev. J. L. Robinson, B.A., Royal Naval College. (London: Murray, 1882.)

THIS book has been written apparently with the view of enabling young naval officers to cram themselves sufficiently to pass the examination in surveying at the Royal Naval College, and it must be conceded displays considerable industry on the part of Mr. Robinson, who has evidently taken pains to go through the examination papers on surveying from their commencement, to see what questions are usually asked, and in what form they could be best answered; and has besides consulted a large number of works bearing on surveying, a list of which he gives at the commencement of his treatise; but we confess we are much disappointed that with such excellent materials, so poor a result should have been produced, for, with the exception of the chapter on tides, which in its way is excellent, the work is of very little value, and rather reminds us of that treatise of—

"The young lady of Buckingham
Who wrote about geese and stuffing 'em,
But found out one day
She'd neglected to say
A word in her book about plucking 'em."

Mr. Robinson says in his preface "he has had no intention to write a handbook for the use of the practical surveyor," and that "such an intention might fairly be regarded as an impertinence in one who has never been engaged in the practical work of the profession," but that he has had rather "the examination room and its requirements before him." But did it not strike Mr. Robinson that the practical surveyor selected to examine the candidates might ask questions upon which he has neglected to touch, and that consequently his treatise might fail to ensure success in the "examination room," notwithstanding the valuable hints he has received from Staff-Commander Johnson and his friend of great experience as a first-class surveyor?

The first chapter consists of extracts from Admiralty publications, but we recommend the officers at the college to consult those publications for themselves, more especially the Admiralty list of abbreviations, as the illustrations in this work give a poor representation of the symbols and signs used by the draughtsman and engraver.

The second chapter, on the Construction and Use of Scales, and the sixth, on Instruments, are derived principally from Heather. Here again we prefer the original to the copy.

The third chapter, on Laying off Angles, merely contains a brief description of the methods of plotting angles by chords with a small radius. On this we would remark that the real value of plotting angles by chords consists in their being plotted with long radii, as any practical draughtsman could have informed the author.

The fourth chapter is a most elaborate analysis of the method of Fixing a Position by Angles, &c. Surveyors take sextant angles, principally, to fix their positions when sounding, and invariably use the station pointer for that purpose; this chapter therefore seems to us to be firing a 12-ton gun at a sparrow.

The fifth chapter, on Charts and Chart Drawing, is rather a description of the method of map construction, and contains some mis-statements. Evidently Mr. Robinson is not well acquainted with the mode of constructing charts at the Admiralty or by surveyors, as he states in one paragraph that circumpolar charts are usually constructed on the gnomonic projection, whereas we are not acquainted with one Admiralty circumpolar chart on this projection. It is true a diagram is published to facilitate the practice of great circle sailing but no circumpolar chart.

The fact is all marine surveyors project their work on the gnomonic projection, and as the smallest scale in use is an inch to a mile, it is evident that the errors of this projection are very slight, as the largest sheet of paper that can be worked at conveniently is about six feet square. When the original surveys arrive at the Admiralty the Hydrographer decides in what form they shall be engraved and published. If the surveys are plans of harbours, they are usually published on the gnomonic projection (as they were originally drawn); if the survey is of a coast, or to be incorporated in a coast, or general sheet, it is transferred to the mercatorial projection, for which the meridional parts of the spheroid are used. Charts of the circumpolar region are however published on an arbitrary projection, in which the parallels of latitude are drawn as concentric circles at equal distances from the pole.

Chapter seven is on Base Lines. Now base lines are principally of use to the marine surveyor as the quickest method of starting his work, which, when it extends over a large area, almost invariably depends eventually for its scales on astronomical observations.

Mr. Robinson states that it is impossible to fix the position *exactly* by means of a sextant. Here we must differ from him, and will give one instance to the contrary. When the question of the boundary between the United States and British North America was decided, and the 49th parallel was fixed on, Admiral Sir George Richards then in command of H.M. surveying vessel *Plumper*, at Vancouver's Island, was directed to ascertain the position of this boundary line on the western seaboard of North America. This he did with a sextant, and buried a mark in the ground on the position of the 49th parallel as ascertained by himself. The Americans sent a party for the same purpose with a zenith sector and altazimuth and when they had fixed the position of the 49th parallel by these means, the difference between the two results was found to be less than 100 feet! It is of course as well that nautical surveyors should know the various methods employed in obtaining accurate bases for geodetical measurements, but for marine surveying the same nicety is not required as in measuring the arc of a meridian, and it cannot be too often impressed on the mind

of the aspirant in surveying that over accuracy (that is such minuteness as cannot be represented on paper) is loss of time.

The eighth chapter is on Triangulation, and is more worthy of attention than those preceding. If we remember rightly, there was only one three-feet theodolite used in the Ordnance Survey of Great Britain, which instrument is the property of the Royal Society. In fact, so far as we are acquainted, there are only two three-feet theodolites in existence, Ramsden's, and another used in the Great Trigonometrical Survey of India.

The observation that very distant stations are generally observed at night is now subject to correction, as the heliostadt has rendered it quite as easy to observe by day, in fact, in some of our marine surveys, triangles whose sides were 60 miles in length, have been obtained with these instruments, and an eight-inch theodolite, with the greatest ease. With the eight-inch theodolite, and by means of repeating the main angles round the circle, very accurate results may be obtained; and the spherical excess has to be allowed for, and deducted, in order to make the triangles plane, for in all nautical surveys the chord of the arc is used both for calculation and plotting.

The ninth chapter, on Levelling, contains not only an account of levelling, but also of obtaining heights by means of the barometer and thermometer, but totally neglects the method in general use by surveyors, viz. by angles of elevation and depression with a theodolite. For travellers the barometer and thermometer give an approximation of the elevation, which is exceedingly useful in an unsurveyed district. For work requiring extreme accuracy careful levelling is required, but for nautical work the principal use of the level is to ascertain the exact difference between the zero of the tide gauge and some permanent mark on shore, so that a fixed datum can always be referred to for reduction of soundings in future. The heights of hills are almost invariably obtained by angles of elevation and depression, and the results so closely approximate to the truth that it is waste of time to do more, unless results less than five or six feet in error are absolutely requisite.

The tenth chapter is on Tides, and is, as has been before mentioned, well worthy of perusal, in fact, it is the most complete popular description we remember to have seen; and if compiled entirely by Mr. Robinson from the books he has consulted reflects great credit on him, and we can but wish he had paid the same attention to the other parts of his treatise.

We think, if we remember rightly, that it was in one of the Arctic voyages, that of Sir John Ross, that the influence of atmospheric pressure on the rise of the tide was first observed, but the fact is well known now, and is always allowed for by surveyors when ascertaining the mean level of the sea. This subject is of considerable practical importance, for it is sometimes the only guide we possess by means of which we can reduce our soundings to the same depth as those obtained at previous epochs. For instance, if the datum-mark to which the soundings have been referred, has, in the course of time, disappeared, the surveyor's first work is to ascertain the height on his gauges of the mean level of the sea. This he does by obtaining day and night observations for five or six consecutive high and low waters, carefully registering the barometer at the same time. Then meaning the results and allowing for the atmospheric pressure, it is astonishing how closely they agree. The mean level having been found, it is very easy to reduce the soundings to the former datum. For instance, if the soundings are reduced to low water ordinary springs, and their rise at springs is 16 feet, it is evident that the soundings must be reduced to 8 feet below mean level of the sea, to enable them to be compared with those previously obtained.

In his paragraph (201) on tide-gauges, Mr. Robinson recommends a string from a float over a pulley. It must be either a chain or wire, as a string is far too subject to contraction and expansion from atmospheric changes.

Self-registering tide-gauges are, we are glad to say, becoming much more common than they were, and we trust to see them established at every important point in the United Kingdom.

The eleventh chapter, on Soundings, may do very well to enable a sub-lieutenant to answer some questions in the examination-room, but is useless in practice.

Few surveyors think it necessary to accurately protract on a chart the position of the objects they use for sounding transits (Art. 210). Often the back mark is too far off to appear at all on the sheet, and the farther off it is the better, provided the atmosphere is clear; for a front mark the first conspicuous object in the foreground is seized—a conspicuous tree, the chimney of a house, the angle of a hedge, a boat hauled up on the beach, &c. If the back object is sufficiently far off, the lines of soundings are practically parallel, and the same mark may be used for the whole survey. It is also requisite to cross the lines of soundings, to avoid any chance of error. The sounding on the chart depends (1) on the leadsman, (2) on the officer fixing and registering, (3) on the tidal register, and (4) on the reductions being correctly made. Now there may be a mistake in either one of these, and consequently it is advisable to always cross the lines of soundings as a check. We have found that as good a method of checking the correctness of the results as any, is by running along the contour lines, as defined by soundings obtained at right angles to those lines.

Another remark on soundings we must also take exception to, viz. that (Art. 213) it is usual to make a reduction of a couple of feet below low water in doubtful cases.

With respect to under-currents (Art. 221), Mr. Robinson appears not to be aware of the methods pursued by Sir G. Nares in the *Challenger*, and Capt. Wharton in the *Shearwater*.

Chapters twelve and thirteen, on Chronometers, and Meridian Distances, are principally derived from Admiral Shadwell's notes on the management of chronometers, and here we recommend the student to the original rather than the copy.

The method of calculating meridian distances expounded by Dr. Tiarks, and fully explained by Sir Chas. Shadwell, is invariably used by nautical surveyors, and the results thus obtained have hitherto closely approximated to the later determinations by means of the electric telegraph.

Chapter fourteen, on the method of Plotting a Survey, deals almost exclusively with the small plans required by the examination papers at the Naval College, and as it

instructs the student to plot *out* from the base line (which is never done in practice) cannot be recommended.

The method of plotting adopted in practice is to calculate out from the base line to the extreme points of the survey, or to the extreme points that will appear on any one sheet of paper, and then to plot *in*. Every practical draughtsman knows that it is far easier to say, "draw a straight line" than to do it, and that an infinite amount of trouble is saved by plotting in towards small triangles from large, as the errors of the plotter are then being constantly reduced, whereas in plotting *out* they are being continually enlarged. In fact we venture to say that no one is competent to write an article on plotting who has not been in the habit of projecting surveys for no one else can understand the extreme nicety required to make three lines from three stations to the same object coincide in one point.

It is possible that Mr. Robinson has compiled this work in hopes the Admiralty may order it to be accepted as the text-book on surveying at the College. We trust, however, that their Lordships may be better advised on the point. Already we have one book, ordered to be used, which contains a theory on winds, not by any means accepted by meteorologists, and this theory has at present to be learnt by all the younger naval officers. Now we have no objection to any one theorising on wind, or any other subject, but what we do object to is that a book containing such theories should be ordered to be the standard work at the colleges, simply because the gentleman who wrote it holds, and worthily holds, a prominent position there. We think that although theories should not be absolutely excluded from textbooks, they should deal principally with well-ascertained facts, leaving the student to develop for himself a theory from those facts.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Natural Selection and Natural Theology

A PERUSAL of Dr. Romanes' article on Natural Selection and Natural Theology, in the *Contemporary Review* for October, 1882, suggests a few remarks upon one or two points, which may not be out of place.

One would quite agree with Dr. Romanes in "insisting on the essentially distinct character of natural science and natural theology as separate departments of human thought." True as that is, in a just sense, how does it follow that there "is no point of logical contact between" the two? Does this mean that because natural phenomena can be reduced to laws and sequences of cause and effect, no legitimate or rational inference can be made by the human mind to a *causa causarum*? It would seem so, and that it must be so to justify his very thorough-going conclusion: (1) That Darwin's theory explodes *particular design* (which he chooses to identify with special or independent creation); and (2) that it does not allow us rationally to introduce the conception of "an ultimate cause of a psychical kind pervading all nature," the theory having "no point of logical contact with the theory of design even in the larger sense." That is, a *raison d'être* in particular is proved to be absurd; behind all secondary causes, one such may possibly exist, but it is not to be legitimately thought of!

Or does he mean only that Darwin's theory need not, and legitimately should not, concern itself with philosophy and natural theology? Very well: then let the disciples practise what they preach, and imitate their revered master, who was content to maintain that species became what they are by descent with modification, instead of by independent creation, leaving untouched the question whether or not they were designed to be what they are. If there be "no logical contact" between Darwin's theory and the theory of design, then this renowned investigator preserved more logical consistency than some of his followers: if he refrained because of "the essentially distinct character of natural science and natural theology," and because of his determination to consider only the former, he was no less consistent.

But after all, such questions may be consistent enough, and moreover they are inevitable; and so it is not wonderful that they are raised—and not rarely prejudged—on the scientific about as freely as on the theological side.

Anyway Darwin did not prejudge the question of design, while declining to discuss it, as is done, for instance, by the *dictum* that if the species of animals and plants were slowly evolved, the evidence of design has been utterly and for ever destroyed. That has been affirmed over and over, formerly in the main by the theologians, but now, when these have seen what it comes to, mainly by the anti-theologians; by both, seemingly, under a misapprehension of the real character of the evidence for design.

Dr. Romanes' view is fairly presented in his denial that, under our present knowledge, "the facts of organic nature furnish *evidence* of design of a quality other or better than any of the facts of inorganic nature." "Or, otherwise stated, there is nothing in the theory of natural selection incompatible with the theory of theism; but neither does the former theory supply *evidence* of the latter. Now this is just what the older theory of special creation did; for it would be proof positive of intelligent design, if it could be shown that all species of plants and animals were created, that is, suddenly introduced into the complex conditions of their life; for it is quite inconceivable that any cause other than intelligence could be competent to adapt an organism to its environment *suddenly*."

Is the writer of this quite sure that any cause other than intelligence could be competent to adapt existing organisms to their environment *gradually*? How has the former presumption—the contrary of which was quite inconceivable—been done away with? For this presumption arose, and had its full force under the consideration of animals and plants produced by natural propagation; and the then irresistible inference of intelligent design was drawn directly from their adaptations in themselves and to their environment; whence it was concluded that the series of phenomena must have been instituted somehow and at some time or times (sudden creation is no doctrine of natural theology) under intelligence. How is this presumption negatived or impaired by the supposition of Darwin's theory, that the ancestors were not always like the offspring, but differed from time to time in small particulars, yet so as always to be in compatible relations to the environment? We do not see how or why the inference, which was so cogent, should under the new showing become at once irrelevant and out of all logical connection with the facts of the case, which *quoad design* are just what they were. *Suddenness*—if that must needs be entertained—is of course incompatible with the Darwinian view, and also with the facts as we understand them; but *gradualness* is in nowise incompatible with design. Under the conception of Nature as the outcome of Divine intelligence, questions of time and mode, of generality and particularity, are well nigh devoid of real significance.

But what may be contended for, and what is probably meant, is that natural selection is a rival hypothesis to design, that it accounts for all adaptations in the organic world upon known physical principles, and so renders the idea of design superfluous, as some would say; or, as it is better stated by Dr. Romanes, renders the evidence of design from these adaptations of no other or better value than that from anything else in Nature. So that the argument from teleology "must now take its stand upon the broader basis of the order of nature as a whole." This last, sensible natural theologians are prepared for. But the whole is made up of parts; and it is a whole in which the designed (if such there be) and the contingent can never be accurately discriminated, in which, indeed, from the very nature of the case, limitation is inconceivable. This need not be wondered at, since we are equally unable to discriminate the two in human

action. The evidence of design may be irresistible in cases where we cannot indicate its limits. We can only infer with greater or less probability, according to circumstances, and especially according to relation to ends. Better evidence than that of exquisite adaptation of means to ends is seldom, if ever, obtainable of human intention, and in the nature of the case it is the only kind of evidence which is scientifically available in regard to superhuman intention. Now if means and ends are predicable of inorganic nature at all, it is only by remote and indirect implication; while in organic nature the inference is direct and unavoidable. With what propriety, then, can it be affirmed that organic nature furnishes no other and no better evidence of underlying intelligence than inorganic nature? The evidence is certainly *other*, and to our thinking *better*.

To make the contrary supposition tenable, it must be shown that natural selection scientifically accounts for the adaptation; that the survival of only the very best adapted, out of the brood of more or less adapted to the environment at the time, gives sufficient scientific explanation of the adaptability or actual adaptation of the organism. Certainly this has not yet been done, and it seems incredible that it ever will be. That organisms have undergone changes as the Darwinian theory predicates, and that these changes have been picked out and led on by natural selection, seems to me most probable. That the action of the environment in some wholly unexplained way induces organisms to movement and change which would not otherwise occur, is also probable; but such change appears to be a response of the organisms to the physical surroundings and stimuli. And this most important factor in the result receives no explanation from the natural selection which operates upon it or co-operates with it. In other words, real causes have been assigned under which, *given the requisite changes*, the actual diversity and adaptations of plants and animals must or may have come to pass. But none have been assigned under which the organisms *must* have responded in the ways they do, or have responded at all, to the influences of the environment. Yet this is the very gist of the matter. The whole tenor of Darwin's writings and many explicit statements assure us that he completely recognised this distinction, which less exact minds overlook. If this distinction is valid, then the conclusion is at least premature which affirms "that the argument from teleology has been dislodged by the theory of natural selection," and its special value, as derived from adaptations in organic nature, utterly and for ever destroyed."

ASA GRAY

Cambridge, U.S.

Intelligence in Animals

MR. ROMANES remarks in his book that there are few recorded instances of intelligence in bears; the following facts may therefore be worth recording:—In the Clifton Zoological Gardens there are two female Polar bears between two and a half and three years old, which came here quite young. One of these shows remarkable intelligence in cracking cocoa-nuts. A nut was thrown to-day into the tank; it sank a long way, and the bear waited quietly till after some time it rose a little out of her reach. She then made a current in the water with her paw, and thus brought it within reach. This habit has already been several times noticed in Polar bears. She then took it on shore, and tried to break it by leaning her weight on it with one paw. Failing in this, she took the nut between her fore-paws, raised herself on her hind-legs to her full height, and threw the nut forwards against the bars of the den, three or four feet off. She then again leant her weight on it, hoping she had cracked it; but failed again. She then repeated the process, this time successfully. The keeper told me she employed the same method to break the leg-bone of a horse. That this is the result of individual experience, and not of instinct, is clear from the fact that her companion has not learnt the trick of opening them thus, nor could this one do it when she first came. The method of throwing is precisely similar to that adopted by the Cebus monkey described by Mr. Romanes.

J. G. GRENFELL

Clifton College, Clifton, Bristol, January 15

On a Relation existing between the Latent Heats, Specific Heats, and Relative Volumes of Volatile Bodies

As I do not find that the following relation between the latent heat of evaporation, the expansion undergone in changing into the gaseous state, and the specific heat of a volatile body has been previously pointed out; and as, if verified, it might be of some value in the determination of one or other of the above quantities I submit it, not however without considerable diffidence, to the readers of NATURE.

Briefly stated the relation stands thus—

The latent heat of gasification at constant pressure of any body, divided by the product of the relative volume of the gas and the specific heat of the body is approximately constant; or, if

λ = latent heat of gasification of any body,

v = relative volume of the gas; *i.e.* the vol. of the body on assuming the gaseous state compared with its vol. as a liquid,

s = specific heat of the body. Then

$$\frac{\lambda}{v \times s} = \text{const.}$$

The calculated value of this constant approximates to 0·8, as will be seen in the following table.

The letters λ, v, and s, heading columns 2, 3, and 4, have the same signification as above.

	λ.	v.	s.	$\frac{\lambda}{v \times s}$
Ether	91·11	228	·515	·775
Carbon disulphide	86·67	414	·235	·890
Wood spirit	263·7	651	·645	·628
Bromine	45·9	510	·107	·841
Oil of turpentine	68·73	204	·410	·837
Formic acid	169·0	548	·536	·575
Ethyl acetate	92·6	209	·527	·840
Methyl acetate	110·2	321	·507	·677
Butyric acid	114·67	271	·503	·841
Ethyl formate	105·3	241	·513	·851
Amyl alcohol	121·0	268	·587	·967
Acetone	129·7	339	·530	·736
Alcohol	208·0	456	·547	·833
Benzene	91·47	282	·395	·821
Chloroform	61·0	318	·232	·828
Perchloride of carbon.	47·0	263	·198	·902
Phosphorus trichloride	51.42	311	·209	·790
Methyl butyrate	87·33	273	·487	·657
Ethyl chloride	93·0	320	·427	·679
Ethyl iodide	46·87	317	·162	·914
Acetic acid	102·0	515	·503[1]	·393
Chloride of arsenic	46·5	324	·176	·813
Tetrachloride of tin	30·53	237	·148	·869
Water	537·0	1612	1·000	·333

It would appear then that the latent heat of a body may be considered as approximately proportional to the expansion of the body in vaporising and to its specific heat; and that the amount of heat required to convert a unit mass of the body at the boiling point from the liquid to the gaseous state, is equal to an amount of heat which would raise through one degree a quantity of the body in the liquid state which is approximately proportional to the expansion undergone by the liquid on evaporating.

It will be noticed that among the bodies instanced in the table there are some which appear to be very far indeed from according with the relationship in question. Notably acetic acid and water; of these, however, water presents so many peculiarities that perhaps it may be allowable to consider this as only adding one more to their number. In the case of acetic acid it is noteworthy that in plotting the curve of the latent heats of the group of acids of which acetic acid is a member, Favre and Silbermann found an irregularity arising from this body. It is, at any rate, possible that this irregularity may mean an error in the determination of the latent heat of this body.

Considering the difficulties which attend the accurate determination of latent heats, relative volumes, and specific heats of the several bodies, and that, of course, an error in any one of these will introduce inaccuracies into the constant, it may well be supposed that some, at least, of the variations noticeable in the results tabulated arise from inaccurate data. Further, there are in many cases two or more distinct determinations of these physical properties extant, of which one might be so selected in each case as to reduce the variations occurring in the constant to a minimum.

F. TROUTON

Trinity College, Dublin

[1] Varies with temperature of determination irregularly.

The Gresham Funds

IN an account of a meeting of the Common Council of the City of London, held last week, I read in the *Times* that the

Gresham Committee reported that they had agreed with the Mercers' Company upon a scheme by which the open area of the Royal Exchange should be roofed over at a cost of 10,000*l*. Does this mean that the funds of the Gresham Estate are particularly flourishing just now, or that they are to be burdened with a new liability which will indefinitely postpone the time when there may be a surplus to be devoted to that advancement of science which Sir Thomas Gresham had in view in forming Gresham College? It is not long since some of the bonds, which represent money borrowed on the Gresham Estate for the building of the Royal Exchange, were advertised for repayment out of its surplus annual income, which afforded a hope that a good time for the scientific part of Sir Thomas Gresham's bequest might be drawing nigh. The public would be glad to know whether this hope is to be falsified or not. W. B.

January 20

Siwalik Carnivora

MAY I ask space to thank your correspondents for their answer to my previous inquiries concerning collections of Siwalik fossils in England, (many of which I have not yet had an opportunity of visiting,) and to add that I am now about undertaking the description of Siwalik Carnivora for the Indian Government? All remains of this order are very scarce, and in general fragmentary, and every specimen is, therefore, important. If any specimens exist in any provincial collections, I should be very glad of any information regarding them, and if possible of the opportunity of describing them in my forthcoming memoir. Any specimens sent to me, to the care of Dr. H. Woodward, F.R.S., British Museum (Natural History), Cromwell Road, S.W., will be thankfully received, and duly returned after comparison and description, if necessary. RICHARD LYDEKKER

The Lodge, Harpenden, Herts, January 17

Earthquakes

EARTHQUAKE phenomena are extremely rare in this highly favoured part of the world; but we had a very decided shake near the close of the year 1882. It occurred last night (Sunday, December 31) at about five minutes past ten o'clock, Halifax time, as nearly as can be determined at present. My observation was made at Lucyfield, ten miles north from the city of Halifax; the house stands on a rounded hill formed of unaltered drift, overlying slate rock, and at an elevation of about 350 feet above sea-level.

The air was perfectly still. There was a sudden rumble as of heavy waggons on a hard road at some little distance, then the sound became louder, I may say deafening, as of heavy loaded waggons running close to the wall; or of a heavy railway train running through a reverberating cutting; then the noise seemed overhead as if caused by rolling heavy furniture on the upper floor; there was a slight vibration of the building, as if something large and heavy had struck the roof, icicles fell from the eaves, fragments of plaster fell down behind the lathing of the walls, and there was a sound like a sudden gust of wind upon the windows and walls outside (there was no wind however). Suddenly noise and vibration ceased, and all was perfectly still. Passing outside, to look for some cause for these remarkable phenomena, nothing particular was noticeable. The country was covered with a thick white mantle of snow, the air was perfectly calm, there had been no rain drops, nor hail, but a faint flash of lightning (unaccompanied by thunder) occurred about a minute and a half or two minutes after cessation of the shock.

For two or three days prior to the shock, the temperature did not fluctuate much. The thermometer stood at zero centigrade (32° F.) at sunset on the 31st and has not varied much since; it was within a degree of the same at sunset of the previous day, but went down at night, rising again in the morning. During the day (31st) the sky was clear with some light fleecy clouds, wind northerly, but veered round to south-west about sunset, and the sky became overcast with clouds; later the clouds seemed to clear away, but the air became foggy, and was so at the time of the shock. (About Truro I am informed the sky was "clear and starry.") The air had been in a highly electric state during the afternoon and evening.

The earthquake shock lasted, as nearly as I can compute from recalling circumstances, something less than a minute, certainly more than half a minute, but probably not more than a whole one. I cannot indicate with any degree of certainty the direction of oscillation; so far as a retrospect of circumstances and sensations indicate, the apparent movement was from south-west to north-east.

Most persons in the city of Halifax to whom I have spoken to day observed the shock more or less distinctly, but it does not appear to have been nearly as violent in the city as in some other places. I ascertained from the conductor of the morning railway train that the shock was felt more or less severely all along the journey traversed by his train this morning, viz. from Truro to Halifax, a distance of sixty-one miles. At Shubenacadie, nearly thirty miles from my point of observation, flower-pots in the railway station house were toppled over on the window-sill and rolled upon the floor.

I have jotted down these particulars, thinking they may possibly prove of some interest if compared with the observations of others at different points. GEORGE LAWSON

Dalhousie College, Halifax, Nova Scotia, January 1

A SHOCK of earthquake was felt in this district on Tuesday, January 16, about 5 p.m. Comparatively few persons perceived it, but to those who did it was a striking phenomenon. The following report has been handed to me by a trustworthy observer:—

"About 5 p.m. on Tuesday, January 16, I was standing in a room, leaning against the foot of an iron bedstead, and facing a window, in front of which, on a table, was a cage containing one of the small African parrakeets known as love birds. The room was perfectly quiet, when this bird, which had settled itself for the night, surprised me by craning out its neck and flattening its plumage with every appearance of alarm, without any sound or movement on my part, or anything in the room which could possibly have frightened it. Immediately afterwards the iron bedstead I was leaning against, as well as the floor, trembled sufficiently to make me wonder what on earth was going on, especially as I heard no sound sufficient to account for it. The trembling ceased in a few seconds, and, while I was still wondering, returned in a greater degree than before, lasting this time about five seconds. The feeling I experienced was similar to that of standing on a bridge while a load was passing over. The second time I speedily came to the conclusion that it was caused by an earthquake." GEORGE F. BURDER

Clifton, January 20

I SHALL feel obliged if you will put on record in your columns that an earthquake was felt at Hastings by my sister and myself in separate rooms, on Tuesday morning last, the 16th inst., at 9½ minutes past 9 a.m. The undulations were between E.S.E. and W.N.W., and lasted about 4 seconds.

R. H. TIDDEMAN

H.M. Geological Survey, 28, Jermyn Street, S.W., Jan. 20

The Sea Serpent

BELLEVING it to be desirable that every well-authenticated observation indicating the existence of large sea serpents should be permanently registered, I send you the following particulars.

About three p.m. on Sunday, September 3, 1882, a party of gentlemen and ladies were standing at the northern extremity of Llandudno pier, looking towards the open sea, when an unusual object was observed in the water near to the Little Orme's Head,



travelling rapidly westwards towards the Great Orme. It appeared to be just outside the mouth of the bay, and would therefore be about a mile distant from the observers. It was watched for about two minutes, and in that interval it traversed about half the width of the bay, and then suddenly disappeared. The bay is two miles wide, and therefore the object, whatever it was, must have travelled at the rate of thirty miles an hour. It is estimated to have been fully as long as a large steamer, say 200 feet; the rapidity of its motion was particularly remarked as being greater than that of any ordinary vessel. The colour

appeared to be black, and the motion either corkscrew like or snake-like, with v rtical undulations. Three of the observers have since made sketches from memory, quite independently of the impression left on their minds, and on comparing these sketches, which slightly varied, they have agreed to sanction the accompanying outline as representing as nearly as possible the object which they saw. The party consisted of W. Barfoot, J.P. of Leicester, F. J. Marlow, solicitor, of Manchester, Mrs. Marlow, and several others. They discard the theories of birds or porpoises as not accounting for this particular phenomenon.

F. T. MOTT

Birstal Hill, Leicester, January 16

A Novel Experiment in Complementary Colours

THE old maxim of an *adjacent gray* in order to give visibility to a complementary colour seems to hold its ground. Mr. Charles T. Whitmell puts it very clearly when he alludes to "the advantage of a reduction of brightness to a level comparable with that of the existing colour."

Mr. Whitmell will find, I think, that this brightness may be still further reduced below the level of the existing colour. This may be shown by one or two remarkable experiments with light admitted through a small needle hole the one-fiftieth of an inch in diameter, made through the bottom of a half ounce pill-box painted inside with lampblack. On placing a sheet of white paper on the table at night in a room lighted with ordinary gas, and looking through the small hole with one eye, *both eyes being open*, he will see on the paper a disc of a beautiful cobalt *blue* colour, evidently the complementary of the yellow light of the gas. On examining the sky in the same way in the morning, there will be seen, especially if the weather is dull and hazy, as it has been of late, a disc of a *primrose yellow* colour, the complementary of the blue sky, which, although invisible, is still making its impression on the sensitive retina. Later on in the day, between five and six o'clock, when the weather is murky, the disc has a well-marked *pink* colour, the atmosphere being evidently tinged with dark green. These several results I have witnessed from day to day for the last fortnight, and they have been verified by others to whom I have shown them. But when the sky is very blue and clear, there is seen, for obvious reasons, a blue disc only.

In the above experiments there is the curious anomaly of having one eye impressed with the exciting colour, the other with its complementary.

JOHN GORHAM

Bordyke Lodge, Tunbridge, January 20

The Projection of the Nasal Bones in Man and the Ape

IN my letter in the last number of NATURE (p. 266) the walls of the human nose were carelessly ascribed to the elevation of the *pre*-maxillary bones. This is not the case. It is only in the ape and lower animals that the ascending processes of the pre-maxillary assist in forming the external nose-case, or muzzle, *above* the nostrils. The frame-work of the nose in the lower types of the negro seems, therefore, in this respect, to differ more from the nose-case of the ape, than it does owing to any great development of the nasal bones.

I take this opportunity to mention that the woodcut of the embryo, which I referred to, appeared first in Quain's "Anatomy." Also, the quotation about the nasal bones of the orang, I have since found from my notes, to have been derived from Prof. Mivart's "Man and the Ape."

January 22

J. PARK HARRISON

HOVERING OF BIRDS

THIS problem, to account for the phenomenon of the motionless hovering of hawks and other birds in mid-air, was the subject of correspondence in NATURE, vol. viii. pp. 86, 324, 362; vol. ix. p. 5; vol. x. pp. 147, 262: vol. xi. p. 364. The only plausible explanation advanced (by Joseph Le Conte, vol. ix. p. 5, and previously by the present writer, vol. viii. p. 362) was that the birds take advantage of slant upward currents of wind sufficiently strong to neutralise the force of gravity. But the arguments brought forward in support of this explanation were perhaps not quite conclusive, for lack of a sufficient series of observations.

During the past six years I have noted such instances as I have chanced to witness in the course of a wandering occupation, and now offer the results as a further contribution towards the solution of the question.

I may state at once that in every case where I have seen a bird hovering, the following three conditions have obtained:—

(1) There was a fresh wind blowing.

(2) The bird was facing the wind.

(3) Beneath the bird there was a steep slope of ground facing the wind.

The particular localities in which I have observed the phenomenon are the following:—

(1) 1877, *September* 17.—Driving from Aberayron to Llanrhystyd (Cardiganshire). Wind W.N.W., moderate. Cliffs facing N.W. Gulls under cliff top, below road, in poise. Hawk under hill top, above road, in perfect poise.

(2) 1877, *October* 13.—Approaching Llantrisant town (Glamorganshire) from Llantrisant Junction. Wind S.W., moderate. Hawk over S.W. slope, barely poising, partly fluttering, tail plainly brushed up.

(3) 1877, *October* 14.—Llantrisant (Glamorganshire). Wind S.S.W. Rooks upborne, above S.S.W. slopes of hill with entrenched fort (Caerau).

(4) 1877, *October* 20.—Cliff facing S. between Longland and Caswell Bays, Gower (Glamorganshire). Gull and crows upborne. Wind moderate, S.S.W.

(5) 1877, *October* 21.—Cefn Bryn, Gower (Glamorganshire), facing S.S.W., climbing from Reynoldston. Rooks upborne. Wind strong, S. by W.

(6) 1879, *October* 17.—On road from Llantrisant to Pontypridd (Glamorganshire). Wind W. Rooks upborne over slopes facing W.

(7). *October* 28.—Killay, near Swansea (Glamorganshire). Hawk poised above hill-side facing N.E., to the west of Killay railway station. Wind N.E. I was almost under the bird, and could see the conduct of wings and tail suiting the ripples of wind.

(8) 1879, *November* 5.—Near Merthyr Tydfil (Glamorganshire). Hawk poised over N. slope of hill above (to S. of) tunnel on Merthyr-Abernant Railway. Wind N.

(9) 1880, *March* 13.—Near Penally (Pembrokeshire). Sea-gulls, rooks, and jackdaws upborne and floating with wings outstretched all along cliff line facing S., between Penally and Lydstep. Wind S., full on cliff from the sea. Gulls up to 200 feet above cliff edge. At greater height and inland, they were flapping. Different behaviour of rooks over inland northern slope.

Further on, over caves at north end of Lydstep Sands, hawk poised for 1 min. and 1½ min. at a time, just over cliff line, in teeth of wind off sea.

(10) 1880, *March* 17.—Near Cardiff (Glamorganshire). Hawk poised about 10 or 12 feet above railway embankment facing E.N.E. (20 or 25 feet high) of Llandaff and Penarth line, near Ely Station. Wind E.

(11) 1880, *March* 27.—Gulls uplifted over E. scarp of Beachy Head Down (Sussex). Wind E.N.E.

(Same day).—Over N.E. slope of Lighthouse Down. Bevy of eight gulls, all in perfect poise, immediately over edge of cliff.

(12) 1880, *August* 8.—Wells next-the-sea (Norfolk). Wind N.W. Hawks poising over W. slope of sea-wall, and over N.W. slope of sand-hills (projecting from the main line of dunes that runs east and west), and trying unsuccessfully over railway embankment which runs N.W. and S.E.

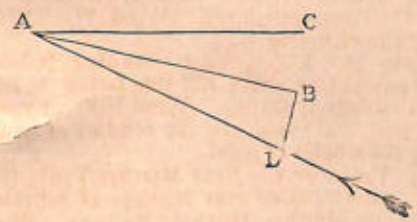
In several of the instances here recorded I was near enough to see that the bird was delicately adapting the slope and spread of its wings to the momentarily varying inclination and force of the wind. Among the sand-hillocks near Wells, on the Norfolk coast, I succeeded in approaching, under cover of ridges and long grass, within about ten yards of a hovering hawk, and saw the

posture of the bird very well for a few seconds, till he became aware of my presence and dashed away. I was much struck by those instances in which the obstacle that caused the upward slant of the wind was only a sea-wall or a railway embankment, and especially by the critical case (No. 12) where the bird was evidently baffled because the wind lay along the embankment, not against it, and therefore gave no upward current.

My list includes four cases (3, 4, 5, 6) of rooks and gulls "up-borne" on outspread wings, under conditions similar to those present in hovering—cases that could not be explained by any theory of *vis viva*, but clearly involved an external mechanical force, which could only be that of the wind, sustaining and uplifting the birds. The close relation between the "up-borne" and the "hovering" action was evident in case (9) where the gulls, &c., were up-borne and sailing, while the hawk was poised and motionless.

These observations, as far as they go, appear to indicate plainly the law which governs the phenomenon in question. I think they strongly confirm the theory already advanced, that the bird in hovering is upheld by a slant upward current of air. A strong wind pressing against a slope of ground is necessarily thrown into a slant upward current, "as slopes a wild brook o'er a hidden stone." There may be a downward eddy if the slope is precipitous, as one may often feel at the foot of a high wall, but the main stream of the air for some considerable height above the slope is forced to take an upward slant, with increased velocity, in order to surmount the obstacle in its path.

Given such a slant upward current, it is easy to see



that a bird, with the exquisite muscular sense that every act of flight demands and denotes, might so adapt the balance of its body and the slope of its wing-surface to the wind as to remain motionless in relation to the earth. The slope of the wing-surface should divide the angle between the horizontal and the direction of the slant wind-current in such proportion that, if the air were at rest, the bird, under the action of gravity, would float forwards, downwards, on outspread wings, with exactly the same velocity as that of the wind (in which it remains motionless) and in exactly the opposite direction. The mechanical conditions are identical in the two cases, whether we consider the air at rest and the bird floating through it, or the bird at rest and the wind rushing under it. In either case the bird has reached, and maintains, its maximum velocity, due to gravity, compatible with the resistance of the air, which resistance is the same in both cases.

I have heard it objected that the mechanical conditions are not the same in these two cases, because in the one case the bird has momentum, in the other not. Need it be said that momentum is a purely relative possession, just as velocity is? In each case the bird has the same velocity, and therefore the same momentum, relative to the air. The mechanics of the situation, as between bird and air, are not affected by the possession or loss of velocity (and with it momentum) relative to the earth.

Perhaps the feasibility of the thing may be best shown by a simple diagram. Let A B represent the slope of the bird's wing (viewed from the right side), dividing the angle between the horizontal, A C, and the direction of the wind, D A. Draw B D at right angles to A B, and take A D to represent the force of the wind. Then D B and B A will represent the force of the wind resolved perpendicular and parallel to the slope of the wing A B. The resolved part, B A, meeting only the resistance of the bird's head and shoulders and front edge of the wings, tends not strongly to push the bird in the direction, B A, that is, backwards and a little upwards. But the resolved part, D B, which meets the full area of the outspread wings and tail, tends powerfully to push the bird in the direction D B, that is, upwards and a little forwards. Then all that is required to keep the bird at rest is that the effect of the forward force exerted by D B should balance the effect of the backward force exerted by B A (both being resolved vertically and horizontally), and that the great upward force exerted by D B, together with the small upward force exerted by B A should exactly neutralise the downward force of gravity.

The only difficulty in the way of the slant-upward-current theory lies in the statement of the Duke of Argyll (NATURE, vol. x. p. 262) that "a hundred times" he has seen birds hovering "when by no possibility could any upward deflection of the wind have arisen from the configuration of the ground." My own observations testify so consistently in favour of slant upward currents that I feel justified in asking for more precise information concerning the instances alluded to by the Duke of Argyll, before relinquishing the theory which I hold. Wherever I have seen a hawk trying to remain in one position over a plain or slightly undulating ground, the feat has only been accomplished by continued vibration of the wings.

The problem of the "soaring" of birds introduces other conditions, which require separate consideration, though I believe it will be found that the two phenomena of "soaring" and "hovering" depend upon essentially similar causes.

(By the bye, does not the provincial name of one of the hawks, the "Windhover" record the constantly observed dependence of the act of hovering on the wind?)

HUBERT AIRY

THE LATE EDWARD B. TAWNEY

BY the death of this young naturalist English geology has lost one of its most enthusiastic and cultivated students. Hardly beyond the threshold of his career, he had already gained for himself a notable place among the geologists of this country, and his friends augured for him a future of distinction and usefulness. But in the fulness of his promise and in the midst of his work he has been struck down so suddenly that few of his friends knew he had been ailing until they were shocked and saddened by the news of his death.

Born in 1841, he was the third child of the Rev. Richard Tawney, Vicar of Willoughby, Warwickshire, who had gained a distinguished place at Rugby, and had been a Fellow of Magdalen College. On the death of his father, young Tawney was placed under the care of his guardian, Dr. Bernard of Clifton, and received his early education there. During these years he seems to have acquired a bent towards natural science mainly through the influence of Dr. Bernard and Dr. Fox of Brislington. He was eventually enabled to gratify this inclination by attending the courses of instruction at the Royal School of Mines, Jermyn Street, from 1860-63, where he greatly distinguished himself. He gained a Royal Scholarship, Duke of Cornwall's Scholarship, the De la Beche Medal for Mining, and the Edward Forbes Medal for Natural Science, and took Associate's diplomas in the Mining and Geological divisions.

With the training in scientific methods thus obtained, he soon betook himself to original research, gaining experience by excursions at home and by travel abroad. In 1872 he accepted the offer of Assistant Curator of the

Bristol Museum. With characteristic energy he at once set to work, re-tableting, re-arranging, and naming the geological collection, taking care to have gaps in the series filled up, and making the museum really serviceable for purposes of instruction. Six years later, in the early part of 1878, he received the appointment of Assistant Curator of the Woodwardian Museum, Cambridge. He soon made his mark there, as was acknowledged in the following year by the bestowal upon him of the honorary M.A. degree. His indefatigable industry and wide range of acquirements so peculiarly fitted him for this position, that his death must for some time to come be an almost irreparable loss to the University.

Looking over his published papers one cannot but be struck with his versatility. At one time we find him discussing the Rhoetic beds of South Wales, at another dealing with that vexed question of Alpine geology—the position of *Terebratula diphya*. From Devonian fossils he passes to the description of new species of Oolitic gasteropods, or to the Cretaceous Aporrhaidæ, or to Palæozoic star-fishes. He could enter minutely into the stratigraphy of the Isle of Wight Tertiary strata, and with not less energy and clearness of insight described the microscopic structure of the crystalline rocks of Wales. Well versed in the Continental languages, he kept himself abreast of the foreign progress of his favourite science. Nor were his tastes wholly scientific. He delighted in Piers Ploughman and the Niebelungenlied. What he might have done who may guess? That with his feebleness of constitution he should have been able to accomplish so much, shows how ardent was his love of nature and how indomitable his spirit of inquiry. His devotion to truth and abhorrence of everything savouring of insincerity or sham led him to speak out freely and uncompromisingly. But no one could mistake the honesty of his purpose. A. G.

REMARKS ON AND OBSERVATIONS OF THE METEORIC AURORAL PHENOMENON OF NOVEMBER 17, 1882

THE interesting meteoric phenomenon seen in England during the aurora of November 17 last, has induced me to endeavour to find the true path of that object. Though I have spent much time in applying the method given by Prof. E. Heis in his "Periodischen Sternschnuppen," I have got no farther than the point to which Mr. H. D. Taylor has brought us, the observations being in no way capable of combinating. In fact, when seeking the lines of intersection, formed by the different planes of the great circles, wherein the apparent path was seen, with the mean horizon (say the plane of a common map), these lines have but little tendency to converge to the same point. Therefore the method of Mr. Taylor seems to me the most convenient. When the object has followed a straight line, all the places where it was seen passing just before the moon, must lie in a plane containing the true path and the moon. This plane must cut the plane of the map in a straight line. Now the four places where observers saw the meteor before the moon's disc are:—Woodbridge, near Ipswich, Lincoln's Inn Fields (London), Windsor, and Ramsbury, near Hungerford, fulfilling, by no means, the above-mentioned condition. Nevertheless the most probable direction of this line seems to be that accepted by Mr. Taylor, N.E. by E.-S.W. by W. (astronomical), because this is the general direction of the lines of section, given by the great circles, mentioned above. Here it is to be remarked that when the meteor was seen from S.E. to S.W. (as in the case at York), but at some height (here 10°) above the horizon, the intersections of the apparent path with the horizon may lie near E. and W. (here, according to the observation of the meteor passing 6° below the moon, at 12° south of E.).[1] We give here bearings as seen from the different places, taken directly from the communications, or deduced indirectly from them:—

Street (3° south of Leeds)	S.E.-S.W.
Clifton (Bristol)	E. 18° N.-W. 18° S.
Greenwich	E.N.E.-(?)
Guildown (p. 149)	E.-W. (nearly)
Bedford	S.E.-S.W.
Clevedon (p. 100)	N. 70° E. (?)-S. 70° W.
Cambridge	E.-S.S.W.
York (H. D. Taylor)	E. 12° S.-W. 12° N.
Woodbridge	E. 10° N.-W. 10° S.
Windsor	E.-W.
Coopers Hill	?-S.W.
Ramsbury	?
Lincoln's Inn Fields	E.-W.

Now we can add to these English observations[1] two others made in the Netherlands.

1. Prof T. A. C. Oudemans gives in the *Utrecht Newspaper* (No. 318) the following (translated) description:—"At 6h. 23m. (6h. 2½m. Greenw. T.) a feather-like appearance, resembling in the beginning a brilliant comet, formed suddenly in the eastern part of the heavens, the end being just before Aldebaran. Within two minutes this feather had prolonged itself above Saturn, through the Pegasus quadrate, and south of the three Eagle-stars, the east or following end shortening, while the other or preceding end advanced. . . . When this arch had obtained the length of 90° (which lasted but a few seconds) a separation was made in the middle of its length, where the arch had a breadth of about 3°. This separation had a length of about 10° and a breadth of ½°, and was pointed at the ends. At 6h. 25m. this arch disappeared wholly in the west." Prof. Oudemans says further that the great circle of the apparent path intersected the equator at 110° and 290° of right ascension. This gives me, combined with the position of Aldebaran, a direction in the horizon of E. 20° N.-W. 20° S.

2. Mr. P. Zeeman observed the same phenomenon at at Zonnemaire, near Zierikzee (51° 42′ lat. and 57′ W. Amsterdam). He wrote me the following on November 19 and 24:—"About 6h. 20m. (I saw) a magnificent, splendid white arch, beginning a little north of east, and prolonging itself to south-west, but in the meantime shortening at the east end and disappearing in a very short time." Mr. Zeeman declares in his second letter that this arch went through Aldebaran, and through α Pegasi. This gives me a horizontal bearing of E. 20° N.-W. 20° S., as the observations of Prof. Oudemans gives also.

Thus we find these two Dutch observations (unhappily the sky in Groningen had just, at 6h. 1m. Greenw. T., got cloudy, the aurora being very splendid before) supplement and confirm the greater part of the English observations. Only the phenomenon seems to have been of greater apparent size, and therefore nearer to the observer. The separation by an obscure streak seems not to have been visible in England, perhaps from the change of its relative position.

The conclusion to which we come after all, regretting earnestly the want of French observations, is that we have here probably a meteoric object, moving, according to the calculations of Mr. H. D. Taylor (vol. xxvii. p. 140), with great velocity through the upper strata of the atmosphere and at the same time of auroral character, as the spectrum observation of Mr. Rand Capron (vol. xxvii. p. 84), makes out beyond any doubt. The separation and the feather-like forms, observed at Utrecht, make it probable that it was a mass of meteoric dust, passing through our atmosphere like an accumulation of little shooting stars. In this way the phenomenon of November 17 brings a confirmation of my own theory of auroræ, proposed by me in the "Appendice alle Mémorie della Società degli Spettroscopisti Italiani," 1878, vol. ii., and received with sympathy

[1] Will Mr. W. M. Flinders Petrie be so kind as to tell us where we can find the Swedish observation mentioned by him, vol. xxvii. p. 140?

by many of the German and Dutch astronomers; but as it seems little known in England, though referred to by Mr. Rand Capron on p. 64 of his beautiful work, "Auroræ." In this theory most of the properties of auroræ are deduced from cosmic dust entering into the atmosphere of the earth. I take the liberty to direct attention to the unexpected argument, that the brilliant object of November 17, 1882, has brought forth in favour of my "Théorie Cosmique," to which I had already the opportunity to refer in this Journal in my article "On Dust, Fogs, and Clouds" (vol. xxiii. p. 195).

Furthermore, I think that this object is not the only example of such a phenomenon. On November 2, 1871, there was seen in Groningen and several places of Germany a strange, brilliant arch, striped parallel to its well defined sides and changing its curve during its two hours of existence. The beginning of the phenomenon (of which I gave a description in the Dutch journal *Isis*) was seen by a student, Mr. Gratama, like an elliptic patch of light round the Pleiades. Dr. Vogel, who observed the same arch at Bothkamp, determined its auroral character by the spectrum. Otherwise it resembled very much the bright spur of a gigantic meteor or fireball. Also it disappeared slowly, beginning at the east end, as the illustration shows. A faint aurora, with dark segment, was visible in the north. The height of this arch was calculated by me approximately at 127 kilometres or 79 miles. I think that the only difference between these two feather-like phenomena of November 2, 1871, and of November 17, 1882, consisted in the different apparent velocity and in the greater mass of meteoric dust, forming in the case of November 17, 1882, but a short, and in that of November 2, 1871, a very long train of incandescent matter. It must be remembered here



Auroral Arch, observed November 2, 1871, at Groningen (Netherlands).

that the tails of great fireballs remain visible for half an hour or more (see *e.g.* the article of Mr. Branfill, vol. xxvii. p. 149). In NATURE, vol. xii. p. 330, is to be found a description of similar arches, seen at Fremantle in Australia by Mr. Lefroy, in presence of the moon, which was obscured by one of them.

This leads us to a question, touched by Mr. Backhouse, NATURE, vol. xxvii. p. 198, that of the halos seen in Siberia (by Von Wrangel, I believe), when an auroral beam was in front of the moon. I watched in vain if such an event should perhaps occur November 17 last, but Mr. Zeeman, whom I have cited above, seems to have been so happy as to have seen a white and bright auroral cloud floating over the moon's disc at 5h. 47 (local time), giving the common interference phenomena. It is unnecessary to remark, that these phenomena can be formed by all kinds of dust, formed of nearly equal particles, and that they in no way require ice-particles. On my inquiry why the observer could decide that it was not a common cloud, he brought forward the following arguments:—(1) Its great brightness; (2) its transparency to the starlight; (3) its very great velocity, unusual in common clouds.

Returning to the meteoric phenomena, visible simultaneously with auroræ, it seems that such phenomena were seen during the marvellous aurora of January 7, 1831, described in *Poggendorff's Annalen* of the same year. We read (p. 440) that Bergrath Senff, in Colberg, at 6.30 o'clock, saw above the west horizon a bright yellow streak, rising upward with a common cloud-velocity, passing at 30° N. Zen. D., and forming an arch from W. to E., beginning to disappear from the west end, almost at the same moment that it reached the east horizon. At p. 458 we see that Prof. Rudberg, at Upsala,

December 7, 1830, saw a very bright patch of double the dimensions of the moon's disc, moving with great velocity behind the common auroral beams. Further, Prof. Bischoff, in Burgbrohl (p. 461), observed, on the occasion of the aurora of January 7, 1831, a moving cloud as bright as the milky way, from E. to W., in five minutes. Prof. Moll saw, in Utrecht a similar object, rising from N.E., through the Pleiades, to S.E. (S.W.?). Similar observations are to be found during the same aurora, p. 471 (one advancing arch), p. 472 (four similar arches, and a dark streak).

In several articles on the aurora of November 17, 1882, the height of auroræ is spoken of. Mr. W. M. F. P. (p. 173) says that the strange object observed is *physically impossible* to auroral nature, because of its height of about 170 miles. It was already observed by Mr. Backhouse that auroræ are often observed at very great heights. The same is also the case with shooting stars. I take the liberty to refer once again to an article of mine in this journal entitled "The Height of the Aurora," where I refer to the beautiful determinations by Prof. Heis and Dr. Flögel, published in the *Zeitschrift der Oesterr. Gesellsch. f. Meteor.*, vii. p. 73. The heights were found from 10 to 100 geogr. miles (46 to 461 Engl. miles). Dr. Sophus Tromholt found, besides apparent low heights of some auroræ in Norway, the height of that of March 17, 1880, to be 17 geogr. miles ("Wochenschrift redigirt," von Dr. H. J. K'ein, 1880, p. 172). Prof. Galle of Breslau calculated by his method, described in the *Zeitschr. f. Met.*, vii. p. 73, and in the *Astr. Nachrichten*, Bd. 79, No. 1882, 40 to 60 geogr. miles, and I found for the great aurora of May 13, 1862, 59 geogr. miles.

H. J. H. GRONEMAN

Groningen (Netherlands), January 14

NOTES

THE Council of the Institution of Civil Engineers have arranged for the delivery at the Institution of a series of six lectures, on the Applications of Electricity, on the following Thursday evenings, at 8 o'clock:—February 15—The Progress of Telegraphy, by Mr. W. H. Preece, F.R.S., M.Inst. C.E. March 1—Telephones, by Sir Frederick Bramwell, F.R.S., V.P.Inst., C.E. March 15—The Electrical Transmission and Storage of Power, by Dr. C. William Siemens, F.R.S., M.Inst. C.E. April 5—Some Points in Electric Lighting, by Dr. J. Hopkinson, F.R.S., M.Inst. C.E. April 19—Electricity applied to Explosive Purposes, by Prof. F. A. Abel, C.B., F.R.S., Hon. M.Inst. C.E. May 3—Electrical Units of Measurement, by Sir W. Thomson, F.R.S., M.Inst. C.E. This is an excellent step which the enterprising Institution has taken, and we are sure will be productive of good both to science and to engineering.

MR. ERNEST H. GLAISHER, B.A., Trinity College, Cambridge, has been appointed Curator of the British Guiana Museum, George Town, Demerara.

MR. W. H. WHITE, one of the Chief Constructors to the Navy, has resigned his position to take up a managerial appointment in the firm of Sir Joseph Whitworth.

AN interesting boring through the chalk is now about to be resumed at Southampton. At the last meeting of the British Association a paper by Mr. T. W. Shore and Mr. E. Westlake on the Artesian well on Southampton Common was read in the Geological Section. The Town Council has now accepted a tender for continuing the boring which was abandoned in 1851, after a depth of 1317 feet had been reached. The boring was then passing through the lower chalk or chalk marl, and we believe it is now intended to continue it to the Lower Greensand. The well at the bottom of which the boring commences is 563 feet deep, and this was reopened last week, after having been closed for thirty-two years. Some observations on the temperature of the water were at once made by Mr. T. W. Shore and Mr. J. Blount Thomas, of Southampton, for the Underground Temperature Committee of the British Association. By means of a heavy elongated sinking weight and a registering windlass, a thermometer was passed down the bore shaft to a depth of 1210 feet, when it was stopped by chalk mud. An outer case which was attached to the sinking weight was much scratched in passing through the Upper Chalk. A temperature of 71°·9 F. was registered at the bottom, that of the outer air being 49° F.

THE City of Neuchâtel celebrated in the beginning of the present month the fiftieth anniversary of the foundation of its Natural History Society. The leader among its founders, who first met for the purpose on December 6, 1832, was Louis Agassiz.

THE biennial Hunterian oration will be delivered on Wednesday, February 14, at three o'clock, by the President of the College of Surgeons, Mr. Spencer Wells, in the theatre of that institution. The biennial festival will be given in the library the same evening, to which the president and vice-presidents have, as usual, invited several distinguished visitors.

THE Pontifical Academy of the Nuovi Lincei have appointed a Committee to take steps for the erection of a monument in Rome to the late eminent astronomer, Father Secchi. The monument will be of a meteorological character. The sculptor Prinzi has already made a model which combines convenience for arranging the meteorological apparatus with features recalling the work of Father Secchi. The statue of the astronomer crowns the monument, and among other emblematical figures will be one of Meteorology holding in one hand a gigantic barometer, which can be seen from a great distance, and another of Physics holding up to view an equally large thermometer.

THE rumour that the fragments of the unfortunate Mr. Powell's balloon have been found in the Sierra del Pedroso, in the far south of Spain, is too vague and incredible to deserve much attention.

AT the meeting of the Essex Field Club, to be held on Saturday evening next, January 27, the attention of the members and the public generally will be directed to the Bill about to be introduced into Parliament for the construction of a line of railway from Chingford to High Beach. In January, 1881, the Club, in conjunction with other Natural History Societies in and around London, strongly protested against any portion of Epping Forest being occupied by a Railway or other Company, to the prejudice of the provisions of the Epping Forest Act, and certainly no sufficient arguments or expressions of public opinion have since been brought forward in favour of the scheme. It is believed that the proposed line is quite unnecessary, as no part of the forest is more than two miles from a railway station, and moreover a railway and its concomitants could not fail to destroy the chief interest and charm of the district—its seclusion and naturalness; qualities of inestimable value so near a large city.

THE following papers are set down for reading at the meetings of the Society of Arts during the part of the Session after Christmas:—At the Ordinary Meetings—W. K. Burton, The Sanitary Inspection of Houses; General Rundall, The Suez Canal; Prof. Thorold Rogers, M.P., Ensilage in the United States; Sir Frederick Bramwell, F.R.S., Some Points in the Practice of the American Patent Office; J. H. Evans, The Modern Lathe; A. J. Hipkins, The History of the Pianoforte; Prof. George Forbes, The Electrical Transmission of Power;

D. Pidgeon, Recent Improvements in Agricultural Machinery; Wilfred Cripps, F.S.A., English and Foreign Silver Work, with some Remarks on Hall-marking. In the Foreign and Colonial Section—Edmond O'Donovan, Life among the Turkoman Nomads; Rev. J. Peill, "Social Conditions and Prospects in Madagascar; Robert W. Felkin, Egypt: Present and to Come; W. Delisle Hay, Social and Commercial Aspects of New Zealand. In the Applied Chemistry and Physics Section—C. F. Cross, F.C.S., Technical Aspects of Lignification; Walter G. McMillan, F.C.S., Chemical Means for Preventing or Extinguishing Fires; W. N. Hartley, F.R.S.E., Self-purification of River Waters; R. W. Atkinson, B.Sc., The Formation of Diastase from Grain by Moulds; James J. Dobbie, D.Sc., and John Hutchinson, On the Application of Electrolysis to Bleaching and Printing. In the Indian Section—Charles H. Lepper, Overland Commercial Communication between India and China, *vid* Assam; W. S. Seton-Karr, Agriculture in Lower Bengal, with some Notice of Tenant Right, &c.; J. M. Maclean, Private Enterprise in India; C. Purdon Clarke, Some Notes on the Domestic Architecture of India.

We have received from Egypt a publication of some interest in the shape of the *Bulletin* of the Chemical Laboratory at Cairo, directed by M. Albert Ismalun. The laboratory is under the Department of Public Works, and judging from the report in the *Bulletin* is doing a fair amount of useful work. The laboratory has been recently much improved, and attached is a museum of specimens in geology, palæontology, and zoology.

A considerable number of names has been added to the list of those who are unfavourable to the meeting of the British Association in Canada in 1884. The request of the protesters to the Council seems to us quite reasonable,—"that it is highly desirable that you should take some further steps in order to ascertain the general feeling of the members of the Association upon the subject, before allowing our kind and liberal friends in Canada to incur any further trouble or expense." There are 141 names appended to the circular, and while some of them are well known, still we note the absence of some of the leading representatives of English science.

The proceedings at the meeting of the Association for the Improvement of Geometrical Teaching were not carried out quite on the lines laid down in a recent number of NATURE (vol. xxvii. p. 247). In consequence of a delay in the delivery of the copies of "the Elements of Plane Geometry," the President was obliged to defer the moving of his resolution till the next meeting, which, it is hoped, will be held about Easter next. The members were also informed that Mr. Levett had, in answer to an appeal made to him, consented to retain office as Hon. Secretary for the present year. Mr. E. B. Sargant, Trinity College, Cambridge, was elected to act as joint secretary with Mr. Levett. The following members were elected: Miss Burstall, Professors G. C. Foster, F.R.S., W. H. H. Hudson, H. Lamb, and G. M. Minchin, Rev. A. Jamson Smith, and Messrs. G. Griffith, E. B. Sargant, Charles Smith, F. Turner, and H. H. Turner.

Dr. Peverati, director of the Meteorological Observatory of Cassine, states that an earthquake shock was felt there on January 16, at 7.42 a.m. (Roman time). The shock was undulating, preceded by a rumbling noise in the direction W.E., and lasted three-quarters of a second. The accompanying noise is compared to that of a very heavy body in motion in contact with another body at rest. The shock is classified as No. 3 in the scale of intensity proposed by Dr. Forel. A similar shock was felt the same day at Demonte (Cuneo) at 5.25 a.m., moving in a west-south direction. On the night of the 14-15th, several shocks were felt at Terranova and Pollino in Basilicata. Twenty-two shocks of earthquake were felt on January 16, at Centi, in the province of Murcia, Spain. Several houses were destroyed, but the inmates escaped unhurt. There was no loss of life.

It is announced from Mexico, January 23, that a new comet near Jupiter has been discovered at the Puebla Observatory.

A new electrical paper *Electricity*, has issued its first number at Buda-Pesth. It is written in the Magyar language. The first paper of this description ever published was called *Les Archives de l'Electricité*, and was published by M. de la Rive at Geneva in 1840, and the first issued in England was edited by the late Mr. Walker in 1843, under the title, *Electrical Magazine*. None of these papers lasted for more than three or four years.

The Algerian Government is preparing an expedition for next spring, in order to protect effectually the southern part of the province of Oran against incursions of the surrounding independent tribes. The results of this expedition are not without interest for English journals, many of which are printed on paper made from alfa, a plant cultivated in those remote regions, and manufactured in England. A curious fact is, that no French paper-maker ever attempts to manufacture alfa for inland consumption.

The oases of the Beni-mzab Confederacy to the south of Algeria, have been annexed to the French Algerian possessions, and a military expedition has established a regular administration in the country. The Algerian section of the French Alpine Club is organising a scientific expedition which will leave shortly in order to take advantage of a favourable season for travelling. Any one wishing to take part in this excursion should communicate with M. Durando, president of the Algerian section. The newly-annexed oases are seven in number, with a population estimated at 40,000, with about 200,000 palm-trees under cultivation. The ruins of several large towns have been covered by sand.

Two of the most important scientific expeditions which attempted to get into the Siberian seas last year were those in the *Dijmphna*, with Lieut. Hovgaard bent on reaching the North Pole, and the Dutch Meteorological Expedition in the *Varna*, bound for Port Dickson. These two vessels succeeded in forcing the ice in the Waigatz Straits in September last, and perhaps the *Dijmphna* would then have got through the Kara Sea, had she not, by mistaking certain signals, been led to leave the open "lead" in which she was, and gone to the assistance of the steamer *Louise*, beset by the ice. She was caught in the pack, as the *Varna* had previously been, and was frozen in on September 17. The last report which we possess from these vessels, is dated September 22, and was brought to Europe by Capt. Dallmann of the *Louise*. Since that date no news whatever has come to hand from the vessels, and the statements which have appeared in the Russian press relating to the discovery by Samoyedes of a wreck, supposed to have been that of the *Dijmphna*, south of Waigatz Island, have been proved to refer to an old Russian whaler, stranded there some years ago. Although the expedition, if it had met with any mishap, would undoubtedly have found its way to the mouth of the Petchora, of which we should have had information before now, it has been decided by the Danish Government to send out a search expedition, under Capt. Norman, from Siberia, in case the *Dijmphna* should be in want of anything. On the other hand the Swedish-Norwegian Consul at Arkangelsk reports under date of December 13, that fishermen who had visited Waigatz Island in November last, had not seen any vessel near that island. In the last message received from Lieut. Hovgaard he expressed the opinion that the ice in the Kara Sea would

break up during the periodical storms in September and October, and enable him to reach Port Dickson, where he intended to winter. If to this is added the statements made by Mr. Leigh Smith and Sir Henry Gore Booth, as to open water north and east of Novaya Zemlya during the summer, it is not improbable that the *Dijmphna* has got free in October, and safely reached Port Dickson, or, perhaps, even Port Aktinia on the Taimur Island. Should this be the case, Lieut. Hovgaard has, no doubt, despatched a messenger to the nearest habitation, viz. Goltochicha, and thence by express to Jeniseisk, and we may therefore look forward to reassuring news from the gallant Danish explorer at the end of January or early in February.

THE results of a fourth years' observations of periodic movements of the ground as indicated by spirit levels at Secheron, are given by M. Ph. Plantamour in the *Archives des Sciences* of December 15. The curves obtained from the east-west spirit level, for the four years, are strikingly similar in the manner (pretty regular generally) in which they follow the thermal oscillations of the air. Different years show a notable difference in the epoch of maximum descent of the east side relatively to the minimum of mean temperature, and maximum rise of the same side relatively to the maximum of temperature. One is led to consider the maximum and minimum of temperature rather as accidents as regards the epoch at which they occur, and to attribute a preponderant influence to the distribution of mean temperatures during the four months November-February, and the four June-September. Probably, too, the degree of moisture influences largely the rapidity with which the deeper ground layers are affected by exterior temperature. The curve for the north-south level is also very similar to the previous ones; but has this peculiarity, that while the south side follows, in general, from October 1 to the end of September, the oscillations of external temperature (descending in winter and rising in summer) the intermediate variations of temperature have an inverse effect. The cause is at present unknown. Col. van Orff's observations at Bogenhausen reveal oscillations of the ground similar to those at Secheron, only with greater amplitude south-north, and less east-west. M. Plantamour regrets that, excepting Col. van Orff and M. d'Abbadie, no one, so far as he knows, has undertaken observations of the kind at any other station. They are easily made, and should yield important results.

NEARLY thirty years ago, Poggendorff described a "fall-machine" of his invention. Its merits, according to Herr Bauer, who spoke warmly in commendation of it at a recent meeting of philologists and schoolmasters in Karlsruhe (*Wied. Ann.*, No. 13), appear to have been somewhat overlooked. Few physical cabinets have it, and the only text-book in which Herr Bauer has found it described is that of Reis. We may state that the two pulleys over which the cord runs are at the middle and one end of a balance beam, a weight being hung from the other end (with which, and a running weight, the beam can be rendered horizontal). The machine is supplied by Herr Karl Sickler in Karlsruhe.

THE *Nation* states that from Mr. Agassiz's annual report on the condition of the Museum of Comparative Zoology it learns that it is his intention, in connection with Prof. Faxon and Dr. Mark, to issue in the Museum Memoirs a "Selection from Embryological Monographs," containing quarto illustrations derived from innumerable scientific transactions and periodicals, and serving as an atlas for any text-book on embryology. By the purchase of the large Schary collection of Bohemian Silurian fossils, and by its own rich amassing in the West and South-west during the year, the museum now contains one of the finest collections of palæozoic fossil invertebrates in existence. Mr. Samuel Garman, whose explorations for mammalian remains in the Western Territories were very successful, was led to believe from the mode of their accumulation that the cause of extinction of the more recent was "a very severe winter, much more extensive and severe" than the occasional blizzards of our time. "As if from freezing, the shafts of the larger bones are generally splintered."

WE have received the *Annuaires* of the Bureau des Longitudes and of Montsouris Observatory, both of them abounding with useful information on many subjects. Gauthier-Villars is the publisher.

THE additions to the Zoological Society's Gardens during the past week include two Macaque Monkeys (*Macacus cynomolgus* ♂ ♂) from India, presented by Mr. J. Steel; a Black-footed Penguin (*Spheniscus demersus*) from South Africa, presented by Mr. John Wormald; a West Indian Rail (*Aramides cayennensis*) from West Indies, presented by Mr. E. H. Blomefield; an Orange-winged Dove (*Leptoptila ochroptera*) from Brazil, presented by Mr. C. A. Craven, C.M.Z.S.; a Long-eared Owl (*Asio otus*), British), presented by Mr. — Dyer; a Great Barbet (*Megalæma virens*), a Silky Starling (*Sturnus sericeus*), two Grey Thrushes (*Turdus cardis*), twelve Red-sided Tits (*Parus varius*) from Japan, a Crested Grebe (*Podiceps cristatus*), four Razorbills (*Alca torda*), a Red-throated Diver (*Colymbus septentrionalis*), British, purchased.

OUR ASTRONOMICAL COLUMN

THE GREAT COMET OF 1882.—The first determination of elliptical elements of this comet by Mr. S. C. Chandler, of Harvard Observatory, U.S., assigned a period of revolution of about 4000 years. Later investigations have diminished this period very considerably, though the length of revolution is not determined within narrow limits. Prof. Frisby, of Washington, employing observations on September 19, October 8, and November 24, gives a period of 794 years, and finds a close agreement between the position indicated by his orbit and the Cape ante-perihelion observation of September 8. Dr. Kreutz, of Berlin, using chiefly meridian observations or normal places from September 8 to November 14, gives 843 years, and finds a pretty close accordance with observation throughout this interval, thus showing no very material perturbation at perihelion passage. Further, Dr. Morrison, of Washington, founding his calculation upon positions for September 19, October 8, and December 11, finds a period of 652½ years. But while these later computations favour a shorter revolution than was at first attributed to the comet, there remains to be ascertained to what extent the abnormal form of the nucleus since the end of September has affected the observations, and hence the deduced elements of the orbit, and a much more complete discussion of the observations than has yet been attempted, when details respecting the point of the comet observed are before us, may be required before confidence can be placed in the result of any calculation. *Primâ facie* the elements assigned by Dr. Kreutz look satisfactory enough.

In view of a possible period of seven or eight hundred years, attention may be again directed to the comet of 1106, which had several characteristics favourable to identity, though the statement in several of the chronicles (chiefly English) that at the latter part of its appearance it was seen between the north and east, is not reconcilable therewith. Pingré, advocating the identity of the comet of 1106 with the great comet of 1680 (in which he followed Halley) rather questioned the authority of the *Chronicon Alberici, Trium-fontium Monachi*, that the tail extended "below the constellation Orion," which might have been the case if the comet were identical with either of the comets of 1843, 1880, or 1882; he remarked, the monk was "ni contemporain, ni exact, ni judicieux." This subject will deserve further attention, and we may return to it shortly.

THE WASHINGTON OBSERVATORY, U.S.—The first volume of "Publications of the Washington Observatory of the University of Wisconsin" has just been issued by the director, Prof. E. S. Holden, and augurs most favourably for the reputation of this institution, which was founded within the last five years through the liberality and scientific spirit of a private individual, the Hon. C. C. Washburn. The volume is especially

valuable, from its containing a large number of measures of double stars made by Mr. S. W. Burnham during his temporary connection with the Observatory, from April 23 to September 30, 1881. The measures are contained in three catalogues: (1) a list of sixty new double stars discovered in the zone observations, chiefly by Prof. Holden; (2) a list of eighty-eight new double stars discovered and measured by Mr. Burnham; and (3) measures by the same eminent observer of 150 double stars from his manuscript general catalogue. A number of difficult objects are included in the third series. 8 Sextantis was not elongated with the highest powers at the epoch 1881·34, nor did γ Coronæ show any sign of duplicity; as Mr. Burnham remarks, "it has been apparently single with all apertures since about 1871." Among the more difficult binary stars there are measures of O.Σ. 235, Σ 3123, 42 Comæ, Σ 2173, β Delphini, δ Equulei, and 85 Pegasi. There are also positions and descriptions of eighty-four red stars, of which twenty-seven are stated to be new, and a list of new nebulæ and clusters discovered in the zone-observations at the Washburn observatory.

The position of the observatory is in latitude 43° 4′ 36″·6 N., and longitude 89° 24′ 28″·3 west of Greenwich. Prof. Watson, of Ann Arbor, Michigan, was first appointed to the superintendence, but at his premature death in November, 1880, he had not been able to commence astronomical observations. Prof. Holden gives some account of the preparations he was making for scientific activity, as the only way of associating his name with the observatory.

CHEMICAL NOTES

Herr Lellmann (*Berichte*, xv. 2835) describes an interesting case of *physical isomerism*. Dibenzyl-diamido-dibromdiphenyl melts at 195°; if the liquid so produced is quickly cooled, the solid now melts at 99°, but on heating again solidifies at 125° to 130°, and melts a second time at 195°; if the substance melting at 195° be slowly cooled and then again heated, the melting point now observed is 195°.

Berthelot and Vielle (*Compt. rend.* xcv. 129) sum up the results of their researches regarding explosive waves. The propagation of an explosive wave occurs when the ignited stratum of gas exerts the maximum pressure on the adjacent stratum; increase of pressure is accompanied by increased velocity of propagation. To produce an explosive wave it is necessary that a considerable mass of gas should be employed and that the cooling by radiation and conduction should not be great; if the temperature fall below 1700°-2000°, or if the volume of the products of combustion is less than one-fourth, or in some cases one-third of the total volume of the final mixture, the propagation of the wave ceases.

According to the experiments of M. Corne (*J. Pharm. Chim.*, [5] vi. 17) the glowing of phosphorus is due to volatilisation of the phosphorus and subsequent production of ozone by electrical energy generated by the volatilisation of the phosphorus. Phosphorus does not glow in pure oxygen at high pressures because, says M. Corne, volatilisation is impeded and at a certain limit becomes too slow to ozonise the oxygen. Gases which hinder the formation of ozone also prevent phosphorescence.

Baeyer (*Berichte*, xv. 2856) has obtained nearly pure indigo-blue by acting on a solution in acetone of ortho-nitro-benzaldehyde. Acetone and nitrobenzaldehyde react to form a condensation product, $C_{10}H_{11}NO_4$, from which alkali withdraws the elements of acetic acid with production of indigo-blue, thus—

$$2C_{10}H_{11}NO_4+2H_2O=C_{16}H_{10}N_2O_2+2C_2H_4O_2+4H_2O.$$

J. Horbaczewski (*Berichte*, xv. 2678) has obtained uric acid by heating together glycocoll and urea to 200°-230°; details of the reaction are promised.

The working of the Food Adulteration Act for the year 1881 is considered in a Report of the Local Government Board lately issued in a Blue Book, and published in *The Analyst* (vii. 218). The total number of districts in which analysts were acting at the close of December 1881, was 260; during the year 17,823 samples were analysed, of which 2613, equal to 14·7 per cent. were reported as adulterated; in 1877, 14,706 samples were analysed, and 19·2 per cent. reported as adulterated. More than a third of the samples analysed, and more than a half of those reported against, were of milk. Birmingham still "maintains the distinction which it has for some years enjoyed, of having a larger proportion of its milk reported as adulterated than any other great town in the kingdom." The adulteration of bread and of butter seems to be steadily on the decrease; in coffee the proportion of adulteration is rather less than last year; chicory is still the commonly used adulterant. The adulteration of sugar is practically a thing of the past. More than one-fourth of all the samples of spirits examined were reported as adulterated, chiefly with water: a good deal of gin is sold containing not much more than 20 per cent. of alcohol.

Prof. Hofmann describes in the *Berichte* (xv. 2656) a number of interesting lecture experiments. To determine that no loss of matter occurs during combustion, he employs a two-litre flask fitted with a cork carrying a small manometer, a glass tube with stopcock, and a straight piece of rather wide tubing to the under-end of which a small porcelain crucible is attached. The wide tube is closed by a cork, about half a gram of dry phosphorus is placed in the little crucible, a portion of the air in the flask is pumped out, and the flask—and also a little bit of stout copper wire—is counterpoised; the little bit of copper wire is heated and dropped down the wide tube, from which the cork is withdrawn for a moment; the phosphorus is thus ignited; after the combustion, which proceeds slowly, is completed, the flask is found to weigh the same as before; the stopcock is now opened, air rushes in, and the flask now weighs more than it did at the beginning of the experiment.

To illustrate the great difference between the volumes of equal weights of liquid and gaseous water, Dr. Hofmann employs a glass bulb of about 300 c.c. capacity, with a narrow glass tube at each end, the upper tube being fitted with a stopcock. This apparatus is supported so that the lower tube reaches to about 1 centim. from the surface of the mercury in a basin; a rapid current of steam is passed into the apparatus; after five minutes or so, when every trace of air is expelled, the stopcock is closed, and at the same moment the lower tube is pushed beneath the mercury, which at once begins to rise into the bulb; after a little time the bulb is almost filled with mercury, on the surface of which the condensed water appears as a thin layer.

Very simple apparatuses are also described for containing considerable quantities of liquefied gases: *e.g.* SO_2: for exhibiting *quantitatively* the reactions on which the manufacture of sulphuric acid is based; and for demonstrating the law of Dalong and Petit. For descriptions of these, and for other experiments, reference must be made to the original paper.

THE HYPOTHESIS OF ACCELERATED DEVELOPMENT BY PRIMOGENITURE, AND ITS PLACE IN THE THEORY OF EVOLUTION[1]

II.

The problem before which we are here placed may be formulated as follows:—How is it, that while the tendency to vary which obtains in all organised beings, and which forms one of the foundation stones of the theory of evolution, how is it that this tendency has exerted upon a number of living beings a so much less considerable influence than upon others, so that even in the present day numerous representatives are found of the most primitive animal groups which belong to the oldest known in the geological succession?

Still more, why are there certain genera which, since the Silurian period, appear to have undergone a stagnation in their development, in their advance towards higher differentiation, whereas within a much shorter period the whole of the living mammalian fauna has developed out of more primitive vertebrates and the important modifications have taken place among these mammalia which have finally led to the appearance of the elephant on the one hand, and of the shrews on the other?

In other words, can it be assumed that this tendency to vary could be totally and persistently neutralised by other causes amongst whole series of living beings during thousands of years, whereas during the same number of years this tendency, aided by natural selection, could lead other series of animals along roads where they have advanced with gigantic strides?

I need not remind you that this objection against the theory of

[1] By Prof. A. A. W. Hubrecht. Inaugural Address delivered in the University of Utrecht, September, 1882. Continued from page 281.

evolution, which has also been felt and combated by Darwin, was very often advanced against it, especially in the beginning. Cuvier had already reminded Lamarck that the absolute identity between the Egyptian animals, as they were embalmed three thousand years ago, with those inhabiting the same provinces in the present day, rendered untenable his ideas about gradual change and perfection of organic beings.

Huxley, to whose close reasoning powers and untiring readiness for battle the rapid progress of evolution is in a great measure due, has devoted several pages to the refutation of this objection. His argument runs as follows [1]:—

The two chief factors in the process of evolution are: the one the tendency to vary, the existence of which in all living forms may be proved by observation; the other, the influence of surrounding conditions, both upon the parent form and upon the variations which are evolved from it. Now, as often as the first factor makes itself felt, and modified forms take their origin out of a common parent form, it will depend entirely on the conditions which give rise to the struggle for existence, whether the variations which are produced shall survive and supplant the parent, or whether the parent form shall survive and supplant the variations. If the surrounding conditions are such that the parent-form is more competent to deal with them and flourish in them, than the derived forms, then, in the struggle for existence, the parent-form will maintain itself, and the derived forms will be exterminated. But if, on the contrary, the conditions are such as to be more favourable to a derived than to a parent-form, the parent-form will be extirpated, and the derived form will take its place. In the first place there will be no progression, no change of structure through any imaginable series of ages; in the second place there will be modification and change of form.

So far Huxley. No doubt but he has made us acquainted with a very reliable explanation of how the variation of any form of animals or plants may be retarded. The hypothesis of degeneration first formulated by Anton Dohrn, and afterwards warmly advocated by Ray Lankester, is no doubt of considerable importance for our comprehension of numerous lower stages of organisation in the animal and vegetable world, which may no longer be looked upon as parent-forms of more highly differentiated groups, but which, on the contrary, have in their lineage much more complicated ancestors than their own stage of organisation would appear to show. At first sight these degenerated animals show different points of similarity with animal forms, lower than those to which they are genetically allied.

So, for example, the Tunicata have for a long time been arranged amongst or close to the Mollusca, but lately-continued researches have evermore tended towards the conclusion that we have here before us the degenerate descendant of animals which had already attained the level of the lowest Vertebrates, but whose descendants, thanks to degeneration, have at present all the appearance of Invertebrates. In this way the number of lower animal types which may be looked upon as primitive, and whose persistence through geological periods gives rise to the questions as formulated above, is deceptively increased by forms, which we must remove from amongst them, and place in the vicinity of their more direct allies.

The process of degeneration is, however, confined within certain limits; it cannot do the same service towards the refutation of the objection here dealt with as can Huxley's argumentation above referred to, which is fully directed against the cardinal point, and the value of which I cannot estimate highly enough.

Still it appears to me that his explanation of the lengthened persistence of so many of the lower organised animals and plants can yet be supplemented by a new hypothesis.

To this I give the name of the hypothesis of accelerated development by primogeniture. If I have the advantage to lay it before you to-day, you will bear in mind that it has as yet only a preliminary shape, and that for its ultimate confirmation extensive researches will yet be required.

The fact is daily confirmed by continuous observation, that not only numerous vertebrates, but also very many invertebrates, can attain a very old age without the capacity for reproduction being essentially diminished. This is confirmed by the recently published researches of Weissmann [2] on the connection between the length of the reproductive period and the duration of life. We may fairly assume that all those animals attaining an old age leave issue which has been born at different periods—issue from a youthful age, which itself has again brought forth children and grandchildren, and issue from old age, which is on a level with the fourth or fifth generation of the first-born descendants. An example of old age combined with successful attempts towards reproduction is furnished by the well known sea-anemone, "Granny," which was captured in 1828 by Dalyell on the Scotch coast, and being still alive, last year gave birth to a certain number of young Actiniæ.

The large Tridacnas and the gigantic Cephalopods which have now and then been observed, must also have attained a considerable age; nothing authorises us to maintain that these have been infertile in all the later years of their lives. We need not stop to consider the higher groups: fishes, birds, and mammalia. They all contribute during a shorter or longer time towards the procreation of the species, and the considerable age which both fishes and birds are known to attain is the cause of a very considerable difference in age of the oldest and the youngest individuals of their own breeding. And so all of them will leave both first-born and last-born posterity. With the first-born this will in their turn be the case, so with *their* posterity, and so forth. Similarly the last-born, when they have attained maturity, will bring forth a series of descendants of very different ages; the last-born of the last-born being the final term of this series.

After centuries the effect will be this: From one pair of parents a large number of descendants will have sprung, a small number of these being the descendants in a direct line of the first-born of every successive generation; another small number being the descendants in a direct line of the last-born of every successive generation, whereas the remainder belong to intermediate stages. The first-born are separated from the primitive parent form by a number of generations, x, which is necessarily a considerable multiple of the number of generations y, which lies between the same parent form and their last-born descendants. Evidently the difference in age between the first-born descendant and his parents is a minimum, for the sole reason of his being the first-born, that between the last-born descendant and these same parents being on similar grounds a maximum. Thus, if we follow up in the direct line of descent the series of first-born of the first-born, &c., we find that the distance between two terms of that series corresponds to a much smaller number of years than the distance between two terms of the series of the continually last-born, which have always descended from last-born.

Comparing these two series simultaneously after the lapse of centuries, the series of the first-born will count numerous terms, many generations, at short distances from each other, whereas the series of the last-born will, on the contrary, consist of a much smaller number of terms, each of which is separated from its predecessor by a much more considerable distance. It is the number of these terms which in the one case I wished to express by x, in the other by y.[1]

From this fact we are led to propose the following question: Is there any reason to expect, that in the struggle for existence, the representatives of each of the two divergent series are collectively provided with different weapons? Or are both these groups quite equal to each other in the struggle?

Both observation and theoretical deduction force the conclusion upon us that a difference is indeed present. A difference, (1) in the external circumstances under which the first-born and the last-born come into existence; (2) in the internal properties and acquirements with which both series are provided; a difference which does not appear sporadically between certain representatives of both groups, but which may indeed be collectively observed between all of them.

As to the first point, the external circumstances, I call your attention to the following example, which shows how nature indeed makes a difference on a large scale in the conditions—under which she awaits the first-born and the last-born progeniture.

[1] American Addresses, p. 39

[2] A. Weissmann, "Ueber die Dauer des Lebens," 1881.

[1] I am doubtful whether there are indeed *first-born descendants* in the pure signification of the word, *i.e.* such which, both from the paternal and from the maternal side, count only first-born in the whole of their ancestry. However, this does not materially influence our argument. We bring together in the series of first-born all those descendants in which mixture and intercrossing with second and third births was always reduced to a minimum, whereas on the other hand, in the group of the last-born, not only those cases which are theoretically pure are brought together, but those in which the number of ancestors on both sides most closely approaches to the number of generations y, which lies between the last born *in ab-tracto* and the common parent form. In the majority of cases, however, intercrossing and blending will have occurred on a large scale, and the average number of generations which leads from them to this parent form may be expressed by $\frac{x+y}{2}$. The calculus of probabilities would be able to furnish us in any given case, supposing enough data are available, with the exact grouping of these numbers.

From the observations which Livingston Stone has made in 1878 in the North American institutions for fish-culture on the McCloud River, it follows that 14,000,000 eggs obtained from ripe but relatively young and smaller salmon were without exception at least one third smaller than the millions of eggs which were before obtained from older, larger salmon of the same species, and that nevertheless they developed quite normally. By these observations the fact is established that the salmon, when older, lays larger eggs than at a more youthful age, and this, more especially, is of great value for our hypothesis. Firstly, the size of the egg must influence the chances which they have for escaping or falling a prey to different voracious animals. In this respect the smaller eggs are exposed to other dangers than the larger ones. Furthermore, the relative size of the egg will, without doubt, exert a certain influence—however insignificant—upon the individual which is developed out of it.

In comparison with the larger egg of the older salmon, either the food-yolk or the formative yolk in the smaller one will be of smaller dimensions, or both together will have been reduced in size. In each of these three cases, even in the last-named, the conditions under which the smaller egg (that is to say, the whole generation of the first-born) attains its development, differ from those of the generation issued from the larger eggs, the generation of the last-born. The first-born will either be of smaller size, or because they possess a smaller food-yolk they will have to provide their own nourishment at an earlier date; or both circumstances are combined.

Nobody will deny that in each of these cases natural selection can freely come into play. In addition to this it must be remarked that however insignificant this difference in external circumstances may be its presence is nevertheless undeniable, since it reappears again with unerring certainty in every successive generation. In this way the effect can gradually accumulate, and finally the path may have been entered upon which leads to a specific differentiation of the descendants of the first and of the last born.

This having taught us that indeed the external circumstances which preside at the birth and at the growth of the first and the last born are different (at least for this species of salmon, reliable observations on a similar scale concerning other animals being for the present wanting), I must now call your attention to the second cardinal point, viz., that the internal properties and acquirements with which each of the two series of births is provided, are also different. Heredity has indeed invested them with peculiarities, part of which show themselves in their organisation, another part remaining latent, and only attaining development in following generations. Such a latent potential tendency towards eventual modification of the individual or his progeny, must needs find more numerous occasions to unfold itself in the first born, simply because these are possessed of a larger number of ancestors. On the contrary those that have a smaller number of ancestors, *i.e.* the last born, have had this occasion for development offered to them at rarer intervals. From this it follows that further modifications under the influence of natural selection will be started by preference in the different series of first born, because *ceteris paribus*, there are here more chances for the appearance of small deviations, which to a certain extent are always due to reversion to the parent forms.

And so there is reason to suppose that also the internal properties of the series of first born differ from those of the last born, in the same way as we have just defined it for external agencies. In my opinion the difference in internal structure is of greater consequence than that in external agencies, although we must at the same time acknowledge that our present methods do not allow us to test this experimentally. Only by extensive and long-continued experiments more light will be thrown on this subject. The example which was mentioned of the seventy-years-old sea anemone, which reproduced itself successfully proves that the material for similar experiments is not deficient. In the vegetable kingdom forms will certainly be hit upon which will fully reward the difficulties of the experiment.

Once a new species, modified and generally higher-differentiated, having arisen out of the first-born by gradual accumulation of the small deviations, intercrossing and bastardising with the last-born descendants of the parent form, becomes rarer, copulation taking place by preference with specimens of the same species, and only exceptionally with representatives of the species which has lagged behind in its development. For this new species the same process sets in; here, too, the first born progeniture will surpass in the course of years the last-born, and will in its turn give rise to new modifications. And so *ad infinitum*.

We now come to another important point, which is in direct connection with the question, which are the last-, which the first-born. With most lower animals—Protozoa, Cœlenterata, Echinoderms, Worms—reproduction by fission is very common by the side of reproduction. Cut arms of starfishes grow to be complete starfishes after having passed the so-called "comet" stage; certain annelids divide themselves after one of the posterior body-segments have become converted into a head; certain Nemertines break themselves into pieces under spasmodic contractions, each fragment being able to reproduce both head and tail; Amœbæ divide themselves into halves.

Now it cannot be admitted that in fissiparous reproduction, heredity can come into play in the same measure as it can in the case of sexual reproduction. It is not even possible to determine which of the two halves represents the older generation. Weissmann has lately humorously said: if we fancy an Amœba gifted with consciousness, she will think upon dividing into two, "I now bring forth a child," and there is no doubt that each half would look upon the other as the child, and upon itself as the mother. Weissmann has thus introduced the idea of the (approximate) immortality of the Protozoa, an idea which can also be adduced in favour of the hypothesis here maintained, and which at all events deserves to be mentioned by the side of the hypothesis proclaimed by Hæckel and others, viz. that the Monera living in the present day are in no genetical connection with older ancestors from earlier periods, but have come into existence by the aid of repeated spontaneous generation.[1]

The same views hold good for the self-division of worms and Cœlenterata. Here too both parts are the direct continuation of a single individual which, although dividing, does not cease to exist. Coral reefs which principally multiply by division may be looked upon in the same way.

Never, in case of fissiparous reproduction, does that mysterious potentiation take place which brings together in the egg-cell and in the spermatazoön, not only the characteristic properties of father and mother, but of whole series of ancestors; never in this case can the special process of fixation of a part of these latent forces, the process which we term *heredity*, take place to its full extent. Never can selection during embryonic and larval life, which, according to recent researches, plays a much more conspicuous part than was originally expected, favour the stability of a variation, and thus lead to modification of the species, where multiplication by division takes place.

In his chapter on pangenesis ("Origin of Species," second edition, pp. 353 and 390) Darwin too touches upon this subject, and insists upon the fact that organisms produced asexually, consequently not passing through the earlier phases of development, "will therefore not be exposed *at that period of life when structure is most readily modified* to the various causes inducing variability in the same manner as are embryos and young larval forms."

The series of generations which owe their origin to a-sexual and not to sexual reproduction, are thus in a much lesser degree liable to vary.[2] And yet a variation of some sort must always first occur, in order that natural selection, acting upon it, may finally produce a definite modification of the species. Nevertheless, fissiparous multiplication continues to play—and has always played—a very important part in the invertebrate kingdom, *by the side* of sexual reproduction. Thus the presumption is allowed, that where in the course of centuries a-sexual reproduction has been more predominant than sexual reproduction, a stagnation in development has resulted, the differentiation of those series of individuals and genera which have originated through sexual reproduction, in the meantime always continuing its regular course onwards.

Both factors—the retardation of development by a-sexual reproduction, and the acceleration of the development of the always first-born, make it very probable, in my opinion, that we have to look upon the more highly-developed groups of animals, and amongst these upon their higher-differentiated representatives, as forms which are separated from the original parent stock by a maximal number of ancestors, the number of times that a-sexual

[1] Lamarck had already, by this same assumption, attempted to overcome the difficulty.

[2] Observation tends to confirm this in a general way (*vide* Darwin, *l.c.*, p. 353.

reproduction has taken place in their ancestry being at the same time reduced to a minimum.

On the contrary, we must expect that a much smaller number of ancestors lies between the lower-developed groups and the common parent form, that a-sexual reproduction has here more repeatedly occurred, and that finally, Darwin's and Huxley's explanation, which we have above alluded to, of the non-occurrence of further modifications, may here have been realised to a greater extent.

Keeping in view the combined action of both these principles, we no longer wonder that even in the present day living representatives are found of genera which were already present in the Silurian epoch, nor that the simplest organised beings have continued to exist in that primitive form.

They are for the greater part the younger sons, and being condemned to a slower rate of development, they could not keep apace of their elder brothers. The latter, which have so much oftener passed through the improving crucible of sexual reproduction, are indebted to that cause for having become the parent stock out of which the higher and highest-developed animal and vegetable forms, now surrounding us, have gradually sprung.

THE ETHER AND ITS FUNCTIONS[1]

I HOPE that no one has been misled by an error in the printing of the title of this lecture, viz. the omission of the definite article before the word ether, into supposing that I am going to discourse on chemistry and the latest anæsthetic; you will have understood, I hope, that "ether" meant *the* ether, and that the ether is the hypothetical medium which is supposed to fill otherwise empty space.

The idea of an ether is by no means a new one. As soon as a notion of the enormous extent of space had been grasped, by means of astronomical discoveries, the question presented itself to men's minds, what was in this space? was it full, or was it empty? and the question was differently answered by different metaphysicians. Some felt that a vacuum was so abhorrent a thing that it could not by any possibility exist anywhere, but that nature would not be satisfied unless space were perfectly full. Others, again, felt that *empty* space could hardly exist, that it would shrink up to nothing like a pricked bladder unless it were kept distended by something material. In other words, they made matter the condition of extension. On the other hand, it was contended that however objectionable the idea of empty space might be, yet emptiness was a necessity in order that bodies might have room to move; that, in fact, if all space were perfectly full of matter everything would be jammed together, and nothing like free attraction or free motion of bodies round one another could go on.

And indeed there are not wanting philosophers at the present day who still believe something of this same kind, who are satisfied to think of matter as consisting of detached small particles acting on one another with forces varying as some inverse power of the distance, and who, if they can account for a phenomenon by an action exerted across empty space, are content to go no further, nor seek the cause and nature of the action more closely.[2]

Now metaphysical arguments, in so far as they have any weight or validity whatever, are unconscious appeals to experience; a person endeavours to find out whether a certain condition of things is by him conceivable, and if it is not conceivable he has some *primâ facie* ground for asserting that it probably does not exist. I say he has *some* ground, but whether it be much or little depends partly on the nature of the thing thought of, whether it be fairly simple or highly complex, and partly on the range of the man's own mental development, whether his experience be wide or narrow.

If a highly-developed mind, or set of minds, find a doctrine about some comparatively simple and fundamental matter absolutely unthinkable, it is an evidence, and it is accepted as good evidence, that the unthinkable state of things is one that has no existence; the argument being that if it did exist, either it or something not wholly unlike it would have come within the range of experience. We have no further evidence than this for the statement that two straight lines cannot inclose a space, or that the three angles of a triangle are equal to two right angles.

Nevertheless there is nothing final about such an argument; all that the inconceivability of a thing really proves, or can prove, is that nothing like it has ever come within the thinker's experience; and this proves nothing as to the reality or non-reality of the thing, unless his experience of the same kind of things has been so extensive as to make it reasonably probable that if such a thing had existed it would not have been so completely overlooked.

The experience of a child or a dog, on ordinary scientific phenomena, therefore, is worth next to nothing; and as the experience of a dog is to ordinary science, so is the experience of the human race to some higher phenomena, of which they at present know nothing, and against the existence of which it is perfectly futile and presumptuous to bring forward arguments about their being inconceivable, as if they were likely to be anything else.

Now if there is one thing with which the human race has been more conversant from time immemorial than another, and concerning which more experience has been unconsciously accumulated than about almost anything else that can be mentioned, it is *the action of one body on another;* the exertion of force by one body upon another, the transference of motion and energy from one body to another; any kind of effect, no matter what, which can be produced in one body by means of another, whether the bodies be animate or inanimate. The action of a man in felling a tree, in thrusting a spear, in drawing a bow; the action of the bow again on the arrow, of powder on a bullet, of a horse on a cart; and again, the action of the earth on the moon, or of a magnet on iron. Every activity of every kind that we are conscious of may be taken as an illustration of the action of one body on another.

Now I wish to appeal to this mass of experience, and to ask, is not the direct action of one body on another across empty space, and with no means of communication whatever, is not this absolutely unthinkable? We must not answer the question offhand, but must give it due consideration, and we shall find, I think, that wherever one body acts on another by obvious contact, we are satisfied and have a feeling that the phenomenon is simple and intelligible; but that whenever one body apparently acts on another at a distance, we are irresistibly impelled to look for the connecting medium.

If a marionette dances in obedience to a prompting hand above it, any intelligent child would feel for the wire, and if no wire or anything corresponding to it were discovered, would feel that there was something uncanny and magical about the whole thing. Ancient attempts at magic were indeed attempts to obtain results without the trouble of properly causing them, to build palaces by rubbing rings or lanterns, to remove mountains by a wish instead of with the spade and pickaxe, and generally to act on bodies without any real means of communication; and modern disbelief in magic is simply a statement of the conviction of mankind that all attempts in this direction have turned out failures, and that action at a distance is impossible.

If a man explained the action of a horse or a cart by saying that there was an attraction between them varying as some high direct power of the distance, he would not be saying other than the truth—the facts may be so expressed—but he would be felt to be giving a wretchedly lame explanation, and any one who simply pointed out the traces would be going much more to the root of the matter. Similarly with the attraction of a magnet for another magnetic pole. To say that there is an attraction as the inverse cube of the distance between them is true, but it is not the whole truth; and we should be obliged to any one who will point out the traces, for traces we feel sure there are. If any one tries to picture clearly to himself the action of one body on another without any medium of communication whatever, he must fail. A medium is instinctively looked for in most cases, and if not in all, as in falling weights or in magnetic attraction, it is only because custom has made us stupidly callous to the real nature of these forces.

When we see a vehicle bowling down-hill without any visible propelling force we ought to regard it with the same mixture of curiosity and wonder as the Chinaman felt when he saw for the first time in the streets of Philadelphia a tram-car driven by a rope buried in a pipe underground. The attachment to these cars comes through a narrow slit in the pipe, and is quite unobtrusive. After regarding the car with open-mouthed astonishment for some time, the Chinaman made use of the following memorable exclamation, "No pullee—No pullee—Go like mad!" He was a philosophic Chinaman.

[1] A lecture by Prof. Oliver Lodge at the London Institution, on December 28, 1882.

[2] In illustration of this statement an article has since appeared in the January number of the *Philosophical Magazine*, by Mr. Walter Browne.

Remember then that whenever we see a thing being moved we must look for the rope; it may be visible or it may be invisible, but unless there is either "pushee" or "pullee" there can be no action. And if you further consider a pull it revolves itself into a push; to pull a thing towards you, you have to put your finger behind it and push; a horse is said to pull a cart, but he is really pushing at the collar; an engine pushes a truck by means of a hook and eye; and so on. There is still the further very important and difficult question as to why the parts hang together, and why when you push one part the rest follows.

Cohesion is a very striking fact, and an explanation of it is much to be desired; I shall have a little more to say about it later, but at present we have nothing more than an indication of the direction in which an explanation seems possible. We cannot speak distinctly about those actions which are as yet mysterious to us, but concerning those which are comparatively simple and intelligible we may make this general statement:—The only way of acting on a body directly is to push it behind.

There must be contact between bodies before they can directly act on each other; and if they are not in contact with each other and yet act, they must both be in contact with some third body which is the medium of communication, the rope.

Consider now for an instant the most complex case, the action of one animate body on another not touching it. To call the attention of a dog, for instance, there are several methods: one plan is to prod him with a stick, another is to heave a stone at him, a third is to whistle or call, while a fourth is to beckon him by gesture, or, what is essentially the same process, to flash sunlight into his eye with a mirror. In the first two of these methods the media of communication are perfectly obvious—the stick and the stone—in the third, the whistle, the medium is not so obvious, and this case might easily seem to a savage like action at a distance, but we know of course that it is the air, and that if the air between be taken away, all communication by sound is interrupted. But the fourth or optical method is not so interrupted; the dog can see through a vacuum perfectly well, though he cannot hear through it; but what the medium now is which conveys the impression is not so well known. The sun's light is conveyed to the earth by such a medium as this across the emptiness of planetary space. The only remaining typical plans of acting on the dog would be either by electric or magnetic attractions, or by mesmerism, and I would have you seek for the medium which conveys these impressions with just as great a certainty that there is one as in any of the other cases.

Leaving these more mysterious and subtle modes of communication, let us return to the two most simple ones, viz., the stick and the stone. These two are representative of the only possible fundamental modes of communication between distant bodies, for one is compelled to believe that every more occult mode of action will ultimately resolve itself into one or other of these two.

The stick represents the method of communication by continuous substance; the stone represents the communication by actual transfer of matter, or, as I shall call it, the projectile method. There are no other known methods for one body to act on another than by these two—by continuous medium, and by projectile.

We know one clear and well-established example of the projectile method, viz., the transmission of pressure by gases. A gas consists of particles perfectly independent of each other, and the only way in which they can act on each other is by blows. The pressure of the air is a bombardment of particles, and actions are transmitted through gases as through a row of ivory balls. Sound is propagated by each particle receiving a knock and passing it on to the next, the final effect being much the same as if the first struck particles had been shot off through the whole distance.

The explanation of the whole behaviour of gases in this manner is so simple and satisfactory, and moreover is so certainly the true account of the matter, that we are naturally tempted to ask whether this projectile theory is not the key to the universe, and whether every kind of action whatever cannot be worked out on this hypothesis of atoms blindly driving about in all directions at perfect random and with complete independence of each other except when they collide.[1] And accordingly we have the corpuscular theories of light and of gravitation; both account for the respective phenomena by a battering of particles. The corpuscular theory of gravitation is, however, full of difficulties, for it is not obvious according to it why the weight of a plate is the same when held edgeways as when held broadside on, in the stream of corpuscles; while it is surprising (as indeed it perhaps is on any hypothesis) that the weight of a body is the same in the solid, liquid, and gaseous states. It has been attempted to explain cohesion also on the same hypothesis, but the difficulties, which were great enough before, are now enormous, and to me at any rate it seems that it is only by violent straining and by improbable hypotheses that we can explain all the actions of the universe by a mere battery of particles.

Moreover, it is difficult to understand what the atoms themselves can be like, or how they can strike and bound off one another without yielding to compression and then springing out again like two elastic balls; it is difficult to understand the elasticity of really ultimate hard particles. And if the atoms are not such hard particles, but are elastic and yielding, and bound from one another according to the same sort of law that ivory balls do, of what are they composed? We shall have to begin all over again, and explain the cohesion and elasticity of the parts of the atom.

The more we think over the matter, the more are we compelled to abandon mere impact as a complete explanation of action in general. But if this be so we are driven back upon the other hypothesis, the only other, viz. communication by continuous medium.

We must begin to imagine a continuous connecting medium between the particles—a substance in which they are imbedded, and which extends into all their interstices, and extends without break to the remotest limits of space. Once grant this and difficulties begin rapidly to disappear. There is now continuous contact between the particles of bodies, and if one is pushed the others naturally receive the motion. The atoms of gas are impinging as before, but we have now a different idea of what impact means.

Gravitation is explainable by differences of pressure in the medium, caused by some action between it and matter not yet understood. Cohesion is explainable also probably in the same way.

Light consists of undulation or waves in the medium; while electricity is turning out quite possibly to be an aspect of a part of the very medium itself.

The medium is now accepted as a necessity by all modern physicists, for without it we are groping in the dark, with it we feel we have a clue which, if followed up, may lead us into the innermost secrets of nature. It has as yet been followed up very partially, but I will try and indicate the directions in which modern science is tending.

The name you choose to give to the medium is a matter of very small importance, but "the Ether" is as good a name for it as another.

As far as we know it appears to be a perfectly homogeneous incompressible continuous body incapable of being resolved into simple elements or atoms; it is, in fact, continuous, not molecular. There is no other body of which we can say this, and hence the properties of ether must be somewhat different from those of ordinary matter. But there is little difficulty in picturing a continuous substance to ourselves, inasmuch as the molecular and porous nature of ordinary matter is by no means evident to the senses, but is an inference of some difficulty.

Ether is often called a fluid, or a liquid, and again it has been called a solid and has been likened to a jelly because of its rigidity; but none of these names are very much good; all these are molecular groupings, and therefore not like ether; let us think simply and solely of a continuous frictionless medium possessing inertia, and the vagueness of the notion will be nothing more than is proper in the present state of our knowledge.

We have now to try and realise the idea of a perfectly continuous, subtle, incompressible substance pervading all space and penetrating between the molecules of all ordinary matter, which are imbedded in it, and connected with one another by its means. And we must regard it as the one universal medium by which all actions between bodies are carried on. This, then, is its function—to act as the transmitter of motion and of energy. First consider the propagation of light.

Sound is propagated by direct excursion and impact of the atoms of ordinary matter. Light is not so propagated. How do we know this?

1. Because of speed, 3×10^{10}, which is greater than anything transmissible by ordinary matter.

2. Because of the kind of vibration, as revealed by the phenomena of polarisation.

[1] To this hypothesis Mr. Tolver Preston has addressed himself with much ingenuity.

The vibrations of light are not such as can be transmitted by a set of disconnected molecules; if by molecules at all, it must be by molecules connected into a solid, *i.e.* by a body with rigidity. Rigidity means active resistance to shearing stress, *i.e.* to alteration in shape; it is also called *elasticity of figure;* it is by the possession of rigidity that a solid differs from a fluid. For a body to transmit vibrations at all it must possess inertia; transverse vibrations can only be transmitted by a body with rigidity. All matter possesses inertia, but fluids only possess volume elasticity, and accordingly can only transmit longitudinal vibrations. Light consists of transverse vibrations; air and water have no rigidity, yet they are transparent, *i.e.* transmit transverse vibrations; hence it must be the ether inside them which really conveys the motion, and the ether must have properties which, if it were ordinary matter, we should style *inertia* and *rigidity*. No highly rarefied air will serve the purpose; the ether must be a distinct body. Air *exists* indeed in planetary space even to infinity, but it is of almost infinitesimal density compared with the ether there. It is easy to calculate the density of the atmosphere at any height above the earth's surface, supposing other bodies absent.

The density of the air at a distance of n earth radii from the centre of the earth is equal to a quarter the density here divided by $10^{350\frac{n-1}{n}}$. So at a height of only 4000 miles above the surface, the atmospheric density is a number with 127 ciphers after the decimal point before the significant figures begin. The density of ether, on the other hand, has been calculated by Sir William Thomson from data furnished by Pouillet's experiments on the energy of sunlight, and from a justifiable guess as to the amplitude of a vibration, and it comes out about 10^{-18}, a number with only 17 ciphers before the significant figures. In inter-planetary space, therefore, all the air that exists is utterly negligible; the density of the ether there, though small, is enormous by comparison.

Once given the density of the ether, its rigidity follows at once, because the ratio of the rigidity to the density is the square of the velocity of transverse wave propagation, viz. in the case of ether, 9×10^{20}. The rigidity of ether comes out, therefore, to be about 900. The most rigid solid we know is steel, and compared with its rigidity, viz. 8×10^{11}, that of ether is insignificant. Neither steel nor glass, however, could transmit vibrations with anything like the speed of light, because of their great density. The rate at which transverse vibrations are propagated by crown glass is half a million centimetres per second—a considerable speed, no doubt, but the ether inside the glass transmits them 40,000 times as quick, viz. at twenty thousand million centimetres per second.

The ether outside the glass can do still better than this, it comes up to thirty thousand million, and the question arises what is the matter with the ether inside the glass that it can only transmit undulations at two-thirds the normal speed. Is it denser than free ether, or is it less rigid? Well, it is not easy to say; but the fact is certain that ether is somehow affected by the immediate neighbourhood of gross matter, and it appears to be concentrated inside it to an extent depending on the density of the matter. Fresnel's hypothesis is that the ether is really denser inside gross matter, that there is a sort of attraction between ether and the molecules of matter which results in an agglomeration or binding of some ether round each atom, and that this additional or bound ether belongs to the matter, and travels about with it. The *rigidity* of the bound ether Fresnel supposes to be the same as that of the free.

If anything like this can be imagined, a measure of the density of the bound ether is easily given. For the inverse velocity-ratio is called μ (the index of refraction), and the density is inversely as the square of the velocity, hence the density-measure is μ^2. The density of ether in free space being called 1, that inside matter has a density μ^2, and the density of the bound portion of this is $\mu^2 - 1$.

This may all sound very fanciful, but something like it is sober truth; not as it is here stated very likely, but the fact that $\left(1 - \frac{1}{\mu^2}\right)$th of the whole ether inside matter is bound to it and travels with it, while the remaining $\frac{1}{\mu^2}$th is free and blows freely through the pores, is fairly well established and confirmed by direct experiment.

(To be continued.)

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—The following further announcements of lectures have been made:—

Prof. Humphry, Circulatory and Respiratory Systems, Jan. 25; senior class, Jan. 29; Demonstrations by the Demonstrator for Natural Science Tripos, Jan. 26; Osteology, for beginners, Jan. 17; Demonstrations for second year students, Jan. 18. Mr. McAlister will give six lectures later in the term, on the Mechanism of the Human Skeleton. Dr. Michael Foster's course of Elementary Physiology, Jan. 23; Mr. Lea, Chemical Physiology, Jan. 24; Dr. Vines, Anatomy of Plants, advanced, with practical work, Jan. 24 (Christ's College); General Elementary course, New Museums, Jan. 23, to extend over two terms, and be illustrated by demonstrations. A class for the practical study of systematic botany, Mr. T. H. Corry, assistant curator of the Herbarium, will be formed. Dr. Hicks will lecture on the Morphology of Flowering Plants, with especial reference to classification, including floral diagrams, in the Hall of Sidney College, beginning Jan. 26; Mr. Glazebrook, advanced Demonstrations in Electricity and Magnetism, Cavendish Laboratory, Jan. 24; Mr. Shaw, Demonstrations in Mechanics and Heat, Jan. 23; if more students attend than can be accommodated in the laboratory at one time, the course will be repeated on the same days. Mr. Trotter, Trinity College, Physical Optics, Jan. 25. Mr. Pattison Muir, Non-metallic Elements, Elementary, Jan. 22, Caius College Laboratory; General Principles of Chemistry, Advanced, Jan. 23. Mr. Solly will give Demonstrations on Minerals in the Lecture Room of the Mineralogical Museum, first lecture, Jan. 22. Prof. Stuart, Jacksonian Lecture Room, Theory of Structures, Jan. 30; the Demonstrator of Mechanism, Mathematics required for Engineering, Jan. 29.

Christ's College Open Scholarships, Natural Science; E. L. Sortain, Bath College, 30*l.*; 3rd year, J. C. Bose, 30*l.*; Caius College, Natural Science, Edgworth, Clifton College, 40*l.*

MR. MARSHALL WARD is giving a course of free public lectures at Owens College, on the Nutrition of Plants.

SCIENTIFIC SERIALS

Journal of the Franklin Institute, January.—Electric lighting in mills, by C. J. H. Woodbury.—Bricks and brick-making machinery, by C. Chambers, Jun.—Experiential principles of controlled combustion, by E. J. Mallett, Jun.—Olsen's testing machines.

Archives des Sciences Physiques et Naturelles, December 15, 1882.—Meteorological *résumé* of the year 1881 for Geneva and the great St. Bernard, by A. Kammermann.—Observations on cometary refraction, by W. Meyer.—Development of the vegetable kingdom in different regions since the tertiary epoch, according to Dr. Engler's work, by A. de Candolle.—Periodical movements of the air indicated by spirit levels, by Ph. Plantamour.—On the same, by C. von Orff.

SOCIETIES AND ACADEMIES

LONDON

Chemical Society, January 18.—Dr. Gilbert, president, in the chair.—It was announced that a ballot for the election of fellows would be held at the next meeting, February 1.—The following papers were read:—The fluorine compounds of uranium, by A. Smithells. The author has investigated the action of aqueous hydrofluoric acid upon the green uranoso-uranic oxide. He finds that a voluminous green powder, uranium tetrafluoride, is left, and that a yellow solution is formed which contains uranium oxyfluoride. The author confirms the results previously obtained by Bolton, and proves those obtained by Ditte to be erroneous.—On a new method of estimating the halogens in volatile organic compounds, by R. T. Plimpton and E. E. Graves. The authors burn the vapour of the compound in a glass Bunsen burner, the products of the combustion are aspirated through caustic soda solution, which is heated with sulphurous acid and the halogen precipitated by silver nitrate, &c., in the usual way. Good results were obtained with various liquids from ethyl bromide boiling at 39° to acetylene bromiodide boiling at 150°.—On a modified Liebig's condenser, by W. A. Shenstone. The author has slightly modified a vertical con

denser so that it can be used for prolonged digestion and subsequent distillation without shifting.—On two new aluminous mineral species Evigtokite and Liskeardite, by W. Flight.—On the volume alteration attending the mixture of salt solutions, by W. W. J. Nicol. The salts employed were NaCl, KCl, KNO_3, $NaNO_3$, $CuSO_4$ and K_2SO_4.

Zoological Society, January 16.—Prof. W. H. Flower, F.R.S., president, in the chair.—Mr. H. E. Dresser, F.Z.S., exhibited and made remarks on a specimen of *Merops philippensis*, which was said to have been obtained near the Snook, Seaton Carew, in August, 1862.—Lieut.-Col. Godwin-Austen, F.R.S., read the third and concluding of a series of papers on the shells which had been collected in Socotra by Prof. J. Bayly Balfour. The present portion treated of the freshwater shells of Socotra, which were stated all to belong to the genera *Planorbis*, *Hydrobia*, and *Melania*. Not a single bivalve was obtained. Four species were described as new, namely, *Planorbis socotrensis*, *P. cockburni*, *Hydrobia balfouri*, and *Melania sclateri*.—Prof. E. Ray Lankester, F.R.S., read a paper on the right cardiac valves of *Echidna* and of *Ornithorhynchus*. Seven additional specimens of the latter animal had been examined since the author's former paper on this subject had been read, all of which, whilst showing interesting variations, agreed in the absence of the septal flap of the right cardiac valve. This character was shown to exist also in *Echidna*, and was therefore presumed to be a distinctive feature in the structure of the Monotremes.—A communication was read from Mr. F. Moore, F.Z.S., containing the descriptions of some new genera and species of Asiatic Lepidoptera Heterocera.—A communication was read from Mr. G. B. Sowerby, jun., in which he gave the descriptions of five new species of shells from various localities.

Anthropological Institute, January 9.—Mr. A. J. Lewis in the chair—The election of Admiral F. S. Tremlett, F.R.G.S., was announced.—Mr. Worthington G. Smith exhibited four palæolithic implements from Madras. One of them weighed 4 lbs. 7¾ oz., and the author believed that it was the second largest specimen of the kind extant.—Mr. W. S. Duncan read a paper on the probable region of man's evolution. Starting with the assumption that man was evolved from a form lower in organisation than that of the lowest type yet discovered, and that his origination formed no exception to the general law of evolution recognised as accounting for the appearance of the lower forms of life, the author said that man's most immediate ancestors must have been similar in structure to that of the existing Anthropoid apes, although it is not necessary to suppose that any of the Anthropoid apes at present existing belong to the same family as that of man. The science of the distribution of animals showed that the higher types of monkeys and apes appear to have had their origin in the Old World, the American continent being entirely destitute of them, either alive or fossil. The distribution of the greater portion of the animals of the Old World was shown to have taken a generally southward direction, owing to the gradual increase of the cold, which culminated in the last Ice Age. This migration was, however, interrupted by the interposition of the Mediterranean and other seas, and thus, although a few of these animals were enabled to journey on until they reached tropical regions, the majority were compelled to remain behind, where they had to exist under altered circumstances. The temperature was much lower, and as a result of the consequent diminution of the number of fruit forests, a change in the food and in the manner in which it was obtained by the apes occurred. A considerable alteration took place also in the manner in which they were forced to use their limbs, and it was due to the operation of these and other causes that the ape form became stamped with human characteristics such as the curvature of the spine and an increase in the breadth of the pelvis. For these reasons the author regarded the south of Europe as the part in which it was most likely that the evolution of man took place. Mr. Duncan concluded by urging the importance of forming a committee to watch discoveries bearing on this branch of anthropology.

Meteorological Society, January 17.—Annual General Meeting.—Mr. J. K. Laughton, F.R.A.S., President, in the chair.—The Secretary read the Report of the Council which showed that the total number of Fellows was 571, 47 new Fellows having been elected during the year.—The President then delivered his Address. He referred briefly to the great importance of the uniform series of observations now taken under the auspices of the Society, and proceeded to speak, at greater length, of certain other points in which the Society might, by its concerted action, further the interests of meteorological science. The first of these was anemometry, which is at present in a condition far from satisfactory. We know nothing positively either as to the pressure or the velocity of the wind; there is no exact standard instrument, and observations, whatever may be their absolute value, are not comparable one with the other. He thought that the Society might properly interfere, so far as to regulate the wide diversity amongst the instruments now used, in order that when the proper time came, and it was known what anemometer could be trusted, the older observations might be reduced. The movement of air in the upper regions of the atmosphere is not measurable by any existing method; but experiments have been made, at the suggestion of the Meteorological Council, in which the drift of the smoke-cloud of a bursting shell may be observed and measured. The observations of the barometer taken at elevated stations in the United States seem to throw considerable doubt on the received formulæ for the reduction of barometric readings to sea-level, and for the calculation of heights. When the observations extend over a long period, and are regularly taken under all conditions of weather, then no doubt the height of a mountain can be calculated with a fair approach to accuracy; but isolated observations, subject to the fluctuations of the different readings are extremely wild in their results. In the same way, the reduction of the barometer to sea-level is complicated by many discrepancies which arise between observations at the upper and lower stations, which have hitherto been ignored. It is impossible to say how far they affect the isobars on which our daily weather charts are based; but it is probable that they are at least one additional source of error and of difficulty. It is much to be wished that systematic and continuous observations at high-level stations could be taken, not only on the top of Ben Nevis, but on the top of some others of the highest peaks in different parts of the country. In this way alone, can these difficulties of reduction be cleared away.—The following gentlemen were elected the Officers and Council for the ensuing year:—President, John Knox Laughton, F.R.A.S., Vice-Presidents: Edmund Douglas Archibald, M.A., Rogers Field, B.A., Baldwin Latham, F.G.S., William Marcet, F.R.S., Treasurer, Henry Perigal, F.R.A.S., Trustees: Hon. Francis Albert Rollo Russell, Stephen William Silver, F.R.G.S., Secretaries: George James Symons, F.R.S., John William Tripe, M.D., Foreign Secretary, Robert Henry Scott, F.R.S., Council: Hon. Ralph Abercromby, William Morris Beaufort, F.R.A.S., John Sanford Dyason, F.R.G.S., Henry Storks Eaton, William Ellis, F.R.A.S., Joseph Henry Gilbert, F.R.S., Charles Harding, Robert John Lecky, F.R.A.S., Capt. John Pearse Maclear, R.N., Edward Mawley, F.R.H.S., George Matthews Whipple, F.R.A.S., Charles Theodore Williams, M.D.

EDINBURGH

Royal Society, January 15.—Prof. Maclagan, vice-president, in the chair.—In a paper on the diurnal variation of the force of the wind on the open sea and near land, Mr. Buchan gave the first instalment of the meteorological results of the *Challenger* expedition. From fully 1200 observations which had been taken, mean diurnal curves were drawn for the different oceans, from which it appeared that in the open sea no clear marked diurnal variation existed, but that near land a very evident maximum showed itself about two in the afternoon, and a much smaller maximum at midnight. Also near land the force of the wind was distinctly less than in the open sea, a fact readily accounted for by the greater friction experienced at the surface in the former case. The wind was strongest in the southern ocean, feeblest in the Pacific. Though the temperature observations had not been completely reduced, enough had been done to show that the surface temperature of the North Atlantic was subject to a very small variation of not more than ·75 of a degree Fahrenheit.—The Rev. Dr. Teape read a long paper on the Semitic and Greek article, in which he pointed out the influence of the Hebrew idiom upon the use of the Greek article, both in the Septuagint and the New Testament, and maintained, in opposition to Prof. Blackie's views, that the use of the Greek article was regulated by definite grammatical rules.—Mr. W. W. J. Nicol, M.A., B.Sc., read a paper on the nature of solution, which he regarded from the point of view of molecular attraction. Solution took place because the particles of water had a greater attraction for the particles of the salt than these had for themselves. The theory was applied to explain various facts

established by himself and other experimenters, such for example as the relation between the density of a crystal and the temperature at which it is made to crystallise out.—An elaborate experimental paper on the relative electro-chemical positions of wrought iron, steels, cast metal, etc., in sea-water and other solutions by Mr. Thomas Andrews, Assoc. M. Inst. C.E., F.C.S., was communicated by Prof. Crum Brown. The time changes in the galvanic relations were very curious, showing in some instances a complete reversal of the poles. This was regarded as probably due to the penetration of the liquid into the plates, which would thus seem to be very far from homogeneous. The experiments have evidently an important bearing on the question of erosion in sea-water.

SYDNEY

Linnean Society of New South Wales, October 25, 1882.—Dr. James C. Cox, F.L.S., &c., president, in the chair.—The following papers were read:—Description of a new species of *Solea* from Port Stephens, by E. P. Ramsay, F.L.S. This new species of sole, of which a drawing was exhibited, was proposed to be named *S. lineata*.—Contributions to Australian oology (continuation), by E. P. Ramsay, F.L.S. In this paper the author gave descriptions of the nests and eggs of nineteen additional species of Australian birds, whose nidification and oology had previously been imperfectly known.—Descriptions of Australian Micro-lepidoptera, by E. Meyrick, B.A. This, the eighth paper by Mr. Meyrick on the Micro-lepidoptera of this country, treats exclusively of the *Oecophoridæ*, a family represented in Australia by about 2000 species. Fifteen genera and 107 species are described at great length in the present paper.—Notes on the geology of the Western coal-fields, by Prof. Stephens, M.A., No. 1. This was a brief account of the Wallerawang and Capertee conglomerates and overlying coal-measures, together with some description of the Devonian beds of the Capertee Valley and Coco Creek. Specimens of *Brachiopoda* and *Favosites*, together with a large *Pleurotomaria* as well as of Porphyry and other rocks obtained from the same locality were shown in illustration of the paper.—Notes on the oyster beds at Cape Hawke, by James C. Cox, M.D., &c. This was a paper in support of the author's views, as expressed in a previous paper, of the undoubted specific difference between the drift oyster and rock oyster of our coasts.

PARIS

Academy of Sciences, January 15.—M. Blanchard in the chair.—The following papers were read:—Choice of a first meridian, by M. Faye (Report in name of Commission). This is favourable to the American proposal.—On the mechanical and physical constitution of the sun (first part), by M. Faye. He presents a *résumé* of his researches on the subject.—Researches on alkaline sulphites, by M. Berthelot.—On alkaline hyposulphites, by the same.—On complex units, by M. Kronecker.—Separation of gallium (continued), by M. Lecoq de Boisbaudran.—Table concerning the ramification of *Isatis tinctoria*, by M. Trécul.—On hydraulic silica, and on the *rôle* it plays in the hardening of hydraulic compounds, by M. Landrin. The pure silica obtained by decomposing a solution of silicate of potash with an acid, and repeatedly washing and drying at a dark red heat, he names *hydraulic silica*, and he considers it the cause of the final hardening of hydraulic mortars. The aluminate of lime cannot concur in this effect, because of solubility, but at the moment of immersion it facilitates the intimate union of the hydraulic elements, hinders water from penetrating the mass of mortar, and so aids the slow reciprocal action of the lime and hydraulic silica.—Chemical studies on maize, &c. (continued), by M. Leplay.—Treatment of typhoid fever at Lyons, in 1883, by M. Glénard. Instead of the *expectant* method, which awaits complications, combating them as they arise, the method of treatment with cold baths has been adopted in Lyons (as in Germany), with a view to preventing those complications. The mortality is thus greatly reduced (*e.g.* in the civil hospitals of Lyons from 26 to 9 per cent., in private practice to 1 or 2 per cent.).—On the proposals of M. Balbiani for opposing phylloxera, and on the winter egg of the phylloxera of American and European vines, by M. Targioni-Tozzetti. He throws doubt on the data on which the Phylloxera Commission have proceeded, in directing effort towards the destruction of the winter-egg. M. Balbiani replies at length to his arguments, none of which, he states, are new.—Treatment of phylloxerised vines, with sulpho-carbonate of potassium in 1882, by M. Mouillefert. The surface treated was 2225 hectares, on 385 properties, and a steady increase is shown since 1877. The amount of sulpho-carbonate used was 821,317 kg.; the cost varied between 200 and 450 francs per hectare; 0·05 fr. and 0·04 fr. per stock.—Observations on the subject of the Circular of the United States Government, concerning the adoption of a common initial meridian and a universal hour, by M. de Chancourtois. He advocates the adoption of a decimal division of the day and of the circle (the latter into 400 degrees the right angle containing 100). The ancient meridian of Ptolemy, about 31·7 degrees from that of Paris, he considers the best for the initial meridian.—On the hypergeometric functions of superior order, by M. Goursat.—On Fourier's series, by M. Halphen.—On a general property of an agent whose action is proportional to the product of the quantities in presence and to any power of the distance, by M. Mercadier.—Methods for determination of the ohm, by M. Brillouin.—Reply to a note of M. Maurice Lévy.—Researches on the relative oxidisability of cast iron, steel, and soft iron, by M. Gruner. Various plates, suspended in a frame, by their four corners, were immersed simultaneously in water acidulated with 0·5 per cent. of sulphuric acid, or sea-water, or were simply exposed in moist air of a terrace. *Inter alia*, in moist air, chromate steels were oxidised most, and tungsten steels less than mere carbon steel. Cast iron, even with manganese, is oxidised less than steel and soft iron, and white specular iron less than grey cast iron. Sea-water, on the other hand, attacks cast iron more than steel, and with special energy white specular iron. Tempered steel is less attacked than the same steel annealed, soft steel less than manganese steel or chromate steel, &c. Acidulated water, like sea-water, dissolves grey cast iron more rapidly than steel, but not white specular iron; the grey impure cast iron is most strongly attacked.—On the losses and gains of nitrogen in arable land, by M. Dehérain. The losses are due not only to the exigencies of crops, but also, and for the most part, to the oxidation of azotised organic matter. When the land is not stirred, but kept as natural or artificial meadows, the combustions are less active, and the gains of nitrogen exceed the losses. Thus a farmer will more easily enrich a soil with nitrogen by keeping it in a meadow than by prodigal manuring.—Physiological action of picoline and lutidine, by MM. de Coninck and Pinet.—New experiments on irian grafts, with a view to establishing the etiology of cysts of the iris, by M. Masse.—On the solutions of continuity produced at the moment of moulting, in the apodemian system of decapod crustaceans, by M. Nevequard.

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THURSDAY, FEBRUARY 1, 1883

POPULAR ASTRONOMY

The Sun, its Planets, and their Satellites. A Course of Lectures upon the Solar System, read in Gresham College, London, in the Years 1881 and 1882. By Edmund Ledger, M.A., Rector of Barham, Suffolk. Pp. 432. (London: Stanford, 1882.)

ANOTHER work on Astronomy! It must have demanded some courage to venture on such an attempt in these days, so unprecedentedly fertile in similar undertakings. We are not speaking of the inundation of lighter productions—the magazines, the lectures, the newspaper articles—by which the lower grounds of modern society are overrun, to the benefit no doubt, in many cases, of those who may thus be led to find out in what a glorious world they live. Provided only that such efforts have something clear and something pleasant about them, we shall not be disposed to say "the fewer the better cheer." It is a worthy and honourable attempt, to introduce one new interest, one fresh and innocent pleasure, into the dull round of a careworn plodding life. The shepherd will love his work none the less for learning something of the movements of his "unfolding star:" the evening of the weary mechanic will bring unalloyed refreshment, if he is enabled to turn an inquiring gaze upon "the fields of light that lie around the throne of GOD." But not only is provision being thus made for the development of thought and intelligence among those whose lives too often are divided between uninteresting labour and debasing gratification, but a corresponding advance has been made in the production of treatises addressed to the possessors of more cultivated minds and leisurely opportunities. A full collection of such treatises during the last half-century would be at once voluminous and interesting. What would come out in strong relief from a comparison of them would be the comprehensiveness and many-sidedness of the subject. It is indeed a glorious subject—the "consideration of the heavens"; the subject of a life-time, of many life-times—in all its complexity of magnificence. No one mind, no one book, can do it justice. It is as boundless as the spaces of which it treats, and the mechanism which it professes to explain. It embraces no small part of the history of human intelligence; it demands the utmost power and subtlety of the most consummate analysis; the picturings of the most poetical imagination will be tame and feeble in the presence of its realities; and yet so simple are many of its elementary truths as to invite and recompense familiar inquiry. There may be room then for another, and another, and yet another work on astronomy; and provided they are thoughtfully designed and accurately wrought out, there will be little question as to their success; for it arises from the very comprehensiveness of the matter, that every writer will address himself to the task from his own point of view, and all readers may find something to interest them in every varied presentation of the subject.

We are pleased to give a welcome reception to the treatise which is now before us. In many respects it will be found worthy to take rank among the best. Where, as we have said, the study is so many-sided, it is obviously better to work on certain lines; not to attempt too wide a grasp, with the inevitable annoyance of bulk and costliness; not to be led into the opposite course of saying a little about everything, and enough about nothing. The author of these Lectures has chosen his own line, preferring to give us a good deal that is explanatory of the mechanism of the solar system, and a good deal that is descriptive of its wonders. And he has executed his task on the whole remarkably well. He has evidently a clear apprehension of what he is going to write about, and therefore succeeds in making it clear to other minds; and there is a pleasant facility in his style which imparts readableness to matters intrinsically somewhat dry. And if we meet with little of vivid and imaginative description, its place is supplied by a truly valuable amount of caution and discretion in dealing with the theories of the day. If he does not lead us far he will certainly not lead us wrong: and "when," as he characteristically tells us, "we know so little, we must not let our ignorance suggest unnecessary difficulties. Rather let it teach us to wait, and watch, and learn." Availing himself of no common extent of reading, he has used his materials with conscientious accuracy; and if we may venture to point out a few matters to which in our view some exception might be taken, we hope it may be looked upon as only the fulfilment of his own express desire to receive friendly communications of this nature.

A comparatively undeveloped point, we venture to think, in the programme, is the very brief notice that has been taken of the theory of the tides. Granted that its minuter details are affected by some complicated considerations, its general outline admits of easy explanation, and is at the same time the cause of occasional misconceptions which ought to be removed; and it would be probably considered by many persons an improvement if the larger space allotted to it were obtained at the expense of the refutation of the fallacy of the exploded Ptolemaic system.

We do not meet with any reference to outbursts of light on the surface of the sun; so interesting a proving that the brilliancy of the photosphere may be far outshone, and so suggestive as to their possible origin.

The author's usual lucidity is scarcely exemplified in the explanation of phases in p. 63, where we venture to think a more familiar treatment might have been adopted.

The larger map by Beer and Mädler, notwithstanding its able reduction by Neison, might have found place in the enumeration of aids to selenographical study.

There seems a little confusion on p. 77 between Sir W. Herschel's idea that Aristarchus and some other spots visible in the earthshine were volcanoes in actual eruption, and the observations by Schröter and others of minute illuminations on the dark side, which seemed to point to an unreflected origin, and are still, unlike the former, not accounted for.

With regard to Mercury, we feel it right to say that Sir W. Herschel's failure to confirm the statements of Schröter may not be entitled to much weight; as is sufficiently indicated by their controversy in the *Phil. Trans.* respecting the phenomena of Venus. As far as this latter planet is concerned, it may be concluded, without accepting the measures of Schröter, that the irregularities

witnessed by many observers prove the existence of elevations much more considerable than any upon the earth: as to Mercury, notwithstanding Schröter's deficiency as an artist, and his occasional mistakes of preconception, his observations are always too honest and faithful to be set wholly aside; and we are not sure that the uselessness of devoting time to this planet may not be found a mistake at some future day.

As to the physical condition of Mars, we venture to think that our author has dealt very fairly and judiciously with a subject of controversy, which might have become less pleasant but for the unassuming modesty of Schiaparelli and the liberal candour of Green, so honourable to each of them. We are not sure that it is always borne in mind, how much of the difference may have been due to the early return of the English observer from Madeira to a far inferior climate, previous to the development of the additional features which were subsequently perceived at Milan, and which may possibly, like their strange gemination, become more visible from prolonged solar influence. The less favourable position of the planet at the next opposition is much to be regretted; but Schiaparelli's experience has warned us that increase of distance may possibly be compensated by improvement in definition: to which we would add on the one hand the constantly verified adage of Sir W. Herschel, that "when an object is once discovered by a superior power, an inferior one will suffice to see it afterwards"; and on the other, the advantage which may be expected from the 18 inches of aperture with which the Italian Government are about to mark their appreciation of their astronomer's ability, and their willingness to enable him to meet the emergency. It will be matter of regret, if in this honourable contest no corresponding preparation should be made among ourselves; though it is difficult indeed to counteract the disadvantage of the English sky. It is not easy to forecast the result; but we think there are indications that possibly the supposed terrestrial analogy has been pushed quite far enough. As to the interesting question of the habitability of Mars by beings like ourselves, it deserves more attention than it perhaps has often received, that none of the supposed correspondence with our own constitution could be maintained excepting on the supposition of a higher internal temperature on the globe of Mars, or possibly a very different composition of atmosphere. We are not so much struck as the author with the progressive diminution of the measured diameter of Mars effected by the employment of modern instruments; at least Schröter's determination by the mode of projection in 1798 scarcely exceeds by 0".5 that adopted by Newcomb for 1850. Irradiation no doubt is a fact; and a very troublesome one; but we suspect that its effects have been sometimes over-estimated, or mixed up with those of diffraction; and possibly the subject might bear further investigation.

As to the internal heat of Jupiter, so interesting an inquiry ought not to have been left so long in abeyance. If it exists, it would hardly be less capable of detection than that of Arcturus; and the bolometer of Langley seems to offer a fair chance for the discovery. The satellite whose strange reappearance is so difficult of explanation was, it will be found, about to enter on transit instead of suffering occultation. It may be noted, *en passant*, that a telescope must have had a marvellous power of indistinctness, that could show M. Flammarion the third satellite with a disc as large as that of Uranus (p. 409).

It seems a pity that the traditional misrepresentation of the ball of Saturn, at p. 358, as carrying a faint shadow on one side, should still be adhered to; and we may venture to suggest that there is a good deal of inequality in the execution of the diagrams in various parts of the book.

We are confident that the author will not misunderstand our remarks, or hesitate to accept our assurances that they are made in the most friendly spirit. If we are in error, he is fully able to hold his own; and he has our cordial wishes not only for his success on the present occasion, but for the extension of his labours, at no distant time, to a wider review of the glorious works of Nature.

THE ZOOLOGICAL RECORD

The Zoological Record for 1881. Being Vol. xviii. of the "Record of Zoological Literature." Edited by E. C. Rye. (London: John Van Voorst, 1882.)

IT is gratifying to be able to announce that the persevering efforts of the editor of the *Zoological Record* to publish the record of one year's work before the termination of the next year have been at last crowned with success; nor do we doubt but that this very desirable effort will be continued, and indeed become even less difficult with the advance of time, so that through the good will of the Recorders the date of publication will recede backwards from December to September or August in each year, enabling the worker to begin his autumn session with the volume in his hand. The facilities of intercourse are now so great with all parts of the world, that the Transactions and Proceedings really published in the month of December in any one year can be, nay are on our bookshelves in these British Isles, long ere the spring is on its wane, and no doubt long ere 1882 was out, some of the Recorders of this very volume had the record for that year well in hand. However grea may be our expectations for the future we cordially welcome this present volume, and acknowledge that our thanks are due to both Editor and Recorders for what they have already done.

To those who have time for reflection and dare to look back over those eighteen years since Dr. Günther and his friends launched this work upon the world of science, the thought naturally arises of the vastness of the amount of work that is year after year being accomplished without apparently in any way leading to exhaustion. The Editor's own comments are naturally in the volume very few, but how full of meaning is the following: "This volume is 36 pages longer than its predecessor," that is, even the very enumeration of the zoological literature of 1881 requires about 800 closely printed pages; and again we read, "the number of new genera and sub-genera contained in the present volume is 1438"—a simply appalling number. The Insecta are credited with 543, and the Protozoa with 517 of these genera. An enthusiastic zoologist once contemplated the posting up to date of Agassiz's "Nomenclator Zoologicus," that was when the generic increase was some 400 to 500 a year; what would he

say or think now of these new births at the average of over 1000 a year. The "examination of this large number of new names, as regards prior occupation," the Editor states was necessarily superficial, we quite sympathise with him; before we read his footnote we rushed into the subject with the A's, but on turning over to page 2 we saw how matters stood and we gave the critical business up at once, and it was obvious at a glance that the greatest genus maker of the year was Ernest Haeckel.

The year 1881 showed a lull so far as the works on recent Mammalia were concerned—at least in comparison with 1880—but the flood of new extinct mammalian forms from North America shows no sign of abatement. In 1881 the lamented Balfour completed his excellent and masterlike treatise on Embryology. The account of the Mammalia in Messrs. Salvin and Godman's work on the Biology of Central America has been finished, and Peters and Doria have published an important work on the Mammals of New Guinea.

The contribution to Bird Literature has been considerable, and the year was marked by the appearance of two more volumes of the Catalogue of the Birds in the British Museum (vols. v. and vi.). Among the Reptiles, Batrachians, and Fishes, no work of any very special importance seems to have appeared. Dr. von Martens still records the Mollusca and Crustacea. The record of the former group extends to 108 pages, and of the latter to 38 pages; both are most painstakingly executed.

The literature of the Arachnida is more extensive than usual, and the year's work is marked by the appearance of several important contributions by the Recorder, Holmberg, Karsch, Keyserling, Koch, Pavesi, Simon, and Thorell, so that it is evident that the Arachnid treasures of the world are at last being worked. Among the Myriopods, Cantoni's Monograph of the Lombardy forms seems to call for notice.

The enormous group of Insecta is recorded by Mr. Kirby, with the exception of the Neuroptera and Orthoptera, which fall to the skilled hands of Mr. McLachlan.

The Vermes and Echinoderms are recorded by Prof. Jeffrey Bell; the Cœlenterata by A. G. Bourne and Sydney J. Hickson. It is remarkable that not a single new genus or species of any recent Octactiniæ seems to have appeared in 1881, nor indeed any separate paper on the group. The Sponges and Protozoa have engaged the attention of Stuart O. Ridley; while nothing very striking seems to occur among the Sponge literature. Kent's Manual of Infusoria, and Haeckel's Prodomus of the Radiolaria mark the year; among the Protozoa, the latter work records 483 new genera and 2000 new species—an almost embarassing number of pretty things.

We are truly glad that the importance of this Record is still practically witnessed to by the generous help rendered to the Zoological Record Association by the British Association for the Advancement of Science and by the Grant Committee of the Royal Society.

OUR BOOK SHELF

The Brewer, Distiller, and Wine Manufacturer. (London: J. and A. Churchill, 1883.)

THE little work before us is the first of a series of technological handbooks to be issued by the publishers, "each of which will be complete in itself, will appear in a handy form and at a low price." Practically they will be a re-issue of articles in Cooley's "Cyclopædia of Practical Receipts and Collateral Information in the Arts, Manufactures, Professions, and Trades," with a somewhat fuller treatment and with reference to the more recent developments which have taken place in industrial processes. As this, the first of these handbooks, treats of Alcohol and Alcoholimetry, Brewing and Beer, Cider, Liqueurs and Cordials, Distillation of Alcoholic Liquors, and lastly, Wine and Wine Making, necessarily much of the Encyclopædic form of treatment must remain, when such important industries are discussed in so small a compass.

Though we cannot endorse the statement of the publishers, that each handbook will be *complete* in itself, we are compelled to admit that the Editor has given a remarkably well condensed *précis* of what has been written on industrial fermentation processes.

The first chapter describes the sources of alcohol, its detection in liquids, and its estimation by volume and by weight: numerous tables are given for this purpose. In addition to the usual distillation process, the methods of Balling, Gröning, Brossard-Vidal, Silbermann, Geissler, and others are described; this part of the book must prove of much use to the technologist.

Brewing is described in fifty pages; this is sufficient to show how condensed the treatment of one of the largest industries in the kingdom must necessarily be. Brief though it be the Editor deserves the highest praise for the manner in which he has condensed the vast mass of facts now accumulated in this department of fermentation chemistry. In addition to the description of the English processes of malting, mashing, and fermentation, a brief account is given of the German Lager beer system now almost universal on the continents of Europe and North America. This is supplemented by a large number of elaborate analyses of English and "Lager" beers, showing the characteristic differences in the products of the two methods. Brief though this part of the handbook is, it will be found of interest to the general reader and of value to the practical brewer, who may not have hitherto given much attention to the scientific part of his manufacturing process.

In Chapter V. we have a full account of the mashing and fermentation processes adopted by the distiller and rectifier, including the methods followed by the latter to remove some of the fusel oils and to flavour "still" spirit so as to produce gins, whiskies, &c., of various taste and aroma. A useful feature in this part of the work will be found in the descriptions and drawings of the stills of Coffey, Siemens, Derosne, Laugier, Dorn, Pistorius, Pontifex and Wood, and others; this will be found of much interest to distillers, more especially in our colonies, where technical information is more difficult to obtain than in the old country.

The sixth and last chapter treats of Wine and its Manufacture.

After a brief description of the soils and manures best suited to the culture of the vine, we have an enumeration and description of some of the better known wines, such as Lafitte, Latour, Margaux, Haut Brion, Leoville, and other red wines of the Gironde, and of the white Graves, as Sauterne, Barsac, Château Yquem, Latour, &c.; of the Burgundies, Romanée Conti, Chambertin, Clos Vougeot, Clos St. George, and La Tache, and of some of the wines of the Champagne, Beaujolais and other vine districts of France.

A brief account is given of the so-called Hocks of the Rhine, and those of the valleys of the Moselle, Ahn, and other rivers of Germany. In the description given of wine-making some use is made of the invaluable treatise by Messrs. Dupré and Thudichum ("On the Origin, Nature, and Varieties of Wine," Macmillan), a work

which will amply repay the technologist who consults it.

In conclusion we may add that this little book, though far from being "complete," or exhaustive of any one subject it treats of, is yet compiled with great care and discrimination by the Editor, and will be found of much value by those unable to consult larger treatises or original papers. C. G.

Il Potentiale Elettrico nell' Insegnamento Elementare della Elettrostatica. Per A. Serpieri, Prof. di Fisica nella Università e nel Liceo Raffaelo di Urbino. (Milano, 1882.)

THIS treatise is an elementary exposition of the theory of the Potential in its application to Electrostatical Phenomena. It is founded, as we learn from the preface, on the author's lectures at the Raphael Lyceum of Urbino; and is intended for the use of the Lyceums and Technical institutes of Italy. It is well known to all who interest themselves in such matters that a promising young school of physicists has recently been springing up in Italy, and that those who wish to be abreast of their time can no longer neglect the Italian scientific literature. If the treatise of Prof. Serpieri may be taken as a fair specimen of the scientific instruction given in the secondary schools of Italy, it is clear that this harvest of physicists is due in no small degree to careful sowing.

The work deserves its title of Elementary, inasmuch as nothing is demanded of the student beyond a knowledge of elementary geometry and algebra, and a slight acquaintance with trigonometry. The author is mistaken, however, in supposing that an elementary treatment of electrical theory has not hitherto been attempted; for the English work of Cumming, published some six years ago, is almost identical in its aims with his own. Although Cumming's treatise is an excellent one in many ways, we cannot help thinking that the Italian one is better fitted for the purposes of elementary instruction. Prof. Serpieri appears to us to have happily kept the middle way alike between poverty and redundance of matter, and between excess of mathematical and excess of merely experimental detail.

In the first four chapters are developed the relation between potential and charge, and the theory of lines of force and equipotential surfaces. The fifth, sixth, and seventh chapters contain the theory of capacity, of electrostatic induction, and of the measure of potential. The eighth chapter contains a short sketch of the centimetre-gramme-second system of units, now universally adopted in accordance with the decision of the Electrical Congress at Paris; farther details on this all-important matter are given in one of the appendices, and a considerable number of numerical examples is provided to familiarise the student with the practical use of the system. The last seven chapters are devoted to the theory of condensers. Not only is the theory explained in a simple and interesting way, but abundance of experimental results and numerical illustrations are given to enable the learner to judge how far the mathematical theory represents the actual facts. The account of the experiments of Villari on the heat developed in the electric spark under various circumstances is interesting, and would probably be new to most English readers.

The main fault we have to find with Prof. Serpieri's work is that he has a tendency to cite second-hand authorities where it would have been quite as easy, more instructive for his youthful readers, and *more just* to give the original sources. Again, why of all the results concerning specific inductive capacity should he quote (p. 69) those of Gordon only, which have been precisely the most questioned, and why on the same page should the results of Boltzmann for the specific inductive capacity of gases not be coupled with the name of their author?

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Hovering of Birds

IN your last number I observe an interesting letter on the "Hovering of Birds," by Mr. Hubert Airy. In that letter he refers to an opinion which I have expressed, that this "hovering" cannot be accounted for by the mere supporting agency of an upward current of air. The writer quotes this opinion as it was expressed in a letter to you (NATURE, vol. x. p. 262). But he does not seem to have read the fuller explanation which I have given on this subject in Chapter III. of the "Reign of Law." To that chapter I must refer your correspondent for an explanation, which shows that hovering can be, and is perpetually accomplished under the ordinary conditions of horizontal currents of air. It is very commonly performed (especially) by the whole tribe of Terns, or sea-swallows, over the surface of the sea, where there are no hills or mountains to deflect aërial currents from the usual horizontal course.

Mr. Airy himself uses words which indicate that this agency of upward currents is quite superfluous. He says: "It is easy to see that a bird, with the exquisite muscular sense that every act of flight demands and denotes, might so adapt the balance of its body, and the slope of its wing-surface to the wind, as to remain motionless in relation to the earth." He prefaces these words by these others: "given such a slant upward current." But no such "gift" is needed. The bird has only to slope his wing-surfaces to the current, and precisely the same effect is produced as if the current had been otherwise "sloped" upwards against a horizontal wing-surface. Mr, Airy's own letter contains an excellent explanation of this correspondence.

Cannes, France, January 29 ARGYLL

WITH respect to Mr. Hubert Airy's interesting note (vol. xxvii, p. 294), I beg to say that I have very often seen the kestrel hovering over the perfectly level meadows of Middlesex with obvious ease, where no undulation of the ground could possibly affect the currents of air. Of the twelve instances Mr. Airy enumerates, I see only six refer to hawks (species undetermined), so this fact must be taken into consideration; the conduct of rooks and crows under such circumstances seems to me to come under quite a different category from that of hawks, and in some instances gulls, thus "prospecting" for their prey. Mr. Airy does not ignore this aspect of the question, but I think that by confusing objective with subjective "hovering" he complicates his theory. HENRY T. WHARTON

39, St. George's Road, Kilburn, N.W., January 27

Action of Light on India-rubber

IT may be in the recollection of some of your readers, that in 1876 I pointed out that the deterioration of ebonite surfaces was due to the combined action of light and air. Some time afterward it was remarked to me that our laboratory (an old greenhouse) was too light, and as a result all our india-rubber tubes would rapidly deteriorate. This led me to submit some pieces of ordinary black india-rubber to the same treatment as the ebonite in the former experiments. On October 11, 1879, four pieces of caoutchouc connector of 5 mm. internal diameter were taken, two were placed in test-tubes plugged with cotton-wool, and the remaining two inclosed in hermetically sealed tubes. One of the sealed tubes, and one of those plugged with cotton wool were placed in a dark drawer, and the other pair in the laboratory window, with a north aspect, and in such a position that they were not under the influence of direct sunlight in the summer. To-day the specimens were examined. Both the sealed tubes were found to be slightly moist inside, and on opening them an organic odour, like that of an india-rubber shop, was perceived. The caoutchouc which had been exposed to air and light, was covered with a thin brown coating, and on being bent this coating cracked; the end which had been most exposed to the light was rather brittle, and could not be stretched

without splitting. The other three specimens were unaltered. All four specimens were slightly acid to test paper, but the quantity of acid was too small to be determined.

Mareck (*Chem. News*, xlvii. 25, from *Zeitschr. für Anal. Chem.* xxi.), has lately recommended the preservation of caoutchouc tubes, by keeping them in water when not in use. This is, no doubt, efficacious in consequence of the exclusion of air.

Cooper's Hill, January 22 HERBERT MCLEOD

A Possible Cause of the Extinction of the Horses of the Post-Tertiary

A TRAVELLER in the Park region of northern Colorado, and the central portion of Wyoming, fifteen years ago, could not fail to notice the immense numbers of skulls and other bones of bisons in districts at that time no longer frequented by these animals. Scattered specimens were to be seen in all directions, some of them bearing marks of bullet and knife which left no doubt as to the agent of destruction. Others were to be found in numbers in localities which suggested that they had been surprised by death while seeking shelter from the weather rather than the human destroyer. In such cases, tumbled and mixed by the scavengers, they were thickly strewed over small areas, and the contour of the surface often was such as to bring them closer together with the movement of water or soil. When asked the cause of the wholesale slaughter, the reply of the natives was almost invariably "the hunters killed a great many, but the most died in the deep snow and cold weather some twenty-five years ago."

The great losses experienced by the cattle men of the Medicine Bow and Elk Mountain region, only a couple of winters ago, are too recent to have been forgotten. The next spring and summer the unfortunate owner found the carcases of his cattle in positions similar to those occupied by the bands of bisons. In small parties they had huddled in sheltered basins or nooks, and some, upheld by the snow through the winter, were still on their feet. Since then these "bone yards" have become similar in appearance to those of earlier date.

Last summer the kindness of Prof. Agassiz enabled me to make some discoveries in the Mauvaises Terres of the eastern slope of the Rocky Mountains which vividly brought to mind the pockets full of recent skeletons. Sections in the Post-Tertiary beds here and there disclosed groups or herds of fossil horses (*Equus*) in circumstances so similar as to leave no alternative to the conclusion that the same causes had filled the bone basins in the olden and in most recent time.

Stripped of the strata above them, the contour of the surface would have been similar, and the old-time Coyotes in their feasting had evidently brought about an equal amount of confusion in the remains. About the time of the deposition of these fossils the horses became extinct. *Why* is still an open question. Such evidence as was gathered there has led to the belief that, in that region at least, occasional "cold waves" of days—perhaps weeks in duration, which deep snows caused, or were the principal causes of the extermination of the horses. Other causes that may be suggested are these: lack of water, and an extended glacial period. A consideration of the character of the deposits, the drainage of the mountains at the time, the absence in these beds of proof of a glacial period affecting them since, and the continued existence in the same locality of other creatures, somewhat less sensitive to the cold, would seem to be sufficient objections to their acceptance. The tradition of the Indians, that there is a winter of terrible destruction to the animals once or twice in the lifetime of a man—say once in about forty years—appears to be confirmed by the testimony of the whites. A few degrees or a few days added to the measure of the "wave," or "blizzard," and a few inches added to the depth of the snow would suffice to sweep the herds from the pastures. Weather of this character is a possibility every winter in the Bad Lands, though we hardly expect it. Apparently the rocks contain evidence of such weather in post-Tertiary times. And it may not have differed so very much from that we are having to-day.

S. GARMAN

Cambridge, Mass., U.S., January 12

Suicide of Scorpions

SPEAKING of scorpion suicide, Mr. G. J. Romanes in his "Animal Intelligence writes: "Still I think that so remarkable a fact unquestionably demands further corroboration before we shall be justified in accepting it unreservedly" (p. 225). Some years ago I made some experiments and observations on a smaller and a larger species of scorpion found on the Cape Peninsula. I am unable to ascertain the specific names; the smaller are found beneath the bark of decaying tree-stumps, the larger, which often weigh upwards of seventy grains, are found beneath stones and ant-balls. I have recently resumed these experiments and observations. The conclusion I come to is that neither of these species have any suicidal instinct. Only in one case have I found, after death, any sign of such a wound as the sting might inflict; in this case, though one of the tergal plates showed a largish irregular fracture, the wound did not seem a fresh one, and was dry and apparently skinned over; in this case, too, though I watched the death of the scorpion (caused by the gradual application of heat to the bottom of the glass vessel in which the creature was inclosed), I was not able to detect anything like the act of suicide. I will now briefly describe the nature of my experiments.

1. Condensing a sun-beam on various parts of the scorpion's body. The creatures always struck with the sting round, across, and over the heated spot, and seemed to try and remove the source of irritation.

2. Heating in a glass bottle. As this admits of most careful watching, I have killed some twenty or thirty individuals in this way. The creatures very commonly pass the sting over the body as if to remove some irritant. The poison exudes from the point of the sting and there coagulates.

3. Surrounding with fire or red hot embers. I first took a newspaper, moistened a ring about a foot in diameter with alcohol, and placed a scorpion within the ring. The paper was, by this time, ignited. He walked without hesitation through the fire, and tried to make his escape. I made a ring of red-hot wood-embers, and placed a scorpion in the middle. He pushed his way out, displacing two of the embers. I made a better fire-wall, and put him in the middle again. He crept over the embers. I placed him in the midst of a ring of embers on the flat and much-heated stones of the fire-place. He crept over the embers again, but this time got baked before he could escape.

4. Placing in burning alcohol. I placed a layer of an eighth of an inch of alcohol in a shallow vessel, lit the alcohol, and placed the scorpion in the midst of the burning spirit.

5. Placing in concentrated sulphuric acid. I moistened the bottom of a large beaker with a very thin layer of concentrated sulphuric acid, and put in a scorpion. The creature died in about ten minutes. (I have also tried other strong acids, a concentrated solution of sodium hydrate, and a potassium cyanide solution.)

6. Burning phosphorus on the creature's body. I placed a small pellet of phosphorus near the root of the scorpion's tail, and lit the phosphorus with a touch of a heated wire. The creature tried to remove the phosphorus with its sting, carrying away some of the burning material.

7. Drowning in water, alcohol, and ether.

8. Placing in a bottle with a piece of cotton-wool moistened with benzene.

9. Exposing to sudden light. I have not tried special experiments as to this point, but have, on turning over an ant-ball, suddenly exposed a scorpion, hitherto in complete or almost complete darkness, to the full glare of South African sunshine.

10. Treating with a series of electric shocks.

11. General and exasperating courses of worry.

I think it will be admitted that some of these experiments were sufficiently barbarous (the sixth is positively sickening) to induce any scorpion who had the slightest suicidal tendency to find relief in self destruction. I have in all cases repeated the experiments on several individuals. I have in nearly all cases examined the dead scorpion with a lens. My belief is that the efforts made by the scorpion to remove the source of irritation are put down by those who are not accustomed to accurate observation as efforts at self-destruction. On one occasion I called in one of my servants to watch the death of a scorpion by gradually heating it in a glass bottle. The creature at once began moving its sting across and over its back, upon which my servant exclaimed, "See it is stinging itself." I do not wish to imply that all the cases of alleged scorpion suicide are merely instances of careless observation. All I wish to do in this note is to record my individual experience, and to state clearly that after making a series of observations as carefully as I could

on a large number of individuals, I cannot place on record a single instance of clear and unmistakable scorpion suicide.

Rondebosch, January 1 C. LLOYD MORGAN

Mimicry in Moths

I HAVE read with great interest the observations of the Duke of Argyll on Mimicry in Moths. I remember more than one similar occurrence during my travels. The most curious was as follows:—

Whilst in Japan, a messmate brought on board, in an ordinary pot, a beautiful trained shrub with a leaf much resembling that of an orange. It was placed on the ward room table where we all sat, the steward removed it from the table to the top of an harmonium at least three times a day, and I watered it when required, and often examined and admired it; in about eight or ten days it began to show signs of failing; and, thinking it might be infected with spider or green fly, I examined it carefully, and in doing so I disturbed a large green smooth-skinned caterpillar. Like all animals on board ship he soon became a great favourite, and we often asked strangers to point him out and in no case did they succeed.

He always lay along the edge of the leaf, with his head to the point and eat at each bite, exactly the breadth necessary to preserve the contour of the leaf as far as possible, when he reached the point, by a few sharp convulsions he returned to the stem and began another row. When he had finished one half of the leaf he began the other; and when nothing but the centre rib of the leaf was left he eat backwards along the stem. He was the most economical feeder I ever saw, only a very little bit of the centre rib of the leaf was bitten off and fell to the ground, and the hard stem of the leaf was left.

I soon observed that he could assume the exact shade of the leaf he was feeding on, and I frequently shifted him and watched the process.

In due time he assumed the chrysalis form; he partly suspended, partly glued, himself to the stem of the plant and it was very difficult to detect him; but not nearly so difficult as in the caterpillar state.

He remained a very short time in the pupa, and one day I was called by a messmate who informed me that "My beastly bug had hatched out," and at first I thought this was the case, as a beautiful black and gold butterfly was expanding his wings and legs on the table, and soon took wing, but was captured and handed over to our bug collector, who by the way took no interest whatever in the prior stages; he was neither butterfly, moth, nor beetle, so nothing to him.

I went to observe how he had broken out of the sheath and was astonished to find that my chrysalis was safe and sound, the butterfly we had certainly did not come from it. Then where did it come from? We were still in Yokohama harbour, and it was a common occurrence that insects flew off to the ships. But how did a butterfly in the state I saw this one get on the ward room table? I came to the conclusion that the pupa had been attached to the plant or pot; I did not anticipate what took place. In a few days another butterfly, to all appearance the brother of the first one, was seen (but not by me), to emerge from the chrysalis we had at first observed; and I have no doubt the first insect had eluded all our prying, and that there were two caterpillars all the time on the plant.

I do not get NATURE until it is a fortnight old, and I have waited with anxiety to see if any one better able than I am would endeavour to show that mere physical causation is sufficient to account for all the phenomena disclosed by the Duke's admirable observation of the moth.

I look upon the Duke as one of the best observers of Nature we have, and his opinions must carry great weight; and believing as I do that in the Theory of Natural Selection the future existence of our race and all hope of advancement in morality is bound up, I am anxious that his doubts on this subject should not carry weight with others.

I think the whole question lies in this—were either of these caterpillars, or the Duke's moth perfect, or even the most perfect of their kind?

I believe I have had more opportunity of observing cases of mimicry than his Grace has, and I have always found that the individuals vary as much in these forms of life as in any other. At Labuan one of the Engineers of the coal works sent a native out and in half an hour he returned with seven leaf-insects. I had picked one up in my walk from the settlement, and although at first each appeared a perfect leaf to my eye, I soon found great differences between the individuals; some being much better specimens than others—just as all sheep are not sheepish to the shepherd—and I think it is quite possible that not one of these eight insects would deceive the eye of an average natural enemy. Let us suppose that anyone of these were so perfect as a mimic, that it would deceive this enemy, it might be wanting in the advantage of perfect rest whilst under inspection, and thus be detected. It was by the movement of the insect that I was enabled to get the one I picked up. The Duke's moth was betrayed by his "beaded eyes and thorax;" and last of all, there was a small hole in the covering of the bright wings, which the Duke considers one of the mysteries of nature, and through all the mimicry of this moth the Duke with very little trouble detects the imposter; as far as he was concerned, all the effort of nature was wasted. If I may be allowed the paradox, it is only when one has come to see what a botch nature has made of its work that its beauties can be properly appreciated. I admire quite as much the quickness of eye that belongs to the lizard that may have been on the watch to capture the moth; these "mysteries" have gone on together; and where a moth or a lizard failed ever so little it went down whilst its better appointed brother was the fittest to survive. Until the mind has taken in how constant the battle is, how small the advantages must be when the enemy is travelling the same path, it is difficult to resist the feeling of wonder and the desire to account for all by a fiat of creation.

I remember some remarks by the Duke of Argyll in a similar strain, when he observed three water-oozels take the water for the first time. He was struck with the way in which they all dived and swam, so perfectly; but I think he failed to consider this view of the matter—did any one of these surpass the others in the art, even were his advantage so little that the Duke was unable to detect it? if so, then provided he was equal of his brothers in all other respects, he was the fittest to survive; and as we evolutionists only claim little by little; its ordinary phrases are no lean and empty formulæ to me.

Nothing but the conviction that, in the new light thrown on nature by Charles Darwin and his numerous disciples, lies the happiness or misery of our race, would have emboldened me, so indifferently educated for the task, to take up the subject and your time. DUNCAN STEWART

Knockrioch, January 25

Clerk-Maxwell on Stress

CAN any of your readers give me a reference to the note in which Maxwell, commenting on or replying to a correspondent of NATURE, gave his ideas as to the nature of stress in a beam or cord? T.

The Comet

MAY I ask space to make some observations about the orbit of the Great Comet of 1882?

Looking on the many elements published in NATURE, in the *Dunecht Circulars*, and in the *Astronomische Nachrichten*, I find very great differences between one and another. Especially the elliptical elements calculated by Mr. S. C. Chandler, Mr. Frisby, Mr. Kreutz, and Mr. Morrison present periods peculiarly different.

Now this fact can be produced but by two causes; either it may be that the different observers considered different parts of the nucleus as the brightest part; or it may be that the movement of the comet has been much perturbed by some bodies of the solar system.

The first hypothesis is very probable, as you remark in the "Astronomical Column" in NATURE, vol. xxvii. p. 300.

The division of the head in two, and perhaps three portions, is a fact well observed by many astronomers, and well shown in the drawings published by Mr. A. A. Common, Dr. Doberck, and Mr. W. T. Sampson in NATURE, vol. xxvii. pp. 109, 129, and 150.

But I observed that with small magnifying power the appearances of the brightest part of the head maintained always a certain unity, which would not admit great mistakes in the observations. Therefore it seems to me that, unless we suppose considerable and unknown variations in the form of the nucleus, only the difference of appreciation of the point observed can hardly explain such a great, and I say regular, difference between one orbit and another.

I say *regular difference*, because I remark a certain peculiarity.

The first elliptical orbit calculated by Mr. J. C. Chandler, using observations from September 18 to October 20, gave a period of about 4000 years.

Afterwards Mr. Kreutz, using observations from September 8 to November 14, gave a period of 843 years, and lately Mr. Morrison, keeping observations from September 19 to December 11, has an elliptical orbit with only 642·5 years.

This fact induces me to believe that an accurate study of the perturbations of the motion of this comet may be as important as it was for Biela's comet.

It is my purpose to go, as far as I can, through a complete discussion of all the observations, and I shall be very glad if those of your readers, who are possessors of good unpublished remarks both about the appearance and about the positions of the comet, would kindly let me know of them.

E. RISTORI

13, Pembridge Crescent, Bayswater, W., January 30

The Aurora of November 17, 1882

I SHOULD like to ask H. J. H. Groneman whether he tried to find out if a curved path for the auroral beam would agree better with the observations than a straight one; because, if it was purely an auroral phenomenon, we should naturally expect its path to be a curve, maintaining a uniform height above the surface of the earth, and to be approximately a small circle having its centre at the magnetic pole, this being the ordinary position of the auroral arches. Of course the motion of the parts of the arch is often not exactly in this direction, because the arch has frequently a transverse motion in addition to the movements that take place longitudinally; and if there was any such transverse motion in the case of this beam, that would prevent its moving strictly along a parallel of magnetic latitude, though it is hardly likely it would deviate far from it. It would be well to ascertain whether such a motion would not agree better with the observations of the beam than Dr. Groneman's hypothesis that it was in a straight line; for the establishment of a curved motion would do away with the idea that the phenomenon was caused by a meteor.

In the other cases cited by Dr. Groneman of supposed meteoric masses passing through our atmosphere and producing auroral effects, the paths, so far as given, seem all to have been approximately along the parallels of magnetic latitude, which circumstance militates against their having had anything to do with meteors, because these traverse the atmosphere in all directions, and would be just as likely to go in a northerly or southerly direction as in an easterly or westerly one. Possibly, however, Dr. Groneman's theory may be that meteors only produce an auroral effect when they happen to go in such directions as may be calculated to produce it.

Sunderland, January 29 THOS. WM. BACKHOUSE

As Dr. Groneman in his most interesting paper on the phenomenon of November 17 asks for my authority for the Swedish observation, I may say that I merely saw it in the "Notes" in NATURE (vol. xxvii. p. 113). There seems a misprint in that statement, however, as "Eskibstuna, fifty-four miles south of Stockholm" would be in the sea, whereas Eskilstuna is fifty-four miles *west* of Stockholm.

As the spectroscope observation is said to put the auroral nature of the "spindle" beyond doubt, I would observe that until we know that gas excited by the passage of particles through it at fifteen miles a second does not give the same spectrum as when incandescent by an electric discharge, the observation of certain lines cannot prove anything of the exciting cause. Further, a good deal of the light might be reflected sunlight, as that would be scattered over the whole spectrum, and would thus be masked by the faint diffused spectrum of the moonlight at the time.

W. M. F. PETRIE

Bromley, Kent

REFERRING to Dr. Groneman's communication, possibly it may be of service to say that at 9 p.m., October 14, 1870, besides some ruddy auroræ, chiefly in the west and north, I saw a band having a very close resemblance to that figured in the illustration, p. 297. It, however, stretched all the way across the sky from west to east, and continued for some time without much apparent alteration in figure or locality. An appointment called me away before it had vanished.

HENRY MUIRHEAD

Cambuslang, January 26

The Sea Serpent

I HAVE seen four or five times something like what your correspondent describes and figures, at Llandudno, crossing from the Little Ormes head across the bay, and have no doubt whatever that the phenomenon was simply a shoal of porpoises. I never, however, saw the *head* your correspondent gives, but in other respects what I have seen was exactly the same; the motions of porpoises might easily be taken for those of a serpent; once I saw them from the top of the Little Orme, they came very near the base of the rock, and kept the line nearly half across the bay.

JOSEPH SIDEBOTHAM

Erlesdene, Bowdon, January 26

Influence of "Environment" upon Plants

REFERRING to Prof. Thiselton Dyer's letter on the above subject in NATURE (vol. xxvii. p. 82), it may interest your readers to know that I have had several trees of *Acacia dealbata* 30 feet high, in the open air, in flower for ten days past, but not so fully as they will be in a fortnight's time. I have had *Desfontainea spinosa* in flower during the past eight months; this shrub is 6¼ feet high, and also in the open air.

Rosehill, Falmouth, January 29 HOWARD FOX

THE PEAK OF TENERIFFE ACTIVE AGAIN

A PRIVATE letter which I have just been privileged to see, from a native lady in Santa Cruz to her sister in this country, tells how the inhabitants of that present capital of Teneriffe had remarked for several months past, that there was no snow on the upper part of the Peak; though all the "Cumbree," or moderately high land over the rest of the island, was whitened with it in the usual manner for the season. But within the past few days, "fire, like three great bonfires" had been seen on the summit of the Peak, and a lava stream had begun to flow down from it.

Now this is interesting both chronologically, and chorographically. Chronologically, I had remarked at p. 150 of my little book "Teneriffe an Astronomer's Experiment," (published in 1858), that the lava eruptions there only break out about once in a century; the last eruption having occurred in 1798, and the previous one in 1703; and now we have one in 1883, but in what part of the mountainous island called Teneriffe has this last eruption appeared?

So far as I can gather from the said private letter, it has issued, if not from the very mouth of the craterlet which forms the tip-top of the Peak, yet from its sides or foot where it stands on a filled up crater of much larger size, otherwise to be looked on as the Peak's proper and effective summit; and it is from that crater's lips that have proceeded all the later, and yet prehistoric, streams of black lava, which score and frill the Peak on every side; and contrast so strikingly with the far more ancient red, and the still more ancient, more abundant, and once hotter yellow streams from the older and larger craters lower down, before ever yet, the Peak, or final cinder heap, was formed.

But though in the Nature-primeval history of the Mountain, the black, unoxydised lava streams of the Peak, were its latest exudations, still nothing more of that kind was locally expected to occur there within the human period. This was partly because no addition to them had been made since the Spanish Conquest; and partly because the lava outflow of 1798 avoided the Peak, and broke out on the Western side of the general mountain mass, while the eruptions of 1703, which threatened the town of Guimar to the south, and destroyed Garachico to the north, filling up its once beautiful bay—broke forth nearer the sea-level than the peak's top. Whence the idea arose, that the central vent of the peak must have clogged up with time, and that nothing more than its merry little jets of steam and sulphurous acid were to be looked for in that quarter; yet now we are told of red hot lava pouring forth.

Nevertheless on the whole, and in the long course of time, the forces of the grand old volcano may be dying out. For in an earlier work than any other that I had ever met with before about Teneriffe, I have lately read a very different account of the average state of the summit crater, to what it has been in, ever since the days of modern travelling and visitation began.

The book I allude to, in the possession of the Earl of Crawford and Balcarres, is an exquisitely illuminated MS. volume in vellum, by the Chevalier Edmund Skory, of the date of about 1582, and dedicated to that name so dear to all the students of Natural Science, viz.:

"Sir Frances Bacon,
"the knower and lover of all good Arts."

The very first dipping into its old MS. pages brought out a quaint proof of its antiquity, by its involuntary allusions to Garachico, as a city that was necessarily the island's chief delight and glory; the seat of its Government, the abode of its commerce, the place of all its shipping, and of course, because it was so prosperous, destined to live a queen for ever, and to be the joy of all peoples. Yet it is now, and has been for nearly two centuries as deserted as another Tyre; hardly fit to be the habitation of foxes, a mere howling wilderness of black rocks, for a few fishermen to spread their nets upon.

This happily preserved author then in the Earl's valuable library, who had abundant experience of Teneriffe more than a century previous to Garachico's Herculaneum fate, speaks of—

"Great stones being, with noyse, fyre and smoke, many "times cast forthe" out of the craterlet on the top of the peak.

Also that, "On the sommer time the fyers doe ofte breake forth from out the hole in the topp of this hill; into which, if you throw a great stone, it soundeth as if a great weight had fallen upon infinite store of hollow Brasse."

C. PIAZZI SMYTH

JOHANN BENEDICT LISTING

ONE of the few remaining links that still continued to connect our time with that in which Gauss had made Göttingen one of the chief intellectual centres of the civilised world has just been broken by the death of Listing

If a man's services to science were to be judged by the mere number of his published papers, Listing would not stand very high. He published little, and (it would seem) was even indebted to another for the publication of the discovery by which he is most widely known. This is what is called, in Physiological Optics, *Listing's Law*. Stripped of mere technicalities, the law asserts that if a person whose head remains fixed turns his eyes from an object situated directly in front of the face to another, the final position of each eye-ball is such as would have been produced by rotation round an axis perpendicular alike to the ray by which the first object was seen and to that by which the second is seen. "Let us call that line in the retina, upon which the visible horizon is portrayed when we look, with upright head, straight at the visible horizon, the horizon of the retina. Now any ordinary person would naturally suppose that if we, keeping our head in an upright position, turn our eyes so as to look, say, up and to the right, the horizon of the retina would remain parallel to the real horizon. This is, however, not so. If we turn our eyes straight up or straight down, straight to the right or straight to the left, it is so, but not if we look up or down, and also to the right or to the left. In *these* cases there is a certain amount of what Helmholtz calls "wheel-turning" (*Rad-drehung*) of the eye, by which the horizon of the retina is tilted so as to make an angle with the real horizon. The relation of this "wheel-turning" to the above-described motion of the optic axis is expressed by Listing's law, in a perfectly simple way, a way so simple that it is only by going back to what we might have thought nature should have done, and from that point of view, looking at what the eye really does, and considering the complexity of the problem, that we see the ingenuity of Listing's law, which is simple in the extreme, and seems to agree with fact quite exactly, except in the case of very short-sighted eyes." The physiologists of the time, unable to make out these things for themselves, welcomed the assistance of the mathematician. And so it has always been in Germany. Few are entirely ignorant of the immense accessions which physical science owes to Helmholtz. Yet few are aware that he *became* a mathematician in order that he might be able to carry out properly his physiological researches. What a pregnant comment on the conduct of those "British geologists" who, not many years ago, treated with outspoken contempt Thomson's thermodynamic investigations into the admissible lengths of geological periods!

Passing over about a dozen short notes on various subjects (published chiefly in the Göttingen "*Nachrichten*"), we come to the two masterpieces, on which (unless, as we hope may prove to be the case, he have left much unpublished matter) Listing's fame must chiefly rest. They seem scarcely to have been noticed in this country, until attention was called to their contents by Clerk-Maxwell.

The first of these appeared in 1847, with the title *Vorstudien zur Topologie*. It formed part of a series, which unfortunately extended to only two volumes, called *Göttinger Studien*. The term Topology was introduced by Listing to distinguish what may be called qualitative geometry from the ordinary geometry in which quantitative relations chiefly are treated. The subject of knots furnishes a typical example of these merely qualitative relations. For, once a knot is made on a cord, and the free ends tied together, its nature remains unchangeable, so long as the continuity of the string is maintained, and is therefore totally independent of the actual or relative dimensions and form of any of its parts. Similarly when two endless cords are linked together. It seems not unlikely, though we can find no proof of it, that Listing was led to such researches by the advice or example of Gauss himself; for Gauss, so long ago as 1833, pointed out their connection with his favourite electromagnetic inquiries.

After a short introductory historical notice, which shows that next to nothing had then been done in his subject, Listing takes up the very interesting questions of Inversion (*Umkehrung*) and Perversion (*Verkehrung*) of a geometrical figure, with specially valuable applications to images as formed by various optical instruments. We cannot enter into details, but we paraphrase one of his examples, which is particularly instructive:—

"A man on the opposite bank of a quiet lake appears in the watery mirror perverted, while in an astronomical telescope he appears inverted. Although both images show the head down and the feet up, it is the dioptric one only which:—if we could examine it:—would, like the original, show the heart on the left side; for the catoptric image would show it on the right side. In type there is a difference between inverted letters and perverted ones. Thus the Roman V becomes, by inversion, the Greek Λ; the Roman R perverted becomes the Russian Я; the Roman L, perverted and inverted, becomes the Greek Γ. Compositors read perverted type without difficulty:—many newspaper readers in England can read inverted type. * * * The numerals on the scale of Gauss' Magnetometer must, in order to appear to the observer in their natural position, be both perverted and inverted, in consequence of the perversion by reflection and the inversion by the telescope."

Listing next takes up helices of various kinds, and discusses the question as to which kind of screws should be

called right-handed. His examples are chiefly taken from vegetable spirals, such as those of the tendrils of the convolvulus, the hop, the vine, &c., some from fir-cones, some from snail-shells, others from the "snail" in clock-work. He points out in great detail the confusion which has been introduced in botanical works by the want of a common nomenclature, and finally proposes to found such a nomenclature on the forms of the Greek δ and λ.

The consideration of double-threaded screws, twisted bundles of fibres, &c., leads to the general theory of paradromic winding. From this follow the properties of a large class of knots which form "clear coils." A special example of these, given by Listing for threads, is the well-known juggler's trick of slitting a ring-formed band up the middle, through its whole length, so that instead of separating into two parts, it remains in a continuous ring. For this purpose it is only necessary to give a strip of paper one *half*-twist before pasting the ends together. If three half-twists be given, the paper still remains a continuous band after slitting, but it cannot be opened into a ring, it is in fact a trefoil knot. This remark of Listing's forms the sole basis of a work which recently had a large sale in Vienna:—showing how, in emulation of the celebrated Slade, to tie an irreducible knot on an endless string!

Listing next gives a few examples of the application of his method to knots. It is greatly to be regretted that this part of his paper is so very brief; and that the opportunity to which he deferred farther development seems never to have arrived. The methods he has given are, as is expressly stated by himself, only of limited application. There seems to be little doubt, however, that he was the first to make any really successful attempt to overcome even the preliminary difficulties of this unique and exceedingly perplexing subject.

The paper next gives examples of the curious problem:—Given a figure consisting of lines, what is the smallest number of *continuous* strokes of the pen by which it can be described, no part of a line being gone over more than once? Thus, for instance, the lines bounding the 64 squares of a chess-board can be drawn at 14 separate pen-strokes. The solution of all such questions depends at once on the enumeration of the points of the complex figure at which an odd number of lines meet.

Then we have the question of the "area" of the projection of a knotted curve on a plane; that of the number of interlinkings of the orbits of the asteroids; and finally some remarks on hemihedry in crystals. This paper, which is throughout elementary, deserves careful translation into English very much more than do many German writings on which that distinction has been conferred.

We have left little space to notice Listing's greatest work, *Der Census räumlicher Complexe* (Göttingen *Abhandlungen*, 1861). This is the less to be regretted, because, as a whole, it is far too profound to be made popular; and, besides, a fair idea of the nature of its contents can be obtained from the introductory chapter of Maxwell's great work on Electricity. For there the importance of Listing's Cyclosis, Periphractic Regions, &c., is fully recognised.

One point, however, which Maxwell did not require, we may briefly mention.

In most works on Trigonometry there is given what is called *Euler's Theorem about polyhedra*:—viz. that if S be the number of solid angles of a polyhedron (not self-cutting), F the number of its faces, and E the number of its edges, then

$$S + F = E + 2.$$

The puzzle with us, when we were beginning mathematics, used to be "What is this mysterious 2, and how came it into the formula?" Listing shows that this is a mere case of a much more general theorem in which corners, edges, faces, *and regions of space*, have a homogeneous numerical relation. Thus the mysterious 2, in Euler's formula, belongs to the two regions of space:—the one inclosed by the polyhedron, the other (the *Amplexum*, as Listing calls it) being the rest of infinite space. The reader, who wishes to have an elementary notion of the higher forms of problems treated by Listing, is advised to investigate the modification which Euler's formula would undergo if the polyhedron were (on the whole) ring-shaped:—as, for instance, an anchor-ring, or a plane slice of a thick cylindrical tube. P. G. T.

CLAUDE BERNARD

UNDER the title of "Notes et Souvenirs sur Claude Bernard," Prof. Jousset de Bellesme, of the School of Medicine of Nantes, has published an interesting sketch of the life and labours of the great French physiologist, his master, which those who are admirers of Claude Bernard will be glad to have their attention called to. The essay was meant for the opening address to be delivered at the commencement of the present session of the Nantes School. It seems to have been a little too outspoken to meet with the approbation of the director of the school. On the representation of a majority of the professors of the school, it was forbidden to be delivered *ex cathedrâ* by the Minister of Public Instruction, in an Order dated October 28, 1882. In the pages of the November number of the *Revue Internationale des Sciences biologiques*, the address appeals in type to a wider audience than the assembled professors and pupils of the School of Nantes. Commencing with an extremely graphic account of the author's first introduction to Claude Bernard, which concludes as follows:—"With a kind gesture of his head he bid me attend his laboratory; I thanked him, and was retiring. Just as I was about to close the door, he, taking his attention off his experiment, turned his eyes upon me and said, 'Have you read Descartes' "Discours de la Méthode?" Read it, and read it again." At the time of this interview Claude Bernard was in his forty-fifth year, and a great number of his striking works had been achieved. Having assisted for many years with astonishment at the apparently inexhaustible series of discoveries, Bellesme ventured to ask him one day, what was the secret which enabled him to penetrate so easily into things hidden from others. "Do not seek for a mystery," said Bernard, "nothing can be simpler, or less mysterious. My secret is open to all. When I was a young man, I lived greedily on the writings of Descartes. His 'Discourse' always completely satisfied my soul, and I was passionately fond of it. His rules appeared to me so just, that I came to the conclusion that by a strict observance of them all questions might be solved. That is all." The most important of these rules, Bellesme reminds his readers, is as follows:—"Ne recevoir jamais aucune chose pour vraie qu'on ne la connaisse évidemment être telle, éviter soigneusement la Précipitation et la Prévention dans ses jugements." The author, then, in a very striking manner, draws a series of comparisons between Descartes and Cl. Bernard. Passing from this, he criticises somewhat severely the tendency of a modern school, which without taking notice of the complexitiness of biological phenomena, seem to have culminated in the idea that no contagious disease can be conceived of which has not some special microbe as its cause; but the disciples of this school, he urges, have not meditated on the third rule of Descartes: "Conduire par ordre ses pensées, en commençant par les objets les plus simples et les plus aisés à connaître, pour monter peu à peu comme par degrés jusqu'à la connaissance des plus composés."

We are afforded a little glimpse of the private life of the great French physiologist, which explains a sadness

about his domestic relations—possibly not understood by many of his foreign admirers and friends. Married late in life—and even in his very youth never having had much place in his mind for love—still his agreeable and quiet character, his inexhaustible kindness, his open frank cordiality, which so often secured the sympathy of others, seemed to promise an abiding union between him and his wife, but the liberal ideas of the husband, and his devotion to his very peculiar studies, did not please Madame Bernard. The state of things became irritable—intolerable; even the birth of two children did not improve the condition of affairs. In 1869 the separation came. The husband and the father was left alone; and from then to the end of his days he lived his solitary life in an apartment in the rue des Écoles, *vis à vis* to the College of France. His life was all too full of work to leave much time for a morbid appreciation of his solitude. Some slight rest was taken each year at the vintage period at Saint Julien, near Villefranche, and he almost every year took part in the French Association for the Advancement of Science, an Association which he assisted in founding, and of which he was the first president. During these latter ten years Bellesme was his very constant visitor, his trusty friend. They were times not to be recalled, he tells us, without emotion, and he regards them as among the happiest of his life. Often he would spend the evening with him by his fire-side in the small bedroom, where by preference he would pass the afternoon, and which his old servant would keep with a quite canonical neatness. In the background was the bed with its curtains of blue damask, to the left the fireplace; at the side of the bed, a large armchair in which Claude Bernard would sit enveloped in a dressing-gown, which, on his ample shoulders, took the folds and plaits of an ancient toga; his head covered with a cap, which he would often remove while talking, with an action peculiar to him, as if his thoughts made him find it too tight. Close to him, opposite the fire, a small square table, on which the lamp is placed amidst a mountain of reviews, *brochures*, new books sent to him from all parts. At this epoch of his life he read, however, but little, nor did he write much. The volumes, which were published during these last ten years, were composed of extempore lectures of his, very carefully edited. "With our feet on the fender," writes Bellesme, "our conversation would begin with the striking events of the day, but speedily we turned to physiology. This was almost the sole object of the master's thoughts. About this he would wax eloquent, and speedily we would be entering on the higher regions of the science. These were charming excursions on the very mountain-tops, with the clear light of his mind illuminating all the dark valleys." No wonder that time was little thought of, or often altogether forgotten.

Up to 1865 Claude Bernard's health was excellent. About then he was attacked by an ill-defined chronic enteritis, from which, after eighteen months, he had only recovered. After this he had some rheumatic attacks, which did not frighten his friends, as he still preserved an alas deceitful appearance of vigorous health. Still nothing seemed to presage his approaching end. Towards the last days of 1877, after passing a long morning in the damp and unhealthy laboratory of the College of France, he returned home shivering, and with a feeling of intense uneasiness. The next day nephritis set in; he kept his room, and was not disquieted as to his state, but after a few days it was evident to all that his career was run. On February 7, 1878, after a six weeks of suffering, he lost all consciousness, and expired on February 10, at half-past nine o'clock in the evening. In Claude Bernard France lost a noble son, one who cultivated science purely and disinterestedly. His works will not ever perish, and in future years they will serve as a demonstration of the excellence of the "Discours de la Méthode," and as a very sure guide towards arriving at a knowledge of truth.

E. P. W.

THE FINSBURY TECHNICAL COLLEGE

THE Finsbury Technical College and the programme of instruction which we have recently received represent a *fait accompli* of the City and Guilds of London Institute.

Judging of the education to be given in the new College from the Programme forwarded to us, we may congratulate the Council of the Institute on having steered clear of the Scylla and Charybdis which overhang the narrow channel of technical education proper. In all such educational movements, there is the danger that the teaching shall either be too exclusively of the ordinary scientific type, or, by being too distinctly practical, shall attempt to take the place of workshop instruction. Theory and practice promise to be judiciously combined in the new school, and the experiment about to be tried in Tabernacle Row is interesting not only as a new departure in education, but also as showing the effect of beginning science teaching from the practical rather than from the theoretical side, as is still so frequently the case.

During the last three years the conception of the Finsbury College has undergone considerable development, and corresponds now much more nearly to what a technical school should be than appeared probable at its inception. According to the plans published in March, 1880, in the Report to the Governors, the College was to consist in the first place of chemical and physical laboratories only. These laboratories were to be adapted to instruction in various departments of applied chemistry and physics, but no provision was made for the teaching of mechanics, drawing, or of other subjects which find a place in the new programme. Such a school would scarcely have realised the idea of a technical college properly so called, least of all a college for the instruction of artisans. It is doubtful whether many of the pupils who frequent the excellent classes of Prof. Ayrton and Prof. Armstrong are really of the artisan class, for which instruction was originally intended to be given by the City Guilds. The progress that is being made in the completion of the Central Institution at South Kensington, which is expressly intended for the education of a higher class of students, renders it the more important, in order that the two schools may not clash with one another, that the instruction at Finsbury should be not only nominally, but really, of a different grade, and adapted to the improvement of artizans and workpeople.

The programme recently published shows that provision has been made for other branches of industry besides electrical lighting and technical chemistry.

The Technical College, Finsbury, consists really of two distinct schools: a day school and an evening school. It has for its objects the education of—

(1) Persons of either sex who wish to receive a scientific and practical preparatory training for intermediate posts in industrial works.

(2) Apprentices, journeymen, and foremen who are engaged during the day-time, and who desire to receive supplementary instruction in the art practice, and in the theory and principles of science connected with the industry in which they are engaged.

(3) Pupils from middle class and other schools who are preparing for the higher scientific and technical courses of instruction to be pursued at the Central Institution.

The College therefore fulfils the functions of a finishing technical school for those entering industrial life at a comparatively early age; of a supplemental school for those already engaged in the factory or workshop; and of a preparatory school for the Central Institution.

The College embraces the following four chief departments: (1) Mathematical and Mechanical; (2) Physical; (3) Chemical; (4) Applied Art.

It is under the general direction of a principal or superintendent of studies; and the Council of the Institute

appear to have acted wisely in asking Mr. Philip Magnus, who has directed the work of the Institute up to the present time with so much ability, and whose exceptional experience of Continental technical schools renders him particularly fitting for such a position—to occupy this post, pending the completion of the Central Institution, and to carry into effect the general scheme of instruction indicated in the programme.

In the day school of the Finsbury College, pupils from middle class and higher elementary schools will have the opportunity of continuing their studies, and of preparing, at the same time, for the particular branch of industry in which they purpose to be engaged.

Such a school is a technical school in the true sense of the word, for it gives the pupil the best training he can receive for his future occupation.

The instruction is not limited to the application of one branch of science only; the future electrician is taught chemistry and mechanics, the chemist is taught mechanics and physics, the mechanician is taught physics and chemistry, and, what is almost equally important, all are taught drawing, French, German, and the manipulation of tools in the workshops.

The evening school is intended for those who are already engaged in practical work, and in this department of the College noteworthy changes have been introduced, with a view of adapting the teaching to the special requirements of artisans. To the courses of Applied Physics and Chemistry originally provided for, courses of Mechanical Engineering have been added; but besides these courses, which are adapted to the higher class of artisans, a complete syllabus of instruction has been added to the programme, suited to the requirements of the special industry of the district of Finsbury, viz. cabinet-making. To provide a systematic course of instruction for cabinet-makers it was necessary to add to the other departments of the College, a Department of Applied Art; and in order to secure a good number of students to start with, the Council affiliated to the College the City School of Art, one of the oldest art schools of the country, and appointed Mr. Brophy as head master.

Moreover, to satisfy the demand of workmen engaged in numerous small industries, the Council have arranged courses of instruction, on a more systematic basis than has been previously attempted in this country, for carpenters, joiners, metal-plate workers, bricklayers, &c., thereby supplying that popular element in the instruction provided by the City Guilds, which at first seemed likely to be wanting in their scheme of technical education.

By undertaking to admit apprentices to the evening classes at half the fees, which are small enough, charged to ordinary workmen, those who have had the direction of the work of the Institute have shown a just appreciation of the importance of encouraging apprentices of fifteen to twenty to follow the evening courses of instruction; for there will be far less difficulty in inducing youths, during their apprenticeship, to attend regular systematically-arranged lessons, covering a period of two or three years, than is generally found in the case of adult workmen.

Indeed, it is in the arrangement of systematised and progressive courses of instruction adapted to various industries and involving the application both of science and of art to the student's occupation, as well as in the practical methods of instruction adopted, that the Technical College, Finsbury, is differentiated from other science schools.

The programme of studies now before us is a publication that can hardly fail to prove useful to all persons who are interested in the establishment of technical schools, and shows unmistakably that the Council of the Institute and their advisers are fully conscious of the difficulties that beset the problem of technical education, and may be trusted to deal judiciously with them in the schools established under their direction.

The fittings of the new College, which are most complete and admirably adapted for practical teaching, have been designed and executed under the direction of Professors Armstrong, Ayrton, and Perry.

ON THE GRADUATION OF GALVANOMETERS FOR THE MEASUREMENT OF CURRENTS AND POTENTIALS IN ABSOLUTE MEASURE[1]

III.

THE determination of H and the measurement of a current in absolute units, can be effected simultaneously by the method devised by Kohlrausch, and described in the *Philosophical Magazine*, vol. xxxix. 1870. This method consists essentially in sending the current to be measured through two coils, of which all the constants are accurately known. One of these is the coil of a standard galvanometer, the other is a coil hung by a bifilar suspension, the wires of which convey the current into the coil. The latter coil rests in equilibrium when no current is passing through it, with its plane in the magnetic meridian. When a current is sent through it, it is acted on by a couple due to electro-magnetic action between the current and the horizontal component of the earth's force, which tends to set it with its plane at right angles to the magnetic meridian; and this couple is resisted by the action of the bifilar. The coil comes to rest, making a certain angle with the magnetic meridian, and as the couple exerted by the bifilar suspension for any angle is supposed to have been determined by experiment, a relation between the value of H and the value of the current is obtained. But, as the same current is sent through the coil of the standard galvanometer, the observed deflection of the needle of that instrument gives another relation between H and C. From the two equations expressing these relations the values of H and C can be found. Full details of the construction of Kohlrausch's apparatus and of the calculation of its constants will be found in the paper above referred to.

In this method it is assumed that the value of H is the same at both instruments, an assumption which for rooms not specially constructed for magnetic experiments cannot safely be made. An instrument which is not liable to this objection has been suggested by Sir William Thomson. A short account of this instrument and its theory will be found in Maxwell's "Electricity and Magnetism," vol. ii. p. 328.

In the application of what has gone before to the graduation of galvanometers, we shall have to deal with the quantities resistance and potential, and in our calculations to measure potentials in volts, resistances in ohms, and currents in amperes. A full explanation of the terms resistance and potential would require a treatise on electricity, but perhaps a very short explanation of what is meant by a volt, by an ohm, and by an ampere may not be here out of place.

Two conductors are at different potentials when, on their being put in contact, electricity passes from one to the other. The difference of potential between them will be made manifest if one of them be connected with an electrically insulated plate which forms one of the scales of a delicate balance, and the other with a second insulated plate parallel to, and at a very small distance from the first plate. If the conductors be at different potentials the plates will attract one another, and the force of attraction may be weighed by means of the balance. With certain arrangements to ensure accuracy, a balance may be constructed by means of which the difference of potentials between two conductors can be measured. Such an instrument has been made by Sir William Thomson, and called by him an Absolute Electrometer.

[1] Continued from p. 108.

It is found experimentally by measuring with a delicate electrometer that, between any two cross-sections A and B of a homogeneous wire, in which a uniform current of electricity is kept flowing by any means, there exists a difference of potentials, and that if the wire be of uniform section throughout, the difference of potentials is in direct proportion to the length of wire between the cross-sections. It is found further that if the difference of potentials between A and B is kept constant, and the length of wire between them is altered, the strength of the current varies inversely as the length of the wire. The strength of the current is thus diminished when the length of the wire is increased, and hence the wire is said to oppose *resistance* to the current; and the resistance between any two cross-sections is proportional to the length of wire connecting them. If the length of wire and the difference of potentials between A and B be kept the same, while the cross-sectional area of the wire is increased or diminished, the current is increased or diminished in the same ratio; and therefore the resistance of a wire is said to be inversely as its cross-sectional area. Again, if for any particular wire, measurements of the current strength in it be made for various measured differences of potentials between its two ends, the current strengths are found to be in simple proportion to the differences of potential so long as there is no sensible heating of the wire. Hence we have the law, due to Ohm, which connects the current C flowing in a wire of resistance R, between the two ends of which a difference of potential V is maintained,

$$C = \frac{V}{R} \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

In this equation the units in which any one of the three quantities is expressed depend on those chosen for the other two. We have defined unit current, and have seen how to measure currents in absolute units; and we have now to show how the absolute units of V and R are to be defined, and from them and the absolute unit of current to derive the practical units—volt, ampere, coulomb, and ohm.

We shall define the absolute units of potential and resistance by a reference to the action of a very simple but ideal magneto-electric machine, of which, however, the modern dynamo is merely a practical realisation. First of all let us imagine a uniform magnetic field of unit intensity. The lines of force in that field are everywhere parallel to one another: to fix the ideas let them be vertical. Now imagine two straight horizontal metallic rails running parallel to one another, and connected together by a sliding bar, which can be carried along with its two ends in contact with them. Also let the rails be connected by means of a wire so that a complete conducting circuit is formed. Suppose the rails, slider, and wire to be all made of the same material, and the length and cross-sectional area of the wire to be such that its resistance is very great in comparison with that of the rest of the circuit, so that, when the slider is moved with any given velocity, the resistance in the circuit remains practically constant. When the slider is moved along the rails it cuts across the lines of force, and so long as it moves with uniform velocity a constant difference of potentials is maintained between its two ends, and a uniform current flows in the wire from the rail which is at the higher potential to that which is at the lower. If the direction of the lines of force be the same as the direction of the vertical component of the earth's magnetic force in the northern hemisphere, so that a blue pole placed in the field would be moved upwards, and if the rails run south and north, the current when the slider is moved northwards will flow from the east rail to the west through the slider, and from the west rail to the east through the wire. If the velocity of the slider be increased the difference of potentials between the rails, or, as it is otherwise called, the electromotive force producing the current, is increased in the same ratio; and therefore by Ohm's law so also is the current. Generally for a slider arranged as we have imagined, and made to move across the lines of force of a magnetic field, the difference of potentials produced would be directly as the field intensity, as the length of the slider, and as the velocity with which the slider cuts across the lines of force. The difference of potentials produced therefore varies as the product of these three quantities; and when each of these is unity, the difference of potentials is taken as unity also. We may write therefore $V = ILv$, where I is the field intensity, L the length of the slider, and v its velocity. Hence if the intensity of the field we have imagined be 1 c.g.s. unit, the distance between the rails 1 cm., and the velocity of the slider 1 cm. per second, the difference of potentials produced will be 1 c.g.s. unit.

This difference of potentials is so small as to be inconvenient for use as a practical unit, and instead of it the difference of potentials which would be produced if, everything else remaining the same, the slider had a velocity of 100,000,000 cms. per second, is taken as the practical unit of electromotive force, and is called one *volt*. It is a little less than the difference of potentials which exists between the two insulated poles of a Daniell's cell.

We have imagined the rails to be connected by a wire of very great resistance in comparison with that of the rest of the circuit, and have supposed the length of this wire to have remained constant. But from what we have seen above, the effect of increasing the length of the wire, the speed of the slider remaining the same, would be to diminish the current in the ratio in which the resistance is increased, and a correspondingly greater speed of the slider would be necessary to maintain the current at the same strength. We may therefore take the speed of the slider as measuring the resistance of the wire. Now suppose that when the slider 1 cm. long was moving at the rate of 1 cm. per second, the current in the wire was 1 c.g.s. unit; the resistance of the wire was then 1 c.g.s. unit of resistance. Unit resistance therefore corresponds to a velocity of 1 cm. per second. This resistance, however, is too small to be practically useful, and a resistance 1,000,000,000 times as great, that is, the resistance of a wire, to maintain 1 c.g.s. unit of current in which it would be necessary that the slider should move with a velocity of 1,000,000,000 cms. (approximately the length of a quadrant of the earth from the equator to either pole) per second, is taken as the practical unit of resistance, and called one *ohm*.

In reducing the numerical expressions of physical quantities from a system involving one set of fundamental units to a system involving another set, as for instance from the British foot-grain-second system, formerly in use for the expression of magnetic quantities, to the c.g.s. system, it is necessary to determine, according to the theory first given by Fourier, and extended to electrical and magnetic quantities by Maxwell, for each a certain reducing factor, by substituting in the formula, which states the relation of the fundamental units to one another in the expression of the quantity, the value of the units we are reducing from in terms of those we are reducing to. For example, in reducing a velocity say from miles per hour, to centimetres per second, we have to multiply the number expressing the velocity in the former units by the number of centimetres in a mile, and divide the result by the number of seconds in an hour; that is, we have to multiply by the ratio of the number of centimetres in a mile to the number of seconds in an hour. The multiplier therefore, or *change-ratio* as it has been called by Professor James Thomson, is for velocity simply the number of the new units of velocity equivalent to one of the old units, and may be expressed by the formula $\frac{L}{T}$, where L is the number of new units of length contained in one of the old, and T is the corresponding number for the unit of time. In the same way the

change-ratio for rate of change of velocity or acceleration is $\frac{L}{T}$; and the change-ratio of any other physical quantity may be found by determining from its definition the manner in which its unit involves the fundamental units of mass, length and time. Now the theory of the change-ratios of electrical and magnetic quantities, in the electro-magnetic system of units, shows that the change-ratio for resistance is the same as that for velocity; that in fact a resistance in electro-magnetic measure is expressible as a velocity; and hence we may with propriety speak of a resistance of one ohm as a velocity of 10^9 centimetres per second.

It is obvious from equation (14) that if V and R, each initially one unit, be increased in the same ratio, C will remain one unit of current; but that of V be, for example, 10^8 c.g.s. units of potential, or one volt, and R be a resistance of 10^9 cms. per second, or one ohm, C will be one-tenth of one c.g.s. unit of current. A current of this strength—that is, the current flowing in a wire of resistance one ohm, between the two ends of which a difference of potentials of one volt is maintained,—has been adopted as the practical unit of current and called one *ampere*. Hence it is to be remembered one ampere is one-tenth of one c.g.s. unit of current.

The amount of electricity conveyed in one second by a current of one ampere is called one *coulomb*. This unit although not quite so frequently required as the others, is very useful, as, for instance, for expressing the quantities of electricity which a secondary cell is capable of yielding in various circumstances. For example, in comparing different cells with one another their capacities, or the total quantities of electricity they are capable of yielding when fully charged, are very conveniently reckoned in coulombs per square centimetre of the area across which the electolytic action in each takes place.

The magneto-electric machine we have imagined gives us a very simple proof of the relation between the work done in maintaining a current, the strength of the current, and the electromotive force producing it. By the definitions given above of a magnetic pole and a magnetic field, a unit pole must produce at unit distance from itself a magnetic field of unit intensity. Again, unit current is defined as that current which flowing in a wire of unit length, bent into an arc of a circle of unit radius, acts on a unit magnetic pole at the centre of the circle with unit force. Hence, as the reaction of the pole on the current must be equal to the action of the current on the pole, this wire carrying the current is acted on by unit force tending to move it in the opposite direction to that in which the pole is moved, and it plainly does not matter which we suppose held fixed and which moved. Therefore a conductor in a magnetic field, and carrying a unit current which flows at right angles to the lines of force, is acted on by a force tending to move it in a direction at right angles to its length, and the magnitude of this force for unit length of conductor, and unit field, is by the definition of unit current equal to unity.

Applying this to our slider in which we may suppose a current of strength C to be kept flowing, say, from a battery in the circuit, let L be the length of the slider, v its velocity, and I the intensity of the field; we have for the force on the moving conductor the value ILC. Hence the rate at which work is done by the electro-magnetic action between the current and the field is $ILC\frac{dx}{dt}$ or $ILCv$, and this must be equal to the rate at which work is done in generating by motion of the slider a current of strength C. But as we have seen above ILv is the electromotive force produced by the motion of the slider. Calling this now E, the symbol usually employed to denote electromotive force, we have EC as the rate of working, that is, the rate at which electrical energy is given out in the circuit.

By Ohm's law this value for the rate of working may be put into either of the two other forms, namely: $\frac{E^2}{R}$, or C^2R. In the latter of these forms the law was discovered by Joule, who measured the amount of heat generated in wires of different resistances by currents flowing through them. This law holds for every electric circuit whether of dynamo, battery, or thermoelectric arrangement.

We have, in what has gone before, supposed the slider to have no resistance comparable with the whole resistance in the circuit. If it has a resistance r, and R be the remainder of the resistance in circuit, the actual difference of potentials between its two ends will not be ILv or E, but $E\frac{R}{R+r}$. The rate per unit of time at which work is given out in the circuit is however still EC, of which the part $EC\frac{r}{R+r}$ is given out in the slider, and the remainder, $EC\frac{R}{R+r}$, in the remainder of the circuit. In short, if V be the actual difference of potentials, as measured by an electrometer, between two points in a metallic wire connecting the terminals of a battery or dynamo, and C be the current flowing in the wire, the rate at which energy is given out is VC, or if R be the resistance of the wire between the two points, C^2R.

One of the great advantages of the system of units of which I have given this brief sketch, is that it gives the value of the rate at which work is given out in the circuit, without its being necessary to introduce any co-efficient such as would have been necessary if the units had been arbitrarily chosen. When the quantities are measured in c.g.s. units, the value of EC is given in terms of the centimeter-dyne or *erg*, the recognized dynamical unit of work. Results thus expressed may be reduced to *horse-power* by dividing by the number $7{\cdot}46\times10^9$; or if E is measured in volts, and C in amperes, EC may be reduced to horse-power by dividing by 746. Thus, if 90 volts be maintained between the terminals of a pair of incandescent lamps joined in series, and a current of 1·3 ampere flows through these lamps, the rate at which energy is given out in the lamps is approximately ·157 horse-power.

ANDREW GRAY

(To be continued.)

NATURAL SCIENCE IN THE OPEN COMPETITIVE EXAMINATIONS FOR CLERKSHIPS (CLASS I.) IN THE CIVIL SERVICE

THE Civil Service Commissioners have done much to encourage the thorough study of natural science in our Universities by the weight which they have assigned to it in the competitive examinations for first-class clerkships in the Government service. These posts are of sufficient value to attract young men of one or two-and-twenty, fresh from the University. It will be seen from the list of marks assigned to subjects, which we print below, that 1000 marks may be made in two branches of natural science, for instance, Zoology and Geology; whilst Greek and Roman language, literature, and history only stand for 1500. Hence a candidate who makes science his strong side and can do something in either English, classical, or foreign literary subjects, is by no means at a disadvantage.

We take this opportunity of prominently drawing attention to the encouragement thus given to the pursuit of natural science as a branch of culture.

The schoolboy who is excused from verse-composition and sent into the chemical laboratory, is distinctly recognised, and has a fair chance given to him by the Commissioners. So too the Oxford undergraduate who breaks with the wearisome iteration of Greek play and Latin

odes in the College lecture-room and escapes to the fascinating microscopes and dissecting troughs of Prof. Moseley, or the verniers and milligram-weighing pans of Prof. Clifton, is marked out for patronage. And not only indeed are Oxford and Cambridge students of science thus benefited.

The courses of instruction in scientific subjects given at the London Colleges, University and King's, are pre-eminently such as will enable a candidate to do justice to his abilities in this examination. The examination is practical, and no mere smattering of a subject will obtain any marks for a candidate. Hence the "crammers" are at a disadvantage, and the teachers in duly-organised and properly-furnished laboratories, are rightly encouraged in their efforts to carry on thorough courses of instruction. It is indeed, a matter for satisfaction that hitherto the various cramming establishments where young men are "prepared" for public examinations have failed to enable any candidate to gain a success in any branch of natural science in these higher competitive examinations, those candidates who have scored marks in natural science having been University students. We subjoin an extract from the Regulations issued by the Civil Service Commission, to the secretary of which body application for further information should be made.

1. The limits of age for these situations are 18 and 24, and candidates must be of the prescribed age on the first day of the competitive examination.

2. At the competitive examinations exercises will be set in the following subjects only; the maximum of marks for each subject being fixed as follows, viz. :—

	Marks.
English Composition (including Précis-writing)... ...	500
History of England—including that of the Laws and Constitution...	500
English Language and Literature	500
Language, Literature, and History of Greece	750
,, ,, ,, Rome	750
,, ,, ,, France	375
,, ,, ,, Germany ...	375
,, ,, ,, Italy	375
Mathematics (pure and mixed)	1250
Natural Science: that is, (1) Chemistry, including Heat; (2) Electricity and Magnetism; (3) Geology and Mineralogy; (4) Zoology; (5) Botany	1000
, The total (1000) marks may be obtained by adequate proficiency in any two or more of the five branches of science included under this head.	
Moral Sciences: that is, Logic, Mental and Moral Philosophy	500
Jurisprudence	375
Political Economy	375

Candidates will be at liberty to offer themselves for examination in any or all of these subjects. No subjects are obligatory.

No candidate will be allowed any marks in respect of any subject of examination unless he shall be considered to possess a *competent knowledge* of that subject.

NOTES

A TELEGRAM, dated December 21, has been received by the Finnish Academy of Sciences from Prof. S. Lemström, chief of the Finnish Meteorological Observatory at Sodankylä. He states that, having placed a galvanic battery with conductors covering an area of 900 square metres on the hill of Oratunturi, he found the cone to be generally surrounded by a halo, yellow-white in colour, which faintly but perfectly yields the spectrum of the aurora borealis. This, he states, furnishes a direct proof of the electrical nature of the aurora, and opens a new field in the study of the physical condition of the earth. A further telegram, dated Sodankylä, January 5, has been received, in which Prof. Lemström states that experiments with the aurora borealis made December 29, in Enare, near Kultala, on the hill of Pietarintunturi, confirm the results of those at Oratunturi. On that date a straight beam of aurora was seen over the galvanic apparatus. It also appears from the magnetic observations that the terrestrial current ceases below the aurora arc, while the atmospheric current rapidly increases, but depends on the area of the galvanic apparatus to which it seems to be proportional. The Professor regrets that with the means at his disposal further experiments cannot be made, and that he intended, on the 13th inst., to withdraw the apparatus.

THE Report of the Royal Gardens, Kew, for 1881, shows what a large amount of varied and highly useful work is got through in the space of a year at that great national establishment; perhaps *imperial* would be more accurate than national, for it is really the botanical and horticultural centre of the whole empire. One important feature is the lessons given during the year to the young gardeners in the science of these subjects; this will certainly tend to secure that the work of the gardens throughout are conducted with intelligence and on a sound scientific basis. The Report contains extracts from the reports of various Colonial curators, on the progress of experiment in the culture of certain important plants, such as Cinchona and india-rubber. Mr. Jamieson reports from the Nilgiris that he has found the Cape Coast and Liberian coffee-plants to be really two varieties. Queensland may yet add coffee to its other industries, a vastly important addition. The Report contains an illustration of *Cinchona Ledgeriana*, Moens.

IN preparation for the International Fisheries Exhibition there is a large number of artificers now employed in erecting and completing enormous buildings for the reception of the exhibits on the ground known as the Royal Horticultural Gardens, South Kensington. Some four or five immense structures have been already erected, two standing side by side on the western side of the gardens—one being about 180 yards, the other some 140 yards in length, with a width of about 20 yards, and of great height and capacity. Arched roofs contain in the centre, running the whole length of the building, a wide breadth of glass, which throws below as ample an amount of light as can be desired. Other similar buildings are in the course of completion at the north-eastern corner of the gardens, close to the Albert Hall; and when the capacity of all these structures is considered, some estimate can be formed of the enormous proportions the International Exhibition will assume. The arcade at the south-western side of the gardens, well known for the horticultural and other expositions which the Royal Horticultural Society has held in it, is being devoted to the purposes of an aquarium, which will soon be completed, and in which both fresh-water and sea fish will be exhibited. The spacious long arcade affords ample room for all the tanks that may be required, and it is expected that the aquarium will form one of the most attractive features of the exhibition. Arrangements will be made to provide easy access from one building to another, and such portions of the gardens as remain uncovered by the necessary structures will serve as an agreeable promenade. All the works are so forward that everything will be ready in good time for the reception of the exhibits of our own and of foreign countries.

CONSIDERABLE success has attended the Sunday Evening Association, its object being to bring together all persons who, estimating highly the elevating influence of music, the sister arts, literature and science, desire, by means of meetings on Sunday evenings, to see them more fully identified with the religious life of the people. The president is Dr. Geo. J. Romanes, F.R.S. The fifth series of meetings will be concluded next Sunday with a lecture by Dr. W. B. Carpenter, F.R.S., C.B., on "Niagara." A sixth series will be commenced on Sunday, February 11, and will include lectures by Dr. G. J. Romanes, F.R.S., on "Star

Fish; " J. Cotter Morison, M.A., on "A Glimpse of England in the Fifteenth Century;" Dr. P. Martin Duncan, F.R.S., on "Metamorphosis of Insects," and J. Norman Lockyer, F.R.S., on "The Recent Eclipse of the Sun." The meetings are held in the Working Men's College, Great Ormond Street.

THE Report of the Commissioner of the Imperial Japanese Mint, Osaka, for the year ending June, 1882, being the twelfth report of the Japanese Mint, shows that the high standard of excellence of the work done at this establishment is still kept up. Rather more gold was coined than during the previous year, viz. 803,645 yen, all in 5 yen pieces; the silver coined during this year was all 1 yen pieces, and amounted to 3,294,988 yen; whilst the nominal value of the copper coins, in 2 sen, 1 sen, and half sen pieces was 1,130,548 yen. The total nominal value of the coins of all denominations struck since the commencement of the Mint to the end of the last financial year is 102,888,478 yen, of which more than one-half is gold and two-fifths silver. Besides this a large number of medals have been struck and refined ingots produced. This year a large number of old bronze guns and field pieces have been melted down, refined, and converted into copper coins, and also additional improvements and economies have been made in the treatment of old Japanese silver coins prior to their re-coinage. The sulphuric acid works in connection with the Mint have been more busy than last year, and nearly a million pounds of acid have been exported to China in addition to that produced for home consumption. The soda works are now in working order, and a considerable outturn of sulphate, black ash, white ash, and crystallised soda has been made; caustic and bicarbonate of soda will shortly be produced, and it is proposed to add works for the production of bleaching powder so as to utilise the whole of the hydrochloric acid formed. There was a considerable increase in the amount of Corean gold dust received during the year, but it was not generally of a high standard. The curve showing the variation in weight of the silver yen issued, as also the report of the trial of the pyx and the reports of the assays on the pyx pieces made by Prof. Chandler Roberts of the Mint in this country, and by Mr. Lawner, of the American Mint, show that the greatest care and attention is given to every department, both by the foreign *employés*, Mr. Wm. Gowland, chemist, assayer, and technical adviser, and Mr. R. MacLagan, engineer, and also by the native officials. The report affords abundant evidence that excellent work is being done by the above-named European technical advisers of the Japanese Government.

WE have received an excellent little pamphlet on "The Rudiments of Cookery, with some Account of Food and its Uses." It is called a manual for the use of schools and homes, is written by "A. C. M.," examiner to the Northern Union of Training Schools for Cookery, dedicated to the Countess of Derby, and published by Simpkin and Marshall. Besides conveying practical information on plain cookery, the writer is careful throughout to explain the why and the wherefore of every point by briefly stating the principles of elementary science which bear upon the subject. We can recommend the pamphlet to the "schools and homes" for whose use it is designed.

AT the meeting of the Royal Geographical Society on Monday evening, Sir Henry Rawlinson, who presided, stated that Mr. Leigh Smith, in acknowledgment of the assistance which the Royal Geographical Society had afforded him in fitting out his expedition, and also to mark the extent of the interest he takes in Arctic discovery, had presented 1000*l.* for the purpose of extended Arctic exploration. Sir Henry referred briefly, also, to the recent services of one of the native explorers which the Indian Government are in the habit of sending beyond the Himalayas, which are closed to Europeans by the jealousy of the natives. The paper from which he quoted said: "One of General Walker's native explorers has returned to India after an absence of four years through Thibet, in which he has obtained a large amount of new geographical information, and has finally disposed of the question of the Sanpo River, which does not, according to him, fall into the Irawaddy, as was generally supposed. The traveller got as far north as Santu, lat. 40° N., 92° E., which is supposed to be the Sorchia of Marco Polo. Returning, he proceeded to Batang, and tried to reach Assam by the direct route, but was stopped at the frontier of the Mishmi country by the assurance that the natives were savages, who would murder him. He, therefore, took a circuitous route to Lhassa, *via* Alanto and Gjamda. But from the latter place he turned and made for Chetang, on the Sanpo, thence by Giangze, Leng, and Phari, to Darjeeling. He reports that Sama is the place where two Europeans coming from Assam were murdered some thirty years ago. If so, it must be Wilcox's Simé, where the priests Kirch and Bsury were murdered in 1854. He is positive he only crossed the Sanpo once at Chetang. He says that on the road from Sama to Gjamda there is a great range of hills to the west, separating the basin of the affluents of the Sanpo, from that of the affluents of the Irawaddy to the east."

LIEUT.-COL. BERESFORD LOVETT, her Majesty's Consul at Astrabad, read at the same meeting a paper, which was illustrated by an itinerary map from his plane table survey of four inches to the mile. The route from Teheran northwards to Asolat is well known, but new ground was traversed between Asolat and the Lur Valley, on the south of Mount Damavand, and again between the Horas River and Firnshuh, and onward to Kurrand, and also between Fulhad Mahala and Shu Kuh. The survey throws considerable light on the untrodden parts of the Elburz Mountains, and on the entire route no part of which had been previously delineated with any approach to accuracy. The author's route was from Teheran to Astrabad, *viâ* Ahar to Sarak, thence to Husan Ikdir, Gutchisir, Wohbad, Towar, and Arsmkern. The route was along the ridge of the Shamran mountain country, which runs south of the Caspian, the author desiring, as the journey was made in the middle of the summer heats, not to descend below 5000 feet, while on the journey an altitude of over 9000 feet was attained, and one mountain 12,500 feet was measured and ascended. The author found in one position a plateau of considerable height full of oyster shells, while in his paper and in the discussion which followed, it was shown that at one geological period the Caspian must have been a sea of very large extent to the north and east.

UNDER the presidency of the Marquis of Exeter, a National Fish Culture Association has been established, its object being to increase the supply of food by increasing the supply of fish of all kinds.

FROM the preliminary report of the Princeton Scientific Expedition (the third of its kind), whose ground was Wyoming, Colorado, and the west, it would seem that the students who formed the party covered a very considerable field, did some good work in geology and natural history, and endured just enough of hardship to give them the feeling of real explorers.

THE trial of the electro-magnetic engine, aërial screw, and bichromate elements constructed by MM. Tissandier for their directing balloon took place in their aëronautical work-shop at Point du Tour, on January 26, before a large number of electricians and aëronauts. It was shown that the twenty-four elements, each of which weighs about six kilogrammes, give during almost three hours a current which rotates a screw of 2·85m. diameter, and about 5 metres of path, with a velocity of 150 turns in a minute. The motive power really developed may be estimated at that of four horses per hour. The weight of all

the machinery and elements is a little less than 250 kilogrammes. The real effect on the air can only be found by experiments in the air, but according to measurements taken with a dynamometer of the horizontal tendency to motion, it is about the same as in the experiment tried by Dupuy de Lome. The motive power of Dupuy de Lome having been obtained with eight men working his large screw, whose diameter was 9 metres, it may be inferred that the results in the present case will be more advantageous in the ratio of *two and a half* to *one*. These results are not very powerful when compared with the immense power of aërial currents. But MM. Tissandier have no intention of directing their balloon against strong winds. Their object is to organise an apparatus with which rational experiments may be made in the air, and they have taken advantage of the most recent improvements of science. If their elongated balloon answer their wishes, a real advance will be registered in the history of aëronautics.

EXCAVATIONS are being carried out on Blackheath for the purpose of exposing the "dencholes" which have puzzled geologists and archæologists, and of which we gave some account in vol. xxiii. p. 365.

IN 1884 a general Italian exhibition will be opened at Turin. Among the exhibits will be works in mathematics, physics, and general chemistry.

THE "Treatise on Marine Surveying," reviewed in last week's NATURE, is published by Messrs. Macmillan and Co., and not by Mr. Murray.

THE additions to the Zoological Society's Gardens during the past week include a Mona Monkey (*Cercopithecus mona* ♀) from West Africa, presented by Mr. J. N. Flatau; a Crested Porcupine (*Hystrix cristatus*) from West Africa, presented by Mr. Joseph J. Doke; two Pileated Jays (*Cyanocorax pileatus*) from La Plata, presented by Capt. Gamble; two Grey-breasted Parrakeets (*Bolborhynchus monachus*) from the Argentine Republic, presented by Mr. Tomas Peacock; an European Tree Frog (*Hyla arborea*), European, presented by Mrs. M. B. Manuel; a Malbrouck Monkey (*Cercopithecus cynosurus* ♂) from East Africa, a Macaque Monkey (*Macacus cynomolgus* ♀) from India, deposited; a Water Chevrotain (*Hyomoschus aquaticus*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

VARIABLE STARS.—The following are Greenwich times of heliocentric minima of Algol:—

	h.	m.			h.	m.
February	3,	8 47		Feb. 26,	7	18
	17,	16 52		March 12,	15	23
	20,	13 41		15,	12	12
	23,	10 29		18,	9	1

The light equation (geocentric—heliocentric) in seconds, may be found from the expression—

$$460{\cdot}2s.\ R.\ \sin(S + 35^\circ\ 28'\ 7),$$

where R is the earth's radius-vector, and S the longitude of the sun. S Cancri will be at a minimum about the following times:—February 2, at 9h. 40m.; February 21, at 8h. 55m.; and March 12, at 8h. 2 m. A minimum of U Cephei occurs on February 5, about 13h. 26m. χ Cygni is at minimum on March 17. This year's maximum of Mira Ceti is not observable. According to the observations of Mr. Knott in 1881 and 1882, a maximum of T Cephei, when the star is about 6·5m., may be expected towards February 17; the position of this variable for 1880 is in R.A. 21h. 7m. 57s., Decl. +68° 0′·1; it is No. 3731 in Feilorenko's catalogue from Lalande.

REPORTED DISCOVERY OF A COMET.—A Reuter's telegram from Puebla, Mexico, January 23, states that a comet had been discovered there near the planet Jupiter, of which no further account has been received at the time we write, nor has a somewhat hurried examination of the vicinity between clouds revealed anything brighter or more cometary in aspect than our very old friend, the first nebula of Messier's catalogue near ζ Tauri, which has proved "a mare's nest" for more than one incipient comet-hunter. Jupiter was close at hand on January 22, but there was a full moon on that date, which hardly favours the suggested explanation. Messier 1, it may be remembered, led to more than a single false alarm when observers were on the look out for Halley's comet in 1835.

THE NEXT RETURN OF D'ARREST'S COMET.—At the sitting of the Paris Academy of Sciences on January 22, M. Leveau communicated elements of the orbit of D'Arrest's comet of short period, for the approaching return to perihelion. He states that on account of the great perturbations suffered by the comet from its passage near Jupiter during the period 1859-1863 (in April, 1861, it passed within 0·36 of the earth's mean distance from the planet), and the want of observations at its third appearance in 1864, it has not been possible to combine in the same system of elements the observations made in 1851 and 1857 with those of 1870 and 1877. He has consequently been obliged to determine the osculating orbit in 1883, from the elements which best represent the observations of 1870 and 1877 alone. The following are the elements of the comet's orbit for 1883, June 12·0, M.T. at Paris:—

Mean anomaly	328	13	20″·3	
Longitude of perihelion	319	11	10·8	Mean
,, ascending node ...	146	7	21·0	equinox
Inclination	15	41	47·1	1880·0
Angle of eccentricity	38	46	33·4	
Mean daily sidereal motion ..	530″·65245			

It is M. Leveau's intention to prepare and circulate among astronomers an ephemeris for what appears to be the most likely period during which to obtain observations, or from April 23 to November 25 in the present year, but from the comet's great distance or unfavourable position it is probable that only the largest telescopes will command it. By the above elements the comet will not arrive at perihelion until 1884, January 13·5765 Greenwich M.T.

MERIDIAN OBSERVATIONS OF NEBULÆ.—Dr. Engelmann publishes the positions of about 120 nebulæ, determined with the 6-inch meridian circle of the Leipsic Observatory, and reduced to the beginning of the year 1870, with the mean epoch of observation and the annual precessions, thus aiding by meridian observations the extension of our knowledge of accurate places of these bodies, which has engaged the attention of d'Arrest, Vogel, Schönfeld, Schultz, and others, with equatorial instruments. Valuable material is thus being collected for the investigation of proper motion amongst the nebulæ, which for want of reliable positions in past times, is not practicable at present, except perhaps in a few isolated cases.

ERRATUM.—In last week's "Astronomical Column," p. 300, lines seven and six from bottom, for *Washington* read *Washburn*.

PHYSICAL NOTES

A DOUBLE-ACTION mercury air-pump, invented by Signor Serravalle, who was awarded a gold medal for it at a recent exhibition in Messina, is described in the *Rivista Scientifico-Industriale* (Nos. 21–22). By a simple mechanical method two similar vessels are raised and lowered alternately with each other on opposite sides of a vertical support. A long caoutchouc tube connecting their bottoms lets mercury pass from one to the other. Each has at top a three-way cock; one port of which in a certain position leads into a small open vessel to receive any excess of mercury, and another is connected by means of a caoutchouc tube with a spherical piece fixed laterally about the middle of the vertical support. This piece has three passages, communicating together; two of them are opposite each other, and lead into the tubes from the mercury vessels; the other is connected by tubing to the vessel to be exhausted of air. The three-way cocks at the tops of the vessels are mechanically shifted at the top and bottom of their course by means of a toothed sector and rack in the one case, and a pin and projecting piece in the other.

TO observe directly the action of gravity on gases, M. Kraievitsch, of the Russian Chemical Society (*Jour. de Phys.*,

December, 1882), sets up two baro-manometers, one on the (low) ground, the other at the top of a high building, or of a hill. The manometric branches are connected by means of a long metallic tube. On rarefying the air in the tube through an adjutage adapted near one of the manometers, the rarefaction is propagated towards the other, but owing to gravity, the lower one always shows a greater pressure than the other. By varying the conditions, the hypsometric formula may be established directly. To ascertain whether gases have or have not a limit of elasticity, two baro-manometers are placed below and connected by separate tubes with the one above. On rarefying through a tube near one of the lower manometers, a limit is reached at which gravity prevents the air of the latter manometer from rising, and it remains stationary while the other continues to fall, if the limit of elasticity exist. The author was fitting up his apparatus for these experiments on a very high old building at St. Petersburg.

In another paper to the same Society (*loc. cit.*), M. Piltschikoff describes an arrangement for measuring the refractive index of liquids of which one has but small quantities. A hollow lens is filled with the liquid, and with the aid of a graduated scale and a microscope, one measures exactly the focal distance of a monochromatic flame placed at a given distance from the lens. The author gives a simple formula for calculating the index of the liquid, when the constants of the apparatus have been determined once for all. In one set of experiments, the index of glycerine was found = 1·47298, with a probable error estimated at ± 0·00001.

In the common practice of referring the electromotive force of galvanic combinations to the Daniell element as unit, some difficulty and confusion have arisen from differences in the construction of that element by different physicists. In a recent investigation of this matter (*Wied. Ann.*, 13, 1882), Herr Kittler gives the name of "normal element" to a combination, which is as follows:—Amalgamated, chemically pure zinc, in dilute sulphuric acid of specific gravity 1·075 at 18° C.; and chemically pure copper in concentrated copper sulphate solution of specific gravity 1·190 to 1·200 He finds that the electromotive force of the Daniell element (Zn, H_2SO_4, $CuSO_4$, Cu) increases with percentage proportion of the acid to a maximum occurring at the same place, whether the copper sulphate solution be concentrated or dilute, viz. with 25 to 30 per cent. of the acid; with further hydration of the acid there is decrease. The increase is greater, however, the more dilute the $CuSO_4$ solution used, and greatest with pure water. It is further found that, if very weak acids are used, there is decrease of the electromotive force with dilution of the copper-sulphate solution. Accordingly, there is a degree of concentration of the acid, with which a Daniell element furnishes the same tension, whether the $CuSO_4$ be concentrated or dilated to any extent. The solution in question has the specific gravity 1·1011 at 16° C., and is compounded of 750 ccm. H_2O and 100 ccm. dilute H_2SO_4 of sp. gr. 1·007. Herr Kittler compares the action of his "normal element" with that of other practical units.

In a recent paper to the Vienna Academy (*Wied. Ann.*, 13, 1882), Prof. Stephan describes an investigation of the magnetic screening action (*Schirmwirkung*) of iron (which is exemplified in Thomson's marine galvanometer and the Gramme machine). His experiments were made with hollow iron cylinders and iron rings, and were of three kinds, viz. deflection, oscillation, and induction.

The sound-vibrations of solid bodies (glass cylinders) in contact with liquids has been lately studied by Herr Auerbach (*Wied. Ann.*, No. 13, 1882). He finds that the *geometrical lowering of tone*, represented by the ratio of the vibration number (n_0) of the empty vessel to that of the same vessel filled with water (n), is smaller the higher the tone of the empty vessel, and greater the narrower the vessel. The *arithmetical lowering of tone* (represented by $(n_0 - n)n_0$) in a vessel of mean pitch, is inversely proportional to the square root of the vibration-number of the empty glass, and (approximately) to that of the number of wave-lengths which the sound of the empty vessel traverses from the wall to the axis. In glasses of different width it is (approximately) inversely proportional to the square root of the width. The specific lowering of tone of a liquid depends primarily on the density, and is greater, the greater this is, though it does not increase so quickly; next, on the compressibility, being greater the smaller this is.

AN INQUIRY INTO THE DEGREE OF SOLUBILITY REQUISITE IN MANURES, WITH SPECIAL REFERENCE TO PRECIPITATED CALCIC AND MAGNESIC PHOSPHATES

Some remarkable field trials, recently conducted in Scotland by Jamieson and others, have tended to raise serious doubts concerning the correctness of the high relative values, hitherto assigned by chemists to dissolved phosphates, commonly termed super-phosphates, for manurial purposes. We propose, therefore, to examine briefly the action of phosphates in the soil; the conditions under which they become available for the nutrition of plants, and the degree of solubility which, considering these facts, would appear to be most advantageous for the purposes of the agriculturist. We hope to be able to show the great value of precipitated calcic and magnesic phosphates as manure-ingredients, and to assign some reasons for the comparative neglect which the salts of magnesia have hitherto received from agricultural chemists.

The careful and elaborate series of experiments undertaken by Dr. Voelcker respecting the "solubility of phosphatic materials" may be said to constitute the basis of our present inquiry, as the behaviour of phosphates in water is perhaps the readiest test of their activity as manures. Dr. Voelcker ascertained that one gallon of distilled water will dissolve the following amount of calcic phosphates, derived from the sources quoted:—

	Per gallon.
Estremadura phosphorite...	0·10 grains.
Norwegian apatite	0·44 ,,
Coprolites (mean of Suffolk and Cambridgeshire)	0·62 ,,
Monk's Island phosphate	1·00 ,,
Pure bone ash (from very hard bone)	1·18 ,,
Pure tribasic phosphate of lime, precipitated, burnt and finely ground	2·20 ,,
Guano	2·52 ,,
Pure tribasic phosphate, precipitated and still moist	5·56 ,,

The general deductions arrived at from these experiments, made about fifteen years ago, were that the phosphates in coprolites, apatite, and other phosphatic minerals were very little acted upon by water, and that "for agricultural purposes phosphatic minerals, as well as bone ash, should be treated with a quantity of sulphuric acid sufficient to convert the whole of the insoluble phosphates, therein contained, as completely as possible into soluble combinations. It is a waste of good raw materials to leave much of the insoluble phosphates unacted upon by acid." Broadly speaking, the above may be said to constitute the creed of the agricultural chemist at the present day, and the farmer buys his manure at a relatively high price, per unit of soluble phosphate.

On applying manures containing dissolved phosphates to the soil, nearly the whole of the phosphoric acid is at once neutralised by the various salts present therein, be they lime, alumina, or iron, and the chemist assures us that the superior estimation in which soluble salts are held arises from the property possessed by these salts of becoming rapidly diffused through the soil and precipitated therein, in an extremely fine state of sub-division. Voelcker, in some recent observations on this question, lays down certain propositions which are thus set forth in the abstract of the *Journal* of the Chemical Society, vol. xi. (1881) p. 640. These appear to us to state very clearly and briefly the accepted theories respecting the action of phosphates in manure.

1. Phosphates are not readily taken up by plants in a soluble form, but must be returned to an insoluble condition before they yield their useful properties.

2. The efficacy of insoluble calcium phosphate corresponds with the minuteness of division in which it is found in a manure.

3. The finer the particles in a phosphatic material, the easier it is dissolved in water, and the more energetic its action as a manure. Coarsely-ground coprolites and other minerals are less useful than the same materials in fine powder.

4. Calcium phosphate in porous soft bones is more soluble and energetic than in hard bones, and is more available in bone meal than in crushed bones.

5. Calcium phosphate in crystallised mineral phosphates—Norwegian, Canadian, and Spanish apatites, for example—is less

soluble and energetic than the same amount contained in porous phosphatic materials, such as certain descriptions of phospho-guano.

6. Treatment with acids renders the material completely soluble in water, and the so-formed superphosphate, when put into the ground, is precipitated in a very fine state of division.

7. In the precipitated state the insoluble phosphate is immeasurably more finely divided than it could be obtained by mechanical means, and is consequently more energetic than any raw material mechanically ground.

8. The author's conclusion is that the chemical treatment with acid is the cheapest and best way of rendering mineral phosphates useful for agricultural purposes.

We think that it will be generally admitted that these propositions give a very reasonable statement of the case; but for the purposes of our inquiry we must supplement them with the following additional proposition. This has reference to a matter which has escaped the attention of Dr. Voelcker, but which is strongly supported by the results of the numerous recently-recorded practical trials.

"By reprecipitating the acid in a super-phosphate previous to its employment for agriculture by means of a suitable base, it becomes possible to obtain a neutral phosphate, possessed of a sufficient degree of solubility to be readily distributed through the soil, in an extremely fine state of subdivision, and capable of affording nutriment to the plant under highly favourable conditions."

It is to this further proposition to which we now desire to call special attention, and we may allude first to the assumed loss of the power of spontaneous diffusion through the soil, which is stated by Sibson, in his work on "Attificial Manures," to render the precipitated phosphates inferior in value to soluble acid phosphate. We think that no chemist will doubt that the phosphates in guano are sufficiently soluble to be available for plant food, and precipitated phosphate is certainly more soluble than the earthy phosphates in Peruvian guano. It must be remembered, moreover, when studying the table of solubilities of phosphates, as ascertained by Dr. Voelcker, that these are stated with reference to distilled water, which does not occur in nature, whereas in water containing small percentages of many of the salts, commonly present in the soil, the solubility of phosphates is largely increased. Thus the addition to the water of a trifling amount of ammonic chloride (1 per cent.) increases the solubility of precipitated calcic phosphate fourfold.

This matter has not then received a due share of attention, for, as we have seen, arguing on the analogy of guano, phosphates, in the precipitated form, are undoubtedly so far soluble as to possess the power of diffusion to an extent amply sufficient for agricultural purposes, and there must be a point, short of perfect solubility, which adequately satisfies all requirements in this respect. A careful consideration of the subject has led us to the conclusion that the effect of phosphoric acid added to the soil, after having been fixed by a suitable base, in a condition sufficiently soluble for every need of the plant, and in a state of subdivision far finer than anything which could be obtained by mechanical means, would be in theory, if not superior, at least equal to that of a similar amount of soluble phosphate, applied to a soil promiscuously, in cases in which it is impossible to predict by what bases the phosphoric acid will be fixed, or even whether it will be fixed at all. Indeed, the foregoing considerations would almost lead us to the belief that the employment of such ready-formed compounds as calcic or magnesic phosphates would be preferable to the haphazard use of soluble phosphoric acid in a super-phosphate.

Chemists in treating of the magnesic phosphates appear to have overlooked the dibasic phosphate and to have conducted their experiments and to have founded their observations mainly, if not entirely, on the behaviour of the far less soluble tribasic phosphate. The freshly-precipitated magnesic phosphate is soluble in about 322 times its weight of pure water, while calcic phosphate, as we have seen when newly precipitated, is soluble to the extent of 5·56 grains per gallon. Both of these salts are therefore much more soluble than the earthy phosphates present in guano. We must not overlook the fact also that although it has not yet received much attention, the magnesia would appear to possess in itself considerable manurial value. A recent French authority assigns to it a value approaching 5*s.* 8*d.* per unit, almost three-fourths of the price he sets down for phosphoric acid, and we are convinced from the study of the composition of numerous fertile soils, the ashes of plants, and recent field-trials, that the day is not far distant when the magnesia will rank as high in a manure as a salt of potash.

Another fact which the foregoing considerations have forcibly brought before us is the value of organic matters, in bringing about the solubility of the phosphates. This is perhaps scarcely within our present scope, but we have mentioned, incidentally, that small quantities of ammonia and carbonic acid, dissolved in the water, produce a very marked effect on the solubility of the phosphates. So valuable is their office in this respect, that it seems a false system to deny that organic matter, when present in a manure, possesses any value whatever. It was formerly the practice with agricultural chemists to allow 1*l.* per ton (2·4*d.* per unit) for organic matter, and we think that the important office which it fulfils in supplying carbonic acid for bringing into solution additional quantities of the phosphates, fully justifies the assignment to it of the above valuation.

We have thus endeavoured to explain the true conditions under which phosphoric acid becomes, in the soil, a source of plant-food. We have shown that there must be a limit to the value of solubility, merely considered as a means of securing diffusion through the soil, because partially soluble salts also possess the property to a degree sufficient for all practical purposes. In conclusion we have claimed for a ready-formed, partially-soluble phosphate, in a finely divided condition, and, in the case of the magnesic phosphate, possessing the property of fixing at the same time a portion of the ammonia, a value at least as great as that of a soluble acid phosphate, which runs the risk of being fixed by iron, or alumina (should lime be deficient in the soil), or, which may sink below the roots of the plants before it is neutralised. We trust we have thus shown a good case for a more liberal valuation of precipitated phosphates, and have indicated, with some measure of success, the reasons for the excellent results that have been recently obtained by the use of manures containing phosphoric acid in this form.

THE ELECTROLYTIC BALANCE OF CHEMICAL CORROSION[1]

THIS paper treats of some fundamental points in silver electroplating, and shows how a large amount of the electric power may be wasted by the use of too large a proportion of free potassic cyanide in the plating solution, or by using the liquid in a heated state.

In it is also described a method of ascertaining the degree of energy of chemical corrosion of metals in electrolytes, by means of the strength of electric current per unit of surface necessary to prevent such corrosion; the metals and liquids employed for the purpose in the present research being silver, and solutions of argento cyanide of potassium containing free potassic cyanide. Numerous examples, chiefly in the form of tables, are given of the strength of current required to enter cathodes of a given amount of surface, in order to exactly balance the chemical corrosive effect upon them at atmospheric temperatures, and at higher ones, of solutions of potassic cyanide of various degrees of strength.

The method employed was to take a given solution of cyanide of potassium, pass through it by means of a sheet of platinum anode and a burnished sheet of silver cathode, a weak electric current, and add gradually to the liquid (with stirring) small portions of argento potassic cyanide, until the faintest perceptible deposit of silver occurred. The verge of deposition thus attained was called "the balance point;" and the conditions which determine and influence it, constitute the subject of this research.

The effect of various conditions upon the point of balance of electric and chemical energy were investigated, and the experiments are described. The influences examined were: composition of the liquid, strength of current, size of cathode and density of current, electro-motive force, temperature, ordinary chemical corrosion, nature of the cathode, etc. The circumstances were also investigated which affect the measurement of the current by the method employed in this research, viz. by depositing silver from a solution of argento potassic cyanide; and the sources of error, (and their limit), in that method, are pointed out. The effect of varying the proportions of free potassic cyanide, and of argento potassic cyanide, upon the strength of current at the balance point, are shown in tables of results. The strengths of current just sufficient to prevent all

[1] Abstract of paper by G. Gore LL.D., F.R.S., read before the Birmingham Philosophical Society, Dec. 14, 1882.

corrosion and to deposit the whole of the silver from a solution of argento potassic cyanide of given composition and containing free cyanide are also shown. The influence of varying the proportions both of argento potassic cyanide, and free cyanide of potassium, upon the transfer resistance [1] of the solution, and thereby upon the balance point, are also investigated and the results described.

A number of results and conclusions were arrived at, some of which are as follows:—variation either of the number of battery elements, the proportion of water, of free potassic cyanide, or of argento potassic cyanide, destroys the balance. The effect of altering the proportion of water is opposite with strong solutions to what it is with weak ones. The electric current at the point of balance appears to be entirely conveyed by the free potassic cyanide, and does not divide itself between the two salts until the liquid contains a certain proportion of argentic salt. In strong solutions of potassic cyanide, decreasing the number of battery cells, necessitates more cyanide of silver to restore the balance. The alteration of the point of balance by alteration of proportion of free potassic cyanide cannot be much accounted for by alteration of corrosive power of the liquid. A current from ten Smee's elements is about sufficiently strong to prevent all corrosion of silver at 60° F. in a solution of cyanide of potassium containing a mere trace of argento potassic cyanide. The addition of nitrate, chloride, iodide, or sulphate of potassium to the cyanide solution has but little effect upon the balance point. Variation of strength and of "density" of current affect greatly the point of balance. Greater "density" irrespective of strength of current usually increases the amount of silver deposited. Difference of electro-motive force of current had no conspicuous effect in altering the balance point. Rise of temperature of the liquid acts in two opposite ways, it increases the corrosive action, and by diminishing conduction-resistance it increases the current, and as the latter effect is usually a little stronger than the former one, rise of temperature alters slightly the point of balance, and enables the current to produce a sparing deposit of silver. The ordinary chemical corrosion of silver in a solution of potassic cyanide without an electric current is increased slightly by partial immersion (through capillary corrosion), and greatly by rise of temperature; it is also slightly greater in a weak solution than in a strong one, with solutions of a certain range of strength; and it is distinctly increased by contact with platinum. In consequence of the latter circumstance, a platinum cathode requires a somewhat stronger current than a silver one to enable the point of balance to be attained. In a mixed solution of potassic and argento potassic cyanides, even the smallest proportion of the former salt conveys a portion of the current, and if the cathode is large or the current is sufficiently weak, the whole of it is conveyed by that salt, however much of the double salt is present, an error is thereby introduced when deposition of silver in such a liquid is used as a measure of current. But with a large amount of the double salt, a small amount of potassic cyanide, and a current sufficiently strong, the proportionate amount of error is small. During the act of deposition the cathode surface is not at all corroded, and any deficiency in the weight of deposit is not due to corrosion, but to a portion of the current being conveyed by other ingredients of the liquid than the argentic salt. A current which produces deposition of silver, prevents all corrosion of a silver cathode in the same liquid. The addition of free potassic cyanide to a solution of the double cyanide alters both the resistance and the balance point. The quantity of current diverted from the argentic salt in solution is directly proportional to the amount of free potassic cyanide present, but not always in the same ratio. The presence of a large proportion of free cyanide, together with the employment of a feeble current conduce to the passage of a large amount of current through the liquid without depositing silver; and a current of ·001057 Ampere (which would deposit ·132 grain of silver in two hours) was hardly strong enough to prevent all corrosion or to deposit any silver from a solution composed of 37·5 grains of argento potassic cyanide and 112·5 grains of free cyanide of potassium in three ounces of water. Whilst also a current if sufficiently weak, may traverse a solution of potassic cyanide containing double cyanide, without any of the current decomposing the latter, it cannot traverse a solution of double salt containing free potassic cyanide without some of it traversing the cyanide of potassium. With a very dense current also, a portion of it enters the cathode without depositing silver, and evolves gas.

It requires a much stronger current to balance the corrosion in a hot solution of the two cyanides than in a cold one, and in an instance given, a rise of temperature from 60 to 120° F. was attended by the passage of 21 per cent increase of current without deposition of silver. Addition of free potassic cyanide to a weak solution of the double salt *at the balance point*, first decreases and then increases the current by altering the transfer resistance, probably at the cathode. An amount of current equal to ·14857 Ampere, entering a surface of ⅛ths. of a square inch, was found to be sufficiently strong to deposit nearly the whole of the silver from a solution at 60° F. composed of 70·11 grains of free potassic cyanide, ·0297 grain of double cyanide, and three ounces of water, the liquid retaining dissolved a little less than that amount of silver at its balance point under those conditions. The strength of current *at the balance point* in a weak solution of potassic cyanide, varies inversely as the amount of silver salt added, and at about eight times the rate. A certain strength of current must enter a given surface of silver in a given liquid under stated conditions in order to prevent all corrosion and produce deposition. The addition of the double cyanide reduces the amount of current conveyed by the free potassic cyanide into the cathode *at the balance point*. Successive additions of double salt to a solution of potassic cyanide *not at the balance point*, first decreases and then increases the current by altering the transfer resistance; it alters the relation of the molecules of potassic cyanide to the cathode so as to diminish their power of transmitting current into that surface without depositing silver. The greater the proportion of double salt present, the greater the tendency to the deposition of silver. Addition of potassic cyanide to a weak solution of the double salt *not at the balance point*, first decreases and then increases the current by altering the transfer resistance at the cathode; in this respect it behaves like addition of the double salt to a weak solution of potassic cyanide. With cathodes of platinum, a solution of potassic cyanide offered less resistance to the current (*not at the balance point*) than one of the double cyanide, but with silver cathodes the reverse effect occurred.

The balance point is a case of equalization of molecular influences, including ordinary chemical corrosion, density of current, nature of cathode, temperature, proportions of water, argento potassic cyanide, free potassic cyanide, and the soluble salts present as impurities, either of which by being disturbed, alters all the others. All these influences also have separate numerical values. A rise of temperature of 60° F. requires an increase of ·000976 Ampere to restore the equipoise. The experiments illustrate the dynamics of electro silver plating; and the method employed in the research is applicable to the detection and measurement of molecular influence in electrolytes. In consequence of the alteration of any one of the conditions having the effect of altering all the remainder, all the above conclusions are limited in their application and are only correct under the conditions given in the paper. The fundamental explanation underlying these conclusions is, that the phenomena are essentially molecular; and that the mere presence and admixture of the double cyanide alters the molecular arrangement of the free cyanide *not at the balance point*, in such a way as to enable the latter to transmit a greater quantity of current into a cathode of given size, notwithstanding its being more diluted by the other salt.

The phenomena of the "balance point" constitute an interesting example of molecular equilibrium, in which the balance point may be compared to a ball suspended by an elastic cord, and having attached to it, a number of other similar cords, each drawing it in a different direction, and all of them being kept in a state of tension. In such a case an alteration of the degree of strain of any one of the cords, changes that of all others, and alters the position of the ball.

The research has a practical bearing both upon the measurement of electric currents by means of deposition of silver from a cyanide solution, and upon the technical process of electro plating. In the former it shows how a large proportion, or even the whole of a current may pass without being measured, and how the error may be reduced to the smallest amount; and in the latter, how a similar waste of current may occur, and how to prevent it.

It is manifest from the foregoing research, that the electrolytic balance of chemical corrosion of cathodes in other depositing solutions, such as those of gold, copper, nickel, etc., might form an extensive subject of experimental investigation.

[1] By transfer resistance is meant the resistance to transfer of the current into the cathode.

Appended Note.—It was constantly found that in using a non-corrodible anode such as platinum, the amount of current passing was very much more easily regulated by varying the size of the anode than that of the cathode, with a corrodible anode however, such as silver, this effect was not observed.

THE ETHER AND ITS FUNCTIONS[1]

II.

Consider the effect of wind on sound. Sound is travelling through the air at a certain definite rate depending simply on the average speed of the atoms in their excursions, and the rate at which they therefore pass the knocks on; if there is a wind carrying all the atoms bodily in one direction, naturally the sound will travel quicker in that direction than in the opposite. Sound travels quicker with the wind than against it. Now is it the same with light: does it too travel quicker with the wind? Well that altogether depends on whether the ether is blowing along as well as the air; if it is, then its motion must help the light on a little; but if the ether is at rest no motion of air or matter of any kind can make any difference. But according to Fresnel's hypothesis it is not wholly at rest nor wholly in motion; the free is at rest, the bound is in motion; and therefore the speed of light with the wind should be increased by an addition of $\left(1 - \frac{1}{\mu^2}\right)$th of the velocity of the wind. Utterly infinitesimal, of course, in the case of air, whose μ is but a trifle greater than 1; but for water the fraction is 7-16ths, and Fizeau thought this not quite hopeless to look for. He accordingly devised a beautiful experiment, executed it successfully, and proved that when light travels with a stream of water, 7-16ths of the velocity of the water must be added to the velocity of the light, and when it travels against the stream the same quantity must be subtracted, to get the true resultant velocity.

Arago suggested another experiment. When light passes through a prism, it is bent out of its course by reason of its diminished velocity inside the glass, and the refraction is strictly dependent on the retardation; now suppose a prism carried rapidly forward through space, say at the rate of eighteen miles a second by the earth in its orbit, which is the quickest accessible carriage; if the ether is streaming freely through the glass, light passing through will be less retarded when going with the ether than when going against it, and hence the bending will be different.

Maxwell tried the experiment in a very perfect form, but found no difference. If all the ether were free there would have been a difference; if all the ether were bound to the glass there would have been a difference the other way; but according to Fresnel's hypothesis there should be no difference, because according to it, the free ether, which is the portion in relative motion, has nothing to do with the refraction; it is the addition of the bound ether which causes the refraction, and this part is stationary relatively to the glass, and is not streaming through it at all. Hence the refraction is the same whether the prism be at rest or in motion through space.

An atom imbedded in ether is vibrating and sending out waves in all directions; the length of the wave depends on the period of the vibration, and different lengths of wave produce the different colour sensations. Now through free ether all kinds of waves appear to travel at the same rate; not so through bound ether; inside matter the short waves are more retarded than the long, and hence the different sizes of waves can be sorted out by a prism. Now a free atom has its own definite period of vibration, like a tuning-fork has, and accordingly sends out light of a certain definite colour or of a few definite colours, just as a tuning-fork emits sound of a certain definite pitch or of a few different pitches called harmonics. By the pitch of the sound it is easy to calculate the rate of vibration of the fork; by the colour of the light one can determine the rate of vibration of the atom.

When we speak of the atoms vibrating, we do not mean that they are wagging to and fro as a whole, but that they are crimping themselves, that they are vibrating as a tuning-fork or or a bell vibrates; we know this because it is easy to make the free atoms of a gas vibrate. It is only in the gaseous state, indeed, that we can study the rate of vibration of an atom; when they are packed closely together in a solid or liquid, they are cramped, and all manner of secondary vibrations are induced. They then, no doubt, wag to and fro also, and in fact these constrained vibrations are executed in every variety, and the simple periodicity of the free atom is lost.

To study the free atoms we take a gas—the rarer the better—heat it, and then sort out the waves it produces in the ether by putting a triangular prism of bound ether in their path.

Why the bound ether retards different waves differently, or *disperses* the light, is quite unknown. It is not easy accurately to explain refraction, but it is extremely difficult to explain dispersion. However, the fact is undoubted, and more light will doubtless soon fall upon its theory.

The result of the prismatic analysis is to prove that every atom of matter has its own definite rate of vibration, as a bell has; it may emit several colours or only one, and the number it emits may depend upon how much it is struck (or heated), but those it can emit are a perfectly definite selection, and depend in no way on the previous history of the atom. Every free atom of sodium, for instance, vibrates in the same way, and has always vibrated in the same way, whatever other element it may have been at intervals combined with, and whether it exists in the sun or in the earth, or in the most distant star. The same is true of every other kind of matter, each has its own mode of vibration which nothing changes; and hence has arisen a new chemical analysis, wherein substances are detected simply by observing the rate of vibration of their free atoms, a branch of physical chemistry called spectrum analysis.

The atoms are small bodies, and accordingly vibrate with inconceivable rapidity.

An atom of sodium vibrates 5×10^{14} times in a second; that is, it executes five hundred million complete vibrations in the millionth part of a second.

This is about a medium pace, and the waves it emits produce in the eye the sensation of a deep yellow.

4×10^{14} corresponds to red light, 7×10^{14} to blue.

An atom of hydrogen has three different periods, viz. 4·577, 6·179, and 6·973, each multiplied by the inevitable 10^{14}.

Atoms may indeed vibrate more slowly than this, but the retina is not constructed so as to be sensible of slower vibrations; however, thanks to Capt. Abney, there are ways now of photographing the effect of much slower vibrations, and thus of making them indirectly visible; so we can now hope to observe the motion of atoms over a much greater range than the purely optical ones and so learn much more about them.

The distinction between free and bound ether is forced on our notice by other phenomena than those of light. When we come to electricity, we find that some kind of matter has more come to electricity, we find that some kind of matter has more electricity associated with it than others, so that for a given electromotive force we get a greater electric displacement; that the electricity is, as it were, denser in some kinds of matter than in others. The density of electricity in space being 1, that inside matter is called κ, the specific inductive capacity. In optics the density of the ether inside matter was μ^2. These numbers appear to be the same.

Is the ether electricity then? I do not say so, neither do I think that in that coarse statement lies the truth; but that they are connected there can be no doubt.

What I have to suggest is that positive and negative electricity together may make up the ether, or that the ether may be sheared by electromotive forces into positive and negative electricity. Transverse vibrations are carried on by shearing forces acting in matter which resists them, or which possesses rigidity. The bound ether inside a conductor has no rigidity; it cannot resist shear; such a body is opaque. Transparent bodies are those whose bound ether, when sheared, resists and springs back again; such bodies are dielectrics.

We have no direct way of exerting force upon ether at all; we can, however, act on it in a very indirect manner, for we have learnt how to arrange matter so as to cause it to exert the required shearing (or electromotive) force upon the ether associated with it. Continuous shearing force applied to the ether in metals produces a continuous and barely resisted stream of the two electricities in opposite directions, or a conduction current.

Continuous shearing force applied to the ether in transparent bodies produces an electric displacement accompanied by elastic resilience, and thus all the phenomena of electric induction.

Some chemical compounds, consisting of binary molecules, *distribute* the bound ether of the molecule, at any rate as soon

[1] A lecture by Prof. Oliver Lodge at the London Institution, on December 28, 1882. Continued from p. 306.

as it is split up by dissociation; and, instead of each nascent radicle or atom taking with it neutral ether, one takes a certain definite quantity of positive, the other the same amount of negative, electricity. In the liquid state the atoms are capable of locomotion; and a continuous shearing force applied to the ether in such liquids causes a continual procession of the matter and associated electricity, the positive one way, and the negative the other, and thus all the phenomena of electrolysis..

What I say about electricity, however, is not to be taken without salt, you will not regard it as recognised truth, but as a tentative belief of your lecturer's which may be found to be more or less, and possibly more rather than less, out of accordance with facts. I can only say that it hangs phenomena together, and that it has been forced upon my belief in various ways.

Now what about the free ether of space, is it a conductor of electricity? There are certain facts which suggest that it is, and Edlund has suggested that it is an almost perfect conductor. When a sun-spot or other disturbance breaks out on the sun, accompanied as it is, no doubt, by violent electric storms, the electric condition of the earth is affected, and we have auroræ and magnetic disturbances. Is this by induction through space? or can it be due to conduction and the arrival of some microscopic portion of a derived current travelling our way?

For my part I cannot think the ether a conductor. Maxwell has shown that conductors must be opaque, and ether is nothing if not transparent; one is driven, then, to conclude that what we call conduction does not go on except in the presence of ordinary matter—in other words, perhaps, that it is a phenomena more connected with bound ether than with free.

But now, looking back to Fresnel's hypothesis of the extra density of the ether inside gross matter, and also to the fact that it must be regarded as incompressible, the question naturally arises how can it be densified by matter or anything else? Perhaps it is not; perhaps matter only strains the ether towards itself, thus slackening its tension, as it were, inside bodies, not producing any real increase of density; and this is roughly McCullagh's form of the undulatory theory. In this form gravitation may be held to be partially explained; for two bodies straining at the ether in this way will tend to pull themselves together. In fact Newton himself pointed out that gravitation could be produced if only matter exerted this kind of strain on all pervading ether, the tension varying as the inverse distance.

He did not follow the idea up, however, because he had then no other facts to confirm him in his impression of the existence of such an ether, or to inform him concerning its properties. We now not only feel sure that an ether exists, but we know something of its properties; and we also have learnt from light and from electricity, that some such action between matter and ether actually occurs, though how or why it occurs we do not yet know. I am therefore compelled to believe that this is certainly the direction in which an ultimate explanation of gravitation and of cohesion is to be looked for.

In thinking over the Fresnel and McCullagh forms of the undulatory theory, with a view to the reconciliation between them which appears necessary and imminent, one naturally asks, is there any such clear distinction to be drawn between ether and matter as we have hitherto tacitly assumed? may they not be different modifications, or even manifestations, of the same thing?

Again, when we speak of atoms vibrating, how can they vibrate? of what are their parts composed?

And now we come to one of the most remarkable and suggestive speculations of modern times—a speculation based on this experimental fact, that the elasticity of a solid may be accounted for by the motion of a fluid; that a fluid in motion may possess rigidity.

I said that rigidity was precisely what no fluid possessed; at rest this is true; in motion it is not true.

Consider a perfectly flexible india-rubber O-shaped tube full of water; nothing is more flaccid and limp. But set the water rapidly circulating, and it becomes at once stiff; it will stand on end for a time without support; kinks in it take force to make, and are more or less permanent. A practicable form of this experiment is the well-known one of a flexible chain over a pulley, which becomes stiff as soon as it is set in rapid motion.

This is called a vortex filament, and a vortex is a thing built up of a number of such filaments. If they are arranged parallel to one another about a straight axis or core, we have a vortex cylinder such as is easily produced by stirring a vessel of water, or by pulling the plug out of a wash-hand basin; or such as are made in the air on a large scale in America, and telegraphed over here, when they are called "cyclones," or "depressions." The *depression* is visible enough in the middle of revolving water. These vortices are wonderfully permanent things, and last a long time, though they sometimes break up unexpectedly.

Vortices need not have straight cores, though they may have cores of various ring forms, the simplest being a circle. To make a vortex ring, we must take a plane disk of the fluid, and at a certain instant give to every atom in the disk a certain velocity forward, graduating the velocity according to its distance from the edge of the disk. We have as yet no means of doing this in a frictionless fluid, but with a fluid such as air and water it happens to be easy; we have only to knock a little of the fluid suddenly out of a box through a sharp-edged hole, and the friction of the edges of the hole does what we want. The central portion travels rapidly forward, and returns round outside the core, rolling back towards the hole. But the impetus sends the whole forward, and none really returns; it rolls on its outer circumference as a wheel rolls along a road. In a perfect fluid it need not so roll forward, as there would be no friction, but in air or water a vortex-ring has always a definite forward velocity, just as a locomotive driving-wheel has when it does not slip on the rails.

We have in these rings a real mass of air moving bodily forward, and it impinges on a face or a gas flame with some force. It is differentiated from the rest of the atmosphere by reason of its peculiar rotational motion.

The cores of these rings are elastic—they possess rigidity; the circular is their stable form, and if this is altered, they oscillate about it. Thus when two vortex rings impinge or even approach fairly near one another, they visibly deflect each other, and also cause each other to vibrate.

The theory of the impact or interference of vortex rings whose paths cross, but which do not come very near together has been quite recently worked out by Mr. J. J. Thomson. It is quite possible to make the rings vibrate without any impact, by serrating the opening out of which they are knocked. The simplest serration of a circle turns it into an ellipse, and here you have an elliptic ring oscillating from a tall to a squat ellipse and back again. Here is a four-waved opening, and the vibrations are by this very well shown. A six-waved opening makes the vibrations almost too small to be perceived at a distance but still they are sometimes distinct.

The rings vibrate very much like a bell vibrates, perhaps very much like an atom vibrates. They have rigidity, although composed of fluid; they are composed of fluid in motion. These vortices, are imperfect they increase in size, and decrease in energy; in a perfect fluid they would not do this, they would then be permanent and indestructible, but then also you would not be able to make them.

Now does not the idea strike you that atoms of matter may be vortices like these—vortices in a perfect fluid, vortices in the ether. This is Sir William Thomson's theory of matter. It is not yet proved to be true, but is it not highly beautiful? a theory about which one may almost dare to say that it deserves to be true. The atoms of matter according to it are not so much foreign particles imbedded in the all-pervading ether as portions of it differentiated off from the rest by reason of their vortex motion, thus becoming virtually solid particles, yet with no transition of substance; atoms indestructible and not able to be manufactured, not mere hard rigid specks, but each composed of whirling ether; elastic, capable of definite vibration, of free movement, of collision. The crispations or crimpings of these rings illustrate the kind of way in which we may suppose an atom to vibrate. They appear to have all the properties of atoms except one, viz. gravitation; and before the theory can be accepted, I think it must account for gravitation. This fundamental property of matter cannot be left over to be explained by an artificial battery of ultra-mundane corpuscles. We cannot go back to mere impact of hard bodies after having allowed ourselves a continuous medium. Vortex atoms must be shown to gravitate.

But then remember how small a force gravitation is. Ask any educated man whether two pound-masses of lead attract each other, and he will reply no. He is wrong, of course, but the force is exceedingly small. Yet it is the aggregate attraction of trillions upon trillions of atoms; the *slightest* effect of each upon the ether would be sufficient to account for gravitation; and no one can

say that vortices do not exert some such residual, but uniform, effect on the fluid in which they exist, till second, third, and every other order of small quantities have been taken into account, and the theory of vortices in a perfect fluid worked out with the most final accuracy.

At present, however, the Thomsonian theory of matter is not a verified one, it is, perhaps, little more than a speculation, but it is one that it is well worth knowing about, working at, and inquiring into. It may stand or it may fall, but if it is the case, as I believe it is, that our notions of natural phenomena, though they often fall short, yet never exceed in grandeur the real truth of things, how splendid must be the real nature of matter if the Thomsonian hypothesis turns out to be inadequate and untrue.

I have now endeavoured to introduce you to the simplest conception of the material universe which has yet occurred to man. The conception that is of one universal substance, perfectly homogeneous and continuous and simple in structure, extending to the furthest limits of space of which we have any knowledge, existing equally everywhere. Some portions either at rest or in simple irrotational motion transmitting the undulations which we call light. Other portions in rotational motion, in vortices that is, and differentiated permanently from the rest of the medium by reason of this motion.

These whirling portions constitute what we call matter; their motion gives them rigidity, and of them our bodies and all other material bodies with which we are acquainted are built up.

One continuous substance filling all space: which can vibrate as light; which can be sheared into positive and negative electricity; which in whirls constitutes matter; and which transmits by continuity, and not by impact, every action and reaction of which matter is capable. This is the modern view of the ether and its functions.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—Lord Rayleigh has resumed his course of lectures on Electrical Measurements.

Dr. Gaskell's lectures this term deal with the Physiology of the Circulation; Mr. Langley is lecturing on the Physiology of Muscle and Nerve, and the Histology and Pathology of the Secretory Organs.

SCIENTIFIC SERIALS

Transactions of the New York Academy of Sciences, Nos. 2-5, 1881-82.—Outlines of the geology of the North-eastern West India Islands, by Prof. Cleve.—The excavation of the bed of the Kaaterskill, New York, by Dr. Julien.—On the cell-doctrine and the bioplasson doctrine, by Prof. Elsberg.—The discovery of the North Pole practicable, by Commander Cheyne.—The volcanic tuffs of Challis, Idaho, and other western localities, by Dr. Jullien.—The mammoth cave of Kentucky, by Mr. Stevens.—On the determination of the heating-surface required in steam pipes employed to produce any required discharge of air through ventilating chimneys, by Prof. Trowbridge.—On a peculiar coal-like transformation of peat, recently discovered at Scranton, Penn., by Prof. Fairchild.—The parallel drift-hills of Western New York, by Dr. Johnson.—Hypothetical high tides as agents of geological action, by Dr. Newberry.—The international time-system, by Prof. Rees. The moral bearing of recent physical theories, by Prof. Martin.—The discovery of emeralds in South Carolina, by Mr. Hidden.—Obituary notice of Prof. J. W. Draper.—On the behaviour of steam in the steam-engine cylinder, and on curves of efficiency, by Prof. Thurston.—Stereoscopic notes, by Prof. Hines.—A new reversible stereoscope, by Mr. Stevens.—Diphenylamine-acrolein, by Prof. Leeds.

Annalen der Physik und Chemie, No. 1, 1883.—On the radiometer, by E. Pringsheim.—A wave-length measurement in the ultra-red solar spectrum, by the same.—Fluorescence according to Stokes' law, by E. Hagenbach.—The isogyrous surfaces of doubly-refractive crystals; general theory of the curves of like direction of vibration, by E. Lommel.—On the heat-conducting power of liquids, by L. Graetz.—On the ratio of the specific heats in gases and vapours, by P. A. Müller.—The product of internal friction and galvanic conduction of liquids is constant with reference to the temperature, by L. Grossmann.—On M. Guébhard's proposed method of determination of equipotential lines, by H. Meyer.—Further researches on the relation of molecular refraction of liquid compounds to their chemical constitution, by H. Schröder.—On the preservation of oxygen gas in the zinc-gasometer, by J. Loewe.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, January 11.—"On the Skeleton of the Marsipobranch Fishes. Part I. The Myxinoids (*Myxine* and *Bdellostoma*)." By W. K. Parker, F.R.S. Abstract.

In their cranio-facial skeleton the Myxinoids are very remarkable; where segmentation is perfect in other fishy types there they only exhibit a lattice-work of continuous growth; in the median region of the skull-base, where other types show but little or only temporary distinctness of parts, these fishes develop and retain large independent cartilages.

The lamprey has a large superficial basket-work of soft cartilage (*extra-branchial*), and its gill-pouches keep this related to the rest of the structures of the mouth and throat. But in the Myxinoids the basket-work is *intra-branchial*, and corresponds to the system of segmented arches of the higher Cartilaginous, the Ganoid, and the Osseous fishes. But these non-segmented arches soon lose all relation to the branchial pouches, which are removed so far backwards that they begin under the *twentieth myotome*; whilst the end of the pericardium is under the *fortieth*.

In seeking light upon the primordial condition of the Vertebrata, one naturally looks to such forms as the Myxinoids. For in these types, even in the adult state, there are neither limbs nor vertebræ, and no distinction between head and body, except the beginning, in the head, of a cartilaginous skull; a *continuous structure*—not showing the least sign of secondary segmentation, and by far the greater part of which is in front of the notochord, or axis of the organism. But here our *gradational* work agrees with the *developmental*, for the continuous skull-bars constantly arise before the secondary cartilaginous segments that are found between the myotomes behind the head. Evidently, therefore, the early "Craniata" grew supports to the enlarged and subdivided front end of their neural axis, long before anything beyond strong fibrous septa were developed between the muscular segments of the body. As for the linear growth, the greater or less extension backwards of the main organs—circulatory, respiratory, digestive, urogenital—that, in the evolution of the primary form, was a thing to be determined by the "surroundings" of the type. "Thereafter as *they* may be" was the tentative idea in this case.

Certainly, in the Marsipobranchs and in their relations, the larval "Anura," we have the most archaic "Craniata" now existing; in these the organs may be extended far backwards in a vermiform creature, as in these low fishes, or kept well swung beneath the head—the body and tail together forming merely a propelling organ, as is seen in Tadpoles, especially the gigantic Tadpole of *Pseudis*.

Thus we see that in low limbless types there is no necessity for the development of more than fibrous "metameres"; but the vesicular brain, the suctorial lips, the branchial pouches, and the special organs of sense—these all call for support from some tissue more dense than a mere fibrous mat or web. In the *Myxinoids* we find that *four* special modifications of the connective tissue series are developed for the support of the properly *cephalic* organs, and for them only; thus these fishes are *Craniata*, but are not *Vertebrata*; that is, if we stick to the letter, which of course we do not.

At first some disappointment is felt, after careful study of these types, for, notwithstanding the low level in which they remain, they are mere specialised *Ammocœtes*, keeping on the same "platform" as the larval Lamprey; yet some parts of their organisation do undergo a marvellous amount of transformation, and are, indeed, as much specialised in conformity with their peculiar habits of life *as any Vertebrates whatever, the highest not excepted*.

Yet, on the whole, the Msxinoids are a sort of *Ammocœtine* type, whilst the transformed Ammocœte, the larval Lamprey, comes nearest to the untransformed Frog or Toad—the *Tadpole*. But the mere putting of this shows (suggests at any rate) what *losses* the fauna of the world has sustained during the evolution of the Craniate forms; *now*, the Myxinoids, Petromyzoids, and anurous Amphibia, must all be kept "within call" of each other; but the types that have been culled out between them

cannot be numbered. Some other kind of fish are evidently the descendants of primordial "Marsipobranchs," notably *Lepidosteus*, the development of which has been lately studied, and the results are being published in the *Philosophical Transactions*. But the *Chimæroids*, *Dipnoi*, and, still more important, the *Myxinoids* themselves, have still to be followed through their early stages. If the present paper is of any value to the morphologist, one on the embryology of these low forms would be worth much more.

The Myxinoids keep on the low "platform" of the larval Lamprey (*Ammocœte*) in the following particulars, namely:—

a. The notochord has no paired cartilaginous vertebral rudiments in the spinal region.

b. The trabeculæ end in the ethnoidal region, without growing forwards into a cornu (or *two continuous* cornua).

c. There are merely "barbels" round the mouth; no *labial cartilages*.

d. The last character involves this, namely, that the special armature of horny teeth, attached to the labials in the adult *Petromyzon*, is absent.

e. The organs of vision are very feeble, and probably almost useless; in the *Ammocœte* they are arrested for a time.

f. The cranium is a mere *floor*, without side-walls or roof.

The Myxinoids come near to the adult Lamprey in the following particulars, namely:—

a. There are developed outside the skull proper, but not segmented from it, palato-pterygoid and hyoid cartilages.

b. There is a very large median cartilage belonging to both the hyoid and branchial regions.

c. The cranium acquires a floor by the development of a special "hinder intertrabecula."

d. There is a large median cartilaginous olfactory capsule.

The Myxinoids go beyond even the adult Lamprey in the following particulars, namely:—

a. The facial basket-work is much more perfect; and as this is a generalised condition of the true *intra-visceral* system of cartilages, it is a very important character; there is not only an equal development of the "suspensorium," but the *suspensorial part* of the hyoid is developed also (it is suppressed in the Lamprey); and there is, in *Bdellostoma* a large complete first branchial arch, and in both kinds pharyngo-branchial rudiments of the second branchial arch.

b. The respiratory (branchial) pouches are much more specialised by being carried far back under the spine.

c. There is not only a distinct sub-cranial intertrabecula, but also a large pre-cranial or nasal median cartilage of the same nature.

d. The opening of the median olfactory sac is not a mere short membranous passage, but a long tube, encased in a series of cartilaginous (imperfect) rings.

e. Correlated with the non-development of the suctorial labial cartilages, there is an enormous development of the lingual, the basal bar becoming not only double, but, in front, quadruple, and the "supra-lingual" cartilages, which are very small in the Lamprey, and carry only one pair of rows of small second teeth, in the Myxinoids are very large, and carry two pairs of rows of large teeth, with the addition of a median antagonistic "ethnoidal tooth."

Lastly, the greater development of the intra-visceral (="intra-branchial") cartilages is correlated with the suppression of the extra-visceral basket-work seen both in the larval and adult Lamprey, and also in the larvæ of the "Anura" generally.

January 18.—"On the Skeleton of the Marsipobranch Fishes. Part II. The Lamprey." By W. K. Parker, F.R.S.

The suctorial mouth has its highest development in the Lamprey; in the Myxinoids (*Myxine*), and Bdellostoma, there is no circular disk with horny teeth, but merely an oral fissure surrounded by barbels, and having inside it a huge tongue beset with two oblique rows of recurved and inturned horny teeth, antagonised by a single ethnoidal tooth. In the larva of the Lamprey the mouth is not circular, and the *lower* lip is far back, covered by the *upper*, which is like a hood; there are no teeth of any kind, only moss-like "barbels" or *papillæ* under the upper lip.

In the Tadpole the mouth is suctorial, the *lower* lip being converted into an imperfect ring, which is completed by the *upper* lip. Here the cartilage of the lower lip is not a perfect ring, as in the Lamprey, but is in two parts, and is formed into a sort of *horseshoe*. Inside this compound ring there are sharp horny plates or teeth, and the folds of the lips, all round the mouth, are covered with a horny rasp.

Correlated with the perfectly suctorial *lower* lip of the Lamprey, which, is a *post-oral* structure, entirely, we have the perfectest form of the superficial branchial skeleton, a basket-work of soft cartilage which appears in the early embryo, and only gains enlargement fore and aft, and all its snags and outgrowths, after metamorphosis. Besides this there are no rudiments of *internal* branchial arches, such as we find in the Tadpole. The only parts developed *inside* the head-cavities and branchial arches are the generalised and rudimentary mandibular and hyoid arches. In the Tadpole there is no *pier* to the hyoid arch, and the *first cleft* is arrested as a small blind pouch; this state is persistent in the Lamprey. But, after metamorphosis—as the lingering latter part of that profound change of structure—the young Frog and Toad acquire a pier to their hyoid arch, right and left. This, however, does not become functional to the arch, much less assist in supporting the mandible, as a "hyo-mandibular," but is transformed into an osseo-cartilaginous chain—a *stapedio-incudal* series, specialised correlatively with the expanded rudiment of the first cleft, now enlarged into a *cavum tympani*, with a large "Eustachian opening." The little mandibles of the Tadpole, which served as arms to carry the divided suctorial disk, and lay across the fore face, become very long, and are often hinged on to their pier behind the occiput, and the cartilages of the suctorial disk straighten out and add to the length of the lower jaw in front. These things show how this *temporary* "Petromyzoid," the Tadpole, blossoms out into unthought-of specialisations; it becomes a *quasi-reptile*, worthy of a place far above the Lamprey, and even far above all other *Ichthyopsida*.

Geological Society, January 10.—J. W. Hulke, F.R.S., president in the chair.—T. W. Edgeworth David, the Earl of Dysart, John James Hamilton, Francis Alfred Lucas, and Meaburn Staniland, were elected Fellows, and Dr. Otto Torell, F.C.G.S., of Stockholm, a Foreign Member of the Society.—The following communications were read:—On the Lower Eocene section between Reculvers and Herne Bay, and some modifications in the classification of the Lower London Tertiaries, by J. S. Gardner, F.G.S.—The author noticed Prof. Prestwich's classification of the Lower London Tertiaries, and the introduction by the Survey of the term "Oldhaven Beds" for some of his basement beds of the London Clay. He next discussed the conditions under which the Lower Tertiaries were produced, and showed that throughout the Eocenes there are indications of the close proximity of land and of the access of fresh water. Two types of faunas are to be recognised, namely, those of the Calcaire Grossier and the London Clay, the latter indicating more temperate climatal conditions. The former is represented in England by the Bracklesham series. The areas of these two faunas were separated by land forming an isthmus, as each formation is bounded by a shore-line and separated from its neighbours by freshwater formations; but this isthmus probably shifted its position to the north and south without ever being broken through. A vast Eocene river existed, draining a great continent stretching westward; the indications of this river in Hampshire and Dorsetshire would show it to have been there seventeen or eighteen miles wide.—The Lower Tertiaries have been divided by Prof. Prestwich and the Survey into the marine Thanet beds, the fluviatile, estuarine and marine Woolwich and Reading Beds, and the marine Oldhaven Beds. The mode of occurrence of these was described by the author, with especial reference to the section between Herne Bay and the Reculvers, from his investigation of which he was led to the following conclusions:—The Thanet Sands were probably deposited by a rough sea outside the estuary of the great Eocene river, but within its influence. This area became silted up, rose above the surface, and became covered with shingle and sand. The Thanet Beds closed with a period of elevation, during which the Reading Beds were formed, and this was followed by a subsidence during the Woolwich period, which finally ushered in the Oldhaven and London Clay deposits. The formation of the Oldhaven Beds may be compared with that of the modern beach at Shellness; and during the period of depression the beaches would advance steadily over the flat area of Sheppey, and the earlier formed ones would sink and become covered up by the silt of the great Eocene river. These beaches, forming vast aggregations of sand and shingle between the Thanet Beds and the London Clay, form integral portions of one or other formation, and cannot be recognised as forming a separate

formation at all equivalent to the other divisions of the Eocene.—On Mr. Dunn's Notes on the Diamond-fields of South Africa, 1880, by Francis Oates, F.G.S.

Anthropological Institute, January 23.—Anniversary Meeting.—John Evans, V.P., D.C.L., F.R.S., in the chair.—The Treasurer's report and the report of the Council were read and adopted.—The Chairman delivered an address, in which he briefly reviewed the work of the past year, and enlarged on the subject of the antiquity of man, discussing the evidence for and against his existence in Tertiary times.—The following Officers and Council for 1883 were elected:—President, Prof. W. H. Flower, F.R.S. Vice-presidents: Hyde Clarke, John Evans, F.R.S., Francis Galton, F.R.S., Major-Gen. Pitt-Rivers, F.R.S., A. Thomson, F.R.S., E. B. Tylor, F.R.S. Director, F. W. Rudler, F.G.S. Treasurer, F. G. H. Price, F.S.A. Council: J. Beddoe, F.R.S., S. E. B. Bouverie-Pusey, E. W. Brabrook, F.S.A., C. H. E. Carmichael, M.A., W. Boyd Dawkins, F.R.S., W. L. Distant, A. W. Franks, F.R.S., Lieut.-Col. H. H. Godwin-Austen, F.R.S., Prof. Huxley, F.R.S., A. H. Keane, B.A., A. L. Lewis, Sir J. Lubbock, M.P., R. Biddulph Martin, M.P., Henry Muirhead, M.D., J. E. Price, F.S.A., Lord Arthur Russell, M.P., Prof. G. D. Thane, Alfred Tylor, F.G.S., M. J. Walhouse, F.R.A.S., R. Worsley.

PARIS

Academy of Sciences, January 22.—M. Blanchard in the chair.—The following papers were read:—On metasulphites, by M. Berthelot.—On seleniide of nitrogen, by MM. Berthelot and Vieille.—On the characters of induced currents resulting from reciprocal movements of two magnetic bodies parallel to their axis, by M. du Moncel. Polarisation of an iron core immobilises a certain quantity of magnetism, which thus remains indifferent to exterior magnetic excitation, and is only affected when, being able to act on the inducing body, which over-excites its energy, it may polarise it in its turn, so that action and reaction are in concordance.—On complex units (continued), by M. Kronecke.—Theory of the most general electro-dynamic actions that can be observed, by M. Le Cordier.—On the construction of a dynamo-electric propeller on a long balloon, by M. Tissandier. The system, with a total weight = three men, gives during three hours the work of twelve to fifteen men. The two-vaned propeller (of steel wire and varnished silk) is driven by a small Siemens' dynamo (120 turns of the former to 1200 of the latter); the battery being of thirty-four elements mounted in tension, and divided into four series. An element consists of a vulcanite box (four litres capacity) holding ten zinc and eleven carbon plates. Strong bichromate solution is let in or drawn off by raising or lowering a separate vessel connected by a tube with the battery.—Observations of the transit of Venus at Bragado (Argentine Republic), by M. Perrin. He observed two direct contacts (the second and the fourth), and a certain number of artificial contacts which will supplement the others. The phenomenon was of distinct and well characterised geometrical appearance.—On the approaching return of the periodic comet of d'Arrest, by M. Leveau. He has calculated an ephemerides (which will be communicated to all astronomers) for the period most favourable to observation, viz. April 23 to November 25 this year. Values for the relative brightness are deduced.—Addition to a note on prime numbers, by M. de Jonquières.—On the relations between covariants and invariants of like character, of a binary form of the sixth order, by M. Stephanos.—On the functions of several imaginary variables, by M. Combescure.—On the functions of two variables, by M. Poincaré.—On the curves of the sextant, by M. Gruey.—Mode of distribution among various points of its small supporting base, of the weight of a hard body, of polished and convex surface, placed on an elastic horizontal ground, by M. Boussinesq.—On a communication of MM. Mercadier and Vaschy on consequences deducible from relations between electric magnitudes, by M. Lévy.—Remarks on the expression of electric magnitudes &c. (continued), by MM. Mercadier and Vaschy.—Observations on Dr. Siemens' last paper, by M. Violle.—Photographic positives on paper obtained directly, by MM. Cros and Vorgerand. Paper is covered with a solution of 2 gr. bichromate of ammonia, 15 gr. glucose, and 100 gr. water, is dried, and exposed to light under a positive (*e.g.* a drawing). When the (yellow) bare parts of the paper have become grey, the paper is immersed in a bath of 1 gr. nitrate of silver to 100 gr. of water, with 10 gr. acetic acid. The image appears at once, with reddish tint, produced by bichromate of silver. Drying in light gives a dark brown tint.—On hydraulic silica, by M. Le Chatelier. The only new fact given by M. Landrin (he says) is the non-hydraulicity of silica obtained from manufacture of hydrofluosilicic acid.—On mutual displacements of bases in neutral salts, the systems remaining homogeneous, by M. Menschutkin.—On the causes capable of affecting the amount of ammonia in rain-water, by M. Houzeau. One important consideration is the time that has elapsed between obtaining and analysing; another the monthly quantity of water (the less the rain, the more ammonia present).—On the action of certain metals on oils, by M. Livache. Instead of metallic plates (which M. Chevreul experimented with), he used metals finely divided, as in precipitation, and got much better effects. Of the three, lead, copper, tin—lead acts most strongly. If some of it be moistened with oil and exposed to air, an increase of weight very soon occurs through oxidation, and it is greater the more siccative the oil. A solid and elastic product is formed. The increments of weight with different oils are sensibly proportional to those in fatty acids of the same oils exposed to air several months (cotton-seed oil alone is anomalous; it is siccative, but its fatty acids increase very little in weight). The transformation of the oil is attributed to direct action of the metal, not to that of the air. It suggests a rapid means of distinguishing siccative and non-siccative oils, and an advantageous substitute for the heating of oils.—Calcification of kidneys, parallel to the decalcification of the bones, in subacute poisoning by corrosive sublimate; increase of the proportion of mineral parts of a tibia, following disarticulation of the other tibia, by MM. Prevost and Frutiger.—Physiological action of sulphate of quinine on the circulatory apparatus in men and animals, by MM. Lee and Bochefontaine. It preserves and increases the force of the heart, and is a powerful antipyretic.—Medullary origin of paralyses following cerebral lesions, by M. Couty.—On the lymphatic system of tadpoles, by M. Jourdain.—On the development of the reproductive apparatus of pulmonate molluscs, by M. Rouzaud.—On Suctociliate Infusoria (a reply), by M. de Merejkowsky.—On the morphological nature of the subterranean branches of the root of adult *Psilotum*, by M. Bertrand.—Contribution to the stratigraphic history of the relief of Sinai, and especially on the age of porphyries of that country, by Abbé Raboisson. The last dislocations of the Sinaitic system were posterior to the eocene.

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THURSDAY, FEBRUARY 8, 1883

ZOOLOGICAL SKETCHES

Zoological Sketches. A Contribution to the Out-door Study of Natural History. By Felix L. Oswald. With Thirty-six Illustrations by Hermann Faber. (London: W. H. Allen and Co., 1883.)

Zoological Notes on the Structure, Affinities, Habits, and Mental Faculties of Wild and Domestic Animals; with Anecdotes concerning and Adventures among them; and some Account of their Fossil Representatives. By Arthur Nicols, F.G.S., F.R.G.S. Illustrated by J. W. Wood and F. Babbage. (London: L. Upcott Gill, 1883.)

WE cannot say much in favour of the first of these two works. The engravings are good, but the subjects chosen scarcely justify the care which has been taken in their execution. For these subjects are nearly all chosen for the sake of a comical or sensational effect, without any reference to utility as illustrating zoological facts or principles. And the essentially unscientific spirit which has led to the choice of the "thirty-six illustrations," is no less apparent throughout the letter-press. We are always ready to welcome any attempt at popularising zoology, more especially when the writer has any first-hand "contributions to the study of natural history" to supply; but surely such study admits of being made sufficiently interesting in itself, without the need of lame attempts at a kind of pleasantry, which in being always forced and never witty, must necessarily become irksome even to the least intelligent of unintelligent readers. We are the more disposed to regret the author's mistake in adopting this artificial style, because in his short preface, where it is not adopted, he shows that he is able to write with marked ability.

Concerning the facts of natural history which are detailed, the most interesting, in our opinion, are those which refer to the intelligence of monkeys and the stupidity of sloth bears. We shall, therefore, give one quotation on each of these topics.

Speaking of a domesticated sloth, the author says:—

"Though fed daily by the same hands, the old pensioner still fails to identify his benefactor, or to recognise his obligations in any way. To his ear the human voice in its most endearing tones is a grunt *et præterea nihil*; you might as well appeal to the affections of a cockroach. You may frighten a pig, a goose, a frog, and even a fly, but you cannot frighten or surprise a sloth. On my last trip to Vera Cruz I procured a pair of black tardos, full grown, and in a normal state of health, so far as I could judge, but after a series of careful experiments I have to conclude that their instinct of self-preservation cannot be acted upon through the medium of their optic or acoustic nerves. They can distinguish their favourite food at a distance of ten or twelve yards, and the female is not deaf, for she answers the call of her mate from an adjoining room; but the approach of a ferocious-looking dog leaves her as calm as the sudden descent of a meat-axe within an inch of her nose. The he-sloth witnessed the accidental conflagration of his straw couch with the coolness of a veteran fireman. War-whoops do not affect his composure. I tried him with French-horn blasts and detonating powder, but he would not budge. One of my visitors exploded some pyrotechnic mixtures of wondrous colours and odours, but the tardo declined to marvel; he is a *nil-admirari* philosopher of the ultra-Horatian school."

Very different was the philosophical temper displayed by another of the author's pets. This was a young Siamese bonnet-macque monkey (*Macacus radiatus*), of which he says:—

"His conduct under circumstances to which no possible ancestral experiences could have furnished any precedent has often convinced me that his intelligence differs from the instinct of the most sagacious dog as essentially as from the routine knack of a cell-building insect. His predilection for a frugal diet equals that of his Buddhistic countrymen, and I have seen him overhaul a large medicine-chest in search of a little vial with tamarind jelly. He remembered the shape of the bottle, for he rejected all the larger and square ones, and after piling the round ones on the floor, began to hold them up against the light, and sub-divide them according to the fluid or pulverous condition of their contents. Having thus reduced the number of the doubtful receptacles to something like a dozen and a half, he proceeded to scrutinise these more closely, and finally selected four, which he managed to uncork by means of his teeth. Number three proved to be the bonanza bottle, and, waiving all precautions in the joy of his discovery, Prince Gautama left the medical miscellanies to their fate, and bolted into the next room to enjoy the fruits of his enterprise."

We have only observed one actual error in natural history, but as it is frequently repeated, we may point it out. The writer speaks of vampire bats as those which suck the blood of sleeping persons, whereas the truth is, as Belt has remarked, "the vampire is the most harmless of all bats."

The over-burdened title of the second of the above-named books serves to show its general character. The author is known from his previous works on "Chapters from the Physical History of the Earth," "The Puzzle of Life, and How it has been Put Together," &c. He is therefore already known as an ardent sportsman, a good naturalist, and an accurate observer; but in our opinion his latest work is his best. Indeed, we have seldom read a more successful and entertaining account of wanderings in which science has been combined with sport. Whether Mr. Nicols is writing about the snakes of India, the marsupials and monotremata of Australia, or the birds of South America, he manages equally to convey such vivid and interesting pictures of the animals, with their habits and surroundings, that while not a few of his first-hand observations are of importance to the scientific zoologist, nearly every page of his book is delightful and instructive to the general reader.

The plan of his work is a simple one. The first four chapters are devoted to Snakes, the second four to Marsupials, and the remaining eleven to Birds. In each case all the more interesting features of classification, anatomy, development, habits, distribution, intelligence, &c., are given with accuracy, and sandwiched between very readable descriptions of scenery, sporting adventure, &c. As an example of the latter, we may give one quotation, and for this purpose we choose one of the scenes in Australia:—

"On every side was desolation and salt-saturated earth, but here, in the midst of it, stately trees, luxuriant vegetation, and, above all, fresh water! This delightful site

was selected by myself and my friend as a camping ground for a fortnight's holiday in the height of summer. Beyond a few pounds of biscuit and the usual allowance of tea and sugar, we had nothing, intending to live on whatever the gun and fishing-line brought to bag, and pick up as much natural history as possible. At a short distance from camp a creek entered the bay, noted for the abundance of wild fowl to be found upon it in the autumn and winter; but now all the birds were away breeding in the vast impenetrable swamps to the southward of the bay, except a few barren or unmated stragglers. . . . The sole result of the day's sport had been a pelican and a small shark, obtained in the first hour after sunrise, when alone it was possible to face the heat. Neither of us being acquainted with the method (if any exist) of rendering pelican a culinary delicacy, and having made the acquaintance of fried shark to such purpose that we would not willingly renew it, nothing remained but to watch for a chance duck. This duty being allotted to me, while my companion prepared the tea, I placed myself towards evening in hiding behind a mangrove stump, with the retriever beside me. For nearly an hour I endured the torture of a mosquito assault on face, hands, and legs, when suddenly a duck turned the seaward bend of the creek, and came skimming along the water to me. I stepped forward and shouted at him, with the usual result of making him hesitate in his flight and rise well into the air, exposing the lower and most vulnerable side of his body to the charge, which in another instant laid him dead upon the water. The retriever dashed in to do his part of the work, and our supper might have been considered secure, had not a swift-winged cloud passed between the very nose of the dog and the bird, and with a splendid swoop a sea eagle bore off the duck, grasped by one strong foot. The sudden and unexpected action deprived me momentarily of all thought but admiration for the consummate ease and grace of a movement performed without any apparent effort, yet so expressive of immense power."

Among the novel or otherwise interesting observations in natural history which are recorded, we may note the following. The young kangaroo, while carried in the pouch of its mother, swallows the nipple, the end of which rests in the stomach; if forcibly withdrawn, the young animal makes no attempt to regain it, nor does it seem to have any idea where to seek for it. Again, speaking of Prof. Owen's theory concerning the object of the pouch as a "perambulator," necessitated by the long droughts in Australia to enable the mother to carry about her young in her search for water, Mr. Nicols observes that this view can only at best apply to the case of kangaroos, and therefore can scarcely be taken as the true *raison d'être* of the marsupium. Moreover, as a matter of fact, the "arboreal marsupials *do not* migrate in the severest drought, and in all probability they depend entirely upon the copious dew which falls at night upon leaves and grass."

The following observation on the king lory seems worth quoting:—

"In a few minutes he flew on to a tree well within range of the binocular, and shortly afterwards a female joined him in answer to his call. The swain was ardent, the damsel coy; they flitted from branch to branch, and whenever she perched he circled round her, threw himself underneath the branch, and swung to and fro with outspread wings, displaying the full glory of his scarlet breast. In every movement, whether on the wing or swaying at the end of a bough, he studied to present in the most effective manner the brilliant adornments of his plumage. . . . I do not think it possible for any one who had seen this little episode in bird life to have resisted the conclusion that the male was conscious of his beautiful breast, and that he adopted the best method of showing it by swinging himself beneath the branch, whence the female could look down and admire the display."

In the course of a discussion on the theory of flight Mr. Nicols has occasion to correct several errors of observation which have been made by previous writers. Thus, Captain F. W. Hutton has said that he has "sometimes watched narrowly one of these birds (albatross) sailing and wheeling about in all directions for more than an hour without seeing the slightest movement of the wings"; and the Duke of Argyll, in his "Reign of Law," repeats the statement, on the testimony, as he says in reply to a letter from Mr. Nicols on the subject, "of many writers and of some friends." But Mr. Nicols is very positive in his assertion that "the soaring does not continue for more than four minutes without a wholly new departure of from twenty to thirty powerful wing impulses, and it would seem an importation of the supernatural into Natural History to admit the possibility of sustaining the [soaring] flight for an hour." Mr. Nicols also questions the accuracy of another statement which was published by the Duke in this Journal concerning the habits of *Picus minor*. His Grace said that, having had an opportunity of closely observing these habits, he found that the cock bird drums upon the hollow parts of trees with his beak, in order to produce "instrumental music" wherewith to charm the sitting hen. On this it is remarked—

"The above is dated May, 1880. Although everything from the pen of so attentive an observer will be received with respect, it must obviously be very difficult to determine whether this is really a substitute for vocal music, or simply the ordinary beating of the tree to procure food."

We shall now conclude this brief selection from Mr. Nicols' observations, by quoting a somewhat remarkable one which he made in the Zoological Gardens of London.

"I saw a most extraordinary performance on the part of a flamingo towards a *cariama*. The two birds were in adjoining pens in the open air, separated by close wire fencing, over which the flamingo could easily reach. The cariama was constantly uttering its harsh metallic cry while standing opposite to the flamingo, both birds being as near the fence as they could stand. They remained gazing at each other thus for at least half an hour, each replying to the cry of the other. The demeanour was not in the least suggestive of anger, but what passion moved the flamingo I cannot imagine, unless it had some reference to the sexual instinct, and may indicate a habit unknown to naturalists. During the whole time that I watched them—and perhaps for long before and after—the flamingo continued to drop from its beak upon or in front of the cariama a bright scarlet fluid, which I cannot doubt was blood. The bird was certainly not wounded or hurt, and the action, from whatever impulse it proceeded, was voluntary, and appeared to afford gratification to the performer and to the recipient of this singular attention. In reply to a note on the subject, which I at once sent to Mr. Darwin, he expressed his surprise at the peculiar behaviour of the flamingo, which had not previously been brought under his notice, but did not see his way to an explanation of it. The keeper told me he had witnessed the performance once before, and assured me that both birds were in perfect health, and on most friendly terms with each other."

From the care with which Mr. Nicols has made most

of his other observations, we should have expected him in this case to have procured some of this scarlet fluid for examination, and also to have obtained permission to place both birds in the same pen; but we have said enough to show that his book seldom lays itself open to criticism of this kind, and we sincerely hope that it will, in the words of its concluding sentence, "find favour with the public," not only because it well deserves to do so, but also because the author promises that if it does, other Series of similar Zoological Notes will be issued by him from time to time. Lastly, we must not take leave of this, the First Series, without noticing the life-like drawings of the illustrations —particularly that of the cobra—and also the novel and effective character of the binding, the boards being covered with thin but continuous plates or shavings of wood, which, without at all adding to their stiffness, give so strikingly pretty a finish that we should like to see this new idea in book-binding largely adopted by other publishers. GEORGE J. ROMANES

THE GOLD COAST

To the Gold Coast for Gold. By Richard F. Burton and Verney Lovett Cameron. Two Vols. Maps and Illustrations. (London: Chatto and Windus, 1883.)

THE exploration undertaken by Captains Burton and Cameron was for the purpose of examining and reporting upon the condition and prospects of certain gold-mining properties in Western Africa, for which early in 1882 upwards of seventy distinct concessions had already been obtained from various native owners. Five only out of these concessions are reported on as having upon them mines actually in operation, while it is more than probable that of the remaining sixty-five a large proportion might be acquired by the British public for a valuable consideration.

It had originally been intended to explore the so-called Kong Mountains, relative to which no additional information has for many years been acquired; but so much work had to be accomplished on the Ancobra river that the latter portion of the programme had of necessity to be relinquished; under this disappointment Capt. Burton philosophically consoles himself with the reflection that "Geography is good but Gold is better.'

Capt. Burton left Trieste on November 18, 1881, for Gibraltar, and thence proceeded to Madeira, about which he very pleasantly gossips through some sixty pages, and where he is joined, on January 8, 1882, by his travelling companion, Capt. Cameron. From Madeira they proceed together to Teneriffe, *à propos* of which place we are given the translation of a Spanish account of the repulse of Lord Nelson from Santa Cruz de Teneriffe, which extends through at least another fifty pages.

From Teneriffe the travellers continued their voyage, touching at Bathurst, Sierra Leone, Cape Palmas, &c., eventually landing at Axim on January 24, 1882. Of Axim Capt. Burton says:—

"There is no better landing-place than Axim upon this part of the African coast. The surf renders it impracticable only on the few days of the worst weather. We hugged the north of the Bobowusúa rock-islet. When the water here breaks there is a clear way further north; the southern passage, paved with rocks and shoals, can be used only when the seas are at their smoothest. A regular and well-defined channel placed us on the shingly and sandy beach. We had a succulent breakfast with Messrs. Gillet and Selby (Linott and Spink), to whose unceasing kindness and hospitality we afterwards ran heavily in debt. There we bade adieu to our genial captain and our jovial fellow-travellers."

From Axim the travellers visited a stretch of country in the vicinity of the coast, extending some distance west of that town, and Prince's River to the east of it; and then ascended the Ancobra as far as Tumentu, situated about twenty-two miles from its mouth. After inspecting this last locality Capt. Burton returned to Madeira, leaving Capt. Cameron to continue his surveys inland as far as Crockerville, a mining settlement about forty-five miles north-east of Axim. Capt. Cameron left the African coast on March 28, 1882, and having picked up Capt. Burton at Madeira they proceeded together to Liverpool.

Rich alluvial deposits as well as auriferous quartz reefs are described as occurring abundantly, but the hydraulicing of the former is strongly recommended in preference to the mining and crushing of the latter. This opinion is so constantly reiterated that the explorers would appear to believe that a large portion of the "Dark Continent" would pay for washing. Water is stated to be plentiful, and facilities for hydraulic mining are represented as being very general.

"Wherever *catas* or 'women's washings' are found we can profitably apply the hydraulic system of sluicing and fluming, not by an upper reservoir only, but also from below, by a force pump. Water is procurable at all seasons by means of Norton's Abyssinian tubes, and the brook-beds dammed above and below will form perennial tanks."

In California as much as fifteen hundred and seventy cubic feet, or forty-three tons of water, under a pressure of four hundred feet, are sometimes discharged per minute from a single nozzle; it is therefore evident that any supply to be obtained from Abyssinian tubes, and pumped against an auriferous bank, would, in comparison with such a power as that described, possess only the force of an ordinary garden squirt.

About five-sixths of the letterpress are from the facile pen of Capt. Burton, the remainder being contributed by his companion, who appears to have done good work by determining the position of various places, of which the exact situation was previously unknown. Collections of plants and birds were also made.

Although somewhat diffuse, these volumes contain much information very pleasantly conveyed, but even in the case of gold-mining, caution is often to be recommended.

OUR BOOK SHELF

A Handbook of Vertebrate Dissection. By H. Newell Martin, D.Sc., M.D., M.A., and William A. Moule, M.D. Part I. How to Dissect a Chelonian. (New York: Macmillan and Co., 1881.)

SINCE the publication of Huxley and Martin's "Elementary Biology," there has been a growing want of more books constructed on the same model. Without such books it is exceedingly difficult for a student to dissect and thoroughly understand the anatomy of an animal which he has never examined before; and though many of the large teaching centres have their own laboratory directions, which, with the help of demonstrators, fulfil all requirements, there are few reliable practical books which can be bought by the private student.

Drs. Martin and Moule have lately brought out a pamphlet in which full directions are given "how to dissect a Chelonian," and we are pleased to see from the preface that they intend to follow it by a series of similar works. It is almost a pity that so exceedingly specialised a reptile as a Chelonian should be the first they treat, especially as they intend to include a lizard in the series, for it is doubtless far better to begin the study of reptiles with the latter than with the former.

The species on which the work is based is *Pseudemys rugosa*, but, as Dr. Martin states, "the end in view is not to provide a monograph on any one species, but to show a student 'how to dissect a Chelonian,'" the fact that, when dissecting another species, the description in the book cannot altogether be relied upon, makes the student examine everything carefully for himself.

Without working through the anatomy of a Chelonian with the help of the book it is impossible fully to appreciate its value, but from the arrangement, accuracy, and clearness of description, it will doubtless prove a great boon to the young herpetologist.

The only fault we can find with it is one under which the "Elementary Biology" equally suffers, and that is the want of illustrations. There is a frontispiece with four rather rough woodcuts of the skull (to which no reference letters are given in the text), but this is all.

Of late several illustrated students' biological books, intended as guides for practical work, have been brought out, but most of these are so inaccurate as to be practically useless. It is therefore to be regretted that a book by so eminent a teacher as Dr. Martin should be so poorly provided with figures, and we hope that the rest of the series will be more fully illustrated, as their value would be thereby greatly increased.

Ferns of Kentucky. By John Williamson. (Louisville, Kentucky: J. P. Morton and Co., 1878).

ALTHOUGH this little volume has only just reached us, it cannot be said to be out of date; for the number of popular works on ferns—those published in England excluded—is so small, that an addition to their number is at any time welcome to the fern-lover who has become well acquainted with the common British species, and would gladly increase his knowledge of the tribe. There is, indeed, no parallel in French or German literature to the number of fern-books which have been issued in England; but it would appear from the volume before us that in America fern-hunting is as popular as it is among ourselves; for Mr. Williamson asks in his preface "Who would think now of going to the country to spend a few days, or even one day, without first inquiring whether ferns are to be found in the locality?"

Mr. Williamson has given descriptions of the species found in Kentucky, and the letterpress is accompanied by sixty plates, in which the characters of the genera and the habit of each species is represented. The descriptions, although short and couched in simple language, seem carefully done; and the absence of pretence about the work does not render it the less attractive. Although, of course, primarily intended for local use, the "Ferns of Kentucky" contains much to interest the British lover of ferns.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Hovering of Birds

I AM much obliged to his Grace the Duke of Argyll and to Mr. H. T. Wharton for the notice they have taken of my letter on the hovering of birds. It may be hoped that the question will not be allowed to rest until it has received its quietus at the hands of some mathematician who shall tell us with authority whether it is possible, according to the received laws of mechanics, for a rigid body, by any disposition of its surfaces, to remain motionless (relative to the earth) for a minute in mid-air in a perfectly horizontal wind.

It is disappointing to find that the 3rd chapter of the "Reign of Law," to which the Duke refers me, contains no satisfactory explanation of the phenomenon of motionless hovering. We read there (p. 161, 5th edition, 1868). "When there is a strong breeze, no flapping is required at all, the force of the wind supplying the whole force necessary to counteract the force of gravity." This is hardly a sufficient explanation. Let us imagine a bird at rest in a horizontal wind. Neglecting friction, the only forces acting on the bird are (1) the vertical force of gravity, and (2) the resultant of the air-pressures on the different surfaces of the bird caused by the horizontal velocity of the wind. These pressures may be resolved parallel and perpendicular to the general plane of the wings, and the direction of their resultant will vary with the slope of the wing-plane; but in every case it will lie to rearward of the vertical, and therefore in every case there will be a resolved horizontal force pushing the bird backwards. Yet in spite of this force the bird is to remain at rest! Shade of Archimedes!

Consider, again, what must be the necessary corollary to the Duke's proposition. If a bird can remain at rest in a horizontal wind, it necessarily follows that in still air a bird can float horizontally without losing velocity. We do indeed see rooks and other birds float long distances descending on outspread wings in still air, and it is marvellous how slow is the descent, so great is the resistance of air to a plane surface when at every successive instant the plane surface covers a fresh body of air that has not yet begun to yield. But no one ever saw a bird maintain a horizontal floating flight in still air. Either the descent is continuous, or the bird loses velocity.

It might be wished, in a matter of such importance, involving as it does nothing less than the establishment of a miracle, that the Duke of Argyll were more precise in his statement, for I feel curious to know by what resolution of forces he would demonstrate the existence of a forward horizontal force to balance the backward force which he cannot deny to be present. A diagram setting forth the Duke's views would be exceedingly welcome. My own diagram, I fear, will not serve his purpose any better than my words, in spite of the attempt he makes to press them into his service. The Duke says: "The bird has only to slope his wing-surfaces to the [horizontal] current, and precisely the same effect is produced as if the current had been otherwise sloped upwards against a horizontal wing-surface." Perfectly true, provided his Grace can tilt the direction of gravity through the same angle. Otherwise not (at the same time there is nothing about a "horizontal wing surface" in my letter).

Mr. Wharton will do good service if, in recording any future observations he will note precisely the local circumstances under which they are made, bearing in mind that such an obstacle as a stack, a barn, a high hedge, or a thick tree might be enough to give the wind an upward throw. I presume he understands that the phenomenon to be explained is that of a bird remaining at rest in mid-air, with wings motionless, not fluttering.

Woodbridge, February 5 HUBERT AIRY

THIS is a very interesting problem, and one which has been very clearly treated by Mr. Hubert Airy, so far as he discusses it.

I think, however, that birds are able to "hover" in other conditions than that he mentions, namely, where the wind is diverted upwards by blowing straight against cliffs or rising ground.

The wind in this country at least is generally of a cyclonic character. Now such wind blows in toward the centre of depression, as has been shown by the Rev. W. Clement Ley, at an average angle of 25 degrees (inward from the tangent to the isobar). If we assume the wind to blow at a distance of 600 miles from such centre at a rate of 20 miles per hour—on an average for one mile in depth—then we have a volume of air converging upon the centre, which must rise into the upper regions there. If we assume, for the sake of obtaining a definite idea of this amount, that the centre of depression covers an area of 70 miles in diameter, say about 4000 square miles, then the air would rise vertically at an average rate of 400 feet per minute over the

area. Owing to the friction which the wind experiences in passing over the surface of the earth, however, this upward current could exist at its maximum only at a considerable height. But it is important to observe that there may frequently be a considerable amount of current upwards in the regions where birds "hover," at least in the neighbourhood of a cyclonic vortex. But how much lifting force is necessary to sustain, say a gull, in the air? A gull moves its wings in ordinary flight at from 160 to 200 (double) strokes per minute, and reckoning 12 inches as the greatest vertical depth through which the bird can raise itself by one (double) stroke, we find that it possesses the power of raising itself about 180 vertical feet per minute. This, however, is less than one-half of the rate at which we have found the currents to rise near the centre of a cyclonic depression. From this we may judge it is likely enough that birds "hover," or suspend themselves motionless in the air through the influence of upgoing currents, which are masked to our observation by the fresh winds which accompany them.

A kestrel may, however, support itself largely by its peculiar quivering play of the wings, and I think it must be difficult to determine how much support a bird may contribute by such motion, when at a height where it is difficult to observe it.

I have frequently observed gulls "hovering" upon currents of air which were heaped up by the wind striking obliquely upon a rising coast line, in which case the head is turned at an angle to the general direction of the wind, so as to face the heaped-up and rising currents. Such passing over irregular ground are irregular or gusty, and tax the bird's utmost muscular agility to prevent a sudden lateral turning to leeward, in which case the rapid flight with the currents may be compared to the fall of a stone to the ground. The same upward direction to the atmospheric currents must be imparted by the contracting sides of a converging valley. But when such local forces derive aid from the upward currents peculiar to cyclonic winds, atmospheric conditions favourable to "hovering" must, I think, frequently occur.

On the other hand, I cannot conceive it to be possible for birds (and I do not think that the third chapter of the "Reign of Law," gives any sufficient explanation) to sustain themselves motionless on currents of air which are purely horizontal, for in such case there is nothing to compensate—when the wings are slanted at the necessary angle to prevent falling—the backward horizontal force, and the creature must inevitably be driven backwards.

DAVID CUNNINGHAM

Dundee, February 5

ON August 12, 1881, I observed a hawk maintaining an apparently stationary position at a height of about 200 feet above the surface of flat ground. He was *as a matter of course* facing the wind, which blew, if I remember rightly, from the west. For the most part his wings remained motionless, but now and then he fluttered them for a little while. This was over the sensibly level plain which lies between Machrihanish Bay and Campbeltown Loch, at the southern end of the Mull of Cantire, and, curiously enough, on or close to the Duke of Argyll's property. The exact spot was about a mile and a half eastwards from Machrihanish Bay, and about three-quarters of a mile northwards from the southern boundary of the plain. There could not be any "slant upward current," such as Mr. Airy supposes, maintaining him in that position; at any rate, there was no sloping ground near.

I watched this bird for about ten minutes, and he verified in a remarkable manner the views I had held on this subject for many years, namely, that, given a steady wind blowing with a velocity which lies somewhere between certain possibly calculable limits, a hawk can remain for a time apparently motionless above a point; he is, in reality, descending a slightly inclined plane, and requires to recover vertically lost ground by the occasional use of his wings.

WILLIAM GALLOWAY

Cardiff, January 30

IN the letters on the above subject that have appeared in some recent numbers of NATURE, the writers lead us to believe that a current of air is *necessary* to enable a bird to "hover" or retain when on wing a motionless position. My observations lead to an opposite conclusion, as I have often seen both hawks and terns remain steadily poised, when there was not a breath of wind. That there was no wind where the birds (*terns*) were, was shown by their heads, when hovering, being turned in different directions, although at only a short distance from each other. Generally, if not always on these occasions, I noticed that the birds spread out their tails in a more or less depressed position, as if to counteract any forward movement likely to be caused by the wing-motion.

J. RAE

4, Addison Gardens, February 3

IN reading the letters published in your last issues of NATURE with regard to the hovering of birds, it struck me that a very similar thing can be seen sometimes, among inanimate objects when an imperfect attempt is made to cause "ducks and drakes" with a flat stone. I have commonly noticed that the missile curves sharply upwards and for a moment "hovers" as it were in mid-air before dropping. In this case and also in the similar one of the motion of the boomerang, the slanting upwards and the apparent hovering do not require, and need not be due to, upward currents, but merely depend upon the force of a horizontal current of air meeting the inherent force of the moving body. It is not unreasonable to suppose a similar simple solution of bird hovering.

C. S. MIDDLEMISS

Linnæus Street, Hull, February 3

Is there not an error in the letter of NATURE (p. 312)? The writer there suggests, as it seems to me, that a bird could maintain a position of rest, with respect to the earth, by a suitable slope of the wings against a horizontal wind. Now, as I pointed out in NATURE, vol. xxiii. p. 78, such lifting action on the part of the wind can only take place in the interval between the time when the bird is first launched from the cliff, and the time when it has by friction attained the velocity of the wind. That this interval is not a long one, is shown when balloons or other objects are launched.

[It may be well to notice, that if there were *no* friction there is no lifting power; so that if we object to the above, that "the bird gives such a *very* small friction with the wind," we thereby do away to the same extent with the lifting power; just as a frictionless ship in a constant stream would be unmoved were it sufficiently tapering.]

From the above considerations I have been compelled, since writing my last letter, to ascribe the hovering power of birds—

1. To the "exquisite muscular sense" by which they can take advantage of all upward currents of air, shifting their positions for this purpose. In an elastic fluid as the air, I imagine that the stream-lines, even over the sea, are far from horizontal. I believe the evidence of balloons over the sea goes to show this.

2. There is, to use a common expression, "flying *and* flying;" just as a man can skate without striking out, so can a bird give itself some support by quiet movements of wings and tail.

I may remark that kestrels keep fluttering their wings at short intervals while hovering; they are never still for long. So also terns and gulls, as seen from the fixed point of a cliff, are always moving and shifting in a quiet way, which may disguise both a seeking of upward currents and the quiet sort of "flying."

W. LARDEN

Cheltenham College

Science and Theology

CAN you tell me by what right the authorities of Cooper's Hill Engineering College, who are in want of a Professor of Physics, make it a condition that he should "be a Protestant," and should "attend morning chapel and Sunday services with reasonable regularity, showing in this respect a good example to the students?" The institution is one supported by the State, and is surely bound to respect the principles which underlie the State's dealings with religious matters. The president (or whoever is responsible for these preposterous conditions) may have forgotten this fact; but I cannot believe that the present Government will allow an appointment to be made until all "religious" limitations are cancelled from its conditions. As the memorandum stands at present, it appears little short of insulting to scientific men.

C.

Intelligence in Animals

MR. GRENFELL's letter in NATURE, vol. xxvii. p. 292, reminded me of a statement in vol. iii. p. 308 of Cook's last voyage, where Capt. King refers to the ordinary sagacity of bears, described in a "thousand stories" which he heard in

Kamtschatka. He gives a single instance, which, he says, "the natives speak of as a well-known fact, and that is, the stratagem they (the bears) have recourse to in order to catch the bareins, which are considerably too swift of foot for them. These animals keep together in large herds, and love to browse at the feet of rocks and precipices. The bear hunts them by scent till he comes in sight, when he advances warily, keeping above them, and concealing himself among the rocks as he makes his approaches, till he gets immediately over them, and nigh enough for his purpose. He then begins to push down with his paws pieces of rock among the herd below. This manœuvre is not followed by any attempt to pursue, until he has maimed one of the flock, upon which a course immediately ensues, that proves successful, or otherwise, according to the hurt the barein receives. I cannot conclude this digression without observing that the Kamtschadales very thankfully acknowledge their obligations to the bears for what little advancement they have hitherto made either in the sciences or polite arts. They confess that they owe to them all their skill in physic and surgery; that by remarking with what herbs these animals rub the wounds they have received, and what they have recourse to when sick and languid, they have become acquainted with most of the simples in use among them, either in the way of internal medicine or external application."

After this we are not surprised when we are told that the Kamtschadales receive instruction from the bears even in the "polite arts," and imitate in their dances the various attitudes and gestures of these animals. It seems that in the *rôles* of master and pupil the proverbial Savoyard and dancing bear would find matters reversed in Kamtschatka.

Millbrook, Tuam, February 3 J. BIRMINGHAM

Electric Railways

PROF. AYRTON speaks as to the advantages obtainable from an electric system of railways. He says:—"The mass of the locomotive adds at least 50 per cent. to the horse-power absolutely necessary to propel the carriages along" (NATURE, vol. xxvii. p. 255). In short, he speaks of the weight of the ordinary locomotive as superfluous, and considers that "a far larger number of passengers may travel at a greater speed and with less fear of danger than at present." Now, speaking practically, it is difficult to conceive of a train of carriages running sixty miles per hour without any massive locomotive in front. It would be easy enough to get up the requisite speed, but the train would certainly leave the road, there being nothing tending to keep the carriages steady, unless they were very heavy. The grip on the rails is directly as the weight of rolling stock, and it is generally found that the light coaches leave the road more readily than the modern heavy carriages. Of course the cant of the rails must not be neglected. I wish that Prof. Ayrton would favour NATURE with a few remarks on these points. V.

The Channel Tunnel

WILL you allow me to correct an unfortunate slip of the pen in my article on "The Silver Streak and the Channel Tunnel" in the current number of the *Contemporary Review*? The rate of the progress of the French Channel Tunnel from the little village of Sangatte towards the English shores was, in November last, 18 yards per day and not "per week." At the present time the Beaumont and English boring machine is cutting the 7-foot driftway at the rate of more than 20 metres per day, and has not arrived at the limit of its capacity. W. BOYD DAWKINS

Owens College, Manchester

The Great Comet of 1882

THE comet not having been visible to my naked eye during the last lunation, I was astonished to find last night that (doubtless owing to its increasing altitude, and the clear, dark sky) its tail is still so visible, quite distinctly, though very faint. I saw it best with a pair of field-glasses, aperture 2·05 inches, power 4; with which it reached to ν^2 Canis Majoris, and was therefore $5\frac{1}{2}^\circ$ long; unless part was really a wisp of the Milky Way: undoubtedly the greater part was the comet. Its axis (which was nearer the north than the south edge) was straight for $3\frac{1}{2}^\circ$, and then appeared to curve southwards somewhat. Its south edge was straight, but its north edge, which was more definite, was convex. Its width was nearly 2°. I could not detect any of the definite features which were so remarkable formerly.

The tail was nearly as long with the naked eye. Its head and two neighbouring stars were plainly visible to the naked eye as one star. One of these stars (Lalande 12,056) was decidedly brighter than the comet's head, which would be about of the 7th mag.

With a 4¼-inch refractor the head continues elongated. With a power of 20, its major axis (which was in the direction of the tail) was 16′ long, and its minor axis 11′. With a power of 38 it was 13′ by 8½′. THOS. WM. BACKHOUSE

Sunderland, January 31

Meteor of November 17

IT is perhaps rather late to revert to the auroral cloud of November 17, but I am away from home, and have only now gained the requisite information. The path which I ventured to assign for it in your issue of November 30, from a digest of the printed reports, as compared with my own observations at Clevedon, proves to have been substantially correct. The cloud passed in the zenith at Brussels, as witnessed by M. Montigny, an eminent Belgian savant; and at Laon it was seen to the northward, as it were, gliding round the upper edge of the great main arch of the aurora. The actual elevation above the surface of the earth may therefore, without much risk of error, be considered as between forty and forty-five miles.

Montreux, February 3 STEPHEN H. SAXBY

The Sea Serpent

LIKE your correspondent, Mr. Sidebotham (in NATURE, vol. xxvii. p. 315), I have frequently seen a shoal of porpoises in Llandudno Bay, as well as in other places, and on the occasion referred to by Mr. Mott, in NATURE, vol. xxvii. p. 293, the idea of porpoises was at first started but immediately abandoned. I will venture to suggest that no one has seen a shoal of these creatures travel at the rate of from twenty-five to thirty miles an hour. I have seen whales in the ocean, and large flocks of sea-birds, such as those of the eider duck, skimming its surface; but the strange appearance seen at Llandudno on September 3 was not to be accounted for by porpoises, whales, birds, or breakers, an opinion which was shared by all present.

WILLIAM BARFOOT

Welford Place, Leicester, February 2

IN the summer of 1881 I was staying for some weeks at Veulettes, on the coast of Normandy. While there, on several occasions, several members of my party, as well as myself, saw, at a distance of three or four miles out at sea, what had the appearance of a huge serpent. Its length was many times that of the largest steamer that ever passed, and its velocity equally exceeded that of the swiftest. What seemed its head was lifted and lowered, and sometimes appeared to show signs of an open mouth. The general appearance of the monster was almost exactly similar to that of the figure in your correspondent's letter published on the 25th ult. Not the slightest appearance of discontinuity in its structure could be perceived by the eye, although it seemed incredible that any muscular mechanism could really drive such an enormous mass through the water with such a prodigious velocity. I carefully watched all that any of us caught sight of, and one day, just as one of these serpent forms was nearly opposite our hotel, it instantaneously turned through a right angle, but instead of going forward in the new direction of its length, proceeded with the same velocity broadside forward. With the same movement it resolved itself into a flock of birds.

We often saw the sea-serpent again without this resolution being effected, and, knowing what it was, could with difficulty still perceive that it was not a continuous body; thus having a new illustration of Herschel's remark, that it is easier to see what has been once discovered than to discover what is unknown. Possibly this experience may afford the solution of your correspondent's difficulty. W. STEADMAN ALDIS

College of Physical Science, Newcastle-upon-Tyne, Feb. 3

Natural Enemies of Butterflies

IT would be very interesting to ascertain what testimony can be brought forward to show that the Rhopalocera are commonly the prey of insect-eating birds. The return of a gentleman who has been collecting butterflies and studying their transformations

for the last five years in Brazil, and who by my request has given especial attention to the matter, is the immediate occasion of my inquiry. Protective mimicry is a fact too well established to admit of its supporters feeling the question a delicate one. Admission into your paper will speedily settle the question.

Liverpool, February 2 HENRY H. HIGGINS

ON THE GRADUATION OF GALVANOMETERS FOR THE MEASUREMENT OF CURRENTS AND POTENTIALS IN ABSOLUTE MEASURE[1]

IV.

WE shall now consider, very briefly, the graduation of instruments for measuring volts and amperes in practical work, and we shall take as our example Sir William Thomson's graded galvanometers. The graduation of these instruments is effected by a comparison of their indications with those of a standard galvanometer such as that described above. We shall consider first the graduation of a potential galvanometer, or galvanometer the resistance of which is so high, that the attachment of its terminals to two points in a conductor carrying a current does not perceptibly change the difference of potentials formerly existing between these points. Of course any galvanometer which measures currents also measures potentials, for, if its resistance is known, the difference of potentials between its terminals can be calculated from Ohm's law; but the convenience of a galvanometer specially made with a high resistance coil is that the difference of potentials, thus calculated as existing between the two points at which its terminals are applied while they are in contact, may be taken as the actual difference of potentials which exists between those two points when nothing but the ordinary conductor connects them. For, let V be this actual difference of potentials in volts, let r ohms be the resistance of the conductor, and R ohms the resistance of the galvanometer. Then by the application of R, V is diminished in the ratio of R to $R + r$, and therefore the difference of potentials between the ends of the coil is now $V\frac{R}{R+r}$. Hence by Ohm's law we have for the current through the galvanometer the value $\frac{V}{R}\frac{R}{R+r}$, or $\frac{V}{R\left(1+\frac{r}{R}\right)}$. If r be only a small fraction of R, $\frac{r}{R}$ is inappreciable, and the difference of potentials calculated from the equation $C = \frac{V}{R}$ will be nearly enough the true value.

The instrument to be graduated is first tested as to the adjustment of its coil, needle, &c. The standard galvanometer and it are then properly set up with their needles pointing to zero, in positions near which there is no iron, and at which the values of H have been determined. The high resistance coil of the standard galvanometer and the coil of the potential instrument are joined in series with a constant battery of as many Daniell's cells as gives a deflection of about 45° on the standard galvanometer, and the magnetometer is adjusted with its index at zero, in such a position on its platform that a deflection of its needle also of nearly 45° is produced. The current actually flowing in the circuit is calculated by equation (11) or (12) from the reading obtained on the standard, and reduced to amperes by multiplying the result by 10. The difference of potentials between the two ends of the coil of the potential galvanometer is found in volts by multiplying the number of amperes thus found by the resistance of the coil in ohms. We can then obtain, by an obvious calculation, the number of divisions of deflection which corresponds to one volt between the two ends of the coil, and thence from the value of H the number of divisions which would correspond to one volt if the intensity of the field were one c.g.s. unit. This would be the number which would be marked at that position on the platform of the instrument; but, except for the position of the magnetometer nearest to the coil, positions the corresponding numbers of which are multiples and submultiples of 2 are alone marked. The numbers corresponding to the two of these positions adjacent on the two sides of the position of the magnetometer in our experiment, may be readily found by keeping the same difference of potential on the coil, and moving the magnetometer nearer to or further from the coil until the deflection is increased or diminished in the proper ratio. For example, let the deflection be 40 divisions for 20 volts, and let the value of H at the instrument which is being graduated be ·17. The number of divisions of deflection which would correspond to one volt for that position, if the field were of unit intensity, would be $\frac{40}{20} \times ·17 = ·34$. Hence the marked positions nearest to this in the two sides of it, are to be those for which the corresponding numbers are $\frac{1}{2}$ and $\frac{1}{4}$. Therefore the magnetometer must be moved further from the coil for the latter until the deflection produced by 20 volts becomes 29·4. This is the position at which the number $\frac{1}{4}$ is to be marked. To find the position at which $\frac{1}{8}$ should be marked a smaller difference of potentials must be used, as the deflection with the same battery as before would be beyond the limits of the scale. Suppose that when its number of cells is diminished to one half; we get a deflection for our first position of 20. While the potential difference in the coil remains constant, the magnetometer is pushed in until the deflection again becomes 29·4. At this position the number $\frac{1}{8}$ is to be marked. From this it is easy to see how the position for the number 1 can be found, and in the same way those for the other numbers of the series, 2, 4, &c. The number corresponding to the position of the magnetometer nearest to the coil, although not one of the terms of this series, is determined in the same way, and marked at that position on the platform. This is the method adopted in practice in the graduation of these instruments.

Another method sometimes convenient is as follows. The standard instrument, a few good Daniell's cells, and a resistance which gives a deflection of about 45° on the standard are joined in series, and the galvanometer to be graduated is applied at two points in the circuit which include between them such a portion of the resistance as gives a deflection of about the same amount. Let R ohms be the portion of the resistance included between the terminals of the galvanometer, and let G ohms be the resistance of the galvanometer coil. Let the current calculated from the deflection on the standard be C amperes, then if V be the potential difference in volts between the terminals of the potential instrument, we have by Ohm's law—

$$C = \frac{V}{R'},$$

where R' is the resistance equivalent to the divided circuit of R and G. But $R' = \frac{RG}{R+G}$, and therefore

$$C = V\frac{R+G}{RG}.$$

Hence,

$$V = C\frac{RG}{R+G}.$$

This last equation gives the number of volts indicated by the deflection on the potential instrument, for the position at which its magnetometer is placed; and from this in precisely the same manner as described above, the series of positions of the magnetometer on its platform are determined and numbered.

[1] Continued from p. 321.

For verifying the accuracy of the graduation of the potential instruments when performed by either of these methods, a standard Daniell's cell of the form proposed by Sir William Thomson at the Southampton meeting of the British Association is used. It is represented in the annexed cut (Fig. 4). It consists of a zinc plate at the bottom of the vessel resting in a stratum of saturated zinc sulphate, on which has been poured, so gently as to give a clear surface of separation, a stratum of half saturated sulphate of copper solution, in which is immersed a horizontal plate of copper. The copper-sulphate solution is introduced by means of the glass tube shown in the diagram dipping down into the liquid, and terminating in a fine point, which is bent into a horizontal direction so as to deliver the liquid with as little disturbance as possible. This tube is connected by a piece of indiarubber tubing with a funnel, by the raising or lowering of which the sulphate of copper can be run into or run out of the cell. By this means the sulphate of copper is run in when the cell is to be used, and at once removed when the cell is no longer wanted.

The electro-motive force of this cell has been determined very carefully and found, according to Lord Rayleigh's latest determination of the ohm, to be 1·07 volt at ordinary temperatures. The direct application of this cell to the galvanometer gives at once a check on the graduation. As the resistance of the galvanometer is always over 6,000 ohms, there is practically no polarization.

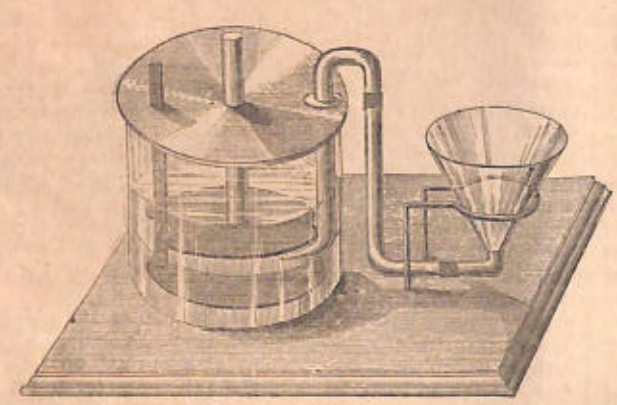


Fig. 4.

The method adopted for the graduation of the current galvanometer is precisely the same as that first described for the potential instrument. The standard galvanometer, of which in this case the low resistance coil is used, and the current galvanometer to be graduated are joined in series with a battery, which with some resistance in circuit is sufficient to give a deflection in each of about 45°, when the magnetometer of the current instrument is at a convenient position on its platform. The current flowing in amperes is given by the standard, and this, of course, is the number of amperes which is indicated by the deflection of the current instrument. By an obvious calculation from the value of H at the current instrument, precisely similar to that above described, the number of divisions of deflection corresponding to a current of one ampere for a field of unit strength is found, and from this the series of positions on the platform and their numbers are found.

The value of the field intensity given at the needles of the magnetometer when in position has generally been determined in the following manner. A battery of about 30 of Sir William Thomson's Tray Daniell's is joined in series with a resistance of about 7,000 ohms. The electrodes of a potential galvanometer, on which the magnetometer is placed without its magnet, are attached at two such points in this resistance that the deflection of the needle produced is from 30 to 40 division on the scale. The current through the galvanometer is now stopped and the magnet placed in position, and the index brought to zero by turning the magnet by means of its screw. The electrodes are now placed so as to include a resistance which makes the deflection nearly what it was in the former case. Let E be the electromotive force of the battery, I the intensity of the horizontal component of the field produced at the needles by the magnet; R_1 R_2 the amount of this resistance included between the electrodes of the galvanometer in the first and second cases respectively; V_1 and V_2 the potential difference in volts on the instrument in the same two cases; D_1 and D_2 the corresponding deflections, and G the resistance of the galvanometer. We have by Ohm's law

$$V_1 = \frac{E R_1 G}{(B + R - R_1)(R_1 + G) + R_1 G} = m H D_1$$

and

$$V_2 = \frac{E R_2 G}{(B + R - R_2)(R_2 + G) + R_2 G} = m I D_2$$

where m is a constant. Therefore we have

$$I = H \frac{D_1 R_2 \{(B + R - R_1)(R_1 + G) + R_1 G\}}{D_2 R_1 \{(B + R - R_2)(R_2 + G) + R_2 G\}}$$

If the resistance B of the battery be small in comparison with G, or if the galvanometer is sensitive enough to allow $\frac{B}{G}$ to be made sufficiently small by resistance added to G, B may be neglected; and it is generally possible, by properly choosing R, R_1, R_2, to simplify very much this formula. The number I thus found, diminished by H, is the number of c.g.s. units which measures the horizontal intensity of the magnetic field produced at the needle by the action of the magnet alone. The value of $I - H$ is the number painted on the magnet, and is generally about 9 or 10.

$I - H$ may be determined by means of a current galvanometer very easily by keeping a constant current flowing through the instrument, and using without the magnet one of the less sensitive positions of the magnetometer, and with the magnet one of the more sensitive positions. If D_1 and D_2 be the deflections and n_1 n_2 the number of divisions of deflections corresponding to one ampere at the two respective positions, the value of I is at once found from the obvious equation

$$\frac{D_1}{n_1 H} = \frac{D_2}{n_2 I}, \text{ or, } I = \frac{n_1 D_2}{n_2 D_1} H.$$

When either instrument has been graduated for a field of intensity equal to 1 c.g.s. unit, and the intensity of the field given by the magnet at the needles has been determined, the graduation of the instrument is complete. In the practical use of the instrument with the magnet in position, the number of volts, or the number of amperes (according as the instrument used is a potential or a current galvanometer) corresponding to a deflection of any number of divisions is found by the following rule:—

Multiply the number of divisions in the deflection by the number on the magnet increased by the horizontal intensity of the earth's field,[1] *and divide by the number at the division on the platform scale exactly under the front of the magnetometer.*

When the magnet is not used the rule is the same as the above, except that the divisor to be used is the value of H for the place of the galvanometer.

For convenience in the ordinary use of either instrument a position of the magnetometer on its platform, which may not be one of the series described above, is determined at which the deflection with the magnet in position, for one volt or one ampere, is one or some other convenient number of divisions. For this position lines

[1] The mean value of H for Great Britain may, *when the magnet is used*, be taken with sufficient accuracy as ·17 c.g.s.

are drawn on the sides of the platform so as to prolong the white lines on the sides of the magnetometer. By this means currents in amperes or potentials in volts can be at once read off without any calculation.

The graduation of the current galvanometer may also be performed by means of electrolysis. The electro-chemical equivalents of a large number of the metals have been determined, and it is only necessary therefore, in order to graduate the instrument, to join it in series with a proper electrolytic cell and a constant battery, and to compare the amount of metal deposited on the negative plate of the cell with the total quantity of electricity which flows through the circuit in a certain time. A convenient cell may be made with two plates of copper, each about 50 square cms. in area, held parallel to one another about 2 cms. apart, and immersed in a nearly neutral solution of copper-sulphate in a glass beaker. A current of one ampere through such a cell gives a very good result. This current will deposit about 1·2 grammes of copper per hour, which, when light plates are used, can be very accurately weighed. The plates should be carefully cleaned, dried, and weighed before being immersed in the liquid, and care must be taken not to send the current through the cell at all until it is made to flow continuously for the experiment. The instant the current through the cell is completed should be noted, and readings of the current galvanometer taken at equal short intervals during the time allowed for the experiment. The average of these readings is to be taken as the average reading of the galvanometer. When the experiment has been completed the plates are to be taken out and very carefully washed in clean water and dried before being weighed. It is generally better to calculate the quantity of electricity from the gain of the negative plate than from the loss of the positive. The electro-chemical equivalent of copper has been recently determined afresh with great care in the Physical Laboratory of the University of Glasgow by Mr. Thomas Gray, and as the result of his experiments ·000331 of a gramme of copper is deposited by the passage of one coulomb of electricity. Dividing the gain of weight of the negative plate by this number, we obtain the total number of coulombs which have flowed through the galvanometer during the experiment. This divided by the time in seconds gives the average current in amperes; from which the number corresponding to the position of the magnetometer on the platform can be determined, and the graduation completed in the manner described above.

Errata in Part II. of this Article—P. 106, line 2 from top, for $2\pi nCA$ read nCA; line 5, for $CD(=x)$ read $CD(=y)$; line 6, for $DE(=y)$ read $DE(=x)$.

ANDREW GRAY

NORWEGIAN GEODETICAL OPERATIONS[1]

THIS publication of the Norwegian Committee of the Association for the Measurement of Degrees in Europe is a report of the observations made at two tidal stations in Norway—Oscarsborg and Drontheim. The report opens with a summary of the previous tidal operations carried out in Norway. From June 8 to 28, 1835, a series of observations were made at a large number of stations at the request of the English Admiralty. These observations, together with those from numerous other stations in Europe, along the North Sea, and Atlantic coasts, were reduced and compared by the Rev. Dr. Whewell, and were published by him in the *Philosophical Transactions* of 1836.

The next tidal operations recorded were undertaken in 1839, to ascertain whether, as supposed, the Norwegian coast is slowly rising. With this object, marks were cut in the rock at twenty-six points along the coast, and the date was inscribed in each case. On the eastern coast, where there is little or no tide, only one mark was cut, at the level of the water; but along the western and northern coasts two marks were made, one at high water, and the other at low water. The report gives a detailed description of the position of these marks. In 1865 observations were made to ascertain if any alteration had taken place in the relative positions of these marks and of the sea, and it was found that on an average the land was rising at the rate of $\frac{3}{10}$" in 26 years. It was, however, seen that such determinations were very inadequate, since the mean level of the sea had not been taken into account, and the marks were not connected by lines of levelling. The whole question was accordingly placed before a Committee of the University. This Committee, however, came to no conclusion, principally on the ground of the expense of the necessary levelling operations. It was then pointed out by Prof. Fearnley, the chairman of the Committee of the Association for the Measurement of Degrees in Europe, that the resolutions of the Association at the General Assembly in 1864, at Berlin, had given an increased importance to the determination of the mean tide level. Nevertheless, it was not until 1876 that any steps were taken to establish a series of tidal stations provided with self-registering apparatus, as enjoined by the Association; six stations have now been established, and two more are to be formed.

Previously however, namely, in 1872, two stations had been established, one at Oscarsborg, in the fortified island of Kaholmen, situated in the Christiana fiord, in connection with submarine mining, and where observations have been recorded from 1872 to 1879; the other at Drontheim in connection with the harbour works, the observations at this station have extended from 1872 to 1878. These two sets of observations form, as already mentioned, the subject of the present report.

At both stations self-registering apparatus of simple designs were used. At Oscarsborg the rise and fall of the tide was marked on a *plane* sheet of paper attached to a frame moving horizontally. The motion was imparted to this frame by a weight, which, in order that the motion might be uniform, was connected to a clock. The float was inclosed in a tube, so as to annul the disturbing action of the waves. At Drontheim the paper was attached to a drum rotating uniformly by means of clock-work. The apparatus was placed at the end of a pier, and the scribing pencil was connected by a system of rods to a bridge, the other end of which was supported by a pontoon. In this manner the action of the waves was eliminated. The mean tide level was obtained at these two stations by taking the arithmetic mean of the heights of the tide recorded on the diagrams for each hour, and not by the more accurate method based on the areas described. But since these observations extended over several years, the result is no doubt practically the same; moreover, neither apparatus was provided with any means of obtaining the areas automatically, and to calculate them would have involved an enormous amount of labour.

The observations at both stations are arranged in a series of tables.[1] Table I. gives the heights of the tide at each hour, commencing at noon, for every day during one month.[2] The arithmetic mean of the heights of the tide at the *same* hour each day during the month, are also given for every hour of the day; the moon's influence does not appear in these means, they therefore show the influence of the sun. These means, for every month during which the observations lasted, are collected together in Table IV., and the mean of these means is also given. Table II. gives the height of high and low

[1] Publication of the Norwegian Committee of the Association for the Measurement of Degrees in Europe, Part I. (Christiana, 1882)

[1] The corresponding tables in each set of observations are denoted by the same number.

[2] This table was not extended further, in order to save space in the Report.

water, the time of their occurrence, and the range of the tide for every day of the same month as before. Table III. is similar to Table I., but the hours are counted each day from the upper culmination of the moon, instead of from noon. By thus arranging the table, the sun's influence is eliminated in the means for each hour, and the moon's influence on the tide is thus made apparent. Table V. corresponds to Table IV. The remaining tables, VI. to XII., give information as regards the interval of time between the moon's upper or lower culmination and high water, and similarly for low water; also the height of the highest and lowest high and low water recorded each month, specifying the direction of the wind and the height of the barometer; and lastly, the greatest and least range of the tide during each month.

The report concludes with a comparison of the semi-menstrual inequality as calculated by Whewell's formulæ, and the actual observed inequalities. The agreement is very close.

There is no discussion in the report of the results obtained, this being reserved for future publications, when the observations at the other stations are available.

The report is accompanied by five plates, showing the registering apparatus, and giving specimens of the diagrams.

SCIENTIFIC HERESIES IN CHINA

THE belief of the old writer who explained the disappearance of the swallows in autumn, by the assumption that they all rolled themselves together into a mass, beak to beak and wing to wing, and plunged "fluminibus lacubusque," there to remain until the return of spring, is but an instance of the thousand and one theories which have from all time been held by unscientific observers on the subject of the annual appearance and disappearance of migrating birds. It is not so very long ago that even among ourselves the question of the migration of sand martens was a moot point; and it need not be a matter of surprise, therefore, that the Chinese, though keen observers of nature, should be guilty of holding heretical hypotheses to account for the presence in summer and the absence in winter of birds of passage, as well as for other processes of nature which are enacted before them. Like most common fallacies, those current in China on these subjects are derived from ancient authorities, but, unfortunately, in the case of the Chinese, whose respect for everything that is old is supreme, this antiquity only entails upon them the more unquestioning faith. They are, therefore, perfectly content to believe that the disappearance of quails in autumn is sufficiently accounted for by the assumption that at that time of the year they are transformed into moles, and that in spring they succeed in reappearing again in beaks and feathers. The explanation of this fallacy is simple enough. The ploughman who in the spring and summer has seen the quails flitting among the mole-hills, finds that when, the birds having disappeared, he ploughs over the same land in winter, the moles, which he has not before seen, are the sole occupants of the ground.

Another generally accepted belief is based on what Max Müller calls a disease of language. At the opening of spring, hawks are said to become pigeons, and at midsummer to be reconverted to their original shapes. Now it happens that *Kiu*, the Chinese name for a pigeon, forms the second syllable of the word for a crested hawk (*Shwang-kiu*), and it would appear that by a corruption of terms and a confusion of ideas the first syllable has been dropped and the last has been allowed to stand in its literal and isolated meaning. Thus the original assertion that during the breeding season hawks become crested has been perverted into a meaningless and self-condemnatory myth. Chinese writers on natural history lay stress on the fact that during the spring, when "the rearing instinct in birds becomes excessive, birds of prey become excited," and when excited or angry, hawks, as is well known, erect the feathers on the head, giving the appearance of a crest.

Much in the same way has arisen the legend that in late autumn certain small birds go into the sea and become crustaceæ. In this case the error depends on the word *wei*, which is here translated "become," but which also means "for," and which, when so read, converts an absurdity into the record of a fact concerning the birds probably referred to, namely, sandpipers. These birds, we know, "frequent sandy sea-shores, some of them congregating in numerous flocks in autumn and winter, and seek their food by probing the sand with their bills, and by catching small crustaceans in pools or within the margin of the sea itself."[1] The same mistake, which is, however, complicated by a further misunderstanding, makes it incumbent on Chinamen to believe that in winter pheasants go into the lakes and become clams. The word here translated "clams" means also "sweet flags and water rushes," and in search of these hungry pheasants might very probably be tempted to seek the margins of swamps and lakes. A curious and unaccountable superstition was anciently and is still connected with this habit of Chinese pheasants. We have it on the authority of one of the Classics that "if within ten days from the beginning of winter pheasants do not go to the great waters, lascivious women will multiply in the country."

Otters and polecats, again, are the subjects of a more sentimental belief. They are said to be in the habit of offering up, the one fish, and the other animals, in sacrifice. This strange myth is accentuated in the "Imperial Encyclopædia," published by order of the Emperor K'ang-he (1661-1722), by an illustration attached to the chapter on otters, in which one of those animals is represented as squatting down on the bank of a river with his two forepaws on a newly-captured trout, and with a most devout expression on his upturned face, which is directed towards the moon. The explanation of this legend is not far to seek.

The habit common to both otters and polecats of destroying many more creatures than they are able to devour, and of leaving their victims apparently untouched after having satisfied their appetites with the flakes at the back of the fishes' necks, and with the blood of the animals, suggests to careless and superstitious observers the semblance of propitiatory sacrifices offered up to the patron saints of vermin.

Many other generally accepted myths might be quoted, which are but the perverted representations of facts. But if we descend to a lower level, to the vulgar superstitions of the masses, we find ourselves in a region where—

"Wisdom and Wit are little seen,
But Folly at full length."

A resemblance in outward form, or even in disposition, is enough to give rise to a belief that the animals are interchangeable. Thus eels are said at times to be transformed into serpents, mice into bats, and sharks into tigers, and *vice versâ*. By a curious connection of ideas, between kings and whales, also comets, those stars so abhorred by rulers, are considered to be as destructive to the lives of the monarchs of the deep as of the sovereigns of the soil.

NOTES OF TRAVEL IN SARDINIA

WHILE Sicily possesses classical associations and remains of the highest interest, together with physical features more or less dependent upon the presence of the most famous volcano in the world; and while Lipari and the associated islands are remarkable for their evidences of past and present volcanic action,

[1] *Chambers' Encyclopædia.*

Sardinia can lay claim to neither the one interest nor the other, to any marked degree. Neither can we compare it with Iceland, or with Majorca, and perhaps the only special interest which belongs to it is the occurrence of large numbers of *nuraghi*—conical stone mounds of prehistoric construction, hollow within, and probably designed as tombs by the earliest inhabitants of the island. These are scattered over the island in large numbers, particularly near Torralba.

In Roman times Sardinia never rose to much importance, hence the relics of that period are but few. The most important is an amphitheatre near Cagliari hewn out of the rock, the major axis of which is 153 feet in length, and the minor axis 98 feet. It is now in a very dilapidated condition, far more so indeed than that of Puzzuoli. There are also a few Roman tombs. The most remarkable is in a suburb of Cagliari called Santa Tenera, and it is known as the *Grutta dessa Pibera*, that is the Grotto of the Viper, from the serpents which are sculptured over the entrance. It was the tomb of Attilia Pamphilla, a noble Roman lady. On the walls there are some interesting inscriptions, which have been published by General La Marmora and by Muratori.

The few travellers who visit Sardinia nowadays are tempted rather by prospects of sport than by anything else. Moufflons still exist among the Gennargentu Mountains, also wild boar, and smaller game, but the amount of sport afforded by the island has been exaggerated, and the sportsman will commonly prefer to go to the north of Norway or to Iceland to running the risk of catching malarious fever in Sardinia.

Malaria has always been very prevalent in the island. There is a large extent of marsh land, and in the autumn a great deal of decomposing vegetable matter. We were glad to notice that some of the Englishmen who have recently acquired land in the island have not only commenced draining operations, but have also planted numbers of Eucalyptus trees, the effect of both operations being undoubtedly to diminish malaria. On the other hand, many of the native landowners are converting their timber into charcoal, for which they can obtain about fifty francs a ton in France and elsewhere. Some thousands of tons are annually shipped, and unfortunately new trees are not planted in place of the old ones. If this wholesale destruction of forests continues, there can be no doubt that the climate of the country will eventually be seriously affected. The exporters do not in the least realise that they are shipping *vis viva* in a very condensed form from their shores, and at the same time diminishing the rainfall.

The chief wealth of Sardinia lies in her mines of argentiferous galena, and of calamine. As much as 120 ounces per ton of silver have been extracted from some of the lead ores. The principal mines are those of Monte Poni, near Iglesias, in the south-west of the island, and of Monte Vecchio, in the west centre. At Monte Poni we noticed that the newest forms of Belgian and German machinery for crushing and washing the ores were in use. At the present time, the operations are very much impeded by the flow of water into the principal shaft, which will probably be to a great extent obviated, by boring a tunnel through the side of the mountain in which the shaft is sunk.

A railway constructed, and to a great extent owned by an English company, now connects the two capital cities of the island—Cagliari and Sassari—a distance of 260 kilometres, with branches to Iglesias, in the south-west, and to Terranova in the north-east, and the line is continued from Sassari for 12½ miles to Portotorres, its port, a miserable and fever-stricken village. Fifty years ago, there were scarcely any roads at all in Sardinia. The Roman roads had become obliterated, and no attempt had been made to construct them afresh.

The railway is well constructed, but the trains are extremely slow, and do not average more than seventeen miles an hour. Between Macomer and Chilirani there are many cuttings and some very steep gradients. The railway connecting the two capitals—Cagliari and Sassari—has only recently been completed. Sassari, a town of 33,000 inhabitants, is nearly as large as Cagliari, and in some respects preferable to it. It stands upon a hill 650 feet above the sea. It has a clean, bright appearance, but in reality is very badly drained and extremely unhealthy. So recently as 1855 the cholera carried off nearly one-third of the population in less than three weeks, at the rate of more than 500 a day.

In Torralba there are a number of *nuraghi*, and in the neighbourhood several extinct volcanoes, the most important of which is Keremule. Nearly midway between Mores and Torralba we saw exposed, in a recent railway cutting, a fine mass of columnar basalt overlying chalk. There is a good deal of pale green, pale pink, and grey trachite in the neighbourhood. The only geological map of the island which now exists is in General La Marmora's fine monograph published in Paris and Turin between 1839 and 1860, and entitled *Voyage en Sardaigne*.

G. F. RODWELL

MATHEMATICS IN SCANDINAVIA[1]

THE first part of the new mathematical journal has reached us. We have not quite reproduced the title; the words *Zeitschrift herausgegeben von* on one side of the axis of symmetry (let us say) of the page, are matched by the words *Journal rédigé par* on the other side. This is significant of one part of the Editor's plan: the journal, though printed and published in Stockholm, is to have its articles written in what the Editor styles the *principal* languages. It may be that English is one of these languages; there is not, however, in the preface, anything definite to relieve our doubts. The prospectus (unintentionally, we hope) is somewhat more informatory. In the language of our "lucid" neighbours it says: "In Germany, in France, in Italy, in Scandinavia, everywhere in fact, where science is held in honour (other side of the line of symmetry—'*überall wo mathematisches Leben herrscht*'), the idea of starting the journal was received with the most lively sympathy." Apparently in regard to the English language and English science, the less said the better. We, for our part, say nothing.

In outward appearance the new journal closely resembles Crelle's. The paper is equally good, the margin equally broad, and the size of page and the number of pages in a part substantially the same in the two serials. Neither is quite so handsome as a third member of the same family, the now five-year old *American Journal of Mathematics*; but then we must not forget the ratio of five dollars to twelve marks.

The list of the editorial staff supporting M. Mittag-Leffler contains many distinguished names. There are five Swedish mathematicians, four Norwegians, three Danish, and one from Finland; and scarcely one of these but is well known far beyond his native country.

The contents of the first part are all that could be expected from such a brotherhood, headed by such a chief. The first paper is by Prof. Poincaré, of Paris, its subject being the *Théorie des groupes fuchsiens*. It extends to 62 pp., and is altogether worthy of its place of honour. One does not know which to admire most—the author's grasp of his subject, or the clearness and simplicity of his exposition. Following this, comes a contribution of 14 pp. by Prof. Malmsten, of Upsala, "Zur Theorie der Leibrenten;" then there is a paper of 16 pp. on "Eine Annäherungsmethode im Probleme der drei Körper," by M. Gyldén, the head of the Stockholm Observatory; and lastly, to complete the 96 pages, there is

[1] Acta Mathematica: Zeitschrift herausgegeben, von G. Mittag-Leffler. (Stockholm, 1882.)

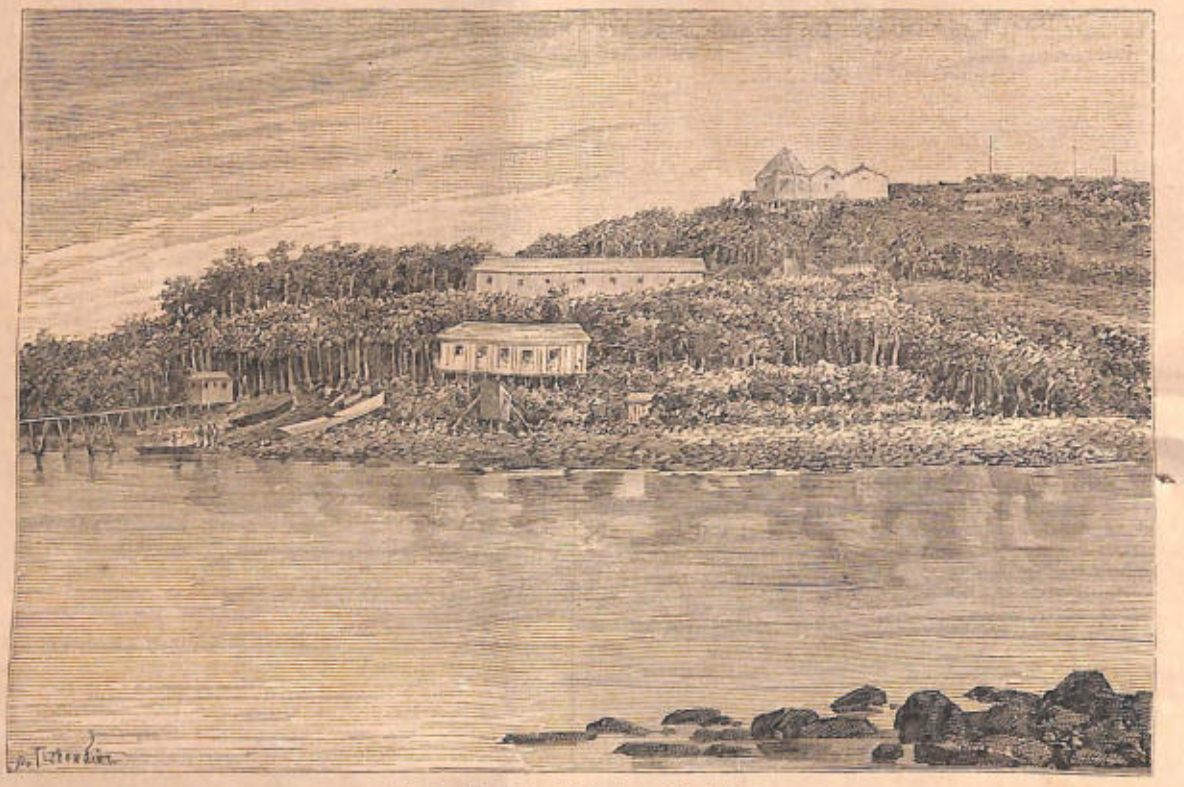
a short communication entitled "Das Problem der Configurationen," by Prof. Reye, of Strassburg. The Editor may be most heartily congratulated on the start he has made: taking everything into account, we have little doubt but that his efforts will be crowned with abundant success.

The most serious difficulty about such an undertaking is that of finance. The *American Journal* began its career with the Johns Hopkins trustees at its back: we suppose, however, that by this time it walks alone. In the present instance, the mainstay is the enlightened King Oscar the Second. Long may he live! The journal is rightly dedicated to him, the dedication being made appropriately in one of the second-rank languages, which it is cheering for us to see, have sometimes their uses.

M. D.

THE FRENCH MISSION TO CAPE HORN

THE members of the French Magnetic and Meteorological Expedition to Cape Horn have taken up their quarters at Orange Bay, and have already begun work. The accompanying illustration, reproduced from *La Nature*, after a photograph transmitted to the Paris Academy of Sciences, will give an idea of the aspect of the station occupied by the expedition. On the summit of the hill are the astronomical cabins, beside which are placed a pluviometer and an actinometer. The large house in the middle distance forms the officers' quarters, while the lower building is for the sailors. Along the shore are other structures partly shown in the illustration, a stockade for the tidal register, and an isolated tent for absolute determinations.



Station of the French Mission to Cape Horn.

The mission arrived at Orange Bay, Terra del Fuego, on September 6 last. They found the country marshy, and were compelled to select a wooded spot in order to obtain firm ground. No time was lost in erecting inclosings and installing the various instruments; and on September 26, the meteorological and magnetical observations were begun. Since the arrival of the party the temperature at Orange Bay has been very mild; the thermometer has never been below 0° C., and several times it has been as high as 16°. The air is very moist, and there has been plentiful rain almost every day, though not lasting long. Squalls have been rare. The magnetic observations will be made partly by instruments which will be read directly,—absolute determinations of declination, inclination, horizontal force, &c., and partly by means of regulating apparatus, which, so far, have worked very satisfactorily, and have given indications agreeing with those obtained from direct-reading magnetometers. The other duties of the expedition consist in astronomical and meteorological observations.

The expedition has been well received by the natives, one of whom speaks and reads English fluently. Indeed, twenty miles off, in Beagle Channel, is an English mission station, which is reported to be very prosperous. On the whole, the French expedition has been very successful; it may be regarded as one of the International Polar Observing Stations.

HEATING BY ACETATE OF SODA

M. A. ANCELIN, Civil Engineer, describes in *La Nature* a method he has devised of heating for domestic purposes, travelling, &c., by means of acetate of soda. His object has been to devise a method that will possess all the advantages of heating by means of hot water, without any of its inconveniences. For this purpose he sought

for a vehicle having a great latent heat of fusion, and after several preliminary experiments, he, in September, 1878, took out a patent for heating carriages, &c., by means of the latent heat stored in solid substances previously liquefied by heat. In the course of his experiments, M. Ancelin's attention was called by M. Camille Vincent to the acetate of soda, the very slow cooling of which during manufacture had struck him. M. Ancelin then experimented with this substance, and obtained satisfactory results. The duration of the heat in a warming-pan with acetate of soda he finds to be four times that of a warming-pan with hot water in spite of the great calorific capacity of water. This is due to the enormous quantity of heat which must be applied to the acetate of soda in order to change it from the solid to the liquid state, a heat which it again gives off as it resumes the solid state. As the result of his experiments, M. Ancelin finds that the quantity of useful heat is in fact four times greater in acetate of soda than in water. A railway warming-pan containing 11 litres of water, in passing from 80° C., the mean temperature at which it is put in the carriage, to 40° the temperature below which the heat is no longer perceptible, disengages 440 calories (11×40). The same pan containing about 50 kilogrammes of acetate, in passing from 80° to 40° disengages 1731 calories instead of 440. Practice is in accord with theory, as may be seen from the curves in Figs. 2 and 3. We see how rapid is the decrease in the temperature of the water warming-pan, while for the acetate pan the curve, at first parallel to that of water, suddenly changes at the point which corresponds to the temperature of crystallisation. The curve then remains almost horizontal, and falls very gently, rendering evident to the eye what takes place inside the pan. We obtain this result at a much less expenditure of heat for the acetate than for water. To raise the pan of water of 11 litres from 10° to 90°, four times, there is required 3520 calories. For the same quantity of acetate only 1987 calories are required, showing a saving of 1500 calories in favour of the acetate. In reality the saving is much greater. In the case of the water-pans raised to 90°, they are only at a maximum of 80° when put in the carriage, and for four heatings we get only 1760 calories, or 50 per cent. of the heat stored. In the case of the acetate, there are only 256 calories unutilised, or about 12 per cent. of the quantity stored. M. Ancelin claims for his method that it required almost one-half less expenditure of heat than in the case of the usual warming-pans, especially when we consider that the water requires four separate heatings, and the acetate only one. Long journeys can thus be made by rail, say from Paris to Havre, Lyons, Bordeaux, &c., without having to change the warming-pans, a great

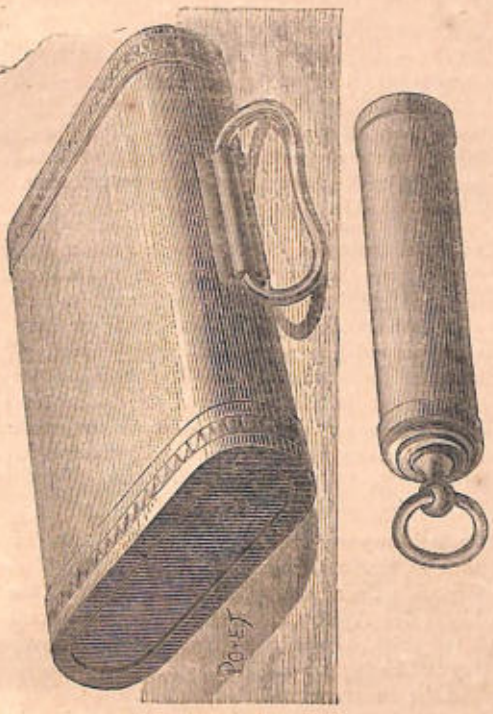


FIG. 1.—Warming-pans with acetate of soda for domestic use: the large form is for apartments or beds, the smaller for a lady's muff.

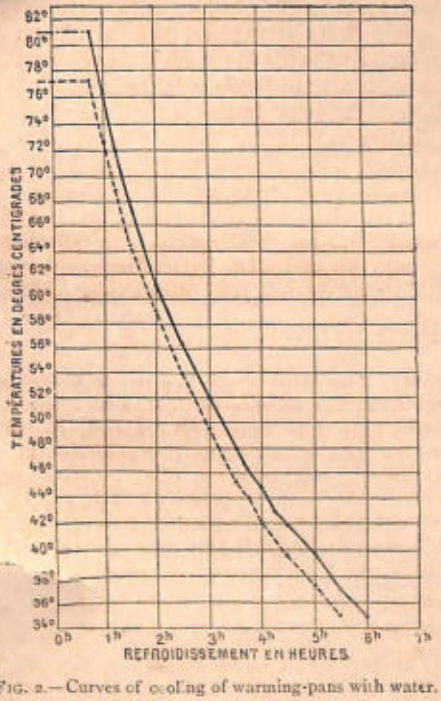


FIG. 2.—Curves of cooling of warming-pans with water.

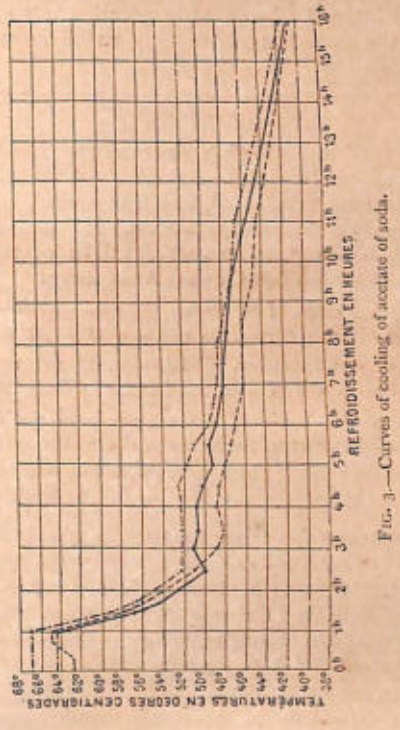


FIG. 3.—Curves of cooling of acetate of soda.

saving of labour and of annoyance to passengers. Several companies both in France and in other countries now employ M. Ancelin's method of heating; the French Western Railway Company use it in their carriages from Paris to Havre, and also to Dieppe. In England, M. Ancelin states, the London and North Western Company had, last winter, 3000 of his acetate pans in use, and double that number during the present year.

He shows that his system may be applied to domestic purposes as well as on railways. It is certainly preferable to charcoal, which, in France, is a fertile cause of death by asphyxia. Fig. 1 shows a portable apparatus, which may be used in private carriages, and even as a foot-pan in bed, and several other purposes, its heat lasting five hours. The smaller figure shows a form of heater which may be used in a lady's muff or even in the pocket. The openings by which the acetate is introduced are hermetically closed, and the substance does not require renewal except at very long intervals. In filling the receptacle certain precautions must be used, which may be easily learned after a little practice. To renew the heat in the pans they have only to be kept in boiling water for half an hour.

NOTES

We believe that two English observers are being sent out to record the approaching eclipse of the sun, and that the American Government have been asked, and have agreed, to find places for them with the American expedition. M. Janssen will be the head of the French expedition, which will be located on one of the smallest islands of the Caroline Archipelago.

On the proposition of M. Fremy, the Academy of Sciences will defray, from its own funds, the expense of sending a naturalist with the Eclipse Expedition. The Austrian Government will send to the same station M. Palisa, the director of Pola Observatory.

For the two last weeks M. Dumas has been unable to attend to his duties of Perpetual Secretary of the Academy of Sciences. He has been suffering from bronchitis, but we are happy to state that no anxiety whatever is being felt by his friends.

A Mathematical Society has been founded at Edinburgh, the initiatory meeting having been held in the University on Friday last. Mr. John S. Mackay, M.A., F.R.S.E., Mathematical Master in the Edinburgh Academy, was chosen the first president, and Dr. Knott, secretary. Professors Tait and Chrystal were elected honorary members.

General Pitt-Rivers has offered his well-known and invaluable collection, now in the South Kensington Museum, to the University of Oxford, on condition that the University provides a suitable building for it. It is to be hoped, for the sake of the University and in the interests of science, that the authorities will accept the collection on the conditions imposed by the generous donor, though we should deeply regret its removal from London.

We are not surprised that the London School Board should have hesitated last week to commit itself to the importation at once of technical education into elementary schools. The adoption of Dr. Gladstone's motion, that a committee be appointed to consider how best the Board could help in the matter, seems to us to be the judicious course to follow.

The following premiums are offered by the Society of Arts for the 129th Session of the Society (1882-83):—Benjamin Shaw Prize.—1. A Society's gold medal, or 20*l*., for the best plan for "obviating or diminishing risk to life in the operations of coal mining." 2. A Society's gold medal, or 20*l*., for the best plan for "obviating or diminishing risk to life in the manufacture, storage, and transport of explosives." The Council of the Society leave it to the competitors to bring the plans under their notice in any way they may think proper, whether by model, written description, or otherwise. Howard Prize.—A prize of 100*l*. for the best essay on the Utilisation of Electricity for Motive Power. Preference is to be given to that essay which, besides setting forth the theory of the subject, contains records with detailed results of actual working or experiment. The Society reserves the right of publishing the prize essay. Fothergill Prize.—A Society's gold medal, or 20*l*., for the best invention having for its object the prevention or extinction of fires in theatres or other places of public amusement. Designs, plans, models, essays, descriptions, inventions, &c., intended to compete for any of the above prizes, must be sent in on or before October 31, 1883, to the Secretary of the Society of Arts, John Street, Adelphi, London.

The Industrial Society of Berlin offers a number of prizes, amongst which we note the following:—1. For a method of precipitating zinc by galvanism from its dilute sulphate solution, 50*l*. 2. For the examination of German crude petroleum, with directions for preparing a good commercial product, 75*l*. 3. For a criticism of the usual indications of the value of iron and a proposal of more useful indications, 15*l*. 4. For a plan of the technical arrangements of a public institute for the examination of tissues, in order to oppose the frequent adulterations met with in textile industries, 15*l*. 5. For ameliorations in salt mines and salt works, 75*l*.

The Birmingham papers report the Town Hall crowded with working men to hear a lecture from the Rev. W. Tuckwell, Rector of Stockton, near Rugby, on "The Midland Boulders and the Great Ice Age." The lecturer described the erratic blocks of the neighbourhood, and some recent discoveries of boulder clay at Birmingham and Stockton. He presented in a popular form the discoveries and theories of Croll, Geikie, Boyd Dawkins, Lubbock, Evans; and drew a picture of early man and his brute contemporaries as revealed by the bones and implements of the caves and river gravels. The lecture was illustrated by lime-light views of glaciers, extinct animals, and human implements; and was followed on the succeeding evening by a *conversazione* at the same place, when Mr. Tuckwell exhibited and explained to successive crowds throughout the evening a splendid collection of implements, ranging from the earliest palæolithic to the latest bronze ornaments and weapons, kindly lent without charge by Mr. Bryce Wright, of Regent Street. Both evenings were arranged by the Sunday Lecture Society, which provides popular scientific lectures every Sunday night throughout the winter half of the year in the four largest Board Schools in Birmingham, with special lectures in the Town Hall three times in the year. The lectures, as was the case with Mr. Tuckwell's, are frequently marked by a religious, though not by a sectarian tone; and the crowded audiences consist of persons rarely or never seen in church or chapel.

The Berlin Academy of Sciences is about to send Dr. Lepsius, Professor of Geology at Darmstadt, with an assistant, to Athens, to make a geological survey of the neighbourhood, and endeavour to decide the question of the origin of the Athenian marbles.

Under the presidency of Count Hans Wilczek and the Baron Victor Erlanger, an International Electric Exhibition will be held in Vienna, opening on August 1, and closing on October 31. It will be an undertaking of a private nature, but is specially favoured by the Government.

There seems to be a serious decline in the once flourishing oyster fisheries of Denmark. Last year only about two million oysters were taken, which is far below the average, nor was the quality so good as usual. There were no new banks discovered during the year. The most important are now those in the Gulf of Vendsel and at Fladstrand.

THE Smithsonian Institution have received from Dr. Stejneger, who was despatched on a scientific mission to Behring Island some time since, *eleven* fairly perfect crania of the extinct Sirenian Mammal, *Rhytina stelleri*, together with sets of nearly all the other bones of the skeleton.

THE Royal Swedish Geographical Society has decided to appoint a committee, consisting of Professors Nordenskjöld and Gyldén, and Consul Elfwing, to consider the proposal for an international meridian and a common time. This committee has requested Consul Elfwing to draw up the Society's report on the question.

THE eminent Swedish palæontologist, Nathorst, has published the results of his examination of the fossil plants collected by the *Vega* expedition at Mogi, in South Japan. According to Hr. Nathorst these remains belong to the Tertiary period, and while they are older than the Glacial age, they would yet seem to be referable to a time when the climate was colder than at present. This instance of the pre-ence in Eastern Asia of a flora of polar origin shortly before the advent of the Glacial period, is considered especially interesting from the strong evidence which it affords in favour of the theory of the migrations of plants in the early ages of the world's history.

FROM a communication of the results of Herr Håkonson-Hansen's observations of the November auroral displays which is printed in the last number of *Naturen*, we learn that while there were as many as nineteen specially vivid and one ordinary manifestation at Trondhjem in that month, the extraordinary length of time during which the auroræ retained their brightness gave a special character to the phenomena. On every night of the week from the 12th to the 18th inclusive, the heavens were illuminated with the auroral light, which on the 13th appeared as early as 4 p.m., while on the 17th it continued visible from 8 p.m. till 6 a.m. on the following morning. During this period it was found almost impossible to work the telegraphic wires by day or night. The most striking display occurred on the 18th, at 4.30 a.m. when a brilliant corona appeared in the zenith, from which vivid streams of light stretched to the horizon, while luminous waves flowed uninterruptedly from the latter towards the corona, diffusing so strong a light as to enable one with ease to read moderately clear print. On the same day, at Gothenburg, it was found impossible to make the wires act.

Naturen warns its readers not to put implicit faith in the statements made in reference to the large yields of silver, which may be expected from the old silver mines in Nordland. According to the prospectus of a company, which offers the public one thousand shares in the Svenningdal mine, near the little town Mosjö, 162,000 kroner's worth of ore was purchased from the owners in 1881 for the silver works at Freiberg in Saxony, yet, as the writer points out, there were only twenty-five men employed in the mine in the course of that year! It is well known that traces of silver are constantly found in the galena and cadmium yielded by these mines, but this fact even the most daring of speculators must admit to be a very insufficient basis on which to maintain the numerous companies, amounting, it is said, to one hundred, which are offering the public at home and abroad shares in one, or other of the more or less exhausted mines of Nordland.

THE Anthropological Society of Paris has received from one of its members, Dr. Benzengre, a report of the autopsy of General Skobeleff, from which it would appear that the weight of the brain, according to Broca's system, was 1457, which is considerably above the mean for ordinary adult Europeans of his height (1·73m.) and even slightly above that hitherto given for men of exceptionally great intellect.

DR. WILLIAM H. STONE will give the first of three lectures, at the Royal Institution, on Singing, Speaking, and Stammering, on Saturday next (February 17); and Prof. Robert S. Ball will give the first of four lectures on the Supreme Discoveries in Astronomy, on Tuesday (February 20).

THE Norwegian Government have commissioned Dr. S. A. Buch during the present year to make researches, practical as well as scientific, into the great herring fisheries which annually take place on the west coast of Norway.

THE new mathematical journal, *Acta mathematica* (noticed in another column), published in Stockholm, Berlin, and Paris, has received a subsidy of kr. 1000 from the Swedish Government, while the Danish have granted a sum of 60*l*. a year to the editors in Denmark—Prof. Lorenz, and Drs. Petersen and Zeuthen.

THE U.S. Commissioners of Fish and Fisheries propose to build a large aquarium at Wood's Hole, Mass., in connection with their new station there. The aquarium will be devoted to biological researches of every description. At the adjoining station preparations are being made for the artificial propagation of cod, mackerel, halibut, and other fishes useful for food, and it is expected to hatch annually a thousand millions of cod and of other kinds in proportion.

AT the meeting of the Essex Field Club held on January 27, a resolution was passed condemning the proposed extension of the Great Eastern Railway from Chingford to High Beach. The Club regards it as "wholly unnecessary for the railway to take the route projected, and that it would not fail to prejudicially affect the advantages secured by the Epping Forest Act, which directs that the forest is to be preserved as far as possible in its natural aspect, and the Society hereby authorises the Council to petition Parliament against the project, and to send copies of this Resolution to the Press."

PROF. VERGA has been recently investigating the subject of intemperance in Milan (*Reale Ist. Lomb.*), a vice which seems to be on the increase there. Among other things he notes that suicide and kleptomania are very rare among the drunkards. With regard to the two sexes, he says that women fall much less easily into intemperance than men, and that drunken women belong to the lowest social strata, and show true brutification. Men appear to give themselves to excessive drinking more in the cold season; women in the mild season. Women relapse more frequently and shamefully into drunkenness than men, and more easily remain victims. The female drunkard excites disgust or laughter by her behaviour, but is not dangerous either to herself or to others; the male drunkard alarms by his excesses, and has often to be severely punished.

THE behaviour of the virus of anthrax in the form of spores and in that of the bacterium (*Bacillus anthracis*) under heat and various reagents, has been investigated fully by Signor Perroncito. The results are published in the *Atti della R. Accademia dei Lincei*, and show well how much greater power of resistance the spore has than the bacterium.

THE Jablochkoff light of the Avenue de l'Opera having been extinguished, the Place de Carrousel is the only street now electrically lighted in Paris at the expense of the Municipal Exchequer. But the electric light is every day finding new fields of display; a building company who are erecting a large structure in the Paris will manufacture the light on their own premises for their own use, and sell it to the tenants at moderate rates.

M. THOLLON has sent from Nice to the Academy of Sciences a determination of the velocity of the motion of the great 1882 comet, calculated from the displacement of the spectral rays.

THE action of very diluted nitromuriatic acid (aqua regia) on meat and other animal substances has been recently studied by Signor Pavesi (*Giorn. Farm. Chim.* xxxi. 529), and he finds the substance an excellent preserving agent; meat in pieces of about 1 kg. kept in the liquid in wooden vessels remains unaltered and savoury for years. The meat treated may also be dried at 15° to 20° without undergoing change, apart from a diminution of volume and the appearance of a brown colour. Put for a few hours in water, the meat recovers its original softness and natural colour. The proportions of the acids in the preserving liquid are not given. The method is also adapted to preservation of animal substances for scientific purposes.

TWO shocks of earthquake occurred at Agram, the first at 8.44 p.m. on the 4th and the other about 1 a.m. on the 5th. Both were of a violent character, accompanied, as the former disturbances were, by a rolling, thundering noise underground. The direction of the motion was from north-east and south-west, and each shock lasted about four seconds. No damage has been done hitherto. A telegram from New York, February 5, states that earthquakes have occurred at Bloomington, Illinois, and Wolfborough, New Hampshire, U.S., but no serious consequences are reported.

THE German Aëronautical Society held its general meeting at Berlin on January 13 last. During 1882 no less than 230 proposals, principally relating to the steerage of balloons, were submitted to the Society, none, however, furthering the question in any material way.

A MEMBER of the Paris "École pratique d'acclimatation" has discovered a species of spider on the African coast, the firm and long web of which resembles yellow silk very closely, and is said to be almost as good as the product of real silk-worms. The syndicate of the Lyons silk-merchants has closely investigated the matter, and the result is reported as highly favourable. There seems to be no difficulty in the way of acclimatising the new silk-producer in France.

NEWS has been received in Bolivia regarding Dr. Crevaux's mission. It appears that several members of this expedition were not killed, as was formerly reported, but are kept prisoners by the Tabo Indians.

THE Museum of the Berlin Society for Commercial Geography will be opened on April 1 next. From time to time there will be in this museum special exhibitions arranged by foreign states. Several of these are already announced. The best part of the Brazilian exhibition will remain in the Museum.

LIEUT. WISSMANN, the intrepid and successful German traveller, arrived at Cairo on January 1. His route from Loanda, by way of Nyangwe, on the Lualaba River, to Zanzibar, which measures about 3600 kilometres, led him for at least one-third of the distance through unexplored country. He has thus solved some of the enigmas of equatorial Africa. It is the southern half of the Congo basin through which Wissmann passed, and he found this to be most densely populated. This fact is remarkable, as it was entirely unexpected. Wissmann also passed through the land of a tribe of dwarf negroes. On the long and dangerous route from Lake Tanganyika to Zanzibar the traveller met with a most hospitable reception at the hands of the renowned brigand chief Mirambo, who supported him in every respect.

IN our last number we stated that M. Tissandier's electromagnetic machine had given a power of 4 horses per hour; it should have been 4 horse-power.

THE additions to the Zoological Society's Gardens during the past week include a Greater Sulphur-crested Cockatoo (*Cacatua galerita*) from Australia, presented by Mrs. Norman; a Roseate Cockatoo (*Cacatua roseicapilla*) from Australia, presented by Mrs. Sims; a Peregrine Falcon (*Falco peregrinus*) from North America, presented by Mr. C. H. Webster; a Vulpine Phalanger (*Phalangista vulpina*) from Australia, presented by Mr. G. S. Northcote; four Ceylonese Terrapins (*Clemmys trijuga*) from Ceylon, four —— River Turtles (*Emyda* ——) from India, a Globose Curassow (*Crax globicera* ?) from South America, deposited; a Blue-cheeked Amazon (*Chrysotis cœligena*) from Guiana, two Maximilian's Parrots (*Pionus maximiliani*) from Brazil, purchased; a Collared Fruit Bat (*Cynonycteris collaris*), two Four-horned Antelopes *Tetraceros quadricornis*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

DENNING'S COMET.—Mr. W. E. Plummer, of the University Observatory, Oxford, has made an interesting contribution to a branch of astronomical investigation, in which we have not shone greatly in this country, in the shape of a definitive determination of the elements of the orbit of the comet discovered by Mr. W. F. Denning, of Bristol, on the morning of October 4, 1881, which proved to be one of short period, though not previously observed. Accurate positions were obtained between October 5 and November 19 at Athens, Dun Echt, Harvard College, U.S., Marseilles, Odessa, Oxford, Palermo, Paris, Rome, and Strasburg. Starting with the second ellipse calculated by Dr. Hartwig, which assigned a revolution of 8·884 years, Mr. Plummer compares all the observations with an ephemeris computed therefrom. He then determines, by the method of variation of constants, for four-day intervals, the effect of perturbations by each of the planets from Mercury to Saturn inclusive, during the period of visibility; the influence of the perturbations upon the observed right ascensions and declinations being inferred by calculating the differential coefficients for variation of elements for the particular epochs, and these coefficients were used in the formation of equations of condition. The tabular longitudes of the sun were corrected by the results of observations at the Royal Observatory, Greenwich, supplied by the Astronomer-Royal. Normal equations were then formed and solved in the usual manner by least squares, and a corrected set of elements was thus found. The positions of the comet computed from them, and the positions inferred from the substitution of the corrections to the elements in the original equations of condition, agreed generally, but owing to their considerable amount, and the neglect of terms of the second order in calculating the differential coefficients, the agreement was not exact. Since Mr. Plummer's principal object was the determination of the comet's mean motion, he preferred to obtain the values of the several unknown quantities in terms of the mean motion, so that by successive small variations of this element, accompanied by the corresponding alterations in the others, several sets of elements could be formed, and the preferable orbit selected by direct comparison with the observations. This additional labour adds much to the value of Mr. Plummer's work. He accepts as the most trustworthy guide the sums of the squares of the errors in right ascension and declination, though the two do not correspond to precisely the same value of the mean motion, and so obtains the following definitive orbit:—

Epoch 1881, September 28·5 Greenwich M.T.

Mean anomaly	1 40 35·39	
Longitude of perihelion ...	18 36 12·8	Mean Eq. 1881·0
,, ,, ascending node	65 52 2·0	
Inclination	6 50 22·6	
Angle of eccentricity	56 8 28·4	
Log. semi-axis major	0·6315148	
Period of revolution	3235 days.	

Hence we find (the unit of distance being the earth's mean distance from the sun)—

Semi-axis major ...	4·28070	Eccentricity	0·8304135
,, minor ...	2·38498	Period in years ...	8·8567
Aphelion distance..	7·83545	The perihelion passage Sept. 13·43493 M.T. at Greenwich.	
Perihelion distance	0·72595		

The orbit of this comet is remarkable for the near approach it makes to the orbits of Venus, the Earth, and Jupiter. By Mr.

Plummer's figures it appears that in longitude 223°·4 the comet is distant from the orbit of Jupiter only 0·145, a sufficient explanation of a probable cause of the short period of revolution.

THE GREAT COMET OF 1882.—The positions subjoined are extracted from an ephemeris published by Herr Stechert, of Berlin (*Astron. Nach.* No. 2486), and founded upon the elliptical elements of Dr. Kreutz:—

At Berlin Midnight

	Right Ascension.	Declination.	Distance from Earth.	Distance from Sun.
	h. m. s.	° ′		
February 8 ...	6 1 57 ...	−20 5·8 ...	2·412 ...	3·008
10 ...	6 0 17 ...	19 34·5		
12 ...	5 58 46 ...	19 3·7 ...	2·506 ...	3·053
14 ...	5 57 25 ...	18 33·4		
16 ...	5 56 12 ...	18 3·6 ...	2·602 ...	3·118
18 ...	5 55 8 ...	17 34·4		
20 ...	5 54 13 ...	−17 5·7 ...	2·700 ...	3·172

ASTRONOMICAL TELEGRAMS.—Mr. Spencer F. Baird, Secretary of the Smithsonian Institution, notifies that arrangements have been completed with the Director of the Harvard College Observatory for conducting the system of telegraphic announcements of astronomical discoveries, which was established by the Institution in 1873, and that henceforward the American centre of reception and distribution of telegrams will be "The Harvard College Observatory, Cambridge, Massachusetts," to which all astronomical telegrams should in future be sent.

THE MATTER OF SPACE

OF late years there has been a growing tendency towards the belief that matter is present everywhere throughout the universe, as well in interstitial space as in the bodies of the spheres. Yet an older hypothesis is still widely held. The phenomena of light seem to require some substantial medium in space, but this substance has been viewed as specifically distinct from matter, and named ether. Another class of thinkers has devised still another species of substance. This is required to meet the demands of the new gravitation hypothesis; and consists of excessively minute particles, moving with intense speed, and pressing vigorously on the larger and slower particles of matter. In the past still other species of substance were imagined; heat, electricity, etc., were each ascribed to a specifically distinct substance.

Now, however, the tide has turned, and the inclination is to believe in only a single form of substance. There are, of course, countless distinct conditions produced by the aggregations of substance, and variations from simplicity to complexity, but this may not necessarily require more than a single kind of basic particle, or whatever we may call it. If the substantial contents of space are similar in constitution to the matter of the spheres, their state of existence must be much more simplified. In the spheres we have matter ranging from the simple elementary gases of the atmosphere, through the complex mineral compounds of the solid surface, to the highly compounded organic molecules. In outer space the variation is probably in the opposite direction, and substance may exist there in a condition much more highly disintegrated than the atmospheric gases. This view is not held by all theorists. Dr. Siemens argues that space holds molecules of considerable intricacy, comprising certain terrestrial elements, and their simpler compounds; as to the contents of space we know that there are very numerous solid masses, some of considerable size, others minute, and possibly ranging through many degrees from the largest to the minutest. Yet these really occupy but an inconsiderable portion of space, and apparently originated in suns or planetary orbs.

Such is, briefly stated, the state of knowledge and of hypothesis concerning the substantial contents of space. We need but add the uncertain reasons for arguing the presence of a resisting medium in space, and the necessity of a highly elastic condition of the light-conducting substance, to exhaust the subject so far as yet pursued.

It is held by some that the gravitation energy of the suns and planets is sufficiently great to sweep space of all contiguous material particles, except those solid masses which are saved from this fate by the vigour of their orbital motions. The atmospheres of suns and planets are retained with an energy very greatly in excess of their reverse energy of molecular motion, and therefore it is quite impossible that any of this material should escape into space, or that any similarly-conditioned material should exist contiguous to the spheres without being forced to become atmospheric matter. The centrifugal energy of the earth's atmosphere at the equator is only $\frac{1}{17}$ of that necessary to overcome gravity. The molecules of the atmosphere have also a vigour of heat vibration about equal to their centrifugal energy. Hence the resisting energy of these molecules is far below the gravitative energy, and they are vigorously held.

The question of the possible existence of gravitating matter in interspheral space depends strictly upon that of its motor energy. If the momentum of any particle, or of the whole sum of particles, be insufficient to constitute a centrifugal energy equal or superior to the centripetal energy of gravitation, then the material contents of space must inevitably be drawn into the attracting spheres, as atmospheric substance, and space be denuded of matter. If, on the contrary, the centrifugal energy of these particles be sufficient to resist gravitation, they will remain free, and space continue peopled by matter.

Such gravitative particles, wherever existing in space, could not be for an instant free from the influence of spheral attraction whatever their energy of motion. If this energy be too small, they must be related to the spheres as falling bodies, and must become atmospheric matter. If the two opposite energies be equal, they must be related to the spheres as planetary bodies, and circle in fixed orbits around the centre of attraction. If the centrifugal energy be in excess they must assume the condition of independent cometary bodies, temporarily influenced but not permanently controlled by any sun, and wandering eternally through space.

Such are the three possible conditions of the material contents of space. If the first obtain, space must be denuded of matter; if the second obtain, it will permanently contain matter in a partially elastic state; if the third obtain, it will permanently contain matter in a highly elastic state, since the pressure upon each other of the vigorously centrifugal particles must be great, and may be extreme. Of course no single particle could long retain its direction of motion, as related to any sphere. Constant impacts must constantly vary the directions of molecular motion. But the motion of each particle is successively transferred to a long series of particles, and thus is virtually continued in force and direction. Each motion pursues its course independently, though not as affecting any fixed particle of matter, and each particle aids in the progression of a vast network of motions, proceeding in every direction throughout the universe. Thus each particle, though not actually changing its place, may have motor relations which extend in every direction to the utmost extremes of space. It is a node in an interminable network of motions, and its incessant leaps throughout the limits of its narrow space are each part of a long motor line, which affects successively myriads of particles. So far as the energy of gravitation is concerned the effect upon this incessantly transferred motion is precisely the same as if the motion was confined to a single particle. If it lack energy the motion will be a falling one; if it equal the gravitative energy it will form a closed orbit. If it exceed the gravitative energy it will form an open curve, and be only temporarily controlled by any sphere.

In this interchange of motor energy certain particles may continually decrease in vigour of motion, and if near solar orbs may be drawn in as atmospheric matter. But they can only lose motion by transferring it to others, which would in consequence become more independent of gravity. The sum of motor energies in the universe must persist unchanged, and the aggregation of atmospheric substance around any planet must cause an outflow of motor energy which will increase the motor vigour of exterior particles. In such a case the height of atmosphere in any sphere will depend, partly on the attractive vigour of the sphere, and partly on the average motor vigour of the whole sum of matter. Every contraction and loss of motor energy by any portion of matter will increase the motor energy of remaining matter, and a fixed limit to the atmospheric control of every sphere must result, since in the outer layers of its atmosphere the centrifugal energy of molecular motion must increase until it equals the energy of gravitation.

Can we arrive at any conclusion as to which of the three possible conditions above considered really exists? If so we can answer the question as to the existence of matter as a constant tenant of space, and also reach some conclusions as to the character of its motor conditions.

There is one line of thought which seems to lead to a settlement of this question. If the nebular hypothesis of the formation of solar systems be accepted as true, either wholly or partly, there can be no doubt as to the interspheral status of matter. The conditions of nebular aggregation indisputably settle it.

This hypothesis holds that the matter now concentrated into suns and planets was once more widely disseminated, so that the substance of each sphere occupied a very considerable extent of space. It even declares that the matter of the solar system was a nebulous cloud, extending far beyond the present limits of that system. From this original condition the existing condition of the spheres has arisen, through a continued concentration of matter. But this concentration was constantly opposed by the heat energy of the particles, or, in other words, by their centrifugal momentum. This momentum could be only got rid of by a redistribution of motor energy. If, for illustration, the average momentum of the particles of the nebula was just equivalent to their gravitative energy, then a portion of this energy must radiate or be conducted outwards ere the internal particles could be held prisoners by gravitation. The loss of momentum inwardly must be correlated with an increase of momentum outwardly.

This is a necessary consequence of the heat relations of matter. As substance condenses its capacity for heat decreases, and its temperature rises, hence a difference of temperature must constantly have arisen between the denser and the rarer portions of the nebulous mass, and equality of temperature could be restored only by heat radiation. This radiation still continues, and must continue until condensation ceases, and the temperatures of the spheres and space become equalised, but this is equivalent to declaring that as the particles of the spheres decrease in heat momentum those of interspheral space increase, and if originally the centrifugal and centripetal energies of matter approached equality, they must become unequal, centripetal energy becoming in excess in spheral matter, centrifugal energy in the matter of space. Thus, as a portion of the originally widely distributed nebulous matter lost its heat, and became permanently fixed in place by gravitative attraction, another portion gained heat, became still more independent of gravity, and assumed a state of greater nebulous diffusion than originally. The condensing spheres only denuded space of a portion of the matter which it formerly held, and left the remainder more thinly distributed than before. The spheres, in their concentration, have emitted, and are emitting, a vast energy of motion. This motor energy yet exists in space as a motion of the particles of matter, which therefore press upon each other, or seek to extend their limits, with increasing vigour, so that the elasticity of interspheral matter is constantly increasing.

It might be hastily imagined that such an excess of heat vigour in the matter of space over that of the spheres should declare itself in temperature. But it must be remembered that temperature is no measure of the absolute heat contents of matter. Condensation increases, rarefaction decreases, temperature with no necessary change in absolute heat contents. The expression "fire mist," so often applied to the matter of uncondensed nebulæ, gives a very erroneous impression. The matter of the solar system nebula, though containing a high degree of absolute heat, was probably of low temperature. Its great rarity must certainly have greatly decreased its temperature. As a differentiation in this matter took place, one portion becoming condensed, another portion more rarefied, the former must have increased, the latter decreased, in temperature. Eventually the extreme condensation of one portion of this matter, and rarefaction of another, caused an extreme difference in temperature. An excessive radiation from the spheres to space has taken place in consequence, the absolute heat of the former constantly decreasing and that of the latter increasing. But the difference in temperature still continues great, the influence producing it acting much more rapidly than the influence tending to obliterate it. Eventually an equality of temperatures may be produced, but only by the production of a very considerable inequality of absolute heat. This must be the final result of spheral condensation and nebulous rarefaction of exterior matter; namely, equalization of temperature, with a change from the original homogeniety to a great heterogeniety of heat contents.

But we are again brought back to the question of the motor energies of matter. Are they sufficiently great to enable a portion of this matter, when reinforced in motor energy by radiations from the spheres, to defy gravitative attraction and, remain free in space? Undoubtedly so, and much greater than would be simply requisite for the purpose, since we find the matter of the planets, after their immense losses by radiation, still possessed of a considerable excess of motor energy. The earth, for instance, has an orbital motion sufficient to maintain it at a considerable distance from the sun. But the motion of the earth is but the combined motion of its molecules. This motion once existed as independent molecular motion, which in time, under the influence of gravity, became dependent molecular motion. We have already spoken of the fact that the particles of space, in consequence of their heat motions, tend to dart off in straight lines of motion, except in so far as the gravitative attraction of spheres causes these lines to become curved. These lines of motion, so far as individual particles are concerned, are checked by the particles coming into contact with others. The motion, however, proceeds onwards, though it is carried by successive, instead of by single particles. If, however, a number of particles move in company in the same direction, they may move much further as individuals, before transferring their energies. And if an immense mass of particles come to thus move in company their individual excursions may be indefinitely extended. The lines of motion, instead of being continued by successive particles, are continued by the same particles, and molecular motion becomes mass motion. The motion of terrestrial molecules, in their revolution around the sun, resemble those of the molecules in Prof. Brooks vacuum tubes, constituting his "fourth state of matter."

Now the degree of resistance of such a mass to centripetal energy will indicate the degree of resistance of the original uncombined molecules. In the earth the motion of the molecules, thus combined, yields a centrifugal energy sufficient to maintain the earth at its present distance from the sun. But this is only a portion of its molecular energies. Its molecules possess considerable independent motion, and form nodes in lines of radiation that extend in every direction. They have also lost a great vigour of motion by radiation to space. It follows that the original momentum of these molecules must have constituted a centrifugal vigour greatly in excess of their centripetal vigour. It secondarily follows that the momentum of those molecules of the nebula which still exist in space, augmented as it has been by radiations from the spheres, yields a very energetic excess of centrifugal vigour. Many of the comets have a centrifugal energy in excess of the centripetal energy of the sun, yet this represents only a fraction of the energy of their molecules, and a much smaller fraction of the energy of the material particles of space.

The combination of the centrifugal energies of terrestrial particles is due to the fact of a secondary centre of gravity having been formed. The heat velocity of its particles, in excess of that displayed in their revolution around the sun, has become partly a revolution around the earth's axis, and is partly retained as heat vibration. But the heat velocity of the material particles of space is not thus secondarily employed. It is affected by the attraction of the sun, or of the nearest sphere; but evidently, from the considerations above taken, this attraction cannot be sufficient to over-balance the centrifugal energy and cause atmospheric aggregation or even to cause orbital revolution. The particles must have energy sufficient to make them independent of spheral gravity. Their straight lines of motion must become to some degree curved in response to gravity, but cannot become closed curves. Instead of becoming planetary, they remain cometary lines, of very open orbit. For if we imagine the earth to be suddenly restored to its nebulous condition, or its particles to be set free in space, they would possess a velocity of motion much in excess of the earth's orbital velocity. Hence they could not be controlled by the sun. The existing particles of space possess a still much greater velocity, and are therefore much more free from gravitative control.

Certain necessary results of this condition have been considered. The lines of centrifugal motion in space are not confined to single particles as in the earth, but are transferred from particle to particle. The effect, however, is precisely the same; this motion of successive particles is in no respect different in effect from what we would have if a single particle were free to move in the same direction. Each particle moves a certain distance, and then transfers its motion in that direction to another. But it immediately pursues some other direction of motion in response to impact, and this aids in the progressive movement of innumerable lines of motor energy. The great centrifugal vigour of these motions must cause an energetic com-

pressing influence upon interspheral matter, and thus produce an elasticity, sufficient perhaps for the requirements of light radiation.

The lines of motion thus transferred through space cannot be unvarying in their orbital directions. Nature knows no great or small in her processes, and each moving particle of the free matter of space is controlled by the same principles which control the motions of a planet. It is subject to perturbations from lateral attractions, similar to those which draw planets and comets out of their orbits, and completely change the orbit of the latter. And its impacts with other particles yield effects such as would arise in impacts between planets of oppositely moving systems. Action and reaction are equal, in this as in every case. The orbit and the speed of a line of motion may be changed through impact or attractive resistance, but only by its causing an opposite change in some other line. Thus the lines of motor energy referred to are not unvarying in speed and direction, but are unvarying in their sum of correlated speeds and directions. The variations which take place in the orbits of spheres and comets through attractive perturbation, and the greater variations which would take place did spheres come frequently into contact, are precisely similar to those which must occur in the case of interspheral particles, and any change in the direction of one orbit is balanced by an equal opposite change in the direction of another orbit, the balance of motor direction and energy in nature being exactly preserved.

If such a line of motion pursues a cometary ellipse and enters the atmosphere of a globe, it must be affected by friction precisely as if the line of moving particles were a single particle, or a minute comet. It might be obliterated by friction or resistance, as the orbital motion of a falling body is obliterated. But this obliteration is really caused by the opposing energy of opposite lines of molecular motion. The single line of motion may be distributed into a thousand lines differing in direction, but the component of these thousand lines must agree with the original line.

The transfer of motion from particle to particle here indicated may take place through attractive resistance as well as through impact resistance. The original disintegration of the matter of space must have increased, as spheral condensation denuded space of much of its material, and as radiation from the spheres increased its motor energy. If matter thus divided up into smaller and smaller particles, these may have continued as closely contiguous in space as are the molecules of spheral atmospheres. In such a case they may present the conditions of excessive rarity so far as weight of matter is concerned; of close contiguity of particles, sufficient to permit the exercise of attractive energy; of great compression, through their vigour of centrifugal motion, and of intense elastic resistance to compression. These are the conditions necessary for the transfer of the radiations of light and heat. In these radiatures motion is conveyed through space by transfer of vibratory motions, not of impacts. The vibrating particle swings between lateral chains of attraction, and causes a like transverse swing in successive particles with which it is attractively connected. Greater energy here causes only greater width of vibration, not greater rapidity of transfer. The latter depends only on the elasticity of the matter concerned. Impact transfer of motion, on the contrary, must differ in speed with every difference in vigour. It is transferred by the motions of what we know as local heat, similar to the incessantly varied heat motions of gaseous matter. As the particles are unvarying in weight, increased momentum can be gained only by increased rapidity of motion, and the lines of motion thus transferred through space vary in speed with every variation in vigour. Every motion, of every particle of matter, is really a minute portion of an orbit, which represents that of a falling body, of a planet, or of a comet, according to its rapidity. Though the momentum affects successive particles of matter the orbit is continous, except to the extent that it is varied by perturbations through attraction and impact.

Wherever any influence aids a translation of interspheral matter—causes a wind to blow through space—the lines of motion continue to be conveyed by the same particles. The orbital motions of the spheres are such winds through space; minor aggregations of moving matter may enter the atmosphere of the sun or other globes. But no atmosphere can become permanently increased in this manner; such masses, checked by friction, must yield motion, which flows outward. The centrifugal energy of the molecules of the external atmosphere is thereby increased, and the gain of matter must be balanced by an equal escape of matter at that critical atmospheric limit where centrifugal and centripetal energies are in balance. But any such fall of interspheral matter must aid the radiant emissions of the sun. Its loss of proper motion, its high degree of absolute heat, its increased temperature through condensation, and its consequent radiation, would make it a source of solar heat. Any such cometary matter must form part of "The Fuel of the Sun."

Philadelphia, U.S. CHARLES MORRIS

THE INSTITUTION OF MECHANICAL ENGINEERS

THE Annual General Meeting of this Institution was held on January 25 and 26, at the Institution of Civil Engineers, Great George Street; and the papers read were of unusual interest, from a scientific point of view, for a society whose aims are so distinctly practical. As it was pointed out by the president, Mr. Westmacott, three out of the five papers on the list were contributed by professors of science, and dealt with aspects more or less theoretical of the subjects treated upon. This forms, in fact, an additional instance of the way in which the old barriers between theory and practice are breaking down, and it is everywhere becoming recognised that neither can flourish without the aid of the other.

The first two papers, though quite independent, were both evolved, as it were, out of the same subject, namely, the research which the Institution has for some time been carrying on into the properties of hardened and unhardened steel. The first of these is an *interim* report by Prof. Abel, C.B., F.R.S., on the present stage of his experiments relating to the condition in which carbon exists in steel. Preliminary trials had shown that the treatment of steel and iron by a chromic acid solution (produced by mixing a solution of potassium bichromate, saturated in the cold, with one-twentieth of its volume of pure concentrated sulphuric acid) gave great promise of success in detecting the chemical differences existing in the same steel, according to the treatment to which it has been subjected. When cold—rolled, and annealed steel was thus treated, it yielded considerable amounts of an insoluble residue, consisting of black spangly particles, strongly attracted by the magnet, and presenting the characteristics of a true carbide, to which was assigned provisionally the formula Fe_6C_5. With hardened steel, on the other hand, but a small quantity of such particles were obtained, mixed with a lighter sediment; and the total residue contained only about one-sixth the carbon in the original steel, whereas in the annealed samples nearly all the original carbon was detected in the residue. The theory to which this points clearly is that in soft steel the carbon exists in a state of chemical combination, forming a carbide which is disseminated as a separate body through the mass of the iron; but that in hard steel this combination is dissolved, and the carbon exists in its pure form, either merely in mechanical admixture, as in the case of grey cast-iron, or in that peculiar and not very well understood form of association which metallargists term an alloy. It would follow that the process of tempering, or rapid cooling, does not leave time for the complete formation of the carbide, and that in tempered steel all or some of the carbon still survives in its free or alloyed condition.

The fresh experiments described by Prof. Abel give, on the whole, great support to this theory. Four preparations were made of steel dissolved in chromic acid solution made as above, but of different degrees of strength. In the last only, where the strength was very high, were the results different, showing that the carbide had not been able to resist the oxidising effects of the solution. In the others, a considerable deposit was found, which, after being kept for several days, first in the original and afterwards in a fresh solution, was washed and dried, and then analysed. Another portion of the same was treated with chlorhydric acid, in order to ascertain what proportion would be converted into hydrocarbon. When this proportion was deducted from the whole, the remainder showed a most remarkable uniformity of composition, the percentages of carbon in three experiments being 5·93, 5·94, and 6·00 respectively. It seems evident that we have here a definite compound, to which Prof. Abel gives the formula Fe_3C. The deviations from this exact composition he accounts for by the presence of a certain amount of water, indicating that a carbo-hydrate had been formed, probably as a result of the action on the carbide first separated.

Prof. Hughes's paper, which was illustrated by a series of very

striking and elegant experiments, performed with the simplest apparatus, may be considered, in its result, as the complement of Prof. Abel's. The latter goes to show that in soft steel the carbon is present as a chemical compound, which is dissolved by hardening; the former, that in hard steel the carbon is present as an alloy, varying with the temper. Between the two, we seem to reach the threshold of a complete theory. They approach the subject, however, from different sides, Prof. Hughes's work being purely electrical, Prof. Abel's purely chemical; and this makes their convergence the more important. Finding that the induction balance was equally sensitive to molecular and to chemical changes, in the metals tested, Prof. Hughes set himself to devise an instrument by which to examine the former class of phenomena by themselves. A wire forming the core of an ordinary magnetic coil, and capable of being shifted, twisted, &c., as desired, supplies what he requires. The coil is joined to a galvanometer, or, better still, to a telephone; the wire is joined to a battery, and currents are sent through it. So long as the wire is at right angles to the coil, no effect is produced; but if we set it at an angle to the coil, sounds are instantly heard, betokening the presence of induced currents in the coil. This is the ordinary effect of electro-magnetic induction, as discovered by Faraday. Now, instead of shifting the wire, let us give it a slight twist, say of 40°: the sounds are instantly heard as before, and we detect induced currents, which are positive for right-hand torsion, negative for left-hand torsion. Prof. Hughes's explanation of this is that the molecules of the wire, which he regards as so many separate magnets, have been given a twist round the axis, and thus set at an angle to the coil, just as the whole wire was by the shifting in the first case. Now let us twist the wire still further, even to several complete turns. No greater strength of current is observed, showing that the angle once given to the molecules is not exceeded, and that the subsequent torsion is of the wire as a whole. Approach to the wire, thus twisted, one pole of a natural magnet, laid parallel to the wire: the sounds cease, indicating that the magnetism has spun the molecules back again into their original directions. Approach the magnet at right angles to the wire: the current returns to zero while it is still two inches distant, and when it is in contact there is a reversed current, which is then at its maximum. Lastly, removing the magnet, untwist the wire by some 40°: the current returns to zero, showing that the molecular torsion has disappeared, while the molar torsion remains almost the same as before.

In all these effects the wire has been supposed to be of soft iron: a remarkable difference appears when we turn to tempered steel. For we now fail to detect more than slight traces of molecular disturbance or rotation, no matter how many turns we give to the wire. Thus, whereas in the iron we appear to have great molecular freedom, with steel we have almost complete molecular rigidity. But this molecular rigidity is found to obtain also in all alloys of steel which have been tested—*e.g.* magnetic oxide, iron and sulphur, iron and tungsten, &c. Hence we draw the conclusion that tempered steel is likewise an alloy, the associated elements in this case being, of course, iron and carbon.

The above is the essential part of this striking paper; but the same idea of molecular freedom and rigidity was illustrated by other examples. Thus, if a tube nearly filled with iron filings be magnetised, the magnetism, though permanent so long as the tube is still, is removed in an instant by shaking, or even by turning the tube gently, so that the filings roll over each other. If, however, we pour in any viscous liquid, the magnetism can indeed be imparted, but it cannot be mechanically removed: the filings are no longer able to revolve back into their former positions. Again, we may magnetise an iron and a steel wire to the same degree, and then give each a slight pull to set it in vibration; it will be found that almost the whole of the magnetism has disappeared in the iron, while it is scarcely affected in the steel. By such illustrations the remarkable physical differences thus shown to exist between iron and steel were brought very clearly home to the audience; and whether they accepted or not the theoretical explanation, they could not fail to recognise the suggestive and practical character of the facts thus put before them.

The third paper on the list, by Mr. Chas. Cochrane, was a sequel to one read by the same author at the Leeds meeting of the Institution, and dealt with Blast-Furnace working, with special reference to the analysis of the escaping gases. It was laid down at the outset that economy of fuel in blast-furnaces is governed by three conditions: (1) the temperature of the blast; (2) that of the escaping gases; (3) the quantity of carbon which can be maintained in the condition, once attained, of carbonic acid, instead of being re-transformed into carbonic oxide by absorption of carbon in the fuel. On the first two of these heads there is, of course, nothing new to be said; but they were illustrated by elaborate and valuable tables, giving, in units of fuel (C burnt to CO), (*a*) the heat carried *in* by blast of a given weight and temperature, (*b*) the heat carried *out* by escaping gases of given weight and temperature. The third is dwelt on at some length; and tables are given, showing, for any given consumption of C per ton of pig, the ratio of CO_2 to CO in the escaping gases, first when all the CO_2, once formed, is retained in that condition; and afterwards when ½ cwt., 1 cwt., 1½ cwt., &c., are afterwards reconverted into CO, or, as the author terms it, when a *transfer* of ½ cwt., 1 cwt., &c., of C has taken place. From this is deduced the conclusion that the mere knowledge of the ratio of CO_2 to CO in the escaping gases, as given by analysis, is useless to indicate what is really going on in the furnace; because the same ratio may appertain to any different conditions, according to the amount of the transfer which has taken place, from CO_2 back to CO. If, however, the consumption of carbon per ton of pig-iron has been at the same time ascertained, then we are at once able to refer the case to its proper position; and the knowledge of the ratio between the two gases enables us at once to see what amount of transfer has been going on, and what prospect there is of effecting an improvement. It was further pointed out that the main causes of this injurious re-conversion of CO_2 into CO were (1) the fact that the limestone, used as flux, contains a proportion of CO_2, which can only be evolved at a red heat, and therefore in contact with red-hot coke, which immediately gives up some of its C to the evolved gases; (2) the fact that the ore, especially in the larger pieces, does not get completely de-oxidised until it reaches the red-hot region, where the CO ascending in the furnace first unites with the oxygen in the ore to form CO_2, and then absorbs another equivalent of C from the coke, so returning again to the condition of CO. It is therefore suggested that both these sources of evil might be removed if (1) the limestone were *calcined* before entering the furnace, so as to have already parted with its oxygen, (2) the ironstone were broken up into pieces small enough to insure their decomposition in the higher parts of the furnace. Another means of accomplishing the latter result was to increase still further, if necessary, the height of furnaces. A sanguine estimate was made of the economy that would attend the application of these two devices, which it was expected might reach over 3 cwt. of coke per ton of pig-iron made.

The value to ironmasters of the elaborate tables annexed to the paper, and of the mode in which the problem of blast-furnace economy is presented, cannot but be very great; but grave doubts were expressed in the discussion, by Mr. I. Lowthian Bell, F.R.S., whether the practical results would answer the author's expectations. As regards the use of calcined limestone, in particular, it was stated that it had already been tried, without effecting any economy, at least in large furnaces; the suggested reason being that the calcined lime, as soon as charged, re-absorbed CO_2 from the escaping gases, and that although heat was no doubt disengaged in the process, yet this was too near the throat of the furnace to have any serious effect. Moreover it is to be remembered that the previous calcining of the limestone must itself require fuel, the amount of which must be deducted from any apparent gain due to the absence of CO_2 in the lime within the furnace.

The fourth and last paper which was read (one on Screw Shafts, by Prof. Greenhill, of Woolwich, being postponed for want of time) was by a Swiss engineer, Herr Wendelstein, of Lucerne, and gave a good and clear account of the mechanical arrangements connected with the construction of the great tunnel under the St. Gothard, the longest in the world. These are beyond our scope; and the important questions of temperature and ventilation, though just touched upon, were reserved for a future communication, which will also deal with the railway approaches. It may be mentioned that the observations of Dr. Stapff, the official geologist at the St. Gothard, were stated to give as the rate of increase of heat in that locality, 2° C. per 100 metres depth (1·1 Fahr. per 100 feet); and that, if this figure be applied to the Simplon tunnel, as at present proposed to be made just above Brieg, the heat to be dealt with would reach the very high figure of 47° C., or 116° Fahr.

THE QUARTERLY JOURNAL OF MICROSCOPICAL SCIENCE

THE January number of this well-known scientific periodical appears in so new a form as to call for special notice. Under the editorship of Prof. R. Lankester it has long since attained a very high standpoint among the high-class journals of Europe, but it wanted a little in its general get-up to bring it to the very highest level of these, in such details as size, paper, and illustrations. No doubt such details are not to be taken for more than they are worth, and of late years it will be conceded by all those whose opinion is worth having that the value of the contents of the quarterly numbers of the journal left it in a great measure independent of mere typographical superfluities. Still it is very pleasant to find this eighty-ninth number of the New Series so splendidly got up—its paper and type are such as we might expect to find associated with some special monograph; while the increased size (royal octavo) enables the illustrations to be given on a scale quite up to anything we have been accustomed to in the very first of the German and French journals. Let us hope that the enterprise of both Editor and Publisher will meet with sufficient reward to enable them to continue to show what can be done in the way of a scientific journal in these countries.

That the contents are worthy of such a shrine is beyond dispute. Never has Prof. Lankester issued a more important number of his journal, as a mere enumeration of the contents as follows will show. Dr. E. Klein, On the relation of Pathogenic to Septic Bacteria, as illustrated by Anthrax cultivations. This paper relates to a most serious question: it is a model of fair and judicious criticism of the labours of others, and of skill in experimental details. Our space forbids an allusion to its conclusions; but every medical man of any culture should read and re-read this memoir. Somewhere Claude Bernard has said, "Nowadays every medical man *thinks* himself a physiologist." Such would profit by a perusal of this paper if they are able to understand its full significance.—E. B. Poulton, M.A., On the tongue of *Perameles nasuta*, with some suggestions as to the origin of taste-bulbs (Plate 1).—Dr. L. Elsberg, Plant-cells and living matter.—F. O. Bower, M.A., Plasmolysis and its bearing upon the relations of cell-wall and protoplasm (Plate 8).—Prof. A. P. Thomas, The life-history of the Liver Fluke (*Fasciola hepatica*), (Plates 2 and 3); a most elaborate, complete, and beautifully illustrated monograph.—W. F. R. Weldon, B.A., Note on the early development of *Lacerta muralis* (Plates 4–6).—R. V. Willemoes-Suhm (the late), On a crustaceous larva, at one time supposed to be the larva of Limulus (Plate 7).—A. G. Bourne, B.Sc., On Haplobranchus, a new genus of Capitobranchiate annelids (Plate 9).—E. Ray Lankester, M.A., and A. G. Bourne, B.Sc., The minute structure of the lateral and the central eyes of Scorpio and of Limulus (Plates 10–12). The authors find, in the essential agreement of the central eyes of Limulus with those of Scorpions, another important detail, which confirms the opinion of Prof. Lankester, that the Scorpions and King Crabs are closely-allied representatives of one class, the Arachnida.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—Dr. E. B. Tylor's first lecture on Anthropology will take place on Thursday, February 15, at 2.30 p.m., at the large lecture room at the University Museum, not on Wednesday, the 14th, as previously announced. The second lecture will be given at the same hour and place on Wednesday, the 21st.

The voting for "elected members" of the new Boards of Faculties took place last Saturday. The electors were the Members of Convocation, authorised by the Colleges to teach in the subjects of the various faculties. Mathematics and Natural Science are included in one faculty—that of Natural Science. As there are more college lecturers in Mathematics than in Natural Science, it was resolved at a preliminary meeting of the electors to choose five mathematicians and five teachers of Natural Science to occupy the ten places which were to be filled up. Ten names were then agreed upon, but at the formal meeting another mathematician and another scientist were proposed so that it became necessary to vote. The ten names before agreed upon were those chosen, the mathematicians naturally heading the list. They are Messrs. W. Esson, F.R.S., Merton, C. J. Faulkner, University, C. Leudesdorf, Pembroke, E. B. Elliott, Queen's, and J. W. Russell, Balliol, as representatives of mathematics; Messrs. R. E. Baynes, Christ Church, as a representative of Physics; Messrs. J. Watts, Merton, and A. G. Vernon Harcourt, F.R.S., Christ Church, as representatives of Chemistry; and Messrs. E. B. Poulton, Keble, and W. H. Jackson, New, as representatives of Biology.

The Professors of Mathematics and Natural Science are *ex officio* members of the Board.

The Examiners for the Radcliffe Travelling Fellowship give notice that the examination will commence at the Museum on February 13.

The Examiners for the Burdett-Coutts Geological Scholarship give notice that the examination will commence on February 12.

The serious illness of Prof. Henry J. S. Smith is causing much anxiety in the University.

CAMBRIDGE.—The syndicate appointed to frame regulations on the subject of the degree of Doctor in Science or Letters report that they think it important that precautions should be taken to secure that whenever a degree in Science or Letters is granted, the provisions of the statute requiring that the candidate shall have given proof of distinction by some original contribution to the advancement of science or learning have been *bonâ fide* complied with; but they think it undesirable to require from candidates any additional examination or special act or exercise. Considering that it is desirable to encourage the more distinguished graduates to turn their thoughts towards original work at a comparatively early age, and that it is not uncommon for able men to be elected Fellows of the Royal Society at the age of thirty or thereabouts, the Syndicate are of opinion that five years will be a sufficient interval between the degrees of M.A. and D.S. or D.L. The Syndicate think that it is to be wished that some of the older graduates in Arts should proceed to their new degrees. They think that the probability of this would be increased if the seniority of all those who so proceed within a limited time were reserved to them. The Syndicate have carefully considered the difficulty which may arise from the ambiguity of the term "Science." They are of opinion that no regulations can be laid down drawing a clear line between claims for a degree in Science and claims for a degree in Letters.

The Syndicate have drawn up a code of regulations to the effect of the above. The fee to be payable for the degree of either Doctor of Science or Letters to be 20*l*.

Candidates' applications are to be sent to the Chairman of the Special Board of Studies with which their original contribution is most closely connected, specifying the printed contribution or contributions for which the degree is sought. The application is to be considered by a committee, and the contributions reported on by at least two persons, who may be members of the committee or not. If the Special Board report in favour of the candidate, the General Board of Studies is to have a voice in the matter, and if they approve, the Vice-Chancellor is to publish the name as approved.

The following are nominated Electors to the Professorships named:—

Chemistry.—Professors A. W. Williamson, Lord Rayleigh, Dewar, Frankland; Doctors Phear (Emmanuel College), and Ferrers (Caius), Prof. Fuller (of Aberdeen), and Mr. Coutts Trotter.

Jacksonian of Natural Philosophy.—Professors A. W. Williamson, G. G. Stokes, G. D. Liveing, Dr. Hugo Müller, F.R.S., Dr. M. Foster, Mr. P. T. Main, Prof. Fuller (Aberdeen), and Mr. Coutts Trotter.

Cavendish of Experimental Physics.—Sir W. R. Grove, Prof. G. G. Stokes, G. H. Darwin, Sir W. Thomson, R. B. Clifton, G. D. Liveing, James Stuart, and Mr. W. D. Niven.

The Smith's Mathematical Prizes are awarded to Messrs. Welsh, Jesus College (1), and Turner, Trinity College (2).

The Balfour Fund now amounts to about 4130*l*., in addition to the 4000*l*. contributed by his relatives and by Dr. Foster.

DURING last year there entered at the University of Upsala 330 students. In 1881, the number was 312; in 1880, 263; in 1879, 259; in 1878, 243.

SCIENTIFIC SERIALS

Journal de Physique, January, 1883.—On the metallic gratings of Mr. Rowland, by M. Mascart.—*Résumé* of experiments made at the Exhibition of Electricity, on magneto- and dynamo-

electric machines, and on electric lights, by M. Potier.—On electric shadows and on various connected phenomena, by M. Righi.—On the surface of the wave, by M. Doyen.—Demonstration of the principle of Archimedes for bodies immersed in various gases, by M. Terquem.

Atti della R. Accademia dei Lincei. Transunti. Vol. xvii., fasc. 1.—On attenuation of the carbuncular virus, by S. Perroncito.—On the tenacity of the carbuncular virus in its forms of spores, or of *Bacillus anthracis*, by the same.—On the presence of yttrium in the sphene of syenite of Biellese, by S. Cossa.—New Sicilian fungi, by Srs. Passerini and Beltiami.—On some unpublished propositions of Fermat, by M. Henry.—On the action of chloride of cyanogen on the potassic compound of pyrrol, by Srs. Ciamician, and Dennstedt.

Vol. xvii. Fasc. 2.—On a class of triple systems of orthogonal surface, by S. Bianchi.—Observations of the Venus transit at the Observatory of Campidoglio, by S. Respighi.—Reports on prize competitions.

Reale Istituto Lombardo di Scienze e Lettere. Rendiconti. Vol. xv. Fasc. xvii.—On compensatory hypertrophy of the kidneys, by S. Golgi.—On drunkenness in Milan (continued), by S. Verga.

Fasc. xix.—Prof. Giacci's "Fundamental theorem in the theory of the canonical equations of motion," by S. Morera.—On drunkenness in Milan (continued), by S. Verga.—On olivil and some of its transformations, by Srs. Körner and Carnelutti.—Congenital pachydactylia from a psychical impression in the mother, by S. Scarenzio.—Observation of the transit of Venus at the Royal Observatory of Brera, on December 6, 1881, by S. Schiaparelli.—Bacteria of anthrax in the fœtus of a heifer that died of the disease, by S. Sangalli. Discussion with S. Golgi.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, February 1.—"On the Affinities of Thylacoleo," by Prof. Owen, C.B., F.R.S., &c.—Since the appearance of Part IV. of the "Fossil Mammals of Australia" in the *Philosophical Transactions* for 1871, the author has omitted no opportunity of promoting the acquisition of additional evidences. The application of a grant by the Legislature of New South Wales, in aid of further exploration of the Limestone Caverns in Wellington Valley, having been confided to Ed. P. Ramsay, F.L.S., the results have furnished the author with additional evidences, including those which form the subject of the present communication. After a brief exposition of the state of the question at the date of the previous paper, a description is given of the complete dentition of the upper and lower jaws of a mature marsupial lion. This is followed by descriptions of the antibrachial bones and ungual phalanges of the same extinct animal, the characters of those parts of the skeleton being compared with the same parts in feline mammals and in the existing kinds of diprotodont marsupials. The paper concludes with a description of an entire mandible; and the conclusions to be drawn from the shape and position of the articular condyles, which harmonise with those deducible from fragmentary fossils previously described, go neatly to complete the reconstruction of what the author deems to be the most extraordinary of the extinct pouched quadrupeds of Australia.

The paper was accompanied by drawings of the natural size of the fossils described.

In the subsequent discussion the author remarked on the correspondence of spælean phenomena, the proportion of the remains of the old British lion in bone caves of this country being paralleled by that of the Australian carnivore in the antipodean caves. They were the retreat of the destroyer in both localities; and the fragmentary, gnawed condition of the remains of the prey, with usual immaturity of the captured kangaroos of great size, the *Diprotodon australis*, *e.g.*, afforded an instructive analogy.

"Preliminary Note on a Theory of Magnetism based upon New Experimental Researches." By Prof. D. E. Hughes, F.R.S.

In the year 1879 (*Proc. Roy. Soc.*, vol. xxix. p. 56, 1879) I communicated to the Royal Society a paper "On an Induction Currents Balance and Experimental Researches made therewith." I continued my researches into the molecular construction of metallic bodies, and communicated the results then obtained in three separate papers (*Proc. Roy. Soc.*, vol. xxxi. p. 525; vol. xxxii. pp. 25, 213, 1881) bearing upon molecular magnetism.

To investigate the molecular construction of magnets, required again special forms of apparatus, and I have since been engaged upon these, and the researches which they have enabled me to follow.

From numerous researches I have gradually formed a theory of magnetism entirely based upon experimental results, and these have led me to the following conclusions:—

1. That each molecule of a piece of iron, steel, or other magnetic metal is a separate and independent magnet, having its two poles and distribution of magnetic polarity exactly the same as its total evident magnetism when noticed upon a steel bar-magnet.

2. That each molecule, or its polarity, can be rotated in either direction upon its axis by torsion, stress, or by physical forces, such as magnetism and electricity.

3. That the inherent polarity or magnetism of each molecule is a constant quantity like gravity; that it can neither be augmented nor destroyed.

4. That when we have external neutrality, or no apparent magnetism, the molecules, or their polarities, arrange themselves so as to satisfy their mutual attraction by the shortest path, and thus form a complete closed circuit of attraction.

5. That when magnetism becomes evident, the molecules or their polarities have all rotated symmetrically in a given direction, producing a north pole if rotated in this direction as regards the piece of steel, or a south pole if rotated in the opposite direction. Also, that in evident magnetism, we have still a symmetrical arrangement, but one whose circles of attraction are not completed except through an external armature joining both poles.

The experimental evidences of the above theory are extremely numerous, and appear so conclusive, that I have ventured upon formulating the results in the above theory.

I hope in a few weeks to bring before the Royal Society the experimental evidence which has led me to the conclusions I have named; conclusions which have not been arrived at hastily, but from a long series of research upon the molecular construction of magnetism now extending over several years.

Linnean Society, January 18.—Sir John Lubbock, Bart., F.R.S., president, in the chair.—E. A. L. Batters, A. J Burrows, E. F. Cooper, Prof. J. A. Harker, and G. Lewis, were elected Fellows of the Society.—Mr. H. Grooves called attention to a specimen of *Ranunculus ophioglossifolius* obtained in Hampshire, and therefore new to Britain.—There was exhibited, on behalf of Mr. Jas. Romanis, a live specimen of *Pieris Rapæ*, which had been found fluttering on the window of his house a few days previously.—A paper was read on the fall of branchlets in the aspen (*Populus tremula*) by Samuel G. Shattock. He shows that in this tree and some few others—in contradiction to the majority of exogenous trees—a process takes place termed "cladoptopsis" by the Rev. M. J. Berkeley many years ago. In the small branchlets only disarticulation is effected by a swollen ring of corky tissue at the base, somewhat as in the ordinary fall of leaves.—Mr. A. G. Bourne gave a contribution on the anatomy of Polynoina, pointing out that the *Polynoë grubiana*, very common in the Mediterranean, is only a variety of the *P. clava*, Montague, of our own coasts. The latter itself has certain constant characteristics, and others much more variable.—Prof. P. Martin Duncan read his observations on the Madreporaria, fam. Fungidæ, with special reference to the hard structures. Edwards and Haime described the synapticula as constituting an essential family structure, and also the absence of endothecal dissepiments. Dr. Duncan describes that the ridges of the continuous synapticula with canals between them is limited by solid and also perforate septa, and he delineates the structures. The synapticula are shown to have no relation to the ornamentation on the ridges of the septa. The basal wall is shown to be of synapticular origin, and the foramina in it to relate to the growth of these binding structures.

Physical Society, January 27.—Prof. Clifton, president, in the chair.—New Member, Mr. Hugh E. Harrison.—Prof. G. Carey Foster read a paper on the determination of the ohm, in which he described the various methods which have been used and proposed in determining the B.A. unit of resistance. He also described a method of his own, proposed in 1874, and recently tried with good results. The method consists in balancing the E.M.F. set up in a coil of wire by spinning it in the earth's magnetic field, against the E.M.F. of a battery or

other electromotor, in a wire whose resistance is to be determined. The two opposing circuits through this wire, R, are composed, the first of the spinning coil and a zero-galvanoscope, and the second of a battery and an absolute galvanometer; these two circuits meeting at the ends of the wire R. The late Mr. Hockin and Prof. Foster find that the best conditions obtain when the resistance of the absolute galvanometer r is equal to R; the resistance of the zero galvanoscope r_2 equal to $\frac{R}{2} + r_3$, and the resistance of the spinning cord, r_3, many times the battery-resistance, which should be so low as to be practically negligible. The E.M.F. of the battery should be double that of the spinning coil. Many other conditions had to be attended to, as explained by Prof. Foster. With this method, and using a thermo-electric battery giving an E.M.F. of 2·2 volts, the coil was spun at about 1800 revolutions per minute; r was 63 ohms, r_2 was 135, r_3 was 50, and R was 73 in one, and of 80 in another experiment. R was made up by coils on a resistance box. The ohm was determined by two trials to be 1·003 and ·999. This general result is so satisfactory that the experiments will be continued with extra precautions. Mr. Glazebrook called attention to the remarkable agreement between the results of Lord Rayleigh's determinations and his own independent ones. Lord Rayleigh's figures are for the unit, ·9893, ·9865, ·9868, and Mr. Glazebrook's is ·9866, or the mean of Lord Rayleigh's results. He also announced that the Clarendon Laboratory, Cambridge, would soon be in a position to test and certify any resistance coils sent there.—Mr. Walter Baily then read a paper on the spectra formed by curved diffraction gratings. In a diffraction grating ruled on a portion of a cylinder, if r is the distance of a point from the centre of the grating, and θ the angle which a line to the point makes with the perpendicular from the centre of the grating, c the radius of curvature of the grating, and d an arbitrary constant, a series of curves may be drawn in the plane perpendicular to the lines of the grating having as the general equation

$$r^{-1}\cos^2\theta = c^{-1}\cos\theta + d^{-1}.$$

If a source of light is placed on a point on one of these, curves the foci of the diffracted light lie on the same curve. The curve consists of two loops, one of which gives the spectra of transmitted and the other those of refracted light. When d is infinite, these curves coincide in a circle, the properties of which have been so used by Prof. Rowland in the construction of his diffraction spectroscope. The paper also describes how the position of the spectra on the curves can be determined for any position of the source of light.

Geological Society, January 24.—J. Gwyn Jeffreys, vice-president, in the chair.—Walter Raleigh Browne, Thomas Charles Maggs, Lieut.-Col. William Alexander Ross, and Cecil Carus Wilson, were elected Fellows of the Society.—The following communications were read:—On *Streptelasma Rœmeri*, sp. nov., from the Wenlock shale, by Prof. P. Martin Duncan, F.R.S., V.P.G.S.—On *Cyathophyllum Fletcheri*, Edw. and H., sp., by Prof. P. Martin Duncan, F.R.S., V.P.G.S.—On the fossil Madreporia of the Great Oolite of the counties of Gloucester and Oxford, by Robert F. Tomes, F.G.S.

Institution of Civil Engineers, January 30.—Mr. Brunlees, president, in the chair.—The paper read was on "Mild Steel for the Fire-boxes of Locomotive Engines in the United States of America," by Mr. John Fernie, M.Inst.C.E.

SYDNEY

Linnean Society of New South Wales, November 29, 1882.—Dr. James C. Cox, president in the chair.—The following papers were read:—"Description of two new birds of Queensland," by Charles W. De Vis, B.A.—One of these birds —*Prionodura Newtoniana* constitutes a new genus and species of the Family Paradiseidæ. It is described from a unique specimen taken in Tully River scrubs, Rockingham Bay. The other bird described—*Cracticus rufescens* came from the same locality.—"Fungi aliquot Australiæ Orientalis," by the Rev. Carl Kalchbrenner.—The following new species were described *Agaricus megalotheles*, *Agaricus Kirtoni*, *A. peltastes*, and *Scleroderma pileolatum*.—The Rev. J. E. Tenison-Woods, vice-president, read the fifth part of his "Botanical Notes on Queensland."—This paper consisted of a description of the "Brigalow" scrubs, which consist mainly of *Acacia harpophylla* (F.v.M.) instead of *A. excelsa* as usually stated. The brigalow forms thickets of from thirty to eighty feet in height, amongst which a peculiar flora occurs. A list of those collected by the author was given at the end of the paper.—"Contribution to a knowledge of the Fishes of New Guinea" No. 3, by William Macleay, F.L.S., &c.—In this paper Mr. Macleay completes the list of the Fishes sent by Mr. Goldie from Port Moresby, bringing the number up to of species 274. The new species described in the present paper are:—*Platyglossus guttulatus*, *Coris cyanea*, *Pseudoscarus Goldiei*, *Pseudoscarus frontalis*, *Pseudoscarus papuensis*, *Pseudoscarus zonatus*, *Pseudoscarus labiosus*, *Pseudoscarus Moresbyensis*, *Monacanthus nigricans*, *Monacanthus fuliginosus*, *Trygon granulata*, and *Tæniura atra*.—"Notes on the Geology of the Western Coal Fields, No. 2, by Prof. Stephens, M.A.—In this paper Prof. Stephens proceeds to an examination of the Wallerawang, Marangeroo and Capertee conglomerates which leads him directly to the conclusion that the continent off whose shores the upper marine carboniferous beds were deposited, was a system of high mountain ranges, snow-capped, and under erosion by glaciers which descended to near the level of the sea. It was shown further that all the subsequent formations were of shore or river formation, in plains skirting the mountains, or in valleys penetrating their recesses, and that these were all fresh water deposits, excepting the coal seams themselves, which were subaerial; and that the most recent sedimentary formations in that district was the Hawkesbury Sandstone, also lacustrine in origin, and due like the underlying strata to a continued rise of the lake waters upon the land.—"Note on an Australian species of Phoronis," by William A. Haswell, M.A., B.Sc.—"Note on a curious instance of Symbiosis," by William A. Haswell, M.A., B.Sc.—"Note on the segmental organs of *Aphrodita*," by William A. Haswell, M.A., B.Sc.

BERLIN

Physiological Society, December 29, 1882.—Prof. du Bois Reymond in the chair.—Dr. Pohl-Pincus spoke about the effect of weak local stimulations of the heart, and about the effect of vagus-stimulation upon the heart.—Prof. Quincke of Kiel, who was present as a visitor, spoke upon the physiological part of the results of experiments and observations which he had made upon the life-history of the red blood corpuscles. It is a well-known fact that large cells with numerous pigment-granules occur in the spleen and in the marrow of bones. These cells, as the micro-chemical reaction teaches us, contain a great deal of iron in combination with albumen. The iron-reaction of the spleen and bone-marrow is more pronounced than the number of pigment-cells can explain; and hence Prof. Quincke hypothesizes the presence in both structures of a colourless iron-albumen, which is, on the one hand, the product of the destruction of red corpuscles, and on the other hand forms the material out of which the new red corpuscles are developed. Both these circumstances were verified experimentally; when by frequent transfusions of equal quantities of blood into an animal, the number of the red blood corpuscles was considerably augmented, and, by this means, the destruction of red corpuscles likewise increased, the number of the pigment-cells and the amount of iron-albumen in the spleen and marrow of the bones was also very much increased, and there was present in the capillaries of the liver a consideable quantity of white blood-corpuscles with iron-albumen, which, under normal circumstances are only found in this organ in very small numbers. When, on the other hand, the number of an animal's red blood-corpuscles had been diminished by repeated bleedings, both the number of pigment cells and the amount of iron-albumen in the spleen and marrow was found after a few days to be considerably diminished and reduced to a minimum. While a part of the iron from the disintegrating red blood corpuscles describes a circle forming a reserve for the newly-forming blood-corpuscles, another portion is eliminated from the blood through the urine and bile. In the experiments, in which the destruction of red corpuscles was increased as a consequence of transfusions, it was possible to demonstrate the intermediate stages in the process of the abnormally-large elimination of the iron, as both the liver-cells and the kidney-epithelium gave a quite distinct iron-reaction.—Dr. Schiffer then read two preliminary communications. One of these was upon the poisonous properties of the mammalian urine. When the urine of either carnivorous or herbivorous mammals was injected under the skin of a frog, symptoms of poisoning manifested themselves, to which the frog soon succumbed. Rabbits also exhibited symptoms of poisoning after subcutaneous injection of

evaporated urine, which had been deprived of the poisonous potash-salts. Dr. Schiffer is still engaged in the investigation of the isolation of the poison.—The second communication was upon his experiments with curare. The striking inoperativeness of this violent poison, when introduced into the stomach cannot be due, as has up to the present been almost universally accepted, to the absorbed poison being quickly eliminated by the kidney, because Dr. Schiffer's experiments showed that the elimination of this substance through the urine is complete, although very slow, so that the animal, if it would absorb the poison, would have had to succumb long before. When Dr. Schiffer introduced a very large dose—about 2 grms.—of curare, into a stomach which he had ligatured at the pyloric orifice, the animal died in about twenty-two hours, which was far too late for curare poisoning, and far too soon as a consequence of the ligature of the pylorus. When introduced into the small intestine, the continuity of which was interrupted above and below by a ligature, the curare was very quickly absorbed; when the small intestine was only occluded above, only a very little curare was absorbed, this absorption taking place slowly. The large intestine behaved like the small one. From the rectum out, curare was very quickly absorbed. Outside the body, curare diffused very well through a stomach wall. To sum up, the inoperativeness of curare when introduced into the stomach is as yet unexplained.

PARIS

Academy of Sciences, January 29.—M. Blanchard in the chair.—The death of M. Sedillot, Member in the Section of Medicine and Surgery, was announced.—Note on the observation of the transit of Venus, by M. Janssen. The conditions were very favourable at the fort du Chateau-Neuf, Oran. Special attention was given to the question as to presence of aqueous vapour in the atmosphere of Venus. This was not demonstrated. Afterwards M. Janssen spent a month at Mecheria, a military station on the high desert plateaux, with the same purpose. The air was so dry and clear that, *e.g.*, Jupiter's satellites could be seen with the naked eye. With very perfect spectroscopic apparatus applied under extremely good conditions, he is yet constrained to great reserve as to the presence of aqueous vapour in Venus's atmosphere. He studied mirage, photographed it several times, and finds its causes, in most cases, to be quite different from those commonly supposed.—On the mechanical and physical composition of the sun (continued), by M. Faye. This relates to spots.—Contributions to the history of the reactions between sulphur, carbon, their oxides, and their salts, by M. Berthelot. The results have a bearing on the reactions produced during explosion of powder.—On the morbid phenomena produced in rabbits by introduction of hydrate of chloral into the ear, by M. Vulpian. The most salient phenomenon is impetuous rotation of the animal on its longitudinal axis; which the author attributes to the inflammation produced in the cavities of the internal ear; to this inflammation, along with more or less broncho-pneumonia, the animals often succumbed. The brain and meninges were not affected, as in the experiment of M. Brown-Séquard, who poured chloroform into the ear. Though the disorders of motility are weakened in time, they are found still to persist in some degree, a month after operation.—Observations on the occasion of a Report of M. Leon Colin, on the mortality produced by typhoid fever in the French army, by M. Vulpian. This Report, by a committee of the Academy of Medicine, throws doubt on the value of M. Glenard's recent statistics (to show the efficacy of cold baths).—Note on the state of natural sciences and on anthropology in Brazil, by M. de Quatrefages. *Inter alia*, Brazil now devotes, on an average, 16 per cent. of her whole revenue to public education; in one of the twenty-one provinces (Goyaz), the proportion reaches 30 per cent. The National Museum in Rio, dating from 1817, has been wholly reorganised by Dom Pedro, and it is of great value. The Emperor is often present at the lectures there. The Museum has its *Archives*, and M. de Quatrefages indicates the contents of the first four volumes sent him; they reveal great scientific activity. The successful Brazilian Anthropological Exhibition held last year is to be followed by one for the entire Continent.—Note on the determination of the phosphoric acid in arable land, by M. de Gasparin. He describes an easy and rapid method. Arrangements were made in connection with a new annual prize of 1000 francs, provided by a widow, Mme. Francœur; it is to be given "to the author of discoveries or works useful to the progress of the mathematical sciences, pure or applied."—On wounds from fire-arms, called seton-wounds, by M. Guérin. These wounds always contain foreign bodies, from crushing of the tissues, and perhaps particles of cloth, &c.; and the conditions are adverse to immediate cicatrisation. The author adopts (with success)—(1) antiseptic washings with continuous currents, (2) pneumatic occlusion; simultaneously, alternately, or successively, as the case may be.—On a class of functions of two independent variables, by M. Picard.—On the algebraic integration of a class of linear equations, by M. Goursat.—On a theorem of M. Tchébychef, by M. Korkine.—Application of a method given by Legendre, by M. Lipschitz.—Observation of a magnetic storm at Cape Horn, by M. Mascart. This was on November 17 and 18 last. The principal perturbation was simultaneous with that at the Parc St. Maur.—Reply to a note by M. Marcel Deprez, by M. Lévy.—M. Deprez presented a translation of the official Report at the Munich Exhibition, on transport of force by dynamo-electric machines.—Reply to M. Lévy, by MM. Mercadier and Vaschy.—New experiment in electrolysis, by M. Semmola. In proof of the law that the quantity of liquid decomposed in a given time is proportional to the quantity of electricity which passes in that time, he uses a voltameter with three platina electrodes inserted equally apart at the bottom. The current coming by one is caused, by a commutator, to pass either by one of the others or by both.—Researches on the passages of alcoholic liquors through porous bodies (second note), by M. Gal. He investigates the influence of temperature, and nature of membrane, and the case in which the membrane is exclusively in contact with the liquid or with its vapour. An alcoholic liquid in contact with a membrane tends to diminish in degree, instead of concentrating, as Soemmering affirmed, and as is everywhere taught; and it is the same with its vapour.—On the vapour of carbamide, by M. Isambert.—On sulphite of manganese, by M. Gorgeu.—On new ammoniocobaltic combinations, by M. Maquenne.—On the crystalline form, specific heat, and atomicity of thorium, by M. Nilson. The crystals form a regular combination between the octahedron and the tetrahedron: the specific heat is 0·02757; the substance is quadrivalent.—On the mutual displacements of bases, &c. (continued), by M. Menschutkine.—Importance of zoological characters furnished by the upper lip in the *Syrphides* (Diptera), by M. Gazagnaire.—On the effects of respiration of air charged with petroleum vapour, by M. Poincaré. Dogs, rabbits, and guinea-pigs were experimented with. Respiration was increased in frequency and amplitude, heart-beats were retarded, (the shock was intensified); there was itchiness, sleepiness, and inappetence. Guinea-pigs alone succumbed, after one to two years.

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THURSDAY, FEBRUARY 15, 1883

THE TERTIARY HISTORY OF THE GRAND CAÑON DISTRICT

A Monograph by Capt. C. E. Dutton. Being Vol. II. of the Monographs of the United States Geological Survey. With Atlas. 4to. [Vol. I. is not yet published.] (Washington, 1882.)

IN a handsome quarto volume, with a large atlas of maps and coloured views, the recently-constituted United States Geological Survey begins its series of memoirs descriptive of the geological structure and history of the country. Most appropriately the subject selected for illustration is at once the grandest and most unique feature in the geology of the United States, and to which indeed there is no parallel elsewhere in the world. Ever since the early Report by Ives and Newberry, in which the marvels of the Rio Colorado of the West were first made known, there has been a strong desire among geologists to learn more of that region, to have accurate measurements and careful drawings, and to be told authoritatively the details and the history of what they could not but admit to be the most stupendous example of river-erosion on the face of the globe.

Major Powell's bold descent of the river and the charming volume in which he described it threw much fresh light on the wonders of the cañons. But he had no opportunity of properly exploring the surrounding regions, though we looked forward to his return to the scene of his exploits and the consequent elaboration of another memoir discussing the whole problem of the origin and history of the geological features of that remarkable area. Pressure of other duties has prevented him from realising this hope. But, though unable himself to resume this task, he deserves our best thanks for having induced the late Director of the Survey, Mr. Clarence King, to intrust the detailed survey of the Grand Cañon to Capt. C. E. Dutton, who had already done excellent service among the high volcanic plateaux farther north. Capt. Dutton unites some of the highest qualities of a geological explorer. He is an excellent stratigrapher, a good petrographer, an enthusiast in the study of rock-sculpture, writes clearly and pleasantly, has a physical frame capable of carrying him triumphantly through any amount of physical fatigue, and is the happy possessor of a bright, cheerful nature, that must lighten the hardships of camp-life in the remote West both for himself and for his companions. We can well imagine how such a man, wandering among the lofty plateaux of Utah that had been assigned to him for exploration, should have cast many a longing gaze southward to that strange wild desert region of rocky platforms and winding *mesas*, through which the gorges of the Colorado and its tributaries have been sunk; how he should have been unable to resist the temptation to stray into that wonderland; and how he must in some measure have almost welcomed the blasts of early winter that drove him down from the survey of the plateaux, and allowed him to journey through the cañon country on his way back to the Mormon settlements and the nearest railroad.

When at last the task of actually exploring and describing that region was intrusted to him, he already possessed a general acquaintance with its character and with many of its details. A stranger who first finds the cañon scenery before him is so excited by its novelty and grandeur, that for a time he feels utterly bewildered. Only after his eye has in some measure recovered its power of grasping the broad effects, without being lost in the details, does he begin to realise what are the elements of this stupendous grandeur. But Capt. Dutton had gone through this preliminary training. He had been led to scrutinise the scenery in detail, to discover the relations of part to part, and to speculate upon the evolution of the whole. Yet no one can read his pages without feeling that this analytic process has in no way dulled his sense of the beauty and majesty of the scenery. His words glow with the light that floods those flaming precipices. The blue aërial perspective of chasm and cliff receding into the dim distance in the central gorge seems to rise before our eyes as we read. With no irreverent hand does he tear the mask off the face of Nature. Rather does he make us feel how deeply the mystery of the scene has entered into his soul, as he gently lifts the veil that we may see a little way within, even as far as he has himself been enabled to penetrate.

And this is the true spirit in which such scenery should be described and discussed. The man who could sit down and dissect these cañons in cold blood, and with as little emotion as he would show in cutting up a joint of beef, would be a creature not to be envied. Nowhere in this world does the scenery appeal so powerfully to the imagination. Among the Alps the rocks have been so stupendously crumpled that we may be pardonably at a loss to tell how far the outlines of a mountain are due to subterranean movements or to subsequent erosion. But among the western cañons there is no room for any such doubt. The rocks lie for thousands of square miles as flat as when they were laid down upon the floors of ancient seas and lakes, and their horizontal undisturbed beds may be followed by the eye, winding in and out from cliff to cliff, preserving the same breadth, colour, features, and serving as so many datum-lines from which to measure the amount of solid rock that has been removed from the gorges. In tracing back the origin of these landscapes, and seeking out the causes of their infinite variety of detail yet marvellous harmony of effect, the mind naturally compares them with the feeble illustrations of erosion with which alone we are usually personally familiar. Such a comparison, however, will almost suggest a doubt as to whether we ever before could have had any proper conception of what the power of running water actually is, so utterly beyond description is the impressiveness with which this power is now realised. Nor is one disposed to deny that nowhere else is the dominant influence of geological structure upon the ultimate contours developed by erosion so significantly displayed. On every part of the scenery the story of its origin is impressed in characters that cannot be mistaken. Yet these characters are on so colossal a scale that the dry prosaic language of ordinary geological description seems utterly incongruous when applied to them. It must be a difficult task to preserve the sober decorum of scientific treatment, and to convey at the same time an adequate impression of the infinite majesty of the subject.

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Capt. Dutton may be congratulated on having accomplished this task with as large a measure of success as probably was achievable. Without entering into stratigraphical details he addresses himself to the problem of the origin and history of the erosion that has converted the level rock-platforms of the Colorado River into their present profoundly trenched condition. Sketching briefly but clearly the general geographical features of the region and their relation to the underlying geological structure, he presents the reader with a series of pictures of the various types of scenery. He shows how everywhere the evidence arises of vast denudation. Not only have the wide valleys and deep gorges been excavated, but an enormous amount of material has been worn away from the broad rocky terraces. From the high plateaux of Utah the Mesozoic and Tertiary formations descend by a succession of broad terraces like a giant staircase to the platform of Palæozoic rocks. Capt. Dutton gives reasons for his belief that the strata which end at the cliffs of these successive terraces once extended over the whole of the Grand Cañon district, and he estimates the amount of rock thus removed to have averaged probably 10,000 feet in thickness over an area 13,000 to 15,000 square miles in extent. He bases this estimate partly upon the obvious continuity of the strata, and the improbability that they could have ended off upon the Carboniferous platform; partly upon the evidence of displacements whereby Palæozoic rocks, formerly buried at least 10,000 feet below the sea-level, under an accumulation of sediment of that depth, have been again uplifted into the lofty plateaux of the Colorado; partly upon an argument from the history of the drainage-lines of the district. In this last argument, developing the views so forcibly expressed by Jukes many years ago for the rivers of the south of Ireland, and more recently applied by Powell to the stupendous illustrations in the Colorado basin, he shows that the present courses of the rivers are so entirely independent of structural features, that their position is inexplicable save on the interpretation that when the streams began to flow these features had not revealed themselves. He thus smoothes over the faulted Carboniferous platform, piles over it a covering somewhere about two miles thick of Mesozoic and Tertiary strata, and makes the rivers begin their first erosion on the surface of this covering. The faulting, plication, and uplifting have taken place subsequently; but meanwhile the rivers have kept their courses, incessantly sawing their way downward into lower layers of rock, and across the dislocations and folds that subterranean disturbance might throw across their path. No thoughtful student of this subject can refuse his assent to the solution of the problem so well worked out.

In tracing the geological history of the cañon region, we find at the bottom of all the visible strata, a foundation of ancient crystalline Archæan rocks, and also crumpled and broken masses of stratified formations, referred with more or less confidence to the Silurian and Devonian periods. The disturbance and extensive denudation of the older Palæozoic masses had been effected before the lowest of the vast conformable series of formations in this region began to be deposited, for the latter lie upon the upturned edges of the former, as on a platform—an impressive feature in the scenery. Continuous sedimentation began some time in the Carboniferous period, and appears to have been carried on with no sensible break up to the close of the Eocene period, until a total depth of at least 15,000 feet of sediment had accumulated. The Carboniferous portion is estimated at a thickness of 4500 feet, the various Mesozoic formations at 9000 or 10,000, and the portion of Eocene lacustrine beds deposited were 1000 or 1200 feet.

Capt. Dutton calls attention to the remarkable uniformity and persistency of the lithological characters of each formation, while at the same time there is great diversity in those respects between the strata of different platforms. By far the larger proportion of the whole mass of conformable strata consists of sandstone, presenting on successive horizons the most extreme contrasts of structure and colour, for they consist along certain platforms of adamantine quartzite, in others of massive cross-bedded sand-rocks, while they graduate also into shales and these into marls. It is this alteration of strata, showing very different degrees of permanence, yet each retaining its normal characters over vast areas, that affords the key to much that is most characteristic in the scenery of the region. The limestones are almost wholly confined to the Carboniferous system, where they occur both in the lower and upper divisions.

Another significant feature brought out by the survey is the evidence that sedimentation went on nearly at sea-level during the whole of Mesozoic time throughout the Cañon province. As the Mesozoic strata are 9000 or 10,000 feet thick, it is obvious that the sediments which were at or near the sea-level at the beginning had sunk to that distance below it by the end of the period. We have here, therefore, a consecutive series of shallow-water deposits not much less than two miles in vertical thickness. The Cretaceous rocks which form the uppermost division of this series are from base to summit banded with seams of lignite or coal, and layers containing marine mollusca. They vary in different parts of the province from 3500 to 8000 feet in thickness. At the close of their deposition, those movements appear to have begun which have culminated in the elevation of the sea-floor into the elevated plateaux that now form so prominent a feature on either side of the watershed of the continent.

With the advent of Eocene time the shallow sea-floor, in which sedimentation had been so continuous during the whole of the Mesozoic ages, began to be converted into wide fresh-water lakes. The Tertiary history of Western America is in large measure a record of the formation, duration, and effacement of these lakes, as the land gradually increased in elevation. In the plateau country the Eocene lacustrine deposits range from 1000 to 5000 feet in thickness. Great as this accumulation is, it unquestionably took place in comparatively shallow water over an area that was generally rising, yet was locally sinking, so that the lake persisted, and remained shallow; for its depth was reduced by the deposit of sediment as fast as it was increased by subsidence. The waters appear to have dried up from south to north, and finally disappeared somewhere in the area of the Uinta mountains.

It was on the floor of this desiccated lake that the drainage system of the Colorado river began, somewhere about the close of the Eocene period. During the vast

succession of ages that have since elapsed, erosion has been continuously in progress, and the result is the scenery of the Cañon region. Capt. Dutton gives what appear to be good reasons for believing that the larger rivers flow along the same channels which they took at the beginning, but that the minor tributaries, where any exist (and they are conspicuously absent in some wide districts), are comparatively recent in origin, and have been determined by modern surface conditions. The excavation of the Grand Cañon of the Colorado has thus been going on ever since the Eocene period. During that enormous interval the climate of the region appears to have passed through successive oscillations. There is no more skilful feature of the volume before us than the way in which the scattered facts that bear on this question are marshalled to their places and made to tell their story. Ancient river-beds, which for ages have been dry and are partly filled up with debris, open on the edge of the great chasm. They doubtless discharged their waters into the main river at a time when rains were abundant and watercourses numerous. But their fountains have long since been dried up, and their fading channels are almost gone. But all the while the Colorado and its larger feeders, drawing their supplies from far well-watered uplands, have continued their task of erosion until they have sunk their channels in some places more than a mile below the level of the plateau across which they flow.

The process of the excavation of the Grand Cañon is treated at length, and much new information is given as to its varying conditions. The details of the erosion are described with great clearness. The two final chapters, wherein these subjects are discussed, contain much that is suggestive, and deserve careful perusal by all who take interest in questions of denudation. They are condensed pieces of reasoning which cannot be intelligibly summarised here, and which indeed one is hardly prepared to find in an official report. Like his colleague, Mr. G. K. Gilbert, Capt. Dutton properly lays great stress upon the influence of an arid climate as one of the chief factors in cañon excavation. He points out how the absence of vegetation exposes the surface of bare rock to the action of rain. But it may be doubted if the scanty rains of the region can do more than remove material already disintegrated. We have to account for the continuous lowering of the level of the plateaux, and the removal of so vast a depth of stratified rock from their surface. Capt. Dutton himself admits that most of the rain which falls upon the country is absorbed by the rocks, and gushes out in copious springs at the base of the cañon-walls, thereby notably increasing the volume of the river. But there is everywhere a perceptible disintegration of the rock at the surface. This decay cannot be attributed to frost, which in so dry a climate can have but small effect. It seems to be due in large measure to the superficial strain induced by a great daily range of temperature. And it is no doubt aided by the action of wind, which removes the loosened particles, and exposes a new surface to the same kind of disintegration.

In conclusion, reference must be made to the truly magnificent series of illustrations by which this monograph is accompanied. The maps of the atlas give the reader a clear mental picture of the general topography and geological structure of the region. But it is by the pictorial illustrations that he will be chiefly fascinated. These are scattered profusely through the text, and form an important feature in the atlas. Mr. Holmes, whose reputation for the accurate and artistic rendering of geological details is so well established, has here far surpassed all his previous efforts, and has produced the most impressive and instructive geological pictures that have ever been made. His large coloured views of the Grand Cañon are in themselves a series of lessons in geology far more interesting and effective than can be supplied in words. The United States may be heartily congratulated on this first of the monographs of their Geological Survey. Let us hope that Congress will continue in the same liberal spirit the annual appropriations that have enabled the Director of the Survey and his associates to produce such splendid results. ARCH. GEIKIE

CENTRAL ASIA

Travels and Adventures East of the Caspian during the Years 1879-81, *including Five Months' Residence among the Tekkés of Merv.* By Edmond O'Donovan. Two Vols. (London: Smith Elder, 1882.)

Wanderings in Baluchistan. By Major-General Sir C. M. MacGregor, K.C.B. (London: Allen and Co., 1882.)

MR. O'DONOVAN'S venturesome excursion to the Merv Oasis stands out conspicuously as perhaps the most romantic episode in the recent annals of Central Asiatic travel. Yet in proceeding eastwards his original goal was not the Mervli Turkomans, but their western kindred, the Akhal Tekkés of the Daman-i-Koh. Sent out as the Special Correspondent of the *Daily News* with the Russian expedition against those nomads in 1879, he was at first well received, and spent some profitable time during the progress of military operations on the Caspian seaboard. But after the death of General Lazareff, having been suddenly banished from Chikislar, his ramblings lay henceforth mainly within the North Persian frontier. Here he again went over the ground, with which we have been made tolerably familiar by V. Baker, Macgregor, Stewart, and other recent explorers. Nevertheless even of this region Mr. O'Donovan has much to tell us, which is both new and interesting. There is a freshness and a fulness of detail in his account of Meshed, Tehrán, Kuchan, Resht, Shahrúd, as well as of the people and scenery of Khorasán and Mazandarán, which lend a peculiar charm to the first of these brilliantly written volumes.

But the chief interest of the work naturally centres in the section devoted to the Merv Oasis and its Tekke Turkoman inhabitants, with whom the traveller passed a forced residence for over five months during the year 1881. How he eluded his Persian escort, crossed the border above Sarakhs, traversed the Tejend river valley, plunged boldly into the heart of the desert, safely reached the Murghab Oasis, allayed the suspicions of the Tekkés, who took him for a Russian spy, gradually gained their confidence, became in fact a "Tekke of the Tekkés" and head of a Turkoman triumvirate, finally, by a rare combination of tact, patience, and courage, again escaping from his too importunate friends, all reads far more like a wild piece of fiction than so much sober history.

Although Colonel Stewart had recently brought home some accurate information regarding the present state of the Oasis, this region had been actually visited by no European traveller since Abbot's expedition in 1840. Hence Mr. O'Donovan is here on comparatively new ground, and his graphic account of the place and its inhabitants will be read with deep interest, especially by those who have not seen the portions already published in the *Daily News*. Since the Russian occupation of the Akhal country, the Oasis has doubtless lost whatever strategical significance it may have hitherto possessed. Nevertheless its position in the desert midway between the Oasis and Caspian, its great fertility and dense population—estimated at about 500,000—its numerous antiquities and grand historic memories, must always ensure for the "Queen of the World" an exceptional importance in the eye of the statesman and historian. The student will here find ample details of its present social and economic condition, of its government and administration, of the organisation of the Toktamish and Otamish Tekke tribes,[1] their local institutions, the water system of the Murghâb, the remains of Bairam Ali, and other cities which successively bore the name of Merv, the home life of the Mervlis, their actual commercial and political relations and future prospects.

A very full description is given of the many ruins scattered over the Oasis, all of which were visited and sketched by the explorer. Of these the most extensive are Giaur Kala, the original site of Merv, destroyed about the end of the seventh century by the Arabs, and Bairam Ali, its successor, destroyed in 1784 by the Amir of Bokhara. A general plan is given of all these crumbling citadels, palaces, tombs, baths, and earthworks, "where now no living creature is to be met with, save an occasional Ersari robber or treasure seeker. For here, as in almost every other part of the East, the popular imagination enriches these ruined vaults and foundations with secret treasures stowed away beneath them" (ii. 241).

From the frequent recurrence of the term *Kalassi*, supposed to be a corruption of *ecclesia*, some archæologists have scattered the remains of ancient Christian churches with a liberal hand over Western Turkestan. But Mr. O'Donovan suggests that there is here a confusion between *Killissi*, which really represents *ecclesia*, and *Kalassi*, a Turki form of the Arabic *Kal'a*, a fort or castle.[2] Hence Kara Kalassi, for instance, would mean, not the "Black Church," but the "Black Castle." In Armenia, a Christian country, *Killissi* certainly occurs; but in the Oasis Mr. O'Donovan "never came upon any structure which could possibly have been a Christian Church" (ii. 177).

Amongst the remains are some earthworks bearing the title of Iskander Kala, or "Alexander's Castle," the local tradition being that the Macedonian army encamped here on its way to India. But here again he pertinently remarks that "in these countries Alexander comes into every story connected with ruins of remote antiquity."

[1] The subdivisions of these two main branches of the Merv Tekkés do not correspond with those given by Mr. Marvin in his "Merv, the Queen of the World." Some of the discrepancies however may be reconciled by restoring to their proper form the names disguised in Mr. O'Donovan's peculiar orthographic system. Thus his *Karatchmet* appears by reference to Marvin's tables to stand for the *Kara-Ahmed* subdivision of the Otamish branch.

[2] At the same time this *Kalassi* would appear in many cases to be simply the Persian *Kalása*, a well, and especially the watering-places maintained at intervals in the desert for the convenience of caravans and pilgrims to Mecca.

Some points of resemblance are discovered between the Turkoman and Kelt, which are probably not intended to be taken seriously. But the description of the Turkoman type, coming from a shrewd and original observer, possesses sufficient ethnological value to deserve quoting:

"The usual Turkoman physical type, both male and female, is rough, rude, and vigorous, and quite in contrast with that of the frontier Persian, which is sleek, cat-like, feeble, and mean. The worst part of the Turkoman is his head, which is decidedly conical, the point being thrown somewhat to the rear. A phrenologist would say that firmness was very pronounced, conscientiousness wanting, and benevolence small. The features are not of that Tartar cast that one would be apt to suppose in denizens of East Caspian districts, and though here and there may be seen a suspicion of peeping eye, a tendency towards flattening of the point of the noise, and occasionally high cheek bones, on the whole the faces are more European than otherwise. In fact I have seen some physiognomies at Gumush Tepé which, if accompanied by an orthodox European dress, would pass muster anywhere as belonging to natives of the West. It is among the women that the absence of European features is most conspicuous. There are many of them who could fairly be reckoned pretty, though it is quite a different order of beauty from that to which we are accustomed. . . . It is among the men that the handsome individuals must be sought for, especially when there has been an admixture of Persian blood. The scanty beard of the pure Turkoman is then replaced by one of much more luxurious proportions, and of a darker tint; the nose assumes a more or less aquiline form, and the eye loses the cold grey expression so characteristic of the pure-blooded dweller on the Steppes" (i. 231-3).

The accompanying portrait of the author in oriental garb might be taken as an apt illustration of this description. There is also an excellent map of North Persia and the Trans-Caspian region, based on that of Colonel Stewart, but with numerous fresh details embodying the results of the explorer's observations in the Tejend Valley and Merv Oasis. But the spelling is as usual at variance with that of the text, and there is unfortunately no index. The appendix contains facsimiles of a number of letters from Turkoman Khans, one or two of which are fine specimens of the beautiful ta'lík penmanship.

The reputation of an intelligent and enterprising explorer secured to General Macgregor by his "Journey through Khorasán (noticed in NATURE, vol. xx. p. 453), will be considerably increased by his "Wanderings through Baluchistan." The trip was made in company with the ill-fated Capt. R. B. Lockwood, of the 3rd Bengal Cavalry, on their return to duty in India, between the months of September, 1876, and March, 1877. During this period the western section of Makrán was thoroughly explored, and the problems connected with the drainage of the Mashkíd and Mashkel rivers at last cleared up. The Mashkíd was supposed by many geographers to flow through the Dasht to the Arabian Sea, while the Mashkel was sent northwards to the Zirreh or Sistán Hamun, that is, to the Helmand basin. But by actual survey the explorers have shown that (1) both of these rivers belong to the same hydrographic system; (2) this system is unconnected either with the Helmand or Arabian Sea; (3) the two streams, after their confluence above the romantic Tank Zorati pass in the Sianeh-kuh range, flow mainly north-west to the Mashkel Hamun in 28° 20′ N., 60° E.; (4) this swamp has no outlet, and is actually separated by another depression, the Kindi Hamun,

and by a range of hills, the Koh-Amir, from the Sistán Hamun. A ride performed under great difficulties across the Kharan desert to the neighbourhood of the Sistán swamp placed all these points beyond doubt, so that the drainage system of the hitherto almost unknown region along the Perso-Baluch frontier, from the Lower Helmand to the Arabian Sea, has now been satisfactorily determined. From Sistán the travellers made their way by two new and parallel routes right across North Baluchistan to Jacobabad in Sind. The numerous typographical points recorded both here and throughout West Makrán are embodied in the accompanying map, which is on a large scale, and which forms an important contribution to our knowledge of the south-eastern section of the Iranian plateau. In the appendix are given the directions, distances, and other useful details of no less than twenty-two routes in the same region. The relief of the land and its salient physical features are also further illustrated by numerous sketches made on the spot by General Macgregor. Much valuable matter regarding the Baluchi and Brahui tribes, and the present political situation of Baluchistan, is scattered over the pages of this pleasantly written volume. A. H. KEANE

PHYSICAL OPTICS

Physical Optics. By R. T. Glazebrook, M.A., F.R.S. (London: Longmans, 1883.)

THIS is the most recent volume of the well-known series of Text-books of Science published by Messrs. Longman. Mr. Glazebrook is already favourably known as an accurate experimenter and an able theorist in the subject of which this volume treats, and it is therefore unnecessary to say that the treatise under notice contains a large amount of authentic and interesting information on all branches of the subject. We must confess, however, to a certain feeling of disappointment after going through the book, arising chiefly from the fact that the author does not appear clearly to have made up his mind as to the class of readers to whom the book is to be useful. Those who have had any experience in real personal teaching of the artisans and students in science schools for whom the volumes of this series are stated to be intended, will soon perceive that Mr. Glazebrook has assumed an amount of mathematical knowledge and ability which very few of them possess. On the other hand results are occasionally assumed, the investigation of which would be quite within the reach of those university students who will probably form the larger part of the readers of the treatise. For instance, the investigation of the focal lines of a pencil refracted in a principal plane through a prism, and the condition of their coincidence in the position of minimum deviation, is settled by an "it may be shown," although the analysis required is certainly not more difficult than much that is given in the book, and the point to be elucidated is of considerable importance.

The author has intentionally introduced a large quantity of matter which is usually considered to belong to the kindred subject of Geometrical Optics, and although there will probably be a difference of opinion as to the advantage of this proceeding, there will be none as to the clearness of the explanations and the excellence of the diagrams employed. There does not seem to be quite so much "matter new to the text-books" as is hinted at in the preface, but on the whole the book furnishes a good account of the subject, comparable with Lloyd's well-known treatise on the Wave Theory of Light, and dealing with many points which have been investigated since the date of the latter work.

Where there is so much of good, it is a pity that it should not be made better, and there are a few points in which perfection has not been reached. In places there is a tendency to a slipshod and "high-falutin" method of expression which may be forgiven in University Extension lectures delivered extempore to popular audiences, but which is hardly suitable for a scientific treatise. On p. 2 we have a graphic representation of the author raising his arm and of the effect thereby produced on his own body. Later on in the book, quitting the solitary first person, he becomes more friendly to his readers, and speaks of "our apertures, our lens, our prism, our eye," and so on. He even presently hands the apparatus entirely over to the reader and directs him to perform the operations for himself. A more serious matter is the want of care in revising the proofs. For instance, on p. 202 an effect is spoken of as "that due to a single aperture multiplied by a number of apertures," which is nonsense, the author's meaning being "multiplied by *the* number of the apertures." Again, on p. 141, the sentence—"They are distinct from the coloured rings of thick plates discovered by Newton, and were described by him as follows," gives an almost opposite meaning to that which the author intended. It ought to read "which were described."

The proper names are not treated with the accuracy and uniformity which are desirable. We have Fraunhofer usually, but on p. 316 Frauonhofer. Huygens appears on p. 15, but more frequently the name is met with as Huyghens, while the possessive case assumes the different forms of Huygben's, Huyghens', and on p. 226 Huyghens's. Defects of this kind mar the pleasure with which the book would otherwise be read, and seem to indicate that more care might have been advantageously bestowed on the original composition as well as on the revision of the proofs. Possibly the author wishes to leave something to be looked for in the second edition, for the speedy arrival of which he has our best wishes.

OUR BOOK SHELF

The Year-Book of Pharmacy, 1882. 8vo. Pp. 607. (London: Churchill, 1883.)

THIS volume contains a number of exceedingly interesting papers and extracts. The most interesting are those which relate to the artificial production of organic alkaloids, for when we obtain such a knowledge of the constitution of these bodies as will enable us to make them artificially, we may hope that a new era will commence in medicine, and that the results of the treatment of disease will be more definite and satisfactory than heretofore.

Prof. Ladenburg, who has been engaged for some time on researches into those alkaloids which dilate the pupil, is still continuing his researches, and has obtained very interesting results indeed. Atropia when heated with strong hydrochloric acid splits up into a base, tropine, and an acid, tropic acid. While pursuing his investigations upon tropine, the author came to the conclusion that this base contained an alcoholic hydroxyl group

which possessed the exceptional property of forming fresh alkaloids, when treated with certain acids in hydrochloric solution as in the preparation from it of atropine and homatropine. By acting upon secondary amines by chlorhydrines, he has succeeded in obtaining a series of bases analogous to tropine, and yielding, like it, other basic compounds, which resemble natural alkaloids in their properties and composition. For these bases, which perform the function of alcohols and amines, the author proposes the name of *alcamines*, and for the basic ethers derived from them, that of *alcameines*. That resulting from the action of phenylacetic acid, for instance, has a composition represented by the formula $C_{15}H_{21}NO_2$, and forms crystallisable salts. It is a powerful poison, acting on the respiration and the heart.

A curious relation has been discovered between theobromine, the active principle of cocoa, caffeine, the active principle of tea and coffee, and xanthine, a substance found in muscle, and largely contained in beef tea and Liebig's extract.

Dr. Fisher shows that xanthine may be converted into theobromine, and theobromine into caffein. The relation between these bodies seems to be that theobromine is dimethyl- and caffein is trimethyl-xanthine.

From the great importance of the cinchona alkaloids and their extensive use in medicine, it is exceedingly desirable that we should be able to make quinine artificially. This has not yet been done, but, with a view towards it, extensive researches are being made into the constitution of the cinchona alkaloids; and Skraup finds that all the four cinchona alkaloids—quinine, quinidine, cinchonine, and cinchonidine—when oxidised with potassium permanganate yield formic acid, and a base apparently related to phenol or carbolic acid. Other modes of oxidation exhibit a relation between quinine and cinchonine.

Cinchonine has been prepared artificially by treating a mixture of nitrobenzol, aniline, and glycerol with sulphuric acid. In its physiological properties it exhibits a certain relation to quinine, and, like it, reduces the temperature in fever and lessens or prevents putrefaction. It is said to differ from quinine in not producing giddiness, or ringing in the ears, and to have very little action on alcoholic fermentation.

An important paper by Plugge relates to the various strengths of aconitine. He finds that Petit's nitrate of aconitine has a poisonous action at least eight times greater than Meret's and 170 times greater than Friedländer's. Such differences as these between preparations bearing the same name have already led to fatal cases of poisoning—when aconitine has been prescribed medicinally; and Mr. Holmes considers that the only way to secure uniformity in the ordinary preparations of aconite is to prepare them only from plants grown in this country, and gathered while the plant is in flower.

In addition to many other interesting papers this volume contains a bibliography of chemistry, pharmacy, botany, and allied subjects.

Mémoires de la Société des Sciences Physiques et Naturelles de Bordeaux. 2e Série, Tome v. 1er Cahier. (Paris, 1882.)

IN this number there are several interesting papers. We note "La Route d'Australie par le Thermomètre," with tables, by M. Hautreux; "Sur les Unités de Gauss," by M. Abria; "Le Téléphone à Bordeaux," by M. Auguste Bonel; "Notice sur les Communications Télégraphiques sous-marines," by the same; "Modification aux Machines à Force centrifuge," by M. O. de Lacolonge; "Températures et Densités de l'Eau dans l'Estuaire de la Gironde," by M. Hautreux; "Vérification expérimentale des Lois de Dalton relatives à l'Évaporation des Liquides," by M. E. Laval. M. P. Tannery contributes one of his useful critical notes, this time "Sur une Critique ancienne d'une Démonstration d'Archimède." The criticism is contained in § 36 of the fourth book of the "Collections" of Pappus (Hultsch's edition), and it charges Archimedes (in Prop. 18 of his "Spirals") with solving as a *solid* problem (*i.e.* with the aid of the conic sections) the following proposition:—*O M A* is a spiral, of which *O* is the pole, *O A* the axis, and *A C*, the tangent at *A*, meets the perpendicular to the axis through *O* in *C*, then *O C* is equal in length to the circumference described with *O A* as radius. M. Tannery supplies the gist of Archimedes' proof, and shows that Archimedes "fait appel simplement à l'intuition et au principe de continuité." Other points of interest turn up in the communication. Dr. Sigismond Günther is engaged upon an extended inquiry into the processes employed by the ancient mathematicians in the extraction of square roots, and in the course of his work has met with some interesting results. Some of these he puts forth in his paper "Sur la dépendance entre certaines méthodes d'extraction de la racine carrée et l'algorithme des fractions continués." The methods examined are those of Mollweide ("Commentationes mathematico-philologicæ tres," Lipsiæ, 1813), and of Alexeieff ("Sur l'extraction de la racine carrée d'un nombre," *Bulletin de la Soc. Math. de France*, t. vii. p. 167).

We have left to the last an article by Dr. Adolf Dux (translated from the *Pester Lloyd* for February 4, 1880), entitled "La Tombe du Savant." Bolyai was professor of mathematics and physics in the "Collège Réformée" at Maros-Vásárhely. No statue, nor marble mausoleum with sides covered with laudatory inscriptions, marks the spot where this *savant* lies; but the tomb, by its occupant's strict direction, is overshadowed by the boughs of an apple-tree, "*en souvenir des trois pommes* qui ont joué un rôle si important dans l'histoire de l'humanité, et il désignait ainsi la pomme d'Ève et celle de Pâris qui réduisirent la terre à l'esclavage, et la pomme de Newton, qui la replaça au rang des astres." Strangely enough, when Dr. Dux visited the tomb there hung on the tree just three apples, "ni plus ni moins."

Bolyai was not only a mathematician, he was also a poet: hence he had not only in his room a portrait of Gauss (with whom he had been associated at Göttingen from 1797 to 1802), but also the portraits of Shakespeare and Schiller.

In 1855 he wrote his own *Necrologe*, and survived its completion about one year. In this "Adieu" occur passages, some grave, and some humorous; of himself he writes, "S'il a été mauvais, la terre est délivrée de lui; s'il a été bon, il est délivré de la terre." He burned his poetical writings, and collected the ashes in a wooden cup, on which he wrote the following lines from Horace:—

"Poesis
Si paulum a summo discessit, vergit ad imum."

This is now preserved as a relic in the library of the College. Here too is shown a photograph taken after Bolyai's death. "Une noble figure, au front et au nez puissants, illuminée par la majesté de l'esprit et du sublime repos, avec de longs cheveux lisses, descendant jusqu'aux épaules. Quand on a vu ces traits, on comprend mieux le sens du nécrologe que Bolyai a lui-même écrit et suivant le texte duquel un noble pommier a été planté sur sa tombe.'

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Natural Selection and Natural Theology

I HAVE just received last week's issue of NATURE from England, and find in it some remarks by Prof. Asa Gray on an

article of mine which appeared in the *Contemporary Review*. As he appears to solicit a further statement of my views, I shall supply a brief explanation of those passages in my article to which he refers.

This article, I must begin by observing, was written in reply to a criticism on my essay in the "NATURE Series" ("Scientific Evidences of Organic Evolution"), and, being intentionally limited to the ground covered by that criticism, it did not require to discuss all the points raised by Prof. Gray. But without reference to the original article, I shall now consider these points *seriatim*.

First, I am requested to state what I mean by urging that in my opinion there is no logical point of contact between natural science and natural theology. My answer is that natural science, *as such*, can only be legitimately concerned with the investigation of natural or physical causes, and that in whatever degree it presumes to pass beyond the territory of such investigation, it ceases to be natural science, and becomes ontological speculation. In other words, there is no point of logical contact between the methods and aims of natural science, as such, and the super-scientific conclusions which constitute the aim of natural theology. For it is the aim of natural theology to establish certain very definite conclusions with reference to the existence and the character of a "*causa causarum*," which is acknowledged to be supernatural at least to the extent of being inscrutable by any of the methods possible to science. But from this sufficiently obvious position it does not follow, as my critic seems to insist, "that because natural phenomena can be reduced to laws and sequences of cause and effect, no legitimate or rational inference can be made by the human mind to a *causa causarum*." Whether or not any such legitimate or rational inference can be made, from the data mentioned, *by the human mind* is not here the question, and therefore I decline to enter upon it. The only question before us is as to whether any such inference can be drawn by the human mind from the province of *natural science*, and I say that this question must be answered in the negative, seeing that there is, as I have just explained, an absence of logical contact between the sphere of natural science and the sphere of supernatural theology. Any inferences of the kind in question, be they legitimate or not, are drawn from the *general* order of Nature—*i.e.* from the *universal* prevalence of "laws and sequences of cause and effect"; therefore they are not really or logically strengthened by a mere enumeration of *particular* instances of such laws and sequences all similar in kind. The so-called law of causation as a whole being known, and its universality recognised, its true argumentative value to the theory of theism is not influenced by the explicit formulation of any number of its specific cases which the progress of science has been able, or may be able, to supply.

I allow, of course, that *the human mind* cannot avoid occupying itself with the most momentous of all questions—the nature of the First Cause and of its relations to the universe—or, in the words of Prof. Gray, that "such questions are inevitable"; I am only concerned with explaining why I conceive that such questions are not affected by any of the methods or results of natural science, which do not stand in any logical relation to them. And this introduces me to the next point in Prof. Gray's criticism. He says that I am inconsistent in first alleging that there is no point of logical contact between natural science and natural theology, and then proceeding to affirm that the theory of natural selection has proved destructive of the evidence of special design in organic nature. But I think this charge must have been made without due reflection; for, if a man believes that there is no logical connection between one thing and another, I do not understand why he should be deemed inconsistent merely because he endeavours to show the fictitious character of the logical connection which has been erroneously supposed to exist. Assuredly I think that "Darwin's theory need not, and legitimately should not, concern itself with natural theology"; but I also think that natural theology should not seek to obtain unreal support from natural science, and it is because natural theology has sought to do this in one conspicuous instance, and in that one instance has been as conspicuously met by Darwin's theory, that, as I explained in my article, it seemed to me desirable, both in the interests of science and of theology, henceforth clearly to recognise the logical gulf which is fixed between these two departments of human thought.

Next Prof. Gray observes, and quite correctly, that my view of the matter as a whole is fairly presented by the following sentences, which he quotes:—

"The facts of organic nature furnish no *evidence* of design of a quality other or better than any of the facts of inorganic nature." "Or, otherwise stated, there is nothing in the theory of natural selection incompatible with the theory of theism; but neither does the former theory supply *evidence* of the latter. Now this is just what the older theory of special creation did; for it would be proof positive of intelligent design if it could be shown that all species of plants and animals were created, that is, suddenly introduced into the complex conditions of their life; for it is quite inconceivable that any cause other than intelligence could be competent to adapt an organism to its environment *suddenly*."

Prof. Gray then asks: "Is the writer of this quite sure that any cause other than intelligence could be competent to adapt existing organisms to their environment *gradually?*" My answer is but too easy. I must leave to others the happy position of being "quite sure" about anything relating to the possibilities of supernatural causation. For aught that I know, or for aught that any living man can ever know, not only all existing organisms, but all existing atoms, may have depended from all time, and for all their changes, sudden or gradual, upon "intelligence," without which they may not have been able either to have lived or to have moved, or even to have had their being. But how does this necessary ignorance on my part affect my statement that "the facts of organic nature present no *evidence* of design of a quality other or better than any of the facts of inorganic nature"? I confess I do not see how this failure in the evidence of design is made good by telling me that, for anything to the contrary of which I can be "quite sure," there *may* have been a designer. For I cannot follow my critic where he argues that the element of a supposed sudden introduction of an organism to its environment makes no difference in the evidence of its adaptations to its environment having been designed. He asks: "How is this presumption [*i.e.* that of special design] negatived or impaired by the supposition of Darwin's theory, that the ancestors were not always like the offspring, but differed from time to time in small particulars, yet so as always to be in compatible relations with their environment?" The answer is, that if we suppose the sudden or special creation of organisms in manifold adaptation to their several environments, we can conceive of no cause other than intelligence as competent to produce the adaptations, whereas, if the adaptations have been effected gradually, and *by the successive elimination of the more favourable variations by a process of natural causation*, we clearly have a totally different case to contemplate, and one which is destitute of any evidence of special design. Assuredly "*gradualness* is in nowise incompatible with design," and I do not suppose that there has ever been any one so foolish as to imagine that it is; but all the same, the progressive adaptations of structures to functions by such a purely physical cause as natural selection when once clearly revealed must destroy all special or particular *evidence* of design, even supposing such design to exist. For under this point of view it was *only* those variations which were "in compatible relations with the environment" which were able to survive. Only if it could be shown that the variations always took place exclusively in the directions required for a development of the adaptations, so as to leave no room for the operation of the physical cause in question—only then would the evidence of design as deduced from the theory of evolution be comparable with that evidence as deduced from the theory of special creation.

Towards the close of his letter, Prof. Gray seems to have anticipated this obvious rejoinder, for he says that, in order to make the purely physical explanation tenable, "it must be shown that natural selection scientifically accounts for the adaptation,"—*i.e.* as I understand, it must be shown that there is not some influence of an intelligent kind guiding the occurrence of the variations in the requisite lines, which, having been thus intelligently caused to arise, are then seized upon by natural selection. If this is Prof. Gray's meaning, he is certainly wrong in attributing it to Mr. Darwin, and I cannot see that it is a meaning of any argumentative use. For the burden of proof lies with the natural theologian to show that there *has* been some such intelligent guidance of the variations, not with the evolutionist to show cause why there *may not* have been such guidance. The evolutionist may freely admit that natural selection has probably not been the only physical cause at work, and even that the variations supplied to natural selection may not have been wholly fortuitous, but may sometimes have occurred along favourable lines as "responses of the organisms to their

physical surroundings."[1] But such admissions would make no change in the logical aspect of the case; for, however many supplementary causes of this kind we may choose to imagine as possible, the evolutionist is bound to regard them as all alike in this—that they are of a physical or natural kind.

And this leads me to the core of the whole subject. Prof. Gray says:—

"What is probably meant is, that natural selection is a rival hypothesis to design, that it accounts for all adaptations in the organic world on physical principles, and so renders . . . the evidence of design from these adaptations of no other or better value than that from anything else in Nature." He then proceeds to object to this view, and says:—"If means and ends are practicable in inorganic nature at all, it is only by remote and indirect implication; while in organic nature the inference is direct and unavoidable. With what propriety, then, can it be affirmed that organic nature furnishes no other and no better evidence of underlying intelligence than inorganic nature? The evidence is certainly *other*, and to our thinking *better*."

This, I say, is the core of the whole subject. If once it is fully admitted and understood that organic nature is one with all the rest of the universe in the matter of physical causation, so that all the wonderful adaptations which we there encounter are the results of natural causes—survival of the fittest *plus* any number of other natural causes—then it appears to me, as I have said in the essay already alluded to, that all such cases of adaptation must fall into the same logical category, with reference to the question of design, as all or any other series of facts in the physical universe. For the only element of difference arises from the greater intricacy of the physical causation in the cases contemplated, rendering it more difficult to perceive the operation of the causes, at work, and therefore, as Prof. Gray truly asserts, rendering their operation more suggestive of design. But this element of difference does not really affect the question. For, *ex hypothesi*, the law of causation is everywhere and equally uniform, and for this reason the evidence of design in organic nature is certainly *not* other than it is in inorganic nature, nor, in view of the same reason, is it, to our thinking, better.

Florence, February 3 GEORGE J. ROMANES

THE letter of Prof. Asa Gray (NATURE, vol. xxvii. p. 291) contains a sentence which seems to me to contain the essence of the difference between the views of organic life, as held by the supporters of Natural Selection and Natural Theology. He says: "How is this presumption negatived or impaired by the supposition of Darwin's theory, that the ancestors were not always like the offspring, but differed from time to time in small particulars, *yet so as always to be in compatible relations to the environment?*" The italicised portion is just such a statement as "Design" would require, but cannot be held by scientific evolutionists, otherwise why are there so many extinct species? With "Design" there ought to be a perfecting of all species; whereas we know of so many which have been ruthlessly swept aside, owing to their having "differed (or owing to their not having sufficiently differed) from time to time in small particulars, yet" *not* "so as to be in compatible relations to the environment." Change is the evolutionist's view of life—change sometimes caused by the environment, sometimes beneficial, sometimes eventually detrimental: where beneficial, the species increases; where detrimental, other changes or extinction must ensue. Design would never have supplied us with a "Nature red in tooth and claw with ravine," nor would it have built up a system by the expensive and cruel mode of trial and error.

Cove Castle, Loch Long, N.B. J. B. HANNAY

Two Kinds of Stamens with Different Functions in the Same Flower

To the Melastomaceæ and Commelynaceæ mentioned in NATURE (vol. xxiv. p. 307, vol. xxvi. p. 386, and vol. xxvii. p. 30), may be added the genera *Mollia* (Tiliaceæ), *Lagerstræmia* (Lythraceæ), and *Heteranthera* (Pontederaceæ), for having differently coloured anthers. In several species of *Mollia*, according to Darwin ("Forms of Flowers," p. 168, footnote), the longer stamens of the five outer cohorts have green pollen, whilst the shorter stamens of the five inner cohorts have yellow pollen; the stigma stands close beneath the uppermost anthers. In a *Lagerstræmia* in my garden the six outer stamens have green pollen, and are much longer than the numerous inner ones, which have bright yellow pollen; the stigma stands on a level with the outer anthers. I have repeatedly seen bees alighting on, and gathering the pollen of, the inner anthers without noticing the outer ones.

In *Heteranthera reniformis* there is one long stamen (belonging to the outer whorl) having pale bluish pollen, and two short stamens (of the inner whorl) with bright yellow pollen. The stigma stands generally on a level with the anther of the long stamen. When the white flower opens, pistil and long stamen diverge, the pistil bending (almost without exception) to the right, and the stamen to the left; at the withering of the flower, they again approach each other, so that the stigma may be fertilised by the pollen of the long stamen. Visiting insects are attracted yet more to the yellow anthers of the two short stamens by their being placed close to a yellow spot, surrounded by a violet border, at the base of the upper petal.

Thus it may be safely assumed that in all these flowers, as well as in the above-mentioned Melastomaceæ and Commelynaceæ, fertilisation is almost exclusively effected by the pollen of the longer stamens, whilst the shorter stamens serve only to attract pollen-gathering or pollen-eating insects. It is far from surprising that the pollen of these latter stamens, though often produced in large quantity, should tend to degeneration. Darwin long ago came to this conclusion with respect to some Melastomaceæ with differently-coloured anthers, of which he had raised seedlings from pollen both of the longer and shorter stamens ("There is reason to believe that the shorter stamens are tending to abortion."—"Cross- and Self-Fertilisation," p. 298, footnote). The *Lagerstræmia* in my garden being self-sterile, I fertilised some flowers with green, and others with yellow pollen of a different variety (or species?) growing in other gardens; both produced fruits with apparently good seeds, but only some of those from the green pollen have germinated.

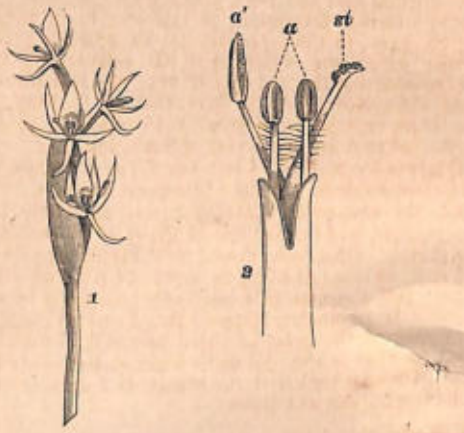


FIG. 1.—Flower-spike of *Heteranthera reniformis* (natural size). FIG. 2.—Upper end of the flower-tube, seen from behind. *a'*, the one anther of the outer whorl, with pale bluish pollen; *a*, the two anthers of the inner whorl, with bright yellow pollen; *st*, stigma.

As in all the flowers above-named, with differently-coloured anthers, the dull colour of those of the longer stamens evidently serves to make them less visible to insects, may not the green colour of the anthers of the long stamens of the mid-styled and short-styled flowers of *Lythrum salicaria* also protect them against the attacks of pollinivorous insects, to which, from protruding far from the corolla, they would be more exposed than those of the shorter stamens?

Even without being differently coloured, the stamens of the same flower may be divided into different sets with different functions. Thus in a species of *Cassia* the visiting humble-bees gather the pollen of the four intermediate stamens (the three upper ones being pollenless), which are short and straight, whilst the three lower ones are very long and curved in such a way that their pollen is deposited on the back of the humble-bees. The pistil is of the same length and curved in the same way as the longer stamens. Another very striking instance has been carefully described by Prof. J. E. Todd of Tabor (Iowa) in

[1] In my "NATURE Series" essay I expressly stated that natural selection is probably not the only cause of organic evolution, and therefore I think it might have been well if my critic had taken the trouble to refer to this essay before indulging in the general proposition at the close of his letter with reference to exactitude.

a plant of a very different family, viz. *Solanum rostratum* (*American Naturalist*, April, 1882, p. 281): one stamen and the pistil are very long and strangely curved; four stamens are short and straight, and serve only to furnish pollen to the visiting insects; all the anthers, as I am informed by Prof. Todd, are of the same dull yellow colour. FRITZ MÜLLER

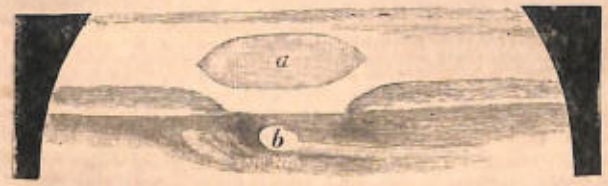
Blumenau, Santa Cattarina, Brazil, December 27, 1882

The Markings on Jupiter

AFTER heavy storms of hail on January 30, the sky cleared and the night was exceptionally fine. I observed Jupiter with my 10-inch reflector about 11h. 30m., and watched the chief markings pass the central meridian of the planet. The well-known equatorial white spot came to transit at 11h. 44m., and it was followed 5 minutes later—at 11h. 49m.—by the great red spot. These objects, therefore, must have been in conjunction on January 30, at 2h. 47m., as the greater velocity of the white spot enables it to gain 13m. 24s. on the red spot daily.

In NATURE, vol. xxv. p. 225, I stated that during the 400d. 0h. 20m. elapsed between 1880, November 19, 9h. 23m. and 1881, December 24, 9h. 43m., the white spot had completed 9 revolutions of Jupiter relatively to the red spot; the number of rotations performed by the former being 976, and by the latter 967. Since 1881, December 24, I have continued to watch the anomalous velocity of these curious markings, and find that between that date and 1883, January 30, the white spot has completed 9 further revolutions of Jupiter. From 1881, December 24, 9h. 43m., to 1883, January 30, 2h. 47m., is 401d. 17h. 4m., during which the white spot has rotated 980 times, while the number for the red spot is 971. In fact my observations since 1880, November 19, show that up to 1883, January 30, the white spot had performed 1956 rotations, as against 1938 by the red spot in the interval of 801d. 17h. 24m.

On January 30, when I last saw these markings, the red spot was remarkable on account of its great faintness. On the other hand, the equatorial white spot was extremely brilliant and conspicuous, and formed one of the most noticeable features on the planet. Observers should now keep a close watch on the red spot, as it seems likely to be on the point of disappearance, though this disappearance need not necessarily be of final character. It fortunately happens that a curious irregularity in the formation of the great southern belt will probably enable the exact position of the spot to be watched for a considerable time. This particular region of the planet, as I drew it on January 30 at midnight, was as follows:—



1883, January 30, 12h. The region south of the equator of Jupiter. *a*, The red spot; *b*, white spot.

The sketch shows that the great south belt is now double, and a very conspicuous object on Jupiter. The south half of this belt is bent abruptly to north, and runs into the other half exactly north of the preceding and following ends of the red spot. There is some explanation to this interesting feature, though it is at present involved in mystery; in any case it may possibly serve as a very accurate indication of the place of the red spot long after that object has become obliterated altogether. W. F. DENNING

Bristol, January 31

Meteor of November 17

I THINK now that more observations of the remarkable phenomenon of November 17 have been brought forward, that we cannot but candidly acknowledge that the evidence is extremely contradictory and impossible to reconcile, that is as applying to one and the same object. Altogether there is something mysterious about it. It is evident that since it appeared to reach the greatest apparent length of about 30° at York, then from all places further south it ought to have attained a length exceeding this, the more so the further south they are. The ends of the beam appeared very well defined from here, and there was very little room for estimates varying according to the observer's sensitiveness to light. If we take the observations made from Clifton, Cirencester, East Clevedon, Woodbridge, and Windsor, as they nearly all agree in estimating the length as *over* 30°, some considerably over, then these may all relate to the same object. But its appearance from York is flatly contradicted by Mr. Batson's observation from Hungerford, that from Halstead, Essex (which seems to agree with Mr. Batson's), also those from Lincoln's Inn Fields, Greenwich, and Cambridge. All these agree in contradicting the others named above, by assigning a much *smaller* angular length. Mr. Batson describes a sudden foreshortening which the meteoroid underwent when passing the moon, and since I saw it pass below the moon at practically the same time, then (on the supposition that we beheld the same object) the same shortening ought to have been visible to me; but there was not the slightest trace of any such thing. I noticed that it very gradually shortened in length (after allowing for perspective) in its journey towards the west, which is significant, and explainable if we suppose the body to have been encountering resistance to its momentum. It is impossible to reconcile all the observations, and yet most extraordinary that no single observer is known to have witnessed more than one such phenomenon at about that time except Mr. Worthington, who says he saw two *at once*. I have reason to believe that a rather similar thing was seen below the moon at about 5 30 on that night from here. I see that from Zierickssee, in Holland, a similar phenomenon was seen to transit α Pegasi (which would be at about 50° altitude, and on the magnetic meridian from there). If this was the one that I saw, then at the time that it was seen to transit α Pegasi, from Holland, it would appear to me to be just forming in the south-east, where it appeared to be about 10° above the horizon, it which case it would have to be under seventy miles high when over Belgium. But it is almost certain that it attained a height of over 150 miles during the latter part of its course. As yet (figuratively speaking) the spectra of these auroral phenomena have not thrown as much light on these things as that which enters the narrow slip of the spectroscope to print its uncertain record on the retina. I only hope that some one with a clear head and much patience will succeed in unravelling the tangled skeins of evidence which surround the mysterious meteoroid of November 17, 1882. H. DENNIS TAYLOR

Heworth Green, York, February 11

Aino Ethnology

IN an article on "Aino Ethnology" which appeared in NATURE, vol. xxvi. p. 524, and which I happened to read only a few days ago, Mr. A. H. Keane makes the following statement:—"Until the appearance of Herr Rein's large work on Japan, one of the most universally-accepted of these conclusions was that, whatever be their affinities, the Ainos must certainly be separated from the Mongolic connection. No little surprise was accordingly produced by Rein's attempt to affiliate them to the surrounding members of the yellow race. But it was soon seen that his arguments, apparently inspired by a love of paradox, were sufficiently refuted by the very illustrations of the Aino type introduced into his work."

I submit that one who has read my work upon Japan will decide, with me, that the spirit of the matter quoted is unfair, in so far as it charges me with "attempts" at affiliation, and with being "inspired" otherwise than by a love of truth—this motive being, as stated in my preface, that which induced me to write.

It is repeatedly mentioned in my book that I had never been in the island of Yezo, and those who have carefully read the whole work—including Mr. Keane, if he has done so—cannot reasonably fail to observe that I speak of the Aino tribe as one who had never visited them in their proper home, nor made them a special subject of study in any respect. My remarks upon their probable racial affinities were based upon good, and the then latest, authorities, whose names I was careful to mention. Thus, on p. 444, occurs a passage of which the following is a rendering:—"Doenitz and Hilgendorf have made thorough investigations of their (the Ainos) physical peculiarities, and have published the results thereof in the *Mittheilungen der Deutschen Gesellschaft Ostasiens*. It appeared as an undoubted fact 'that the Ainos are Mongolians, who are separated from the Japanese in a perhaps less degree than the Germans from the Romans.'"

In the comments which I added to the views of these eminent observers, wherein I mentioned those circumstances that seemed to me to tend in the direction of their support, I was, of course, unable to include, as I would if possible have done, a statement of the opinions of such noted authorities as Dr. Steube and Herr von Siebold, the results of whose investigations have so recently been given to the press, and which are cited in the article that elicits this letter. J. J. REIN

Marburg, Germany, February 8

Hovering of Birds

I REGRET that I did not notice until to-day that Mr. Airy, in his letter published in NATURE, vol. xxvii. p. 294, specially referred to "hovering with perfectly motionless wings" as being that for which an upward slant of wind is, as he believes, absolutely requisite to enable the bird to do so.

Is the term "hovering" applicable to the examples given by Mr. Airy of gulls and hawks floating as it were with motionless wing along hillsides and cliffs?

I have always associated "hovering" with the flapping or fluttering of the wings, as is invariably noticed when terns or hawks are looking for their prey either over land or water.

February 10 J. R.

Intelligence in Animals

IN his letter in NATURE, vol. xxvii. p. 337, Mr. J. Birmingham does not mention what kind of bear it is that throws down pieces of rock "in order to catch the bareins," as told by the Kamtschadales; but the Eskimos have a somewhat similar story of the white bear, when attacking the walrus, the largest of which, with their formidable tusks, Bruin generally avoids.

The circumstance was told me by an eye-witness, a very truthful and honest Eskimo of Repulse Bay. He said: "I and two or three other Innuit were attempting to approach some walrus in winter, lying on the ice close to the water, kept open by the strong current, in Fox's Channel. As we were getting near we saw that a large white bear was before us. He had reached in the most stealthy manner a high ridge of ice, immediately above where the walrus were lying; he then seized a mass of ice[1] in his paws, reared himself on his hind legs, and threw the ice with great force on the head of a half-grown walrus, and then sprang down upon it."

The Eskimos then ran up, speared the bear, and found the walrus all but dead, thus securing both animals. I should add that the bear threw the ice as if he was "left-pawed."

Kensington, February 10 J. RAE

WHILE spending the late winter months at Paignton, in Devon, I frequently watched, through a telescope, shore birds of various kinds stalking game on the low tide sands. These abound with sand-eels, which lie, perfectly concealed, about an inch below the surface, and are caught in the following way by the gulls.

Standing close to the water's edge, the birds tread the wet sand into soft puddles by rapid alternate movements of their feet, and when a sand-eel, thus disturbed, makes a dart for the sea, he is instantly taken by a skilful but leisurely-looking snap of the beak.

Sand-eels bury themselves without leaving any marks on wet sand, and the gulls were always seen steadily and tentatively beating over the ground in the way I have described. They took, each, a fish a minute, perhaps, and impressed me with the idea that some thoughtful ancestral gull had deserved well of his race for the invention of such an easy logical way of picking up a living. D. PIDGEON

Holmwood, Putney Hill, February 7

The Sea-Serpent

ON reading the letter of W. Steadman Aldis in NATURE (vol. xxvii. p. 338) yesterday, I was reminded by a person present that some years ago, when in Orkney, I pointed out an appearance that most people unaccustomed to witness it might have taken for a great sea-monster. This was no hing more or less than some hundreds of cormorants or "scarps" flying in a continuous line close to the water, the deception being increased by the resemblance of a head caused by several "scarps" in a cluster *heading* the column, and by the "*lumpy*" seas of a swift tideway frequently intervening and hiding for an instant part of the black lines, causing the observer to—not unnaturally—imagine that the portions so hidden had gone under water. The speed of the cormorant on the wing may be fairly estimated at thirty miles an hour or more. J. RAE

Kensington, February 10

[1] It may be questioned how the bear could find a lump of detached ice. The strong current mentioned is constantly breaking up the ice into small pieces.

The "Zoological Record"

I SHOULD like to point out a slight error in the last impression of NATURE (p. 311). In your notice of the *Zoological Record*, 1881, it is stated that no separate paper seems to have appeared in 1881 exclusively devoted to the group *Octactiniæ*. I should mention that Prof. Nicholson's book on "Monticulipora," his paper on the skeleton of "Tubipora," and Mr. Wilson's paper on the development of "Renilla," all appeared in 1881, and were duly recorded by me. SYDNEY J. HICKSON

Anatomical Department, Museum, Oxford, February 5

SIEVE-TUBES

A CAREFUL examination by E. Russow (*Ann. Sci. Nat.* xiv. 1882, Nos. 3 and 4) of the structure and development of sieve-tubes leads him to the following general conclusions.

In all vascular plants examined, the sieve-tubes exhibit a remarkable agreement in structure, always expressed by the presence of callus. The sieve-punctation appears to be wanting in *Isoetes*, and possibly also in the Marattiaceæ. It is not, when present, confined to the sieve-tubes, but occurs also in the parenchyma of the secondary liber. It is often difficult to decide whether these punctations are actually perforated; but this is clearly the case wherever the sieve is traversed by callose cushions or striæ, or by connecting filaments; the presence of callus is not of itself sufficient to indicate perforation, for its formation certainly precedes the perforation of the membrane. In conifers the punctations between the sieve-tubes and the cells of the medullary rays are provided with callose cushions only on the side of the sieve-tubes, and the punctations remain closed.

The development, accumulation, and final disappearance of the callus indicate that it is not a product of transformation of the cellulose, but that it is separated from the contents of the sieve-tubes; its accumulation round the perforations is proportionate to the freedom and duration of the intercommunication that takes place through them; this communication probably continues as long as the striæ of the callus remain clearly developed, and ceases when these disappear, close up the sieve-pores, and end the function of the sieve-tubes.

In gymnosperms and vascular cryptogams, mucilaginous filaments are never to be seen traversing the callose cushions, although there is always a certain amount of communication between the elements of the sieve-tubes. The special function of the sieve-tubes is probably always maintained wherever striæ cross the callose cushions. The large number of plants in which the sieves are traversed, both in summer and winter, by mucilaginous filaments, and the large number in which no such filaments are at any time observable, contradicts the idea that the function of the callus is to close the sieve-pores during the dormant season.

Much less callus is deposited in the sieve-tubes of closed fibrovascular bundles, especially in permanent organs, than in those of open bundles which increase in thickness from the activity of their cambium. This difference corresponds to a difference in the nature of the contents, and in the duration of the activity of the sieve-tubes. While in gymnosperms and dicotyledons the active period of the sieve-tubes rarely exceeds two years, in monocotyledons and vascular cryptogams it lasts as long as the organ itself. A stem of *Alsophila*, at least

twenty years old, had all the sieve-tubes at its base still in a state of full activity. In a stem of *Yucca aloifolia*, about fifteen years old, the sieve-tubes of all the fibro-vascular bundles, even the innermost, were active, and had their sieves covered with callus; but this was no thicker in the oldest than in the youngest tubes. In a stem of *Dracæna draco*, at least twenty years old, the callus had nearly or entirely disappeared from many of the sieve-tubes; but the plant was otherwise in bad health.

The callus is not a reserve-substance; for in gymnosperms and dicotyledons it often remains unchanged for years in the dead sieve-tubes, and even in leaves which have fallen in the autumn, and in aërial branches which die in the winter. It behaves rather like a secretory product; and this view is confirmed by the study of its development. The organised structure which the callus sometimes exhibits is not a sufficient objection to this view.

Under which class of organic compounds the callus should be placed cannot at present be determined with certainty. Its behaviour to iodine-reagents and to aniline blue appears to indicate an alliance with proteinaceous substances, and especially with nuclein; in this respect it differs altogether from the solid carbohydrates, such as cellulose and starch.

All sieve-tubes resemble one another in their contents, at least as far as relates to the parietal protoplasm and water. The mucilage, which is undoubtedly a non-granular protoplasm, only exists in large quantities in dicotyledons; no mucilaginous threads can be detected in monocotyledons or vascular cryptogams; in some monocotyledons there is simply an accumulation of mucilage in the sieve-tubes. The sieve-tubes of these two classes contain, on the other hand, a large quantity of smaller or larger refringent globules, which are also proteinaceous. Similar globules have been observed in the closed vascular bundles of *Hippuris vulgaris*.

Although starch is almost always present in the sieve-tubes of open vascular bundles, it is seldom to be met with in those of closed bundles. The diameter of the starch-grains is always greater than that of the canals which are clothed with callus, which renders it impossible for them to pass from cell to cell as long as the sieve-tubes are in an active state. The reddish-violet or brick-red colour which these starch-grains take with iodine reagents indicates the presence of a diastase among the contents of the sieve-tubes.

A series of observations on the same organs by E. Janczewski (*Ann. Sci. Nat.* xiv. 1882, Parts 1 and 2) was directed mainly to a comparison of their structure in the different primary groups of the vegetable kingdom.

In vascular cryptogams the elements of the sieve-tubes are not much larger than those of the parenchymatous tissue. They have no nucleus, and contain proteinaceous globules, adhering to the parietal protoplasm, and collected below the pores. Both the lateral and terminal walls have a larger or smaller number of pores. The membrane of these pores is never perforated, and prevents the intercommunication of the contents of adjoining elements; it is sometimes (as in *Pteris aquilina*) pierced by callose cylinders. The time of year exercises no influence on the sieve-tubes, which remain in the same condition through the whole of their existence.

In gymnosperms the life of the sieve-tubes may be divided into two periods, *evolutive* and *passive*. During the first period the pores in the walls of the young tube produce callose substance, and are transformed into sieves covered and closed by the callus; the elements of the tubes contain, at this period, parietal protoplasm. During the second period the tubes entirely lose their protoplasm, and become inert; but at its very commencement the sieves also lose their callus, and free communication is established between adjacent elements.

In dicotyledons the structure of the tubes is still more complicated; their life may be divided into four periods: *evolutive*, *active*, *transitional*, and *passive*. During the first period the cambial cell is not transformed immediately into an element of the tube, as in gymnosperms; it divides longitudinally, and produces on one side an element of the tubes, on the other side one or two cells of the liber-parenchyma. In the elements thus separated, the pores of the walls, or the entire horizontal septa, become covered with callus, and perforated into true sieves composed of a delicate network of cellulose and a callose envelope. The tubes now enter the second or active period, characterised by the sieve-structure and the free intercommunication of the protoplasmic contents of adjacent elements. It may last for months or years. In some cases the sieves are closed before winter by a fresh formation of callus, and open again in the spring. During this period the tubes contain protoplasm, a larger or smaller quantity of a mucilaginous proteinaceous substance, and sometimes starch. During the transitional period the tubes gradually lose their contents; the sieves are closed by callus, and reopen again by the complete absorption of the callose substance. They have now entered the passive period; they are completely inert, and contain no organic matter; the sieves are reduced to a delicate network of cellulose.

The development and behaviour of the sieve-tubes of monocotyledons resemble that of dicotyledons, and their life may be divided into the same four periods. But from the fact of the vascular bundles being closed, and having no cambial zone capable of forming fresh tubes, the active period of the tubes may last as long as the life of the organ which contains them requires it. The passive period is, in fact, rarely manifested. In our climate the sieve-tubes have the power of closing their sieves in autumn, and reopening them in spring. The elements of the tubes contain no starch or mucilaginous substance; and their parietal protoplasm only contains proteinaceous particles which seem to disappear in the spring, and to add to the density and refrangibility of the protoplasm.

CASSELL'S NATURAL HISTORY[1]

WITH the sixth volume, this well-illustrated account of the natural history of the animal kingdom is brought to a close, and the six handsome volumes leave nothing to be desired, so far as good covers inclosing excellent paper and beautiful typography are concerned. Indeed, the general get-up of the series is quite unexceptional, and as to the average value of the scientific contents we feel fully justified, on the strength of such contributors as Parker, Sharpe, Carpenter, Dallas, Solias, &c., in strongly recommending the series to the majority of our readers.

From a purely scientific point of view, we regret the title selected by the Editor. He should not have launched so important a book in these days upon the sea of science under an obviously wrong title. The "Historia naturalis" embraces, as the Professor of Geology in King's College, London, well knows, something more than an account of the members of one of nature's kingdoms, and of their distribution in space and time. It is therefore certainly not scientific, and we take it as against modern culture to adhere to such a style. If, indeed, the eminent firm of publishers were to extend this natural history so that in another half-dozen volumes we should have an account of the equally interesting, and even more important vegetable kingdom, the title of the series would the more approach exactness.

Although in the title of his work the Editor has followed in the footsteps of the mere compiler, he has by no means

[1] "Cassell's Natural History." Edited by P. Martin Duncan, M.D. Lond., F.R.S., Professor of Geology, King's College, London. Volumes 1 to 6, illustrated. Volume 6. (London, Paris, and New York: Cassell, Petter, Galpin, and Co., 1883.)

followed this example in the direction of writing on all the groups of the animal kingdom with his own pen, but has been fortunate in getting together a number of contributors, whose very names command respect for their contributions. The table of contents of the just published volume shows that the subjects of the Insects, Myriopods, and Arachnids have been written by Mr. W. S. Dallas, with the exception of the Lepidopterous Insects written about by Mr. W. F. Kirby, the Crustacea are described by Mr. Henry Woodward, the Echinoderms by Mr. Herbert Carpenter, the Sponges by Prof. Sollas, the Rhizopods by Prof. Rupert Jones, and the Worms, Zoophytes, and Infusoria by the Editor.

We have been greatly struck by the immense amount of information given to us by Mr. Kirby in Chapter IX., which treats of the characteristics of the order of Lepidoptera, gives an account of the evolution of these insects from the egg to the perfect state; describes the imago condition; gives a condensed but very clear account of their anatomy, food, and geographical distribution, and concludes with a few hints on collecting, killing, and setting. Among the statistics of lepidopterous life, we note that the present census gives about 10,000 species of butterflies, and 40,000 moths; but then Mr. Kirby adds: "Hundreds of new species are being added to our lists every year." The abundance of species in a district would seem to be in proportion to the variety of the vegetation, which latter is intimately connected with variety of elevation, and so it is "that Lepidoptera are far more numerous in Switzerland than in the peninsulas of Italy and Spain:" but is it not possible that the mountainous regions of Spain will still yield many as yet unknown forms? The illustrations in this portion of the volume are often very beautiful, and comparatively new. Of the next order, Diptera, says Mr. Dallas, "it is not easy to arrive at any trustworthy estimate of the total number of species, yet allowing Dr. Schiner's estimate of 9000 species as European, it has been calculated that the total fly population of the world would be from 150,000 to 160,000. Only a very few of this great army could be of course alluded to, but the information given about the gnats, midgets and crane flies is very full and interesting. Many of these forms are injurious to our crops, as well as irritating to ourselves. The Gall Midges (Cecidomyidæ) are among the most delicate species of all these gnat-like Diptera. The larvæ of these elegant little insects feed



FIG. 1.—Cecidomyid with viviparous larva. A, adult insect; B, B, pupæ; C, larva, showing young larvæ at *a a*.

upon various species of plants. The number of species is very considerable, about 100 being recorded as European. Many of them by attacking useful plants, frequently do much mischief. Among these may be mentioned the Hessian Fly (*Cecidomyia destructor*), which has done so much damage to the grain crops of the United States, and which received its name from a belief that it was introduced into the States with the baggage brought by the Hessian troops in the pay of the British Government about the year 1776. The Wheat Midge (*C. tritici*) is an enemy of the wheat crops in this country, sometimes doing much damage; several other species form the flower-like galls oftentimes found on willows.

In 1860 Dr. Nicolas Wagner, of Kasan, made the startling discovery that in certain of these Cecidomyids the larval stages could give rise by a kind of budding, to several small larval-like forms, and that when these latter got free, they in their turn produced still other larval forms in the same curious fashion, and so one generation succeeds another throughout the autumn, winter, and spring. In the summer the last generation undergoes a change to the pupa state, and from these pupæ the perfect winged males and females emerge. The latter lay eggs in the bark of trees, and the larvæ produced from these commence once more a fresh series of organic broods. This strange circle of development is in part represented in the accompanying illustration, which will serve as a fair example of those which abound in this volume. All the families of the flies, ending with that of the flea, which, however, is placed in an order by itself, are well and judiciously treated.

The chapter on the Rynchota is also, despite its subject, a very interesting one, and a great deal of useful information is crowded into a small space. Mentioning the noise produced by the male Cicadæ, the author says: "During the heat of the day they sit concealed amongst the foliage of the trees and shrubs, and sing incessantly;" but is it not rather their wont to select the end of some dead twig, or the extremity of some vine pole, and there out in the full glare vibrate violently. A little space might have been spared for an account or figure of the vine phylloxera.

The chapter on the Orthoptera begins with the crickets and ends with the springtails. Among the Myriopods

we find the orders Pauropoda and Onychophora, the latter for Lansdowne Guilding genus Peripatus, which, by the way, he referred in his original description to the mollusca, and not, as here stated, to the worms.

The chapter on the Arachnida includes the Scorpions and their allies the Spiders, Mites, Tardigrades, and Pantopods; in the sketch of the latter a half page might usefully have been devoted to recent researches on the distribution of this extraordinary group of marine forms in the depths of the sea.

The class of Crustacea is well illustrated, and in the

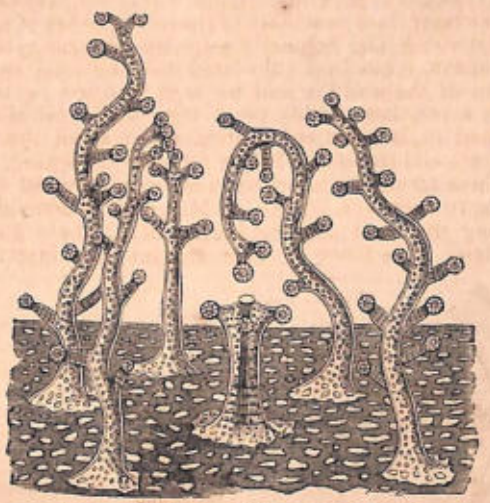


FIG. 2.—Millepora, showing expanded zooids.

introductory chapter we have an excellent account of the general anatomy and strange development of the group. Even Fritz Müller's account of the metamorphosis of Penæus is given, with figures of the Nauplius, Zoea, and Mysis stages. The typographical arrangements of the headings of the orders of the Crustacea seem faulty. The Editor's eye has failed him here, and though the sense is in no ways altered by the want of uniformity in the type used for the headings Brachyura (p. 197), Anomoura (p. 202), Macroura (204), yet there is a utility in the case of a classification of appealing to the eye. The

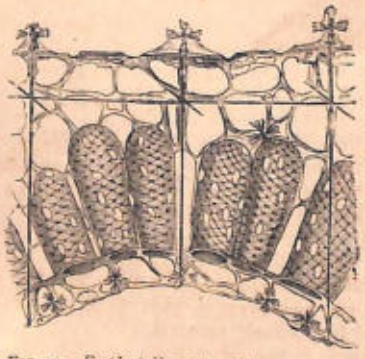


FIG. 3.—*Euplectella aspergillum*, structure.

King Crabs are placed as usual among the Crustacea, but the joint authors (Messrs. Dallas and Woodward), in their concluding remarks on the Arthropods write: "the structural relations of these to the scorpions would seem to be very close, and certainly raise a difficult problem, one which is rendered still more interesting by the fact that, according to the researches of Dr. Jules Barrois, a Limuloid, or King Crab-like stage occurs in the development within the egg of certain true spiders. For the present, this and many other such questions must, however, remain open. In all biological problems relating to the past developmental history of the organic world, we must for a long time yet expect to come continually upon obscure and puzzling points which only a more extended knowledge of minute details can clear up."

The various classes of the "grand division" of the worms are treated rather unevenly. This grand division is, no doubt, a somewhat heterogeneous one. "Thus it is found that an animal does not exactly correspond with one of the articulate groups, and another resembles in certain points, but not in all, an Infusorian. They are then placed with the Vermes [worms] because of the existence of certain fundamental structures." There is a good deal of minute anatomical detail given about the Leeches and Rotifers, while the Land Planarians are dismissed with the following:—"They have eyes, no tentacles, a proboscis, and a narrow body. They are found in the United Kingdom and generally in Western

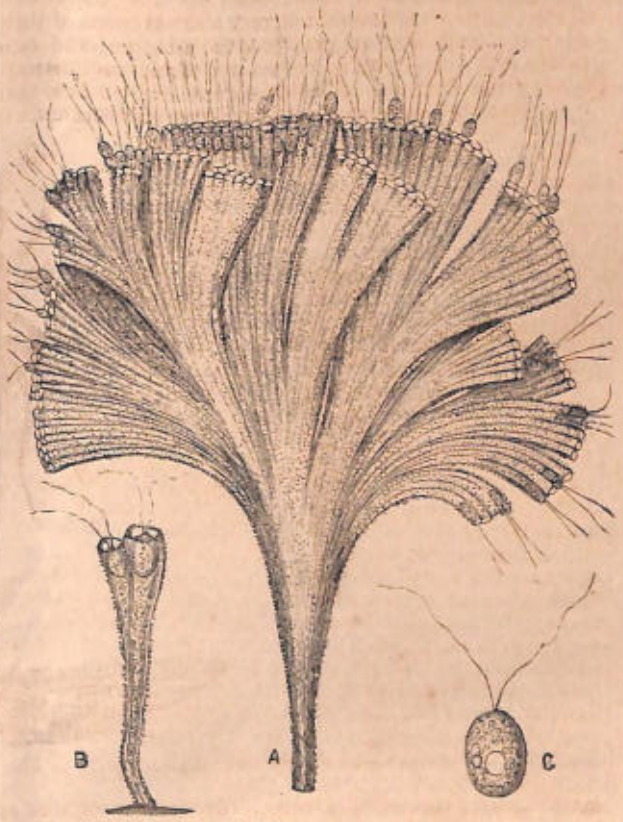


FIG. 4.—*Rhipidodendron splendidum*. A, colony; B, two monads; C, free monad.

and Central Europe. They have been found in America and on continental as well as on oceanic islands. Moseley says that they are nocturnal in their habits, when in the light getting under leaves. Some contain chlorophyll and seek the light, but die in the sunshine. They eat small snails, worms, and flies. An American kind secretes a mucous thread, and suspends itself in the water, and another lets itself down from the leaves by one."

If we were introduced to the Worms as a Grand Division, we are told that the Echinoderms form a Sub-Kingdom of the Animal Kingdom, but there is nothing to guide us to this in the heading of the portion of this volume in which Mr. Herbert Carpenter so well, though succinctly describes this important group, an account that we would have wished to have been much more detailed. The classes of this group getting about three pages of text to each, and several of the pages are devoted to new and excellent woodcuts.

The group of the Zoophyta embraces the Hydrozoa and

Actinozoa. These "are distinct from the Spongida, although some synthetic-minded morphologists classify all together as Cœlenterata." In treating of the freshwater Hydra we notice the "old story" repeated that "if the body be turned inside out, the old ectoderm [why the adjective?] takes on the digestive power and the former endoderm that [takes on the function] of the skin."

The order Hydrocorallina is placed as the last of the Hydrozoa, with the families Milleporidæ and Stylasteridæ, as indicated by Moseley, to whose researches and those of Agassiz we are indebted for all we know about the order. *Millepora alcicornis* was obtained by Moseley at Bermuda. The calcareous tissue of the coral is very hard and compact, and the polyps are extremely small. It is very difficult to prevail on the polyps to protrude themselves from their cells, but Mr. J. Murray, of the *Challenger* Expedition, succeeded in procuring them in this state on two occasions, and the accompanying drawing (Fig. 2) of one of the expanded polyps, and of five of its surrounding zooids, is from Mr. Moseley's memoir on the structure of this genus. In the centre is seen the short polyp form provided with a mouth and with only four short knobby tentacles, while grouped around are the five polyps without mouths, and for the sake of letting the central zooid be clearly seen a sixth mouthless zooid is omitted from the sketch; these latter zooids have from five to twenty tentacles; they are much more active than the mouth-bearer, and do the work of catching food for it. When alarmed all disappear within the framework.

The article on the group of the Sponges is excellent. The author now regards the sponges as forming a separate class independent of the Cœlenterata, and situated at the very bottom of the Metazoic sub-kingdom, and gives a brief sketch of the orders and sub-orders.

The figures of sponge structure are refreshingly new, many of them being from quite recent sources—such as the memoirs of Haeckel, Schulze, and Prof. Sollas himself. The beautiful sponge belonging to the genus Euplectella, now known to live anchored in the mud in deep seas, or attached to the hard bottoms of shallower waters, has had its structure ably described by Prof. Schulze, from whose memoir the annexed woodcut (Fig. 3) is taken. The membranous wall—very delicate and thin—which surrounds the skeleton is furnished with smooth-edged roundish *pores* of different sizes, irregularly arranged, and varying very much in number. These form an open communication between the cavities of the chambers and the duct-like spaces surrounding them, which penetrate everywhere between the ciliated chambers and extend even to their mouths, where they terminate on a tougher membrane, which binds together and connects laterally the chamber walls. The figure shows the outer portion of a thin section taken perpendicularly to the outer surface through the side wall of a ridge, and is magnified × 150. Several of the ciliated chambers are seen.

Although the Rhizopods are described as standing "first in the scale of animal organisation," we find them treated of in a chapter before that relating to the Infusoria, and we are told in the same paragraph that "they have in a great degree the same simple constitution as several other kinds of animalcules which are grouped by naturalists as Protozoa." We venture to think that such a description will be apt to lead the general reader astray; nor was it quite fair of the Editor to allow the writer of the article on the Rhizopods to go somewhat out of his way in his forty-ninth paragraph to give a view of the organisation of the Sponges which will be apt to puzzle the reader who has perused the more accurate account of the sponge structure given by Prof. Sollas.

As an example of the beautiful illustrations of the Infusoria, which are for the most part taken from Saville Kent's excellently illustrated Manual of Infusoria, we give the woodcut of *Rhipidodendron splendidum*. There are few workers with the microscope who devote themselves to the study of the Infusoria but must be familiar with the stems of that group of animalcules, which gravitate about the well known *Anthophysa vegetans* of Müller; the attached colony stocks putting one in mind of some minute fucoid stem. Of this group the species figured after Stein is one of the most remarkable, originally described and most beautifully illustrated in Prof. Stein's great work. This freshwater form has apparently not yet been found in this country, but a nearly allied species, *R. Huxleyi*, has been met with in South Devon. The figure shows the compound colony stock at A, the quite young colony stock at B, which latter was built up by a single monad, which divided by longitudinal fission, producing two parallel, or nearly so, tubes, and one of these monads is seen at C free, without a tube.

In congratulating the Editor on the successful termination of his labours, we are not unmindful of the difficulties he has had to encounter in trying to secure a more or less uniform style of treatment of subjects so varied as the different classes and sub-divisions of the animal kingdom.

THE CONDENSATION OF LIQUID FILMS ON WETTED SOLIDS

IN *Poggendorff's Annalen* for 1877, and in the *Philosophical Magazine* for 1880, I have recorded some facts which are satisfactorily explicable only on the supposition that the liquid in contact with the glass undergoes condensation upon the surface of the latter. In the latter paper I was able to show that this condensed film visibly altered the resistance experienced by the liquid in flowing through the tube. In the paper in the *Poggendorff's Annalen* it was shown that a difference of potential was set up between the two ends of a capillary tube through which water was forced, and that the effect of leaving the water in contact with the tube was that this difference of potential rapidly diminished. No doubt this finds its explanation in the effect of the condensation of the liquid on the sides of the capillary tube, causing the friction of the water against the tube to become less and less, whilst the friction of the water upon the condensed water-film becomes progressively greater, as the latter adheres more strongly to the glass. Probably simple drying would suffice to restore to the tube the originally observed difference of potential between its ends.

Whilst working upon this subject I noticed the large E.M.F. produced by a small air-bubble slowly ascending through the vertical capillary tube which was full of water (see Dr. Dorn, *Ann. d. Phys. u. Chem.* 1880, S. 73). At the time I could not account for this, but not long ago I constructed an apparatus which allowed of alternate drops of water and bubbles of air being driven through the capillary tube. This produced a very large E.M.F. Probably this increase in the E.M.F. is dependent upon (α) the increased electrical resistance consequent upon breaking up the water in the tube into drops separated by air-bubbles, and (β) upon an increased disturbance of the liquid film adhering to the glass. Experimentally these effects, (α) and (β), might be separated by substituting for water a (practically) perfectly insulating liquid.

Another and very interesting illustration of a liquid condensed on the surface of a solid is probably to be seen in the familiar fact that water will not clean a greasy sheet of glass.

As is well known to all workers on surface tension, almost the only way of getting a *physically* clean surface of glass is by heating the glass in concentrated sulphuric acid, to which a little nitric acid has been added, and then heating, after washing in pure water to remove the acid. Such a glass surface exposed to the air for a short time is generally imperfectly

wetted by water. This no doubt arises, partly at least, from the condensation of *gases* on the surface, which Quincke has shown will produce this effect to a remarkable extent under certain conditions described by him. To this, also, Barrett and Stoney have referred certain modifications of Leidenfrost's phenomena; and the floating shells, &c., of Hennessey are due to the same general cause. But a very minute trace of oil on a physically clean surface produces the familiar greasy surface. Why is this? Oil is not insoluble in water, and when the quantity of water used is sufficient to dissolve the quantity of oil placed on the glass, it ought to wash off. Every one knows, however, how difficult it is to wash oil off glass. *Is this then due to a diminution in the solubility of the oil in the water owing to its* CONDENSATION *on the glass surface?* I believe it to be very probable that this is the case, and think that the experimental proof would be possible by placing estimated quantities of oil on a physically clean glass surface, and subsequently washing in quantities of water, such as under ordinary circumstances would readily suffice to dissolve it. By dissolving the oil in a volatile medium, its quantity might be readily estimated. No doubt other liquids of somewhat greater and better known solubility might be advantageously substituted for the oil, and perhaps, as Dr. Japp has suggested to me, by employing a coloured liquid the result might be rendered evident to the eye.

My inability to complete these experiments at the present time, and the great interest attaching to a determination as to whether the condensation experienced by the liquid-film alters the physical or chemical properties of the liquid must be the excuse for the publication of incomplete results, which I much hope may be taken up by others.

J. W. CLARK

THE STOCKHOLM ETHNOGRAPHICAL EXHIBITION[1]

DR. STOLPE was asked to arrange and describe the Ethnographical Exhibition of Stockholm in the year 1878. This exhibition was brought together from all, or at least nearly all, Swedish public and private collections; no less than 217 exhibitors with about 10,000 objects participated, the King himself opened the galleries, and general interest was raised by an ethnographical exhibition as indeed no other country has realised till now. We took occasion to visit the exhibition, and were astonished to see so rich a material, as well as a thoroughly scientific arrangement.

Both works named below are a result of the meritorious undertaking. The second was partly a guide through some parts of the exhibition, especially China and Japan, with a general introduction, and many valuable and interesting special remarks, partly, in its second volume, a determination of all, about 6200 numbers of the exhibition, arranged after the exhibitors. The first-named work illustrates in geographical order the more important objects of the exhibition, partly in groups, but chiefly in single representations. There may be represented in all about 1500 objects, and we hear that a fourth supplementary volume is in the press.

The first volume of this album contains, on 84 plates, Australia, Oceania, Malaysia, Madagascar, Malayo-Chinese, and Tibet: the second, on 116 plates, China, Japan, Samoyedes, and Turks; the third, on 78 plates, America, Africa, Circassia, Persia, and India. Japan and China, as well as Oceania, are relatively best represented; among the last-named division figures the fine collection from the Savage Islands, which the expedition of the *Eugénie* brought home in the year 1853.

[1] H. Stolpe, "Exposition ethnographique de Stockholm, 1878–1879." Photographies par L. F. Lindberg. 3 vols. 4to. 36 pp. 278 plates. (Stockholm, 1881).—"Den allmänna etnografiska utställningen, 1878–1879" (The General Ethnographical Exhibition). 2 vols. 8vo. 80 pp. 1878-1879, and viii. 112 pp. 1880.

This photographic album must be regarded as the best existing ethnographical atlas; it gives, notwithstanding the inequality in the representation of the single countries, a good idea of a high-class ethnographical museum. The editor has had a full appreciation of the problem which was to be solved, and no ethnologist who works scientifically can do well without this album. It was therefore right that the International Geographical Congress of Venice in the year 1881 should bestow a prize on this beautiful work. Copies of the album are, we believe, only printed to order, and may be obtained direct from Herr Lindberg, R. Archæological Museum, Stockholm.

A. B. MEYER

BARON MIKLOUHO-MACLAY

LETTERS have been received from Baron N. de Miklouho-Maclay from the Suez Canal, the distinguished traveller being now on his way back to Australia. During his prolonged and arduous experience of eleven years' life amongst Melanesian and other savages of the Pacific his health has, we are sorry to say, suffered very seriously, and he returns to Sydney mainly on this account, since he finds that the climate of New South Wales suits him best. He intends to call at Batavia on the way out, where he left a part of his collections in 1878, in order to convey these to Sydney, where the main bulk of the gatherings of his many journeys is already stored. The Emperor of Russia, with enlightened liberality, has promised to defray the cost of the publication of the scientific account of Baron de Maclay's results, and the collections have been brought together at Sydney in order that they may be available for the preparation of the work for the press there.

Baron de Maclay hopes to be able to get ready the whole of his numerous diaries, notes, and papers for publication in about two years' time. The complete work to be issued by him will, if his present plan be carried out, consist of an anthropological and ethnographical section, a section treating of comparative anatomy, and a general narrative of his travels, together with appendices containing meteorological observations and information on physical geography.

The work will be published first in Russian, but translations in other languages will probably soon follow.

He intends to do a good deal of the anatomical work needed to complete his researches on animals collected by him in Australia and New Guinea at the Zoological Station at Watson's Bay, of which he is the founder. This Zoological Laboratory at the very first received most important support from the Linnean Society of New South Wales, and by the influence of this Society a grant of land was obtained from the New South Wales Government for the erection of the building. Scientific men in other colonies, and notably in Victoria, recognising the great importance of the establishment to the progress of biological research, have come forward nobly to support the enterprise, and the Australian Biological Association has been formed, a Society including men of science of all the Australian colonies and some distinguished European naturalists, the object being to support biological stations in Australia. It is very gratifying to find so enlightened a sympathy with scientific progress developed, and that the different colonies are able to work together in so excellent a cause. We hope to refer shortly again to the constitution and aims of the Australian Biological Association.

NOTES

WE can only for the present express the deep regret with which we learn of the death, on the 9th inst., of Prof. H. J. S. Smith, of the Savilian Chair of Geometry at Oxford, at the

comparatively early age of fifty-six years. We hope next week to refer to his career and work in detail.

THE death is announced at Basle of Prof. Peter Merian, the Nestor of Swiss geologists. He was born on December 20, 1795. His first important work, on the Jura of the Canton of Basle, was published in 1821, followed a few years later by a geological account of the Southern Schwarzwald. In 1821 he was appointed Professor of Physics and Chemistry in his native University, and at a later period he accepted the Chair of Mineralogy and Geology, which he held for half a century. He was more than once chosen rector of his University, and throughout his life not only continued his geological activity, but took an active interest in scientific work of all kinds as well as in public affairs.

WE learn that Dr. Oscar Dickson arrived in Christiana on the 9th inst. to confer with King Oscar, who is sojourning there, as to an Arctic expedition to be despatched this year, under the command of Baron Nordenskjöld, to North Greenland.

M. JANSSEN, as leader of the French Eclipse Expedition, will embark on March 6 for Panama. He will cross the isthmus by rail, and the *Éclaireur* will be ready at Colon to take him to Sable Island, near Caroline Island in the Marquesas group.

MR. DEANS COWAN, well known for his explorations in Madagascar (see *Proc. R. Geogr. Soc.*, vol. iv. p. 521), has now fully settled to return there, in order to explore the southern part of the island. Of this district little is known, and it may be fully expected that Mr. Cowan will get valuable additions to our knowledge of the natural history of the country. Mr. Cowan calculates that his journey will occupy about two years. His plan is to begin at Ambahy on the south-east coast, and to proceed inland to the most southern point reached when he made his survey of the Bara-land, working southward amongst the Tausay and Taudroy people, thence westward towards the district of the Mahafaly tribe, and on to the River Onylahy. This will occupy one year. From the Onylahy the route will be nearly north through western Bara-land and the Sakalava country, ending at Mojanga. As these journeys will be amongst the aborigines, and even in different geological formations to that from which nearly all our Madagascar specimens are obtained, Mr. Cowan expects that the results will be of a most valuable character, and help to a solution of many interesting questions in regard to Madagascar.

COLONEL ÉMILE GAUTIER has been appointed Director of the Observatory of Geneva, in succession to the late Prof. E. Plantamour, who had filled the position from 1839.

ON Tuesday, February 6, a large number of scientific men and social and political notabilities assembled at the Northern Station, Paris, to witness the transmission of energy by an iron wire used like an ordinary telegraphic line, and extending to Sevran, near Le Bourget, and returning to the station, thus completing a distance of 20 kilometres. The primary engine was moved by a force of about 5 horse-power, and the force of the secondary was said to be 2½. No precise measure was taken. The experiment was in continuation of the much spoken of Munich transmission of energy from Mierbach, according to the Marcel-Deprez system. It is not believed that this new experiment will put an end to the controversy. Many papers have reported enthusiastically on the proceedings, and letters have been written by electricians claiming to have executed more successful trials. It is stated that new experiments on the Marcel-Deprez system of transmission of energy will take place under the superintendence of M. Tresca, with the same machines as on February 6. The *Lumière Électrique* states that the percentage of force is about 37½ per cent., and that the dynamometer for measuring the motive power of the primary machine was not in order at the time of the first experiment.

PROF. FLOWER will commence, at the Royal College of Surgeons, on Monday, the 26th inst., a course of nine lectures upon the Anatomy of the Horse and its allies. In the first three lectures the general position of the horse in the animal kingdom, and its relations to other existing and extinct species will be treated of; the remainder of the course being devoted to a more detailed account of the osseous, dental, muscular, nervous, and other systems, as compared with those of the generalised Mammalian type, the allied forms of Ungulates, and Man. The lectures will be given on Mondays, Wednesdays, and Fridays, and are free to all who take an interest in the subject.

IT seems that the season of 1882 has, on account of the state of the ice in the Arctic seas, undoubtedly been one of the most adverse on record. Thus while the Norwegian walrus and white fish hunters were unable to get to the north of Spitzbergen and the Swedish Meteorological Expedition to Mossel Bay, no vessel succeeded in reaching the Siberian rivers. It appears from information just to hand that the summer along the coast of Siberia has been unusually cold, while incessant north-east winds have accumulated drift-ice on the shores to such an extent that the estuaries of the Yenissei and the Obi were not once navigable in the season. Thus the small steamer *Dallmann*, of Yenisseisk, belonging to Baron Knop, was quite unable to get from the Yenissei into the Obi, and all she accomplished during the year was to transfer a few thousand poods of grain from Mr. Sibiriakoff's depôt, where it had been lying for some years, to Baron Knop's, to be eventually forwarded to Europe. When it is remembered that this was the state of the ice in the eastern and southern parts of the Arctic seas, and we remember the reports of Mr. Leigh Smith and Sir Henry Gore Booth of open water north and east of Novaya Zemblya, it becomes apparent that some other part of the Polar basin must have been very free from ice during the summer. It seems to be the opinion of several authorities, as for instance Baron Nordenskjöld, that any vessel which had attempted to penetrate by way of Behring Strait would, no doubt, have demonstrated the practicability of navigating the Siberian seas every summer from one end or the other. This year fresh attempts will be made by Mr. Sibiriakoff, Baron Knop, and Dr. Oscar Dickson to open up a trade route with Siberia from Europe; those however acquainted with the Arctic seasons would not be surprised to see the ice in the summer of 1883 as adverse to Arctic voyaging as it was in 1882.

THE first news has been received at St. Petersburg from the Russian Lena Expedition. Lieut. Harder, who was searching for the remains of the victims of the *Jeannette* disaster, met Dr. Bunge and Jürgens on October 3. He found the members of the Lena Expedition in excellent health, and already comfortably settled in their winter quarters.

M. WOLF, chief of the Physical Department in the Paris Observatory, delivered last Saturday evening a lecture at the Sorbonne, on the Methods employed in Astronomical Physics, before a very large and enthusiastic audience. M. Wolf insisted upon the three methods employed by astronomers, viz. ocular inspection with telescopes, spectroscopic analysis, and photography. He dwelt upon the difficulties of vision with instruments possessed of a great magnifying power, and he tried to oppose the popular delusion that any description of celestial phenomena could be photographed with advantage. He explained that this method should be almost exclusively confined to the sun and moon. The lecture was illustrated by many experiments and projections.

AN important advance is in course of realisation in the use of telegraphy for French newspapers. The *Reforme* has hired a

direct cable from London to Paris. The instruments are in the London and in the Paris office of the paper, so that the transmission is instantaneous. According to circumstances, the *Reforme* telegraphists use the Calais, Boulogne, or Dieppe cables. None of these gives a sensible retardation through crossing the sea; but it is remarked that, contrary to expectation, the Dieppe cable is the best of the three. The transmission is made with an ordinary Hughes apparatus.

THE following are the subjects of the lectures to be given at the Royal Institution by Prof. Robert S. Ball, the Royal Astronomer of Ireland, on the Supreme Discoveries in Astronomy:—"The Scale on which the Universe is Built," "The Sun no more than a Star, the Stars no less than Suns," "The Law of Gravitation," and "The Astronomical Significance of Heat." The first lecture will be given on Tuesday, February 20.

PROF. JOHNSTRUP, Rector of the University of Copenhagen, in a paper on "The Glacial Phenomena manifested in Denmark," has shown that the Cyprina-mud deposits overlying the gravel in many parts of the Danish territories afford evidence that an interval of lesser cold must have followed the great glacial period. He moreover regards the presence of the shells of *Cyprina islandia*, and other boreal forms of similar habit, as a proof that the climate in this intermediate period must have been similar to that of the North Sea and the Cattegate in the present day. His views of the connection between these Cyprina deposits and the varied manifestations of glacial action are based on the hypothesis that the ice, which advanced from the interior of Scandinavia and covered Denmark and Northern Germany, must have been driven back, and that on its disappearance, the Cyprina mud was deposited in horizontal layers. On the recurrence of another glacial period these deposits were crushed, dislocated, and often thrust into a vertical position by drifting ice-fields, which had ploughed and broken up the sea-bottom in their advance. This view is in opposition to the opinion more generally held by geologists, that Denmark was twice completely buried under one connected ice-pall, which owed its origin to the continental ice of the Scandinavian peninsula. The direction of the striations and scouring lines in the island of Bornholm, and in some parts of Iceland, which are now being carefully investigated, are, however, admitted to be favourable to the views advanced by Prof. Johnstrup.

MR. GEORGE STALLARD, B.A., of Keble College, Oxford, at present Science Master of St. Paul's School, has been appointed Science Master at Rugby, in place of the Rev. C. M. Hutchinson.

THE Council of the Meteorological Society have determined upon holding at the Institution of Civil Engineers, 25, Great George Street, S.W., on the evening of March 21 next, an Exhibition of Meteorological Instruments which have been designed for, or used by, travellers and explorers. The Exhibition Committee invite co-operation, as they are anxious to obtain as large a collection as possible of such instruments. The Committee will also be glad to show any new meteorological apparatus invented or first constructed since last March, as well as photographs and drawings possessing meteorological interest.

WITH reference to the recent explosion of a zinc-plate oxygen gasometer, described by Herr Pfaundler in *Wiedemann's Annalen*, Dr. Loewe states (*Wied. Ann.* No. 1) that to protect oxygen or atmospheric air from admixture of carbonic acid or acid vapour from the air of the laboratory, he has for many years placed them over lime-water. Some 20 to 30 gr. freshly slacked lime, in a powdered state, is placed in a strong linen bag, which is tied with cord just above the contents, and hung near the outflow tube of the water vessel of the gasometer. This ensures that all carbonic acid and acid vapours which the water of the gasometer may in time absorb from the air, are neutralised by lime-hydrate, and rendered innocuous. There is the further advantage, for elementary analysis, that the potash or soda lye, which is preferred for washing the gases, remains long quite caustic, and thus serves—as it ought to do—less for purification of the gas than as an indicator of the gas current. After long use the linen bag of lime-powder is renewed.

THE February part of Hartleben's *Geographische Rundschen* contains an interesting account of Potanin's journey through Mongolia in the years 1876–77; also some well-written articles on the Samoa Islands, on Eastern Africa, and the European census of 1881, together with a memoir of Count Hans Wilczek.

SOME time ago we announced the commencement of the publication of an "Elektro-technische Bibliothek," by Hartleben, of Vienna. The second volume of this series has just appeared, entitled "Die elektrische Kraftübertragung und ihre Anwendung in der Praxis," by Eduard Japing.

WE are requested by the Council of the Society of Telegraph Engineers and of Electricians to state that an International Electrical Exhibition will be held in Vienna under the patronage of the Austro-Hungarian Government, in the months of August, September, and October next. At the request of the Austrian Minister of Commerce, and of the Managing Committee of the Exhibition, the Council of the Society have appointed a Committee for the purpose of receiving applications for space from intending British exhibitors, and for promoting generally the formation of a British section. Application should be made as early as possible, and should be addressed to the Secretary of the Society of Telegraph Engineers and of Electricians, 4, The Sanctuary, Westminster, S.W.

THE Municipal Council of Paris is proposing to the administration to organise a medical service for the inspection of the eyes, ears, and teeth of the pupils of the public schools, in order to see how to cure constitutional or chronic diseases by which they may be affected.

A LOCAL committee has been established in Annonay for the erection of a statue to commemorate the invention of the *Montgolfier* balloon.

UPWARDS of one hundred Palæolithic implements from the collection of Mr. Worthington G. Smith have been transferred to the collection of Mr. John Evans at Nash Mills, Hemel Hempstead.

A "SECOND London edition" has been issued by Macmillan and Co. of Prof. Newcomb's "Popular Astronomy." The principal additions relate to Dr. Draper's investigation on the existence of oxygen in the sun; Janssen's conclusions from his solar photographs; Prof. Langley's investigation on the solar spectrum on Mount Whitney, California; the satellites of Mars; the supposed intra-Mercurial planet; preliminary results from the late (1874) transit of Venus, and other recent methods of determining the solar parallax; the transit of Venus on Dec. 6, 1882; the great telescopes completed within the last four years; and recent developments in cometary astronomy. The preface is dated Washington, July, 1882.

A DEPOSIT of remains of mammals from the diluvial period has been laid bare by the waves of the Wolga on the bank of that river between Zarizyn and Sarepta. M. Al. Knobloch, of Sarepta, has made a valuable collection of the bones found, which belonged to *Elephas primigenius*, *Bos priscus*, *Elasmotherium*, *Camelus Knoblochi*, besides several antelopes, stags, &c.

On the evening of January 24 an aurora was observed at Geestemünde, which was remarkable both for its duration as well as for the intensity of its light. The sky was quite clear and the moon shining brightly, when about 7.30 p.m. a semicircle of light appeared in the north-east. Soon afterwards long rays shot out from this across the sky, forming an immense fan of light; the middle one of these rays crossed the sky right down to the south-west, and remained visible in the same brightness for two hours. The size and brightness of the other rays changed constantly. The light was perfectly white.

A violent earthquake is reported from Freiburg-im-Breisgau January 24, at 5.30 a.m., accompanied by loud subterranean noise. At the same time two strong shocks were felt at Bischoffingen. On the same date, at 7.58 a.m., an earthquake was observed in Herzegowina. It lasted for four seconds, and its direction was from north to south.

During the coming summer a Fine Art and Industrial Exhibition will be held at Huddersfield in connection with the opening of the New Technical School.

The additions to the Zoological Society's Gardens during the past week include two Macaque Monkeys (*Macacus cynomolgus*) from India, presented respectively by Mr. T. W. Davidson and Miss M. Sutton; two Common Marmosets (*Hapale jacchus*) from Brazil, presented by Mr. A. Pariss; an Oak Dormouse (*Myoxus dryas*) from Russia, presented by M. A. Wrzesniowski; a Common Marmoset (*Hapale jacchus*) from Brazil, presented by Mrs. Lynch; two Common Gulls (*Larus canus*), British, presented by Mr. W. K. Stanley; two Herring Gulls (*Larus argentatus*), British, presented by Capt. C. R. Suckley; a Brant Goose (*Bernicla brenta*), European, presented by Mr. J. C. Robin on; a Black Lemur (*Lemur macaco*) from Madagascar, four Impeyan Pheasants (*Lophophorus impeyanus* ♂ ♀ ♀ ♀) from the Himalayas, a Black-necked Swan (*Cygnus nigricollis*) from Antarctic America, deposited; two Philantomba Antelopes (*Cephalophus maxwelli*), a Crowned Hawk Eagle (*Spizaetus coronatus*) from West Africa, four Snow Buntings (*Plectrophanes nivalis*), two Brant Geese (*Bernicla brenta*), European, a Red-throated Diver (*Colymbus septentrionalis*), British, purchased; a Schomburgk's Deer (*Cervus schomburgki*), from Siam, received in exchange; two Hybrid Peccaries (between *Dicotyles labiatus* ♂ and *D. tajacu* ♀), five Ring-hals Snakes (*Sepedon hæmachates*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

The Comet of 1771.—The comet discovered by Messier at Paris on April 1, 1771, and last observed by St. Jacques de Silvabelle at Marseilles on July 17, has long been mentioned in our treatises on Astronomy as undoubtedly moving in a hyperbolic orbit. This inference was first drawn by Burckhardt, who considered that of all the comets calculated up to the time he wrote (*Mémoires présentés par Savans étrangers*, 1805) that of 1771 was the only one of which it could be stated with some degree of certainty that the orbit was hyperbolic. Encke considered the case worthy of further investigation; remarking that from the nature of the conditions it might be demonstrated that a comet could not rigorously describe a parabola, and that experience so far rather gave the preference to the ellipse over the hyperbola, he insisted that a comet, whose track could not be represented completely except by hyperbolic motion, merited the greatest attention. He accordingly reduced anew the six observations employed by Burckhardt, and after their careful discussion found that the most probable elements were hyperbolic with eccentricity = 1·00937, which is almost identical with Burckhardt's value (1·00944). Nevertheless he did not regard the decided superiority of the hyperbola in the representation of the six places as an indubitable proof of the necessity of admitting motion in that curve; the positions used were not normal positions, but the results of single and isolated observations, and as such, the errors exhibited by a parabolic orbit had not so great a preponderance in his opinion as to enforce such necessity. He concluded that the subject still required examination by a combination of all the observations, and especially if the originals of those at Marseilles could be found. On this point Zach stated, in a note to Encke's communication (*Correspondance Astronomique*, t. v.), that during a recent visit to Marseilles he had searched in vain amongst the papers of St. Jacques de Silvabelle for these originals.

Lately, the orbit of the comet of 1771 has formed the subject of two memoirs, containing very rigorous discussions of the observations, the first by Mr. W. Beebe, in the *Transactions* of the Connecticut Academy of Arts and Sciences, vol. v.; the second by Dr. H. Kreutz, published in the *Proceedings* of the Vienna Academy. Mr. Beebe gives also a hyperbolic orbit, accompanied by the most probable parabola for comparison. The investigation by Dr. Kreutz, a very complete one, gives perhaps a more definite result. He is led to a parabolic orbit for the closest representation of the comet's path, and though the original observations at Marseilles had again been sought for unsuccessfully, he does not think their recovery would affect the conclusion at which he had arrived. The elements of the definitive parabola are as follow:—

Perihelion passage, 1771, April 19·14144 M.T. at Paris.

Longitude of perihelion ...	104 ° 1 ′ 21·″7	M. Eq. 1771·0
,, ,, ascending node	27 53 11·7	
Inclination	11 15 53·1	
Logarithm of perihelion distance, 9·955127		

Motion—direct.

The Cassini Division of Saturn's Ring.—At the January meeting of the Royal Astronomical Society, Prof. J. C. Adams made a very interesting communication on William Ball's observations of Saturn, upon which much confusion and misapprehension have existed. Attention has been directed to the subject lately by several astronomical contemporaries, mainly with the view to show that William Ball was not, as he has been considered, the discoverer of the chief division of Saturn's ring. Prof. Adams has carefully examined letters from Ball preserved in the *Archives* of the Royal Society, Huyghen's *Opera Varia*, &c., and remarks: "I find no evidence that Ball, any more than Huyghens, had noticed any indication of a division in the ring." This statement may be accepted as conclusive that the impression of several English writers as to Ball's claim to the discovery of a double ring is a mistaken one, and the credit of the discovery rests with Cassini. The announcement of it made by the French astronomer to the Academy of Sciences is in the following terms:—"Après la sortie de Saturne hors des rayons du soleil l'an 1675 dans le crépuscule du matin, le globe de cette planète parut avec une bande obscure semblable à celles de Jupiter, étendue selon la longueur de l'anneau d'orient en occident, comme elle se voit presque toujours par la lunette de 34 pieds, et la largeur de l'anneau étoit divisée par une ligne obscure en deux parties égales, dont l'intérieure et plus proche du globe étoit fort claire, et l'intérieure un peu obscure. Il y avoit entre les couleurs de ces deux parties, à-peu-près la même différence qui est entre l'argent mat et l'argent bruni (ce qui n'avoit jamais été observé auparavant), et ce qui s'est depuis vû toujours par la même lunette, mais plus clairement dans la crépuscule et à la clarte de la lune que dans une nuit plus obscure. Cette apparence donna une idée comme d'un anneau double, dont l'inférieur plus large et plus obscur fût chargé d'un plus étroit et plus clair." In two figures attached to this announcement the ring is shown with the outer half shaded and the inner half white, and there is a central band across the globe.

ON THE CHEMICAL CORROSION OF CATHODES[1]

THIS paper contains a description of the influence of various circumstances upon the chemical corrosion of metallic cathodes in different liquids.

Several preliminary experiments are described by means of which it was found that in some cases the chemical corrosion of a metal is increased, and in others decreased, by making the metal a cathode. Also, that the loss of weight of a cathode in an electrolyte is dependent upon several conditions, such as difference of metal, of liquid, or of strength of liquid, some of

[1] By G. Gore, LL.D., F.R.S. Abstract of paper read before the Birmingham Philosophical Society, December 14, 1882.

which tend to increase, and others to decrease the corrosion. In a solution of potassic cyanide pure silver is always protected by being made a cathode. The influence of variations of strength of acid was tried in several cases.

The results, which at first were apparently contradictory, were found to depend upon a number of conditions, and it would require an extensive research to determine the limits of those conditions, and what the proportions are, in which all those separate influences participate in producing the effect. Unequal capillary action is one of them, and its effect is described in a separate paper entitled, "The Electrolytic Balance of Chemical Corrosion." Another is unequal corrodibility of the metal itself. This was investigated, but how it arose was not clearly ascertained. Traces of certain kinds of soluble impurity in the liquid was also a disturbing circumstance. The altered chemical composition of the liquid around the cathode, caused by substances set free or formed by electrolysis, was another influence; this was investigated in the case of a silver cathode in a solution of potassic cyanide, and the protective influence of the current upon the cathode was found to be partly due to the formation of potassic hydrate; the current, however, operates also in some other manner. The effect of temperature was also examined, and it was found that the current exercised a greater protective power when the liquid was hot than when it was cold; the corrosive effect without a current was also greatest (as might have been anticipated) in the hottest liquid. The effects were further influenced by the degree of strength of the current; the greatest strength of current exercised the most protective power, and a large number of experiments were made expressly to test the question whether difference of electro-motive force alone, independently of difference of strength of current, affected the rate of corrosion, but the difficulty of insuring perfect uniformity in all the other conditions which affected the corrosion was so great that sufficiently decisive results were not obtained.

THE MOVEMENTS OF AIR IN FISSURES AND THE BAROMETER

FROM time to time attention has been called to the property exhibited by certain wells in different parts of this country of maintaining an active and permanent circulation of air. It was observed that currents alternately entered or issued from fissures in the sides of the wells, and though in some cases the first emission on sinking the well consisted of choke-damp, the gas subsequently passing consisted of no more than atmospheric air. While it was clear that the currents were not due to the evolution of any gas by chemical action in the rock or the water, an explanation of the phenomenon was found in the fact that the changes in the direction of the circulation coincided precisely with the changes of movement of the barometer, the current being outwards with a falling glass, inwards when the barometer was rising, and ceasing altogether when no change in the atmospheric pressure was taking place. The strength of the currents moreover was found to be proportionate to the rapidity of the barometric movements.

The name of Blowing Wells has come to be applied to such wells in consequence of these properties. From their extreme sensitiveness to changes in the atmospheric pressure, they have been found to give useful indications of the approach of bad weather. Their warnings are rendered audible by fixing horns or whistles in an air-tight covering, in such a way as to sound readily to the outward current, or to give a different note for an outward or inward movement of the air.

The first blowing well of which we have an account appears to have been of an entirely artificial origin. A well was sunk at Whittingham, near Preston, to a depth of eighty feet, and being afterwards abandoned, was covered with a large flagstone pierced by a small hole. Currents of air were observed to enter or issue from this hole, according as the barometer was rising or falling, and a tin horn fixed in it became audible at a considerable distance. Similar phenomena were exhibited by a cesspool, intended to receive offensive residue from some chemical works. The pool was arched over, a small hole being left for the passage of the refuse; a fall in the barometer was made unpleasantly evident by the issue of offensive vapours.[1]

Subsequently it was noticed that three wells in the New Red Sandstone, in the neighbourhood of Northallerton exhibited the same peculiarity. The wells "blow" through fissures in the sandstone just above the water-level. The changes in the direction of the currents coincide precisely with the movements of the barometer, and the outward current is made to blow a "buzzer," which is said to be audible at a mile distance.[1] In the years 1879-80 a series of interesting experiments on one of these wells, situated near Solberge, three and a half miles south of Northallerton, was made by Mr. Thomas Fairley, F.R.S.E.[2] After stating that the water has a composition similar to that coming from chalk or limestone, and that, though on the first opening of the fissure a violent outburst of choke-damp had taken place, the gas subsequently issuing did not differ appreciably from common air, Mr. Fairley gives a detailed account of observations made on the volume of air passing. The currents passed through fissures in the sandstone at a depth of forty-five feet from the surface of the ground, and just above the level of the water. The measurements were made firstly by a vane-anemometer, and subsequently by two large dry meters, constructed to pass 3,000 cubic feet per hour; these had been substituted for two of the largest meters in the possession of the Leeds Corporation, which had been thrown out of gear by their incapacity to pass the air fast enough. As a result of these experiments it was found that a fall of the barometer of 0·26 inch was accompanied by an outflow of 83,900 cubic feet of air, and by an application of Boyle's law it was calculated the total capacity of the fissures must amount to nearly 10,000,000 cubic feet.

The existence of currents obeying the same laws is equally obvious in a well at Langton at a few miles distance. The well has been long disused, and the water is exceedingly foul, notwithstanding which a candle burns clearly at the bottom. A third instance occurs at Ornhams near Boroughbridge, where the roar of the air-currents passing into the crevices of the rock has been compared by a workman to that of the water in a mill-race. No observations, further than those necessary to prove the existence of the currents, have yet been made on these wells.

At Hopwas a well has been sunk for the supply of Tamworth to a depth of 168 feet, the water standing naturally a depth of 129 feet. The shaft passes through alternations of shale and sandstone, one of the beds of the latter, met with at a depth of ninety-six feet eight inches, being described as "light fissured sandstone thirty feet four inches."[3] From a fissure in this bed, at 115 feet from the surface, there issued a violent rush of atmospheric air, which soon spent itself, and was succeeded by currents showing variations coincident with the barometic changes. The currents have been noticed in one fissure only, an irregular opening, of two and a half inches in height by one inch in width, in a nearly close-sided vertical joint. Experiments on the amount of air traversing this fissure are now in progress.

The same properties are exhibited in an equally well-marked degree in a well belonging to Mr. A. Potts at Hoole Hall, near Chester. The well is eighty-one feet deep and contains ten feet nine inches of water; it is sunk through glacial deposits, consisting of a tough clay overlying a sand of variable thickness, into the New Red Sandstone, but, being an old well and lined with brick to the water-level, the exact nature of the strata and the position of the fissures is unknown. Communicating with the interior of the well by pipes, are two whistles of a different tone, and a pressure gauge; the deeper-toned whistle sounds to an inward, the shriller-toned to an outward current, and were they allowed to act freely during unsettled weather, these whistles would render sleep in the adjoining house impossible. It is stated by Mr. Potts that changes in the atmospheric pressure are shown more rapidly by the pressure gauge of the well than by a mercurial barometer, and that whenever there is a sudden change for rain, the water in the well becomes agitated and slightly discoloured. An appearance of ebullition was noticed also in the Solberge well, but has been attributed by Mr. Cameron to the falling of fragments of mortar. The movements of the water in the Hoole well are being made the subject of experiment by Mr. Potts. Similar, though less powerful, currents have been observed in two other wells within a distance of 500 yards of Hoole Hall. The wells are in a situation where a similar sequence of glacial deposits probably exists, but further particulars are at present wanting.

The fissures from which the currents in blowing wells issue occur usually near, but just above, the water-level. Above them there is provided an air-tight covering in the glacial clays,

[1] J. Rofe, F.G.S., *Geological Magazine*, vol. iv. p. 106, 1867.

[1] A. G. Cameron, *Geological Magazine*, vol. vii. p. 95, 1880.

[2] *Proc.* York Geol. and Polyt. Soc., N.S., vol. vii. p. 409. 1881.

[3] Mr. H. J. Marten, Eighth Report on the Circulation of Underground Waters to the British Association, 1882.

or in beds of shale interstratified with the sandstone, cutting off communication with the open air in this direction. Fissures traversing a dry sandstone in such a situation constitute an air-chamber which may clearly be of great capacity. On cutting one of a system of connected fissures, the first effect is frequently to liberate a quantity of pent-up air or choke-damp, as at Solberge; subsequently the opening becomes the sole channel by which equilibrium is preserved between the enclosed air and the atmosphere. It would however seem as probable that the opening should occur below the water-level as above it. In such a case the first effect of an expansion of the pent-up gases would be to force out water, and raise the level of the water in the well. The agitation of the water noticed in the well at Chester is probably due to the openings being partly above and partly just below the surface of the water. That they not infrequently are wholly below appears probable from observations on springs and wells, for it has been noticed that in certain chalk-springs there is an increase in the amount of water flowing when there is a rapid fall in the barometer, though no rain may have fallen, and that under the same circumstances water recommences to flow from land-drains and percolation gauges. The gaugings of deep wells in the chalk have confirmed these observations and show that there is a rise in the water-level under a decrease of atmospheric pressure. These movements have been attributed to the expansion of the dissolved gases.[1] It is probable that the gases when given off by the water, rise into and occupy cavities from which there is no escape upwards.

It is noticeable however that five certainly, and two probably, of the blowing wells described above derive their properties from fissures in the New Red Sandstone; no case is known in either chalk or limestone, though these are soluble rocks peculiarly liable to contain caverns or widened joints. It is not improbable that the fissures are too numerous in these rocks, so that wherever large hollows occur, there are also communications upwards with the open air. In sandstone on the other hand large hollows are of extremely rare occurrence, and in view of this difficulty it has been suggested that the Magnesian Limestone which underlies it about Northallerton, at a depth of about 400 feet, and is known to be extremely cavernous, may have given way in places, and led to the formation of hollows in the sandstone. This explanation however is not applicable to the wells at Tamworth or Chester, where the sandstone is not underlain by limestone. It seems more probable that the strength of the air-currents should be taken in connection with the copiousness of the water-supply as indicative of the great extent of small ramifying fissures in some of the triassic sandstones. That the united capacity of such fissures must be very great to account for the phenomena is undeniable. The volume of air contained in the cavities at the Solberge well was estimated at about 10,000,000 cubic feet, or as much as would fill a chamber measuring 217 feet each way, length, width, and height.[2] In making this estimate no allowance was made for aqueous vapour, or for air held in solution in the water, both of which would come off in increased quantities with a decreasing pressure. The former was known by the state of the meter to have been present in large quantities. But making every allowance for these causes of error, it is impossible to escape the conclusion that the fissures, small as they are individually, must in the aggregate form a reservoir of immense capacity.

In concluding these remarks we may refer to the practical application of the knowledge of these properties in fissures. It has been noticed that the drains of large works begin to smell on the approach of rain, and there can be little doubt that this is partly due to the setting up of an outward current corresponding to a fall in the barometer. In fact every network of covered drains, and every covered cess-pool, where special provision for ventilation is not made, must constitute a natural blowing well. It is not our intention to discuss here the engineering details of drainage. It is sufficient to point out that by a faulty system of ventilation, or by the derangement of a system originally good, sewer-gas might be forced into a house with every fall of the barometer.

Lastly we would allude to the effect of the barometer on the escape of fire-damp from coal-seams. Coal is a rock subject to jointing; seams are not only broken through and displaced by faults, but for some distance from the main fracture are traversed by joints and smaller shifts resulting from the general strain. A brief visit to a fiery portion of a mine is sufficient to show the part played by these small clefts. On every side is heard the monotonous hissing or bubbling of the escaping gas, often accompanied by the deeper note of a "blower," or one of those larger channels often observed in connection with faults. The gas is continously given off as a result of a slow decomposition taking place in the coal, and the amount that comes off indicates a great extent of connected fissuring. For though cavities charged with gas under pressure and liable to exhaustion are found, yet large "blowers" commonly continue active for years, and must therefore drain a large area of the seam. While the movement of the gas in the blower differs from that of the air in sandstone fissures, in being always in one direction, namely outwards, it is at the same time evident that the same cause which induces an outward current in the well would cause an increase in the outward current from the coal. The increase would be proportional to the capacity of the fissures; a fall in the barometer from thirty to twenty-nine inches for example would cause $\frac{1}{30}$th of the body of gas stored in the fissures to be added to the ordinary outflow. The liability to explosion with a falling glass has long been a subject of observation. When it is considered that a wide margin is usually allowed in the ventilation to ensure the sufficient dilution and removal of fire-damp, and that a number of other contingencies may bring about an explosion, it becomes evident that a powerful cause must be operating to make the influence of the barometric changes perceptible.

A. STRAHAN

[1] Baldwin Latham, Report of the British Association for 1881, p. 614.
[2] *Proc.* York Geol. and Polyt. Soc., *op. cit.*

SOCIETIES AND ACADEMIES

LONDON

Royal Society, January 25.—"Internal Reflections in the Eye," by H. Frank Newall, B.A.

The author in this paper describes the appearance and investigates the cause of a faint light seen under certain circumstances now to be related:—Stand opposite a uniformly dark wall in a darkened room. Direct the eye to any point in front, and keeping the eye fixed, and being ready to perceive any appearance out of the line of direct vision without moving the eyes towards it, hold up a candle at arm's length, and move it to and fro over about two inches on a level with the point fixed, and a little to the right or left of it. The faint light may be seen moving with a motion opposite to that of the candle on the other side of the point of direct vision.

Near inspection of the light shows it to be an inverted image of the candle, about equal in size, very faint.

Reasons related in the paper lead the author to offer the following explanation: the physical cause of the faint light or "ghost," is light which, proceeding outwards from the image of the candle, formed on the retina by the lens, is reflected back on to the retina by the anterior surface of the lens. This second image is "referred" outwards, and seems as if produced by a faint source of light outside the eye.

The effects of alterations of the state of accommodation on the appearance of the ghost are described; the question as to whether the retina is to be regarded as a screen or as a *regular* reflector is discussed; and the results of calculations based on numbers given by Helmholtz for his schematic eye are noted as forming a difficulty in the explanation.

If the candle be replaced by sunlight, further observations are to be made: (1) signs of the faulty centering of eye-surfaces, as shown by the fact that the sun and its ghost do not arrive at the centre of the field of vision together; (2) signs of oblique reflection at a concave mirror, as shown by the fact that the ghost is circular in only one state of accommodation, whilst in other states it is extended either in a horizontal direction for near focus, or in a vertical direction for distant focus.

To about four out of fifteen persons the author has failed to show the ghost; but no relation is as yet observed between the visibility of the ghost and the kind of sight of the observer as defined by the ordinary terms, long- and short-sightedness.

A second "ghost," probably due to reflections entirely within the lens, is referred to in the paper: but this, on account of its indistinctness, has not been investigated, except to establish the fact that its motion is the same in direction as that of the candle in the circumstances above related.

February 1.—On the Electrical Resistance of Carbon Contacts, by Shelford Bidwell, M.A., LL.B.

The experiments described in the paper were undertaken with the object of investigating the changes of resistance occurring in carbon contacts under various conditions.

A short movable carbon rod was placed across and at right angles to a similar rod which was fixed in a horizontal position, and arrangements were made for varying and accurately measuring the pressure of the one upon the other, for varying and measuring the current passing through them, and for measuring the resistance at the points of contact. The following are the most important results arrived at:—

1. *Carbon Contacts.*—Changes of pressure produce proportionately greater changes of resistance with small pressures than with great pressures. Thus, when the pressure was increased from ·25 to ·5 grms., the resistance fell from 16·1 to 11·0 ohms., the difference being 5·1 ohms; whereas, when the pressure was increased from 25 to 50 grms., the resistance fell from 2·1 to 1·8 ohms., the difference being only ·3 ohms.

Changes of pressure produce proportionately greater changes of resistance with weak currents than with strong currents. Thus when the pressure was increased from ·25 to ·5 grm., the resistance fell from 9·27 to 8·45 ohms with a current of ·1 ampere; and from 25·50 to 17·75 ohms with a current of ·001 ampere.

Changes of current, the pressure remaining constant, produce greater changes of resistance with small currents than with large currents, and with light pressures than with heavy pressures.

When the resistance of a carbon contact has been reduced by an increase of pressure, it will, on the removal of the added pressure, rise to approximately its original value.

The passage of a current whose strength does not exceed a certain limit, depending upon the pressure, causes a permanent diminution in the resistance (so long, of course, as the contacts are undisturbed), and the stronger the current, the greater will be such diminution.

When the strength of the current exceeds a certain limit, the resistance is greatly and permanently increased (generally becoming infinite). The greater the pressure, the higher will be such limit.

Unless special means are adopted for maintaining a constant current, the fall in the resistance which attends increased pressure is greater than that which is due to increased pressure alone being partly due also to the increased current.

It is not proved that the diminished resistance which follows an increase of current is an effect of temperature.

2. *Metallic Contacts.*—For the sake of comparison, a few experiments were made with metals. The metal principally used was bismuth, which was selected on account of its high specific resistance, but experiments were also made with copper and platinum.

In the case of bismuth, and probably of other metals:—With a given pressure, the weaker the current the higher will be the resistance. This effect is most marked when the current is small. Thus, with a pressure of ·1 grm. the resistance, with a current of ·1 ampere, was 2 ohms; with ·01 ampere it was 16·92 ohms; and with a current of ·001 ampere it was 143·3 ohms. With a pressure of ·5 grm., the resistance with the same currents as before was 1·45, 1·47, and 3·8 ohms.

The passage of a current, even when very small, causes a permanent adhesion between metallic contacts. This effect had been previously observed by Mr. Stroh.

Increase in the current is accompanied by a fall of resistance, and if the current be again reduced to its original strength, the resulting change in the resistance will be small, and it will in no case return to its original value.

Diminution in the strength of the current is followed by a small fall in the resistance if the metal is clean, and by a small rise in the resistance if the metal is not clean.

Increased pressure produces a greater fall in the resistance with small pressures than with great pressures, and with weak currents than with strong currents.

The resistance, after having been reduced by increased pressure, does not return to its original value when the added pressure is removed.

3. *Reasons for the Superiority of Carbon over Metal in the Microphone.*—The above observations may perhaps furnish an answer to the question, Why does carbon give far better results than any metal when used in the microphone? The mere fact that a current causes delicately-adjusted metal contacts to adhere to each other seems sufficient to account for the superior efficiency of carbon. In addition to this phenomenon of adhesion, and probably connected with it, are the facts that metallic contacts, unlike those of carbon, do not even approximately recover their original resistance when once it has been reduced by increased pressure or increased current, unless indeed complete separation occurs; and even the initial effect of pressure upon resistance is in general much more marked with carbon than with metals.

Lastly, it is to be noticed that in the case of carbon, pressure and current act in consonance with each other: pressure diminishes the resistance, and in so doing, increases the strength of the current; and the current thus strengthened effects a further diminution in the resistance. In the case of metals, on the other hand (or at least in the case of clean bismuth) pressure and current tend to produce opposite effects. The resistance is diminished by pressure, and the current consequently strengthened; but by reason of the increased strength of current, the resistance is *higher* than it would have been if the current had remained unchanged. The effect of this antagonism is not very great, but it seems sufficient to give a material advantage to carbon.[1]

The paper contains fifteen tables, four curves, and three diagrams, illustrative of the apparatus used.

February 8.—"Note on Terrestrial Radiation." By John Tyndall, F.R.S.

On Hind Head, a fine moorland plateau about three miles from Haslemere, with an elevation of 900 feet above the sea, I have recently erected a small iron hut, which forms, not only a place of rest, but an extremely suitable station for meteorological observations. Here, since the beginning of last November, I have continued to record from time to time the temperature of the earth's surface as compared with that of the air above the surface. My object was to apply, if possible, the results which my experiments had established regarding the action of aqueous vapour upon radiant heat.

Two stout poles about 6 feet high were firmly fixed in the earth 8 feet asunder. From one pole to the other was stretched a string, from the centre of which the air thermometer was suspended. Its bulb was 4 feet above the earth. The surface thermometer was placed upon a layer of cotton wool, on a spot cleared of heather, which thickly covered the rest of the ground. The outlook from the thermometers was free and extensive; with the exception of the iron hut just referred to, there was no house near, the hut being about 50 yards distant from the thermometers.

On November 11, at 5.45 p.m., these were placed in position, and observed from time to time afterwards. Here are the results:—

6	p.m.	...	Air 36° Fahr.	...	Wool 26° Fahr.
8.10	,,	...	,, 36	...	,, 25
9.15	,,	...	,, 36	...	,, 25

air almost dead calm, sky clear, and stars shining.

November 12, the wind had veered to the east, and was rather strong. The thermometers, exposed at 5 p.m., yielded the following results:—

5.15	p.m.	...	Air 38°	...	Wool 33°
5.45	,,	...	,, 38	...	,, 34
6.45	,,	...	,, 38	...	,, 35
9	,,	...	,, 39	...	,, 36

During the first and last of these observations the sky was entirely overcast, during the other two a few stars were dimly visible.

On November 13, 25, and 26, observations were also made, but they presented nothing remarkable.

It was otherwise, however, on December 10. On the morning of that day the temperature was very low, snow a foot deep covered the heather, while there was a very light movement of the air from the north-east. Assuming aqueous vapour to play the part that I have ascribed to it, the conditions were exactly such as would entitle us on *a priori* grounds to expect a considerable waste of the earth's heat. At 8.5 a.m. the thermometers were placed in position, having left the hut at a common temperature of 35°. The cotton wool on which the surface thermometer was laid was of the same temperature. A single minute's exposure sufficed to establish a difference of 5° between the two thermometers. The following observations were then made:—

8.10	a.m.	...	Air 29°	..	Wool 16°
8.15	,,	...	,, 29	...	,, 12

Thus, in ten minutes, a difference of no less than 17° had established itself between the two thermometers.

Up to this time the sun was invisible: a dense dark cloud,

[1] In April, 1882, the author communicated this observation to Mr. Preece, who referred to it in a paper read at the Southampton meeting of the Brit. Assoc., on "Recent Progress in Telephony."

resting on the opposite ridge of Blackdown, virtually retarded his rising.

8.20 a.m.	...	Air 27°	...	Wool 12°
8.30 ,,	...	,, 26	...	,, 11
8.40 ,,	...	,, 26	...	,, 10
8.45 ,,	...	,, 27	...	,, 11
8.50 ,,	...	,, 29	...	,, 11

During the last two observations, the newly-risen sun shone upon the air thermometer. As the day advanced, the difference between air and wool became gradually less. From 18° at 8.50 a.m., it had sunk at 9.25 to 15°, at 9.50 to 13°, while at 10.25, the sun being unclouded at the time, the difference was 11°; the air at that hour being 31° and the wool 20°.

In the celebrated experiments of Patrick Wilson, the greatest difference observed between a surface of snow and the air 2 feet above the snow, was 16°; while the greatest difference noticed by Wells during his long-continued observations fell short of this amount. Had Wilson employed swansdown or cotton wool, and had he placed his thermometer 4 feet instead of 2 feet above the surface, his difference would probably have surpassed mine, for his temperatures were much lower than those observed by me. There is, however, considerable similarity in the conditions under which we operated. Snow in both cases was on the ground, and with him there was a light movement of the air from the east, while with me the motion was from the north-east. The great differences of temperature between earth and air, which both his observations and mine reveal, are due to a common cause, namely, the withdrawal of the check to terrestrial radiation which is imposed by the presence of aqueous vapour.

Let us now compare these results with others obtained at a time of extreme atmospheric serenity, when the air was almost a dead calm, and the sky without a cloud. At 3.30 p.m., January 16, the thermometers were placed in position, and observed afterwards with the following results:—

3.40 p.m.	...	Air 43°	...	Wool 37°
3.50 ,,	...	,, 42	...	,, 35
4 ,,	...	,, 41	...	,, 35
4.15 ,,	...	,, 40	...	,, 34
4.30 ,,	...	,, 38	...	,, 32
5 ,,	...	,, 37	...	,, 28
5.30 ,,	...	,, 37	...	,, 30
6 ,,	...	,, 36	...	,, 32

These observations, and especially the last of them, merit our attention. There was no visible impediment to terrestrial radiation. The sky was extremely clear, the moon was shining; Orion, the Pleiades, Charles's Wain, including the small companion star at the bend of the shaft, the north star, and many others, were clearly visible. On no previous occasion during these observations had I seen the firmament purer; and still, under these favourable conditions, the difference between air and wool at 6 p.m. was only 4°, or less than one-fourth of that observed on the morning of December 10.

We have here, I submit, a very striking illustration of the action of that invisible constituent of the atmosphere, to the influence of which I drew attention more than twenty-two years ago. On December 10 the wind was light from the north-east, with a low temperature. On January 16 it was very light from the south-west, with a higher temperature. The one was a dry air, the other was a humid air; the latter, therefore, though of great optical transparency, proved competent to arrest the invisible heat of the earth.

The variations in the temperatures of the wool recorded in the last column of figures are, moreover, not without a cause. The advance of temperature from 28° at 5 p.m. to 32° at 6 p.m., is not to be accounted for by any visible change in the atmosphere, or by any alteration in the motion of the air. The advance was due to the intrusion at 6 p.m. of an invisible screen between the earth and firmament.

As the night advanced the serenity of the air became, if possible, more perfect, and the observations were continued with the following results:—

6.30 p.m.	...	Air 36°	...	Wool 31°
7 ,,	...	,, 36	...	,, 28
7.30 ,,	...	,, 35½	...	,, 28
8 ,,	...	,, 35	...	,, 26
8.30 ,,	...	,, 34	...	,, 25
9 ,,	...	,, 35	...	,, 27
10 ,,	...	,, 35	...	,, 28
10.30 ,,	...	,, 35	...	,, 29

After this last observation, my notes contain the remark, "Atmosphere exquisitely clear. From zenith to horizon cloudless all round."

Here, again, the difference of 4° between the temperature of the wool at 8.30 p.m., and its temperature at 10.30 p.m., is not to be referred to any sensible change in the condition of the atmosphere.

The observations were continued on January 17, 23, 24, 25, and 30; but I will confine myself to the results obtained on the evening of the day last-mentioned. The thermometers were exposed at 6.45 p.m., and by aid of a lamp read off from time to time afterwards.

7.15 p.m.	...	Air 32°	...	Wool 26°
8 ,,	...	,, 31	...	,, 26
9.30 ,,	...	,, 31	...	,, 27

During these observations the atmosphere was very serene. There was no moon, but the firmament was powdered with stars. The serenity, however, had been preceded by heavy rain, which doubtless had left the atmosphere charged with aqueous vapour. The movement of the air was from the south-west and light. Here again, with an atmosphere at least as clear as that on December 10, the difference between air and wool did not amount to one-fourth of that observed on the latter occasion.

The results obtained on February 3 were corroborative. The thermometers were exposed at 6.15 p.m.

7.15 p.m.	...	Air 34°	...	Wool 28°
8.25 ,,	...	,, 34	...	,, 30

Here again, the difference between air and wool is only 4°, although the sky was cloudless, and the stars were bright. The movement of the air was from the south-west and light.

On the forenoon of this day there had been a heavy and persistent rain storm. Heavy rain and high wind also occurred on the night following. The serene interval during which the observations were made lay, therefore, between the two storms. Doubtless the gap was well filled with pure aqueous vapour.

Further observations were made in considerable numbers, but they need not here be dwelt upon, my object being to illustrate a principle rather than to add to the multitudinous records of meteorology. It will be sufficient to say that, with atmospheric conditions sensibly alike, the waste of heat from the earth varies from day to day; a result due to the action of a body which escapes the sense of vision. It is hardly necessary for me to repeat here my references to the observations of Leslie, Hennessey, and others, which revealed variations in the earth's emission for which the observers could not account. A close inspection of the observations of the late Principal Forbes on the Faulhorn proves, I think, that the action of aqueous vapour came there into play, and his detection of this action, while unacquainted with its cause, is in my opinion a cogent proof of the accuracy of his work as a meteorologist.

Postcript.—In the *Philosophical Transactions* for 1882, Part I. p. 348, I refer to certain experiments executed by Prof. Soret of Geneva. My friend has recently drawn my attention to a communication made by him to the French Association for the Advancement of Science in 1872. It gives me great pleasure to cite here the conclusions at which he has arrived.

"The influence of humidity is shown by the whole of the observations; and it may be stated generally that, other circumstances being equal, the greater the tension of aqueous vapour the less intense is the radiation.

"In winter, when the air is drier, the radiation is much more intense than in summer, for the same height of the sun above the horizon.

"On several occasions a more intense radiation has been observed in dry than in humid weather, although the atmosphere was incontestably purer and more transparent in the second case than in the first.

"The maximum intensity of radiation, particularly in the summer, corresponds habitually to days exceptionally cold and dry."

Such are the results of experiments, executed by a most excellent observer, on the radiation of the sun. They apply word for word to terrestrial radiation. They are, moreover, in complete harmony with the results published by General Strachey in the *Philosophical Magazine* for 1866. Meteorologists will not, I trust, be offended with me if I say that from such outsiders, fresh to the work and equipped with the neces-

sary physical knowledge, they may expect efficient aid towards introducing order and causality among their valuable observations.

Mathematical Society, February 8.—Prof. Henrici, F.R.S., president, in the chair.—Capt. P. A. MacMahon, R.A., was admitted into the Society.—The following communications were made:—On the Sylvester-Kempe quadruplane, by Mr. H. Hart.—On curves obtained by an extension of Maclaurin's method of constructing conics, by Mr. S. Roberts, F.R.S.—A generalisation of the "nine-point" properties of a triangle, by Capt. P. A. MacMahon.—On the use of certain differential operators in the theory of equations, by Mr. J. Hammond.—A method for reducing the differential expression $dt/\sqrt{\{(t-\alpha, t-\beta, t-\gamma, t-\delta\}}$ to the standard form, by Mr. J. Griffiths. The "nine-point" property was the following:—If through the centre of the circle ABC, and the ortho-centre of the triangle ABC, lines be drawn making angles α and $\pi-\alpha$ with the sides of the triangle, twelve points will be obtained on the sides, and these lie six and six on two circles of radius $\frac{1}{2}R$ cosec α. Each circle also passes through six other points, and they are inscribed circles of the two three-cusped hypocycloids, which are the envelopes of the two tangents, equally inclined to the axis (at angles α), to a parabola inscribed in the triangle ABC. Of course, when $\alpha = \frac{\pi}{2}$, the circles become the ordinary nine-point circle of ABC.

Linnean Society, February 1.—Sir John Lubbock, Bart., F.R.S., president, in the chair.—Messrs. F. W. Burbidge and Joseph Johnson were elected Fellows of the Society.—Dr. W. C. Ondaatje called attention to examples of red coral from Ceylon.—Mr. W. T. Thiselton Dyer exhibited a model of the fruit of the Double Cocoa-nut (*Lodoicea Seychellarum*, Lab.), of an unusual form, obtained from Major-General C. G. Gordon, R.E.—A series of microscopic sections of coal-plants were shown on behalf of Mr. J. Norman.—The following paper was then read:—On the structure, development, and life-history of a tropical epiphyllous lichen, by H. Marshall Ward. The author's observations lead him to believe that the epiphyllous cryptogam in question supports the view that a lichen is a compound organism composed of an alga on which an ascomycetous fungus has become more or less intimately affixed and dependent. It is developed on the leaves of many plants, but it has been more closely watched on *Michelia furcata*. The lichen presents four types, orange-red stellate patches, greyish-green blotches, clear grey spots, and white shining circles, but these pass imperceptibly into one another, and vary in size from a speck to a quarter of an inch in diameter. The reddish spots of the earlier stages is an alga of which the radiating filaments are in part reproductive organs, and in part barren hairs. It subsequently passes into the grey and green stages, and by a modification of growth the invasion of a fungus mycelium succeeds. The white matrix of the complete lichen consists of the same algal thallus invested by dense masses of the fungus hyphæ, which produce shining black dots, viz. the fruit bodies. The author describes in detail the peculiarities of growth and reproduction of the alga and fungus, and formation of the lichen. He alludes to and criticises Dr. Cunningham's account of *Mycoidea parasitica*, which latter is evidently closely related to that described by himself. Assuming that Mycoidea and Ward's Alga are generically the same, either Cunningham discovered a female organ of reproduction which becomes fertilised and produces zoospores, or he confounded these with certain fertile hair organs described by Ward. As regards the systematic position of the alga, a comparison with *Coleochæta* suggests that there is very little in common beyond mode of growth of the disc-like thallus, and the production of zoospores from certain cells. The genus *Chroolepus*, however, presents features which agree in several important points, viz., orange-red oily-cell contents, habitat, production of zoospores in ovoid cells developed terminally and laterally. The structure of the thallus, and relative positions of the main masses of fungal and algal portions, agree with what occurs in heteromerous crustaceous lichens, as Graphidea; but the perithecia indicate an angiocarpous alliance, bringing this form nearer such families as Pertusaria and Verrucaria, to the latter of which it may ultimately be referred.—A paper was read by F. Maule Campbell, on the pairing of *Tegenaria Guyonii*, and description of certain organs in the male abdominal sexual region. Two cases are related in which during confinement the males killed the females after union, and an instance is also given of an attempt to impregnate an immature female which was also destroyed by the male. In neither case could hunger have been the cause of the attack. The writer explains the occurrences, and also the accounts of females destroying males after union, on the ground "that those instincts which are habitually practised throughout the far greater portion of the life of the species, and on which it is dependent, would scarcely be suspended for a longer period than necessary for the sexual union." Some of the habits of spiders, and especially of this species, are mentioned as bearing on these sexual conflicts, and the specific benefits which would arise from them are referred to. The paper concluded by a note on some glands (probably for spinning) situated on the convexity of the abdominal sexual region. The ducts are considerably convoluted, and open through transparent tubular spines which are arranged transversely to the axis of the body of the spider. Two papilla-like processes below the opening of the genital sinus are described.

Zoological Society, February 6.—Prof. W. H. Flower, LL.D., F.R.S., president, in the chair.—A letter was read from Mr. F. C. Selous, dated from the Matabele Country, on the possibility of obtaining a White Rhinoceros.—Extracts were read from a letter received from the Rev. G. H. R. Fisk, C.M.Z.S., of Cape Town, giving an account of the habits of some reptiles which he had had in captivity.—A communication was read from Messrs. Salvin and Godman, containing the description of a new species of Pigeon of the genus *Otidiphaps* from Ferguson Island, one of the D'Entrecasteaux group, which they proposed to call *O. insularis*.—Mr. Sclater read some further notes on *Tragelaphus gratus*, and exhibited drawings of both sexes of this antelope, taken from specimens living in the Menagerie of the Jardin des Plantes, Paris.—A communication was read from Mr. E. W. White, F.Z.S., containing some supplementary notes to a former paper on the birds of the Argentine Republic.—A communication was read from the Rev. G. A. Shaw, containing some notes on the habits of an Aye-Aye which he had had in confinement for several months, and other information respecting this animal.—Mr G. A. Boulenger, F.Z.S., read a paper containing the description of a new species of Lizard of the genus *Enyalius* from Peru, which he proposed to name *E. palpebralis*.

BERLIN

Physiological Society, January 12.—Prof. du Bois Reymond, in the chair.—Dr. Falk read a contribution upon the phenomenon lately demonstrated by experiments on animals, that great oedema of the lungs can be produced in a very short time, even in a quarter of an hour, by compressing or otherwise interrupting the function of the left side of the heart; whereas a similar action on the right side does not produce such an effect. Dr. Falk has had an opportunity in two post-mortems of proving the correctness of these observations in respect to man. In one of these cases, a strong, healthy man died in consequence of a discharge of shot, and the post-mortem showed that the cause of death was the penetration of a shot into the wall of the left ventricle. The lung of this previously healthy man exhibited a high degree of oedema. In the second case, a healthy railway-workman was killed by a blow of a buffer upon the chest. The post-mortem showed a rupture of the right ventricle to have been the cause of death; the lungs which were carefully examined, did not show a trace of oedema.—Dr. W. Wolff described the structure of the tactile corpuscles, according to his researches, they contain no nerve-branchings, but consist of a rugose sheath, granular contents, and the free ends of the entering nerve-fibres. In opposition to other histologists he further found the epithelium-cells which he studied in the corneæ of small mammals to be devoid of nerves; and in agreement with this he has always found gland-cells to be without nerve. The sympathic nerve-fibres which enter into the glands according to Dr. Wolff always end in unstriped muscles.—Prof. Kronecker reported on experiments of Dr. Meiss, upon the irritability of the heart under abnormal conditions of nutrition. During experiments, undertaken to study the comparative effects of concentrated and diluted blood upon the frog's heart, and which established the occurrence of a more energetic activity by nutrition with concentrated blood, certain remarkable deviations occasionally occurred from the general law that frog's hearts (always) respond to every stimulation with maximal contractions. These deviations consisted in the occurrence of smaller submaximal beats between the maximal beats. Further investigation of this appearance led to the conclusion that this was a

result of abnormal conditions of nutrition which was not easily or certainly to be produced. These sub-maximal contractions and the irregular pulse, were chiefly observed when passing a current of asphyxiated blood through the heart, but they always disappeared on supplying the heart with fresh blood.

Physical Society, January 19.—Prof. Roeber in the chair.—Prof. Schwalbe supplemented his former communications to the Society on ice-caves, wtth additional facts he had recently come to know through literature. He noted, as a most interesting phenomenon, that the occurrence of ice-caves was not confined to limestones, basalts, and lavas, but that they have also been observed in gypsum-hills. Thus, in the gypsum-hill Illetzkaja Satscha, in the Southern Ural, is an ice-cave which Murchison visited in August; situated in the street, it was closed with a simple wooden door, and it was utilised by the inhabitants of the district as a store-room. Its temperature was so low that the drinks kept in it only a short distance from the mouth were frozen. And as with all other caves distinguished by their low temperature and ice-formation, it was stated of this one, that in winter the air in it was very warm, so that people slept in it at night without requiring their sheep-skin furs. Murchison had applied to Sir John Herschel for an explanation of the phenomenon, and Herschel had offered the hypothesis, that it was a case of cold and heat waves, which, penetrating inwards from the surface of the earth, were retarded, and so caused the low temperature in summer and the warmth in winter. Prof. Schwalbe, however, has convinced himself that this explanation is inadequate; for the summers in which ice-caves have been visited and found filled with ice, have been preceded by very mild winters, *e.g.* the winter 1881-2 was very mild, yet the ice-cave in Bohemia, which he had himself visited, was covered with ice; besides, the retardation of cold several months is very improbable. Before a sufficient explanation of these peculiar conditions of temperature can be reached, continuous scientific observations must be made, for a long time, of the course of the temperature throughout the year. As to the warmth of air in the caves in winter, and the melting of the ice in winter, there are only observations by lay-persons, which, however, strikingly agree, even in the assertion that the ice-formation regularly takes place on a large scale in the month of May. With regard to the immediate cause of the ice formation no one (according to the author) can be in doubt, who has visited an ice-cave, and seen how the dropping water from the roof solidifies directly on falling. Water that has trickled through is over-cooled, and solidifies just as after falling to the ground, even when it meets a different solid body; the ice, further, is only met with where water drops. The strong cooling of the water and of the rock through which it has trickled, is, perhaps, connected with the process of filtration through the earth-strata. On this point experimental research must decide, following up the investigation made by Jungk in 1865, who observed a lowering of temperature in filtration of water through porous partitions. Such laboratory experiments, exact long-continued temperature-observations in accessible ice-caves, and topographical examinations of as many as possible of these caves (which are not rare), will surely bring about a solution of this still obscure natural problem.

PARIS

Academy of Sciences, February 5.—M. Blanchard in the chair.—The following papers were read:—On the physical and mechanical constitution of the sun (third and last part), by M. Faye. He deals with the depth of spots, the movements of hydrogen and their effects, the height of protuberances and an illusion attending them, the clouds of the photosphere, &c.—M. Hirn presented an analysis of a *brochure* by himself and M. Hollauer. "Refutations of a second critique of M. G. Zeuner." It relates to the theory of steam-engines.—On the spherical representation of surfaces, by M. Darboux.—On the functions satisfying the equation $\Delta F = 0$, by M. Appell.—On the displacement of the sodium lines, observed in the spectrum of the great comet of 1882, by MM. Thollon and Guy. From the displacement observed with a single-prism spectroscope, he had estimated the rate of recession of the comet at from 61 to 76 km. per second. This is confirmed by M. Bigourdan, who, from a determination of the trajectory of the comet, estimates the velocity at the time in question (3 p.m. on September 18) at 73 km. The spectroscope is thus shown to be reliable for the purpose referred to.—Magnetic action of the sun and the planets; it does not produce secular variation in the great axes of orbits: note by M. Quet.—The distribution of energy in the solar spectrum and chlorophyll, by M. Timiriazeff. Prof. Langley finds, with his bolometer, the maximum of radiant energy in the orange, precisely that part of the spectrum which corresponds to the characteristic band of chlorophyll. M. Timiriazeff is studying the quantitative relation between solar energy absorbed by the chlorophyll of a leaf and that stored up through chemical work produced. It appears that the leaf can transform into useful work as much as 40 per cent. of the energy absorbed.—On some combinations of sulphite of magnesia with alkaline sulphites, by M. Gorgeu.—On hydraulic silica; reply to M. Le Châtelier, by M. Landrin.—On the mutual displacements of bases in neutral salts, the systems remaining homogeneous, by M. Menschutkine.—The microbes of marine fishes, by MM. Olivier and Richet. In all the fishes they examined (about 150) there were, in the peritoneal liquid, the lymph, the blood, and consequently in the tissues, microbes more or less numerous, having all the characters of terrestrial microbes, and reproducing similarly. Besides direct observation, the authors had recourse to experiments (1) of cultivation, (2) of occlusion. (In the latter case the fishes, or parts of them, were put into melted paraffine, which, after solidifying, was coated with several layers of collodion and Canada balsam, to protect from atmospheric germs. In a few weeks microbes were always abundantly developed.) The organisms were mostly *Bacillus*.—On the reaction-time of olfactory sensations, by M. Beaunis. He gives numerical results for this quantity (which is the time between sense-excitation and the moment when the person indicates by a signal that he has experienced the sensation) in the case of ten substances: ammonia, acetic acid, camphor, &c. They range from 37 to to 67 hundredths of a second. The time is longer than that for tactile, visual, and auditory sensations (in the author's case shorter than for tactile sensations).—On the respiration of aquatic plants or submerged aquatic-aërial plants, by M. Barthelemy. He considers the phenomena brought forward as proof and measure of the chlorophyllian function merely exceptional, and produced by the mode of experimentation. Under normal conditions, the special respiration of green organs cannot have the universal importance attributed to it.—Note on the morphological nature of the aërial branches of adult *Psilotum*, by M. Bertrand.—Influence of temperature on the production of wheat, by M. Duchaussoy. He gives in a table the yield of wheat in the department of Cher, and the mean temperature of spring and summer, from 1872 to 1881. The descending scale of the yield is nearly that of the mean temperature of summer. The years 1873 and 1876 are exceptions, and their small yield is explained by the dryness of the summer.

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THURSDAY, FEBRUARY 22, 1883

PROFESSOR HENRY SMITH

ON Friday, the 9th inst., we lost one of our most gifted men. By the death of Prof. Henry Smith there has dropped out from our roll-call a name which was already known among a wide circle of friends and admirers, but which would assuredly have been more widely known and more fully recognised if he had remained longer in our ranks.

Henry John Stephen Smith was born in Dublin, but when he was about two years old his family, at his father's death, removed to England. His precocity from the earliest age was remarkable; but what was perhaps still more remarkable, the talents which he thus showed did not, as is so often the case, fail him in after life. He was a fair-haired child, and was known among his relations as the "white crow." When he was two years old it was understood that he could read; and on his third birthday it was agreed that he should be tried, on the condition that, in the case of failure, the white crow should be allowed to fly out of the window, which was set open for the purpose. It is needless to add that there was no occasion for flight. At the age of four he was found one day lying flat on the floor, with his face raised slightly above his book (his sight being, even then, short) teaching himself Greek from an old-fashioned grammar full of antique contractions in the characters. His subsequent education was carried on until he was eleven by his mother, and then by tutors. For an account of the rapidity with which he galloped over the ground with one of them, we are indebted to an interesting letter in the *Times* of the 12th inst. With a view to his education the family removed to Oxford in 1840, whence he was transplanted to Rugby. He entered the school in August, 1841, the commencement of the last year of Dr. Arnold's Head Mastership, and was in the Boarding House of the late Rev. Henry Highton, who was himself an old Rugbeian, a pupil of Arnold, and Co-Exhibitioner from the school with the present Dean of Llandaff and the late Dean of Westminster, and had lately graduated at Oxford, taking a First Class in Classics and a Second in Mathematics. Henry Smith had been Highton's private pupil at Oxford, and was so well taught that when he entered Rugby he was (although only then fourteen) placed in the fifth form, which is the highest form but one below the sixth, and which, by the rules of the school, is the highest in which a new boy can be placed. He was distinguished at Rugby for his unvarying gentleness of character, and was a favourite alike with masters and boys. An old schoolfellow writes of him thus: "I was a young boy in the house, and remember being struck with his great gentleness and amiability. It did me good at once, and I felt it, as I believe, to my lasting benefit." He was always much attached to his old friend and tutor, Highton; and ever since the latter's death, in December, 1874, no one has shown more kindness to his widow and children than Henry Smith. At Rugby he progressed as rapidly as elsewhere, and was kept back from entering the sixth form under Arnold, only on account of his age. He was the first boy promoted to that form under Dr. Tait, Dr. Arnold's successor. Nothing in fact seemed capable of stopping his intellectual career. The death of his only brother and his consequent withdrawal from school, which would have thrown most boys entirely out of gear, did not interfere with his gaining, at the age of eighteen the scholarship at Balliol. A severe illness delayed his residence at college, but neither the malady itself, nor absence from England, nor severance from books prevented him in 1848, winning the blue ribbon in classics among Oxford undergraduates—the Ireland Scholarship. In 1850 he took his degree, obtaining an old-fashioned "Double First," namely, in classics and mathematics. The next year he gained the Senior Mathematical Scholarship; and if in this he had but few competitors, it was because his strength and powers were already known. After such a University career, almost unparalleled in the annals of Oxford, it seems but a natural consequence that he should be elected, as was the case, to a Fellowship at his College. In 1861 he was elected successor to the late Baden Powell in the Savilian Professorship of Geometry, which chair he retained until his death. With a view to relieving him from the labour and duties of College tuition, which he had faithfully discharged for five-and-twenty years, Corpus Christi College offered him a Fellowship free from such duties. Notwithstanding his regret at leaving (although, as it subsequently proved, temporarily) his old college, he decided, having reference to the growing calls upon his time, to accept the offer. But Balliol, unwilling to lose all connection with its distinguished alumnus, afterwards bestowed upon him an honorary Fellowship, and, under the recent Statutes, a full Fellowship without emolument.

The malady under which he ultimately sank may be considered hereditary, for his father died from the same cause, and the son showed symptoms of it even at an early age. It is idle now to speculate whether a quieter or less exhausting life would have prolonged his years. There is some truth in the idea that a man can first and last perform a certain amount of work and no more. On this supposition it may be even a gain to the individual to have performed his task in the minimum of time, while those who remain must rest thankful at having lived in his day, and having retained him amongst us as long as was the case.

The testimony of his friends to his ability and other qualities is from all quarters abundant. Prof. Huxley writes: "Henry Smith impressed me as one of the ablest men I ever met with; and the effect of his great powers was almost whimsically exaggerated by his extreme gentleness of manner, and the playful way in which his epigrams were scattered about. They were so bright and sharp that they transfixed their object without hurting him. I think that he would have been one of the greatest men of our time, if he had added to his wonderfully keen intellect and strangely varied and extensive knowledge the power of caring very strongly about the attainment of any object." Although the present writer is not likely to differ much from Prof. Huxley in his estimate of the man, he would still suggest that Henry Smith's care for the attainment of an object was measured rather by his estimate of its ultimate value than by its present advantage. For those who knew him best were most fully aware of the effort which it cost him to postpone (as he

often did, with apparent readiness) his beloved mathematics to other claims. Another friend says: "He was a man of rare powers, and as guileless as he was richly gifted."

Of some men it is said that they were never young, of others that they became old while their contemporaries were still lads; and it has been stated as a general law, in scientific thought at least, that the best and most original ideas have always been conceived before the age of thirty. But whatever may be the case in this respect with the generality of men, Henry Smith was as young and vigorous in intellect at the age of fifty-six, the limit to which he attained, as he was when he gained the first of his many University honours. It was his freshness of mind, his vivid appreciation and intelligent enjoyment of everything going on, not only in science, but also in life, whether social or political, which made us forget that his years, like ours, were passing away, and that the number of them was finite. It was his genial presence, his sympathetic attention, his ready counsel, his sound judgment, his happy mode of dealing with both men and things, which make us already feel a loss which we cannot as yet fully appreciate, but which we can never hope again completely to replace.

Of many Greek towns it is related that each has claimed for itself the honour of having been the birthplace of Homer; in like manner, many branches of knowledge, and avocations of life, might claim to have been the favourite pursuit of Henry Smith. But however proficient, or even prominent he may have been in other subjects, it was in mathematics that he mainly showed the originality of his genius, and that he has left any permanent record of work of the highest kind.

Among the great works which it was long hoped that he would have accomplished was his treatise on the Theory of Numbers. This subject, which during the present generation has been so marvellously generalised as to undergo a complete transfiguration since it was presented to us in the work of Barlow and in the ordinary educational books on Algebra, formed for many years a serious study on the part of Prof. Henry Smith. The papers in which the researches of mathematicians on this subject are recorded are scattered through the pages of various periodicals, so that it is not easy to realise the steps which each writer has contributed to the general progress, nor to assign to each his relative position. But this is not all, nor even the worst; it has been a prevailing custom, too prevalent we think, among mathematicians of late years, to publish results alone, without proof of their statements, and even without indication of the train of argument which led them to their conclusions. This naturally entails on the part of the reader either a strong act of faith or a difficult and, as we hold, unnecessary effort. It need hardly be added that in endeavouring to digest and present to his readers what had been done by others in his subject, Henry Smith adopted the latter course; and, with a sagacity in which few could have rivalled him, he assimilated all these fragments, and utilising the valuable among these *disjecta membra*, and rejecting the worthless, he brought them into harmony, and was in a fair way to produce from them a structure intelligible in itself, and capable of forming a groundwork for further developments. But while our author was discussing what had been already done, the very materials upon which he was engaged were growing apace, and his self-imposed task accumulated upon him. Of unfinished work, or of "ragged ends" as he used to call them, he was as nearly intolerant as he could be of anything; and it is not clearly known whether he ever made up his mind to complete what he had undertaken up to a certain date or not. In any case what he had already long ago achieved in this matter must have been a gigantic work; and it remains only to hope that his manuscripts have been left in such a state that others may be able to wield the weapons which he had forged.

The results of his preliminary studies were given in his six invaluable Reports on the Theory of Numbers, published in the volumes of the British Association for 1859 and following nearly consecutive years; and these alone are sufficient to show the extent of his reading and the firm grasp which he had of the subject. The following extracts from the first and third of these Reports indicate both the wide range of the theory and the magnitude of the portion which still remains to be achieved:—

"There are two principal branches of the higher arithmetic: the Theory of Congruences and the Theory of Homogeneous Forms. In a general point of view these two theories are hardly more distinct from one another than are in algebra the two theories to which they respectively correspond, namely, the Theory of Equations and that of Homogeneous Functions; and it might, at first sight, appear as if there were not sufficient foundation for the distinction. But, in the present state of our knowledge, the methods applicable to, and the researches suggested by, these two problems, are sufficiently distinct to justify their separation from one another."

"It is hardly necessary to state that what has been done towards obtaining a complete solution of the Representation of Numbers by Forms, and the Transformation of Forms, is but very little compared with what remains to be done. Our knowledge of the algebra of homogeneous forms (notwithstanding the accessions which it has received in recent times [1861]), is far too incomplete to enable us even to attempt a solution of them co-extensive with their general expression. And even if our algebra were so far advanced as to supply us with that knowledge of the invariants and other concomitants of homogeneous forms, which is an essential preliminary to an investigation of their arithmetical properties, it is probable that this arithmetical investigation itself would present equal difficulties. The science, therefore, has as yet had to confine itself to the study of particular sorts of forms; and of these (excepting linear forms, and forms containing only one indeterminate) the only sort of which our knowledge can be said to have any approach to completeness are the binary quadratic forms, the first in order of simplicity, as they doubtless are in importance."

Prof. Smith's sphere of utility was, as indeed is pretty well known, not confined to his University, nor to science as such, but extended, among other directions, even to departments of the State. Passing over the Royal Commissions on Scientific Education and on the University of Oxford, of both of which he was a leading member, mention must not be omitted of the Meteorological Council of which he was chairman. That body, nominated by the Royal Society, but appointed by the Government,

holds a position intermediate between a public department and an independent institution. While on the one hand this intermediate position presents some advantages, at all events in the present stage of the subject as a science, it undoubtedly, on the other, requires no inconsiderable tact and judgment in its management. For the yearly administration of a large sum of public money, for the management of a considerable staff at home, and of a variety of observers at out-stations in all parts of the country, and for communication with similar departments of State in foreign countries, science alone would not have sufficed. But at the same time few branches of natural knowledge stand more in need of a strong scientific guide to keep it from the crotchets of dabblers in the subject, or from relapsing into an indiscriminate accumulation of loose observations from which no valuable result can ever be derived. For this post the President and Council of the Royal Society unanimously nominated him, nor had they ever reason to regret the step which they then took.

The case of the Meteorological Council was, however, but one instance out of many in which his name came uppermost in the minds of men when they were looking for a leader, or a chairman, or a president. Whether as President of the Mathematical Society (1874-6), or of the Mathematical and Physical Section of the British Association (1873), or as Chairman of Committees too many to enumerate, he always succeeded in commanding the respect of those with whom he was associated, and in carrying through the business to a satisfactory issue.

In one matter only did he fail of success; but in that case the failure was not really his, but that of those who should have given him support. The case was that of his candidature for the representation of the University of Oxford, when, in 1878, Lord Cranbrook received his peerage. Instances of the candidature of leading University men, both in Oxford and in Cambridge, have not been unknown, from the time of the late Sir John Lefevre to that of Prof. Stuart; but all have terminated in the same result, namely, the total defeat of every man of University distinction, whatever other qualifications he may have for the office. With these instances we may compare, not without interest and instruction, the choice which has been made by the University of London on the only two occasions on which a vacancy has yet occurred.

It was sometimes thought that his mind became diverted from mathematics by his many other distracting avocations; there are, however, reasons for doubting this. It is true that he did not pour out the amount of mathematical papers of which he was certainly capable; but those which he did publish showed that he cared little to add fringe-work to the borders of our knowledge, and that he reserved himself for questions of real importance. We remember his alluding to the subject of one of his later papers contributed to the Mathematical Society, on Modular Equations, as relating to "a point on which people had puzzled themselves for a long time," and the following passages from his address to the London Mathematical Society were certainly not penned by a president for whom that subject had lost its charm. "Of all branches of mathematical inquiry, this is the most remote from practical applications; and yet, more perhaps than any other, it has kindled an extraordinary enthusiasm in the minds of some of the greatest mathematicians." Then he quotes Gauss as having held Mathematics to be the Queen of the Sciences, and Arithmetic to be the Queen of Mathematics. I do not know that the great achievements of such men as Tchébychef and Riemann can fairly be cited to encourage less highly gifted investigators; but at least they may serve to show two things—first, that nature has placed no insuperable barrier against the further advance of mathematical science in this direction; and secondly, that the boundaries of our present knowledge lie so close at hand that the inquirer has no very long journey to take before he finds himself in the unknown land. It is this peculiarity, perhaps, which gives such perpetual freshness to the higher arithmetic. It is one of the oldest branches perhaps the very oldest branch, of human knowledge; and yet some of its most abstruse secrets lie close to its tritest truths. I do not know that a more striking example of this could be found than that which is furnished by the theorem of M. Tchébychef. To understand his demonstration requires only such algebra and mathematics as are at the command of many a schoolboy; and the method itself might have been invented by a schoolboy, if there were again a schoolboy with such an early maturity of genius as characterised Pascal, Gauss, or Evariste Galois."

The following is another instance of the interest which he retained in mathematics to the very last. In the address above quoted he alluded to a problem, at that time still unsolved, in the following terms:—"It was first shown by M. Liouville that irrational quantities exist which cannot be roots of any equation whatever, having integral coefficients. We may perhaps be allowed to designate by the terms arithmetical and transcendental the two classes of irrational quantities which M. Liouville has taught us to distinguish; and it becomes a problem of great interest to decide to which of these two classes we are to assign the irrational numbers, such as e and π, which have acquired a fundamental importance in analysis. To Lambert, the eminent Berlin mathematician of last century, the first great step in this direction is due. He showed that neither π nor π^2 is rational; with regard to e he was even more successful, for he was able to prove that no power of e, of which the exponent is rational, can itself be rational. There (with one slight exception) the question remained for more than a century; and it was reserved for M. Hermite, in the year 1873, to complete, by a singularly profound and beautiful analysis, the exponential theorem of Lambert, and to prove that the base of the Napierian logarithms is a transcendental irrational. But, in a letter to M. Borchardt, M. Hermite declines to enter on a similar research with regard to the number π. 'Je ne me hasarderai point,' he says, 'à la recherche d'une démonstration de la transcendence du nombre π. Que d'autres tentent l'entreprise; nul ne sera plus heureux que moi de leur succès; mais croyez m'en, mon cher ami, il ne laissera pas que de leur en coûter quelques efforts.' It is a little mortifying to the pride which mathematicians naturally feel in the advance of their science to find that no progress should have been made for a hundred years and more toward answering the last question, which still

remains unanswered, with regard to the rectification of the circle."

Last year, Lindemann, starting from Hermite's researches, succeeded in supplying the proof required with reference to the number π. And while speaking of this achievement with the satisfaction which his generous nature prompted, Henry Smith added that it was a problem on which he had long fixed his eye with a view to attacking it seriously so soon as he had leisure for the undertaking.

He was doubtless then looking forward to some University vacation; for vacation time formed the period for his original investigations, while term time was devoted to current work and to society, which he himself so keenly enjoyed, and in which he was always an honoured and a welcome guest.

It has been much the fashion of late years to raise memorials to the departed; and in some cases it may be doubted whether a wise discrimination has been exercised in the matter. No one, however, who has any interest in science, would doubt for a moment that the memory of Henry Smith was in the highest degree deserving of perpetuation. But in our opinion the best and only suitable memorial of him will be the publication of his works, in the fullest and most complete form of which they are now capable. And it is sincerely to be hoped that his MSS. may be placed in the hands of a mathematician who may prefix to them an introduction as worthy of these works as was Prof. Smith's introduction to the remains of Clifford.

During his last few years he lived, as Keeper of the University Museum, at the house adjoining the main building, previously occupied by his predecessor, John Phillips. His companion was his sister, whose useful and sympathetic life worthily supplemented his own. It is to be hoped when the wave of sorrow which is now passing over her has in some degree subsided, and when time has brought an alleviation which may now seem impossible, that she may derive satisfaction, although it be a melancholy one, in having learnt through the event how much her brother was appreciated and beloved by many, and by some even unknown, friends.

W. SPOTTISWOODE

PUBLIC ELECTRIC LIGHTING

MUCH attention is being given at the present moment to the operation of the Electric Lighting Act passed during the last session of Parliament. Under the terms of that Act, licenses and provisional orders will be granted to local authorities, companies, and private individuals to supply electricity for the purpose of electric lighting over definite areas. A large number of applications for licenses and provisional orders have already been submitted to the Board of Trade, in a few instances by local authorities, but in the majority of cases by joint-stock companies formed for working one or other of the different systems for electric lighting. A number of the "Provisional Orders" now being promoted lie before us, the majority of them being drawn in almost identical terms. A perusal of these documents cannot fail to impress the reader, firstly, with the great complexity of the question, secondly, with the extreme difficulty of striking a fair balance between vested interests and public convenience, thirdly, with the great amount of knowledge and skill displayed in the drafting of these provisional orders. It is an open secret that not only the main outlines but also most of the details of these orders are from the hand of Mr. J. Fletcher Moulton, F.R.S., whom we must congratulate upon the success with which he has applied himself to the task of preparing them. Now that Parliament is once more in session we shall probably hear of further legislative proposals; but if all provisional orders are as well and as wisely drawn as the majority of those before us appear to be, there can be little doubt that the necessity for separate further legislation and for the promotion of private bills for electric lighting will be removed.

As to the provisional orders themselves it would be impossible within reasonable limits to deal with a tithe of the important topics which are therein set forth. Many of the provisions are naturally directed toward questions of municipal rights and parochial law. Leaving aside all these matters we come to the more scientific points. Four separate systems of distribution are recognised in the provisional orders. These are (*a*) "direct" system, more familiar under the name of distribution in parallel arc, with "distributing mains" throwing off "service lines" for individual consumers; (*b*) "storage" system, with service lines in parallel arc from storage batteries charged in series intermittently from a generating station; (*c*) either of the above with "earth" returns; (*d*) "series" system, supplying customers in one undivided circuit. We may remark in passing that it appears to us that the use of "earth" for return should be in every case forbidden. If currents of the intensity employed for electric lighting are sent through earth in our crowded cities we shall have constant derangements of telegraphs, telephones, electric bells, in fact of all electric appliances which work by feeble currents and use earth returns. Moreover, as "earth" in practice means usually the employment of existing gas-pipes or water-pipes as returns the proposal to utilise "earth" for electric light returns, is doubly to be deprecated. Amongst other limitations set forth in the provisional orders are some which bind the "undertakers" to lay down their mains within two years, some which prescribe the hours during which the supply of currents must be maintained, and some which limit the conditions of supply. Amongst the latter we observe in several of the orders before us that it is proposed that "the potential at corresponding points of the positive and negative distributing mains shall differ at each point by a constant difference, not being less than thirty volts, and not being more than four hundred volts." And that "such constant difference of potential" is to be termed "the standard pressure." It is to be hoped that the Board of Trade will be much more precise in its limitations. Thirty volts is so low a "pressure" as to be practically out of the question except with a gigantic outlay in copper conductors, whilst four hundred volts is equally inadmissible on account of the danger to person. No less an authority than Sir W. Thomson has said that nothing above two hundred volts should ever be admitted into a dwelling-house. The provision that "the standard pressure may be different for different points of the said mains, and for different hours during the period of supply"

is bad, and if permitted will greatly militate against convenience and uniformity in using the current both for light and for motive-power. Where the undertakers distribute "alternating" currents it is provided that the mains should have a "constant (?) difference of potential" or standard pressure of not less than forty-five and not more than six hundred volts. Here again we think that the Board of Trade might very wisely insist on a further restriction. If steady currents at a pressure of four hundred volts are dangerous, alternating currents at four hundred are far more so. Yet here the undertakers talk of six hundred! Indeed, considering the risks involved, and the difficulty in distributing alternating currents through long lines or lines where there is great self-induction; and also considering that the supply of electric currents is not for lighting alone but for the providing also of motive-power, it would not be any loss to the public if the use of alternating currents under the provisional orders were absolutely disallowed. It is true that the patentees of certain specific forms of machine might cry out loudly against such a prohibition; but the public would be guaranteed against one source of danger and difficulty. According to the orders the undertakers may charge consumers either by the amount of electric energy consumed, or by the quantity of electricity supplied, or by time, or by a yearly agreement. In connection with the first of these methods the proposal is made to call by the name "one unit" the energy contained in a current of 1000 amperes flowing under an electromotive force of one volt during one hour. Most of the provisional orders name sevenpence per unit as the price of electrical energy. We have here for the first time an actual quotation-price for *energy*; a fact which should be interesting to those who have striven so hard to drive into the popular mind exact ideas concerning energy and its conservation. One "unit" thus defined for commercial purposes being 1000 volt-amperes (*i.e.* 1000 watts) for one hour, and one horse-power being 746 watts, we see that the scale of payment is about 5⅓ pence per hour per (electrical) horse-power.

Into the further provisions for the inspection and testing of mains, the inspection of meters, the testing of insulation, provisions for safety, and penalties for default in supply, we cannot here enter. Suffice it to say that there is no detail that does not appear to have had thought expended upon it, no provision that is really superfluous or harassing, no possible want or eventuality that does not appear to have been anticipated. Such masterly treatment cannot but greatly facilitate the work of the Board of Trade in agreeing to orders and licenses, and will tend to bring about unity of method in the organisation of the actual work of laying down town supplies so soon as such orders and licenses shall have been granted. If it be true that the effect of the Electric Lighting Act has been to produce a temporary lull in the progress of electric lighting, we are convinced that such a lull will be in the end an unmixed good, since it gives the opportunity for thought to ripen, and for projects and inventions to mature, if not to survive. Two dangers indeed seem yet possible in the future of public electric lighting, and either of them may be sufficiently serious to damage public confidence in this new industrial factor. Firstly, some better guarantees ought to be insisted on that the Companies or other parties who obtain orders or licenses as undertakers should be possessed of capital adequate to carry out the projects in hand. A very hasty glance at the list of applicants for provisional orders will suffice to show that this fear is not unfounded. Secondly, it ought to be made impossible for a Company which has obtained an order for any limited district to delegate the responsibility of supplying any section of such district to a sub-company. No Company should be allowed to hold a monopoly (if the limited monopoly created by the provisions of the Electric Lighting Act be a monopoly at all) of a single square yard of territory which it cannot with its own resources supply under the terms of the order or license which has been granted. If this principle be not upheld, serious abuses will creep in, to the detriment of progress and in contravention of the interests of the public.

CRYPTOGAMIC FLORA OF GERMANY, AUSTRIA, AND SWITZERLAND

Dr. L. Rabenhorst's Kryptogamen-Flora von Deutschland, Oesterreich, und der Schweiz. Zweiter Band: Die Meeresalgen Deutschlands und Oesterreichs. Bearbeitet von Ferdinand Hauck. 1-3 Lieferung. (Leipzig: Eduard Kummer, 1883.)

SINCE the appearance of the original work (1845-53) the systematic study of living algæ has, through a more accurate knowledge of the structure and fructification of these plants, led to great changes in their diagnosis and classification. Hence the necessity of a new edition of Rabenhorst's work.

In order to render it more valuable, the preparation of the parts of which it is composed have been intrusted to authors specially conversant with the subjects of which they treat. The first volume, five numbers of which have already appeared, contains the Fungi, and is edited by Dr. G. Winter of Zurich; the second comprises the Marine Algæ (exclusive of the Diatomaceæ); then will follow the Fresh-water Algæ, edited by Herr Paul Richter of Leipzig; the Diatomaceæ, by Dr. A. Grunow of Vienna; and the Frondose Mosses and Hepaticæ, by Herr G. Limpricht of Breslau. To these will succeed works on the Lichens, Charicæ, and Vascular Cryptogams.

The second volume, which forms the immediate subject of this notice, has been intrusted to M. F. Hauck, who, from his residence on the coast at Triest, has, during many years, had opportunities of studying marine algæ in a living state; and by his connection with German and other algologists, has been able to obtain authentic examples of most of the species. It may also be mentioned that M. Hauck has published "A List of the Algæ of the Adriatic" (*Beitr. z. Kenntn. d. adriat. Algen*, Wien, 1878).

The present work, of which three numbers have appeared, includes not only the algæ inhabiting the Austrian coast and islands of the Adriatic, but also those of the Baltic and North Seas, and the coasts of Heligoland with the adjacent islands: the latter have been found especially rich in species.

In the Introduction to his work, M. Hauck gives instructions for the collection and preparation of the various

kinds of marine algæ. The list of instruments and appliances used in collecting is rather formidable, but it must be remembered that the object of the algologist is to obtain specimens in as perfect a state as possible, for the purpose of instituting a searching examination into the structure and fructification of the plants; and this cannot be done without much labour and pains. In the case of small plants which adhere closely and spread over rocks and other objects, M. Hauck recommends that, instead of scraping off the algæ, portions of the rocks on which they grow should be chipped away with a geological hammer, and preserved with the growing plants upon them. Directions are also given for the treatment of the Corallinæ and other algæ which are covered with carbonate of lime, in order to divest them of the lime, and thus prepare them for microscopic examination. There are also instructions for preparing and mounting specimens of algæ for the microscope.

Every one who has endeavoured to cut sections of algæ for microscopic observation, must be aware of the difficulty, occasioned by the different structures of the plants, of performing this operation. The author shows how some of these difficulties may be avoided; but he has omitted to mention whether the sections should be made with a machine, or in the old-fashioned way, by holding the portion to be cut firmly with the forefinger nail of the left hand, while cutting the section with a sharp, thin knife.

We now come to the work itself. M. Hauck thus classifies the marine algæ: I. RHODOPHYCEÆ, plasma coloured red; II. PHÆOPHYCEÆ, plasma coloured brown; III. CHLOROPHYCEÆ, plasma chlorophyll-green; IV. CYANOPHYCEÆ, plasma bluish-green. Commencing with the Rhodophyceæ, he treats of the Florideæ, describing their structure and fructification. A summary of the families, with the names of the genera contained in each family, follows. M. Hauck's classification of the Florideæ is novel; it remains to be seen whether it will meet with the general approval of algologists. We have next a description of the genera and species. This part of the work is illustrated with figures drawn on zinc, of at least one species of each genus, as seen by transmitted, not reflected, light, the objects being represented as if transparent. Some of these illustrations are original, but the greater part are borrowed from Kützing, Thuret, Zanardini, and others. They are inserted in the text near to the species delineated,—an extremely convenient arrangement.

Besides these illustrations, there are five plates, representing different species of Lithophyllum and Lithothamnion. They were printed by the "Albertotype" process, from negatives executed under the supervision of the author. These plates are admirable, and give more correct and characteristic figures of these singular and in this country but little-known vegetable productions than can be obtained by any other process. Several species of Lithophyllum and Lithothamnion have been found in our seas, and it is probable that more would be found if sought for. They abound in the Adriatic and Mediterranean, and some species are known on the French coast.

M. Hauck seems to have bestowed much pains and care in the preparation of the work, and it will be seen that he has added very considerably to our knowledge of the fructification of numerous species. It may, however, be as well to remind him that the cystocarpic fruit of *Callithamnion Thuyoides*, *Call. polyspermum*, *Call. Borreri*, *Ceramium tenuissimum*, and *Grateloupia filicina*, which he does not mention, were described and figured in Harvey's *Phyc. Brit.* (Pls. 269, 281, 259, 90, 100). Also that the tetraspores of Nemaleon, which M. Hauck says (pp. 14, 59) are unknown, were described by Dr. Agardh, who had examined the living plant (see "Sp. Gen. et Ord. Algarum," vol. ii. p. 417 (1852).

It is to be hoped that we have found in this work the solution of a problem which for a long time has exercised the minds of algologists, namely, Does Porphyra belong to the Chlorosperms or to the Florideæ?

Although the colouring of Porphyra assimilates it to the Florideæ, yet the apparent agreement of its vegetative structure with that of the Ulvas, and especially of some of the species of Monostroma, had induced the elder algologists to place Porphyra among the Chlorophyllaceæ. The discovery of the fructification of the plants of both genera has however shown that they are widely separated. In Monostroma the only kind of fructification known consists of zoospores, which, when they first issue from the mother-cell, are endowed with active motion. In Porphyra the tetraspores were first discovered, then the antheridia; the antherozoïds are motionless. Algologists, however, still hesitated to admit Porphyra among the Florideæ, because no cystocarpic fruit had yet been found. M. Hauck now tells us that the cystocarps of some species are known (p. 21), and he describes those of *P. leucosticta*, as well as the tetraspores and antheridia of this plant (p. 25). There can, therefore, be no longer any hesitation as to including Porphyra among the Florideæ, of which it constitutes the lowest family.

On looking through the present instalment of this work, it will be seen that out of the 122 species, or thereabouts, which are described in it, about seventy are found on the British coasts—nineteen of the latter are common to the North Sea and Adriatic—twenty-seven of them inhabit the Adriatic, and twenty-four the North Sea. The work, when complete, cannot fail therefore to prove of great interest to algologists in this country.

The type is good, as well as the figures with which it is illustrated, and readers will no doubt be glad to know that in the printing German characters have not been used.

MARY P. MERRIFIELD

THE CHURCHMAN'S ALMANAC

The Churchman's Almanac for Eight Centuries (1201 *to* 2000), *giving the Name and Date of every Sunday.* By W. A. Whitworth. Pp. 23. (London: Wells Gardner, Darton, and Co., 1883.)

THERE never surely was such an age of almanacs. The social change whose effects meet us on every side has worked a revolution here. Some of us can call to mind the time when "Old Moore" ruled the reckoning in his peculiar, old-fashioned way, and Murphy blazed out like a meteor to expire like a farthing candle, and Zadkiel "Tao Sze" began to trade on human curiosity and credulity. But those days are past. Instead of being left, as of old, to make our own quiet, though limited, choice,

as the year draws to its close we find ourselves surrounded by a swarm of calendars; the insurance-office, the journalist, the general-storekeeper, the stationer, the watchmaker, the grocer, all vie in pressing on our acceptance something to remind us how time flies; often padded with most irrelevant pieces of innovation, but sometimes, it must be owned, got up in a very attractive form. We do not quite see how all this can be made to pay. We should have thought it a very expensive and often uncalled-for mode of advertising. But that is no affair of ours. Living in "a nation of shopkeepers," whatever may be our private impressions, we are bound to believe that it is found a remunerative mode of expressing gratitude, or anxiety, as the case may be. But whatever may be the donor's purpose it is not quite easy to see what corresponding purpose is, generally speaking, to be answered on the part of the receiver: for, with certain exceptions, it really signifies very little to the bulk of the community, how the fifty-two ensuing weeks are arranged. One great exception of course is the festival of Easter, and the others that depend upon it. But as to these there is always a sufficient general understanding, as there was in our least educated days, when there were comparatively few that knew how to use a calendar: and as to the phases of the moon, the only other leading feature in ordinary almanacs, their notification is rather convenient than necessary, excepting for those who believe, as old-fashioned people still do, with Prince Bismarck at their head, that the moon has an influence distinct from its attractive power. But, say what we will against the necessity of a general diffusion of almanacs, public feeling is on the other side, and even those who could do very well without these favourite articles, and seldom refer to them, would not feel satisfied if they did not possess them.

One curious feature in the case, however, is that so few comparatively have any correct idea of the principles on which almanac-making proceeds. We suspect that even among such as pass for educated people it would be easy to find those who would not be very comfortable if they were required to explain the want of correspondence between the reckoning by weeks and that by months, the unequal length of the latter, the necessity of intercalation, or the cause of the difference between the "styles"—important as that was thought in its day, even to the excitement of popular indignation. As to such matters, if it is true that "we take no note of time but by its loss," it is nearly as true of a large portion of even civilised society, that they take no note of the arrangement of time—except perhaps by misunderstanding it. However, there is no excuse for such ignorance (if we may be forgiven the expression) for the future, if people will take the trouble of referring to the little work whose title we have quoted above. It will not indeed enlighten us much as to the root of all the difficulties—the incommensurable durations of the day and month and year, or help us to make out the strange old machinery of cycles and epacts and golden numbers by which the calendar was kept right, but it will do what is practically of much more value, set before us something of the processes, and all needful results, of the most accurate computations.

The title was a puzzle to us at first, for we had been for so many years acquainted with a very unpretending though most useful *Churchman's Almanac*, that we did not comprehend how it should now find its place in NATURE, till we remarked the continuation of the title; this, promising perennial instead of annual information, at once made a claim to attention which we find is well deserved. There are a good many curious and out-of-the way pieces of information in the three pages of introduction—among which we may mention the explanation of the reason, hitherto to us so incomprehensible, why the accounts of the public revenue are made up to the odd-looking epoch of April 5—and this is followed by a perpetual calendar, as far as Sundays are concerned, to a period that the youngest now living will never see; while, for historical purposes, the retrospective portion is an authentic and valuable resource as to many indefinite matters in chronology, the correct determination of which, as antiquaries well know, often involves considerable trouble. The author has fulfilled his undertaking, as far as we can judge, with especial care and attention; and if his work, which is one of the thinnest of folios, is so far less in accordance with the ideas of this "handy-book"-loving age, we must bear in mind that the form was imposed by the extent of its tabular matter, and that though there is little to attract in its formidable array of figures, its intrinsic value is, for those who need such aid, of a high and enduring character.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Hovering of Birds

MR. AIRY asks for a diagram explaining my views as to the hovering of the kestrel and of other birds, asserting at the same time that these views would establish a "miracle."

If Mr. Airy will be so good as to look at the beautiful drawing of a kestrel in the act of hovering, by Mr. W. Wolf, at p. 160 of the "Reign of Law," he will see an illustration far better than any diagram. Mr. Wolf is an excellent naturalist as well as an accomplished artist, and his drawing of the kestrel was made to represent his own knowledge and observation of the act of hovering, and not to set off any theory of mine.

It will be seen that the body of the bird is represented as at a considerable angle to the horizon, and (of course) to any horizontal current of wind.

It is by placing itself in this position to the wind, and by a wing-action accurately proportioned to the strength of the breeze, that the bird accomplishes the feat of hovering—which is no miracle, but the mechanical result of the "resolution of forces."

The hovering of a boy's kite is a miracle of the same kind. The element of weight is here represented by the string, held at the surface of the ground.

Mr. Airy is, however, mistaken in his description of the facts. He speaks of hovering being performed with "wings motionless, not fluttering." Now I have never seen a kestrel's wings motionless when hovering. Always when the air is still, and always when the breeze is only moderate, the wings have a rapid and tremulous action, varying from moment to moment according as the "muscular sense" directs it, and feels it to be needed for the "poise." But sometimes when the breeze is very stiff this action may be suspended for a moment or two. I have seen this occasionally. But even in this case I could detect the quivering of the quills.

The sea-swallows perform the evolution perpetually over the water when it is as still as a millpond. In all cases the inclined

position of the body of the bird to the plane of the horizon is observable. The miracle is always performed by the use of the appropriate means. ARGYLL

Cannes, February 12

I AGREE with "J. R." that the term "hovering" is likely to be misunderstood. I used it because it had been used in the earlier correspondence in NATURE to which I referred. If "J. R." (or any other of your correspondents on this subject) has never seen a hawk hanging in motionless poise above a hillside, I would ask leave to refer him to NATURE, vol. viii. pages 86 and 324, for a description of the act.

February 19 HUBERT AIRY

I HAD once a very unusual opportunity of observing accurately the flight of buzzards, from the summit of Acro-Corinthus. As this unique natural fortress rises sheer from the plain, on the side toward Attica, to the height of eighteen or nineteen hundred feet, a group of these birds, hanging at that height above the surface, were thus brought in a line with the eye. I could detect the minutest movement of wings or tail. Again and again there were considerable intervals, of many seconds' duration, during which one bird and another would hang, with pinions horizontally outstretched, absolutely motionless, neither descending nor drifting, but as if his balance in the air were one of delicately adjusted equipoise. And when, by a just perceptible movement of wing, he stirred again, it seemed rather to be to change his position than that he needed any kind or degree of effort to maintain it. The kestrel is an unfortunately chosen bird for Mr. Hubert Airy's observation, because though it hangs for a minute or two over the same spot watching its prey, it is always "by short and rapid motion of its wings"; from which fanning motion it has acquired, I think, its popular name of windhover, and not because, as Mr. Airy supposes, it is upborne by the wind. But were my Corinthian buzzards upborne by the wind? There was none. The day was one of dead calm. No doubt of necessity there was some upward current of air from the sun-warmed surface of the ground by which the birds profited; but if at all sufficient to sustain them, their actual gravity, when in that position and so willing it (by which I mean nothing so absurd as that gravitation can be counteracted by the *vis viæ*, but that by inflating its lungs, and perhaps suspending its respiration, the bird may have the power at will of lessening its comparative weight in the air), must be very near to that of the atmosphere around and underneath them. It is evident that Mr. Airy could only claim my observation as being in favour of his theory if there had been a breeze from Attica striking against the face of the citadel. There was none perceptible; and I drew the attention of my companions to the curious problem presented by such an ease of flight. HENRY CECIL

Bregner, Bournemouth, February 13

P.S.—Will you allow me just to mention that the letter reprinted from NATURE by Dr. George J. Romanes in his "Animal Intelligence," as mine, is by Mr. Merlin, our present Consul in Athens. I sent it, but he wrote it, and the observation is wholly his.

The Auroral Meteoric Phenomena of November 17, 1882

MR. BACKHOUSE remarks in his letter (NATURE, vol. xxvii. p. 315): "It would be well to ascertain whether such a motion (in a curve) would not agree better with the observations of the beam than Dr. Groneman's hypothesis that it was a straight line."

When a straight line lies within or without the (celestial) sphere, on whose surface we wish to trace the perspective projection of that line (the eye being placed in the centre of the sphere), the perspective of the line will of course always be a great circle. When inversely the apparent path of the same meteor, seen from *one* place of observation is a great circle, the true path must lie in a plane. When the apparent paths, seen at the same time from two different places, not situated in the direction of the apparent path, are both great circles, the true path lies in two different planes, and *must be a straight line.* Now Prof. Oudemans at Utrecht says positively that the apparent path of the phenomena of November 17 was a great circle, cutting the horizon (and also the equator) in two opposite points. Of the English observers I will cite Mr. Saxby (p. 86), who describes "the trajectory as much flatter than that of the stars." Moreover the general fact is, as I proved in my paper, that this trajectory, having been seen of regular form and consequently probably of equal curvature in its whole length, intersected the great circle of the horizon in two opposite points, and therefore must have been a great circle itself. The abovementioned condition being fulfilled, I was under the necessity of taking the true path as a right one. I think this peculiarity indicates the meteoric nature of the phenomenon and of all the auroral arcs (*les arcs proprement dites* of my theory) showing as great circles of the sphere. In fact a curve cited by Mr. Backhouse, lying at equal height above a terrestrial parallel, will show itself *but in one case* as a great circle, namely where the observer is within its plane. From all other places it will be seen as a small circle of the sphere. In this case is the apparent boundary of an aurora in the north, the arch of the dark segment cutting the horizon in two not opposite points.

I dare not occupy more space to answer Mr. Backhouse further on the influence exercised by cosmic matter on terrestrial magnetism, and the consequence of the general direction east to west of these currents when passing in the neighbourhood of the earth, but I think that this direction east to west *must* be deduced from the observed facts.

I am much obliged to Messrs. Petrie and Muirhead for their information. As to the remark of the former on the spectrum observed by Dr. Rand Capron, I think that the auroral character of some phenomena will be proved the best when it shows the auroral lines, whatever may be the origin of its light. When its other properties point out its meteoric character, a strong argument is found in favour of the cosmic theory of auroræ. H. J. H. GRONEMAN

Groningen, Netherlands, February 14

The Orbit of the Great Comet of 1882

I AM very much obliged to those gentlemen who have kindly given me the information required in my letter published in NATURE, vol. xxvii. p. 314.

They all agree on the same point, which confirms my opinion that in all the good observations the same or very nearly the same point of the head was observed during the brightest appearance of the comet.

I remarked especially in the sketches shown to me by Mr. A. A. Common, who was the first to see the comet in England, on September 16, and who continually made careful observations of it, that, although the nucleus was seen since October 30 divided into two parts, always one of these (which I shall call the main part next to the following end of the nucleus) remained the brightest. Mr. Common in every drawing marks this part with the word "brightest." At the Washington Observatory also this same bright point was always observed with the transit instrument, as it is stated by Mr. W. C. Winlock in his letter (NATURE, vol. xxvii. p. 129).

Mr. W. L. Elkin, Cape Observatory, in a communication to the *Astronomische Nachrichten*, No. 2490, speaks about this orbit. He used the first observation made at the Cape on September 8, the observation (of the) disappearance of the comet at the sun's limb on the 17th of the same month, and a normal observation on November 17, to calculate either a parabolic orbit or an elliptic one; but none of these gave the positions of the comet according to intermediate observations.

Mr. Elkin believes it is possible to take as the most probable value of $\frac{1}{a}$ the value 0·0075, and consequently the comet has a very long period, while Mr. Morrison in his calculation of the orbit had $e = 0{\cdot}9998968$, and a period of 652·5 years.

As errors of observations are of course inadmissible, it is now the question to study what produces such great differences in calculating the orbits.

Are they due to disturbances during the comet's passage through the solar system, and especially at its passage through the sun's corona? or are they due to the hypothesis specified by Mr. Elkin and others that the centre of the nucleus is not the point gravitating around the sun? This question cannot be decided but by a careful discussion of all the positions of the comet during the whole period.

The observations before perihelion are of course very important. Unfortunately at the Cape the astronomers were prevented making observations between September 8 and September 17 because of bad weather; but there are some observations made in Melbourne and in other observatories before September

17; and besides, the important observation of the disappearance of the comet at the sun's limb is very valuable. Now then, if it will be possible to secure some observations in the remaining days the comet will be visible, I am sure we shall have a large amount of material to study upon.

I may add that Mr. Common and I saw the comet a few days ago. With magnifying power of 120 and 150 we were not able to distinguish the division of the nucleus, but with a higher power we saw five bright points; one of these, corresponding to that seen before, remains the brightest. The comet has all the appearance of a little curve convex to the horizon, and is still a very bright object, as Mr. Common was able to see it pretty well with only six inches aperture and in moonlight.

13, Pembridge Crescent, Bayswater, W. E. RISTORI

Aino Ethnology

LET me hasten to assure Herr Rein that nothing could have been further from my intention than to question the "love of truth," which is conspicuous in his work on Japan. I trust he will consider as absolutely withdrawn any expression of mine which he fancies might at all bear such a construction. His authorities I did not quote, because I attached much more importance to the weight of his name than to theirs. The almost unanimous opinion of original observers is opposed to their conclusions, which I was certainly somewhat surprised to find adopted by Herr Rein. But as he has not himself visited the Aino people, the question of their affinities need not be further argued here. I may state, however, that to Steube and von Siebold must now be added Herr Kreitner, of the Szechenyi expedition, who emphatically removes them from the Mongolic, and "assimilates them to the Caucasic type" ("Im Fernen Osten," Vienna, 1881, p. 318). A. H. KEANE

Auroral Experiments in Finland

IN the note in NATURE, vol. xxvii. p. 322, in which you refer to my telegrams from Sodankylä, there is a misunderstanding concerning the apparatus which I made use of in the experiments. This apparatus, which I call in Swedish "Utfrömningz-apparat" (streaming apparatus), was constructed of uncovered copper wire, provided at each half-metre with fine erected points. That wire was led in slings to the top of the hill, and reposed on the usual telegraph insulators. From one end of this wire was conducted a covered copper wire on insulators to the foot of the hill (600 feet high), and there joined a plate of zinc interred in the earth. In this circuit was put a galvanometer.

It was this apparatus which produced both the yellow-white halo at Oratunturi and the straight beam of aurora borealis at Pietarintunturi, as the positive current in the galvanometer at both places. The terrestrial current diminishes (or ceases) below the belt of maxima of the aurora borealis. S. LEMSTRÖM

Helsingfors

Flamingoes and Cariamas

IN NATURE, vol. xxvii. p. 334, an account is given of the curious behaviour of a flamingo towards a cariama. May I point out that this habit of the flamingo was observed in 1869 by Mr. Bartlett, and will be found in a P.S. to a paper of his entitled "Remarks upon the Habits of the Hornbills," read before the Zoological Society, February 25, 1869. The liquid was examined by Dr. Murie, and is said to have consisted almost entirely of blood. A short notice of the habit, communicated by Mr. Bartlett, appears also in Buckland's Edition of "White's Selborne." JAMES CURRIE

Cambridge, February 19

THE APPROACHING FISHERY EXHIBITION

FROM the cheerful note of preparation which is now being sounded, we presume the opening of the International Fisheries Exhibition will take place punctually on the day which has been fixed for that event—May 1. That the Exhibition will be successful, both in a pecuniary sense and as an exposition of fishery economy and of the natural history of our food fishes, may, we think, be even now predicted. The two exhibitions by which it has been preceded, those of Edinburgh and Norwich, not only paid all expenses, but left a handsome surplus; so that, with the vast population of London and the strangers who daily come within its gates to work upon, the promoters of the exposition are warranted in believing that it will prove a success. It will undoubtedly be the greatest affair of the sort which has yet been designed, and will occupy a site twice as large as the Norwich and Edinburgh exhibitions joined together. The fishery exhibition which was held at Berlin three years ago was visited by nearly half a million persons, but it was only open for ten weeks, whilst the show to be held at South Kensington will remain open for six months, and as the population of London is more than four times greater than that of Berlin, we may calculate on the visitors to the Fishery Exhibition running into big figures;—two million persons at a shilling each would represent a sum of one hundred thousand pounds. Already a large guarantee fund has been subscribed by corporations and private persons, and there is no reason why Parliament should not be asked for a grant in aid, although any money that might be granted may not be required. It is right to say that as a nation we play a rather "mean" part in such matters, and are quite outdone in liberality by other countries. America, for instance, is sending us an "exhibit" which will cost that country ten thousand pounds, and other foreign countries are acting in an equally liberal spirit. If we were asked on any occasion to reciprocate, what answer could we make? We have positively nothing that we could send. With the exception of the toy museum left to the country by Mr. Frank Buckland, we possess nothing in the shape of a national collection illustrative of fishery economy; hence the Exhibition which is about to open assumes very much the shape of a commercial enterprise, and becomes a gate-money show. But that is better than nothing, and it is to be hoped that from the debris of the approaching exposition a substantial addition may be made to the Buckland Museum of economic fish culture, and if we may be permitted to make such a suggestion, the aquarium should, if that is possible, be so arranged that it could be left as a permanent attraction for all who are interested in the natural history of fish and in the proper ingathering of the harvest of the sea.

Great expectations are entertained as to the value of the lessons to be taught at the approaching Exhibition. We are undoubtedly in need of knowledge of all kinds regarding the natural history of our fishes. From the whitebait to the whale we are singularly deficient in those details of fish life that would prove valuable to persons engaged in fishery enterprise. In the matter of well-planned investigation into the natural history of the British food fishes we are far behind America, where information of the most valuable kind is systematically collected and disseminated. As a matter of fact, we have (as a nation) done almost nothing in respect of adding to the knowledge of the public. Some individuals have been toying with the subject of *Pisciculture*, whilst in the seas that pertain to the United States fish-breeding on an extended scale has been long in operation under the auspices of the Government. It will not be the fault of the promoters of the approaching Exhibition if attention is not aroused to our want of interest (as a people) in the sea-fisheries of the country. We have therefore every reason to be grateful to those who have stepped to the front in order to promote this enterprise; the men who have assumed the lead have nothing to gain personally by its success—they are working in the interests of the public, knowing well that the fisheries of the surrounding coasts contribute largely to the commissariat of the country.

A portly prospectus, so far as its contents are concerned, has been issued, indicative of what will be shown in the Exposition, and from that document we gather that a large

sum of money will be distributed in prizes for inventions and improvements of fishing gear; the special prizes in this department alone will number over 100, ranging in value from 600*l.* to 2*l.* 10*s.* Over 1000*l.* will also be given for essays on various topics connected with the economy of the fisheries and the natural history of our more important food fishes, as also for papers on fishery legislation. The dissemination of the knowledge to be obtained from such essays as may be awarded prizes is important. None of the essays contributed to the Norwich Exhibition have been published, except that of Sir James Maitland, printed presumably at his own expense, so that whatever information was contained in the Norwich prize essays remains only in the cognisance of those who read them. The Edinburgh prize essays are, we believe, being printed. Surely they might have been published ere this, and it might be taken into consideration by the executive of the present Exhibition, whether it is possible to have the essays judged, the prizes awarded, and a print of such as are worthy of being published on sale in the building in the course of the summer: a popular "handbook" to the Exposition will, we may presume, be issued. As to "exhibits" of a useful kind, such as those of fishing gear of every description, men with a practical turn of mind will be able to take stock of them and perceive at a glance how far they can be utilised. As a class, fishermen are slow to learn and chary in the way of trying experiments, but it is not impossible that the approaching Exhibition may contain the germs of some new ideas which may prove alike practical and profitable.

THE PROGRESS OF TELEGRAPHY

THE first of the series of six lectures on the Applications of Electricity was delivered on Thursday evening, February 15, at the Institution of Civil Engineers, on "The Progress of Telegraphy," by Mr. W. H. Preece, F.R.S., M.Inst.C.E., of which the following is an abstract:—

Telegraphy is the oldest practical application of electricity. It grew about the railway system, and was rendered a practical agent by the foresight of Robert Stephenson, I. K. Brunel, Joseph Locke, and G. P. Bidder, who were its godfathers in England. Electric currents are, as a rule, maintained for telegraphic purposes by the combustion of zinc, and in the innumerable forms of batteries in use, the conversion of zinc into sulphate of zinc is the root of the transformation of energy into that form which was utilised as electric currents. There are three forms of battery in use in the British Post-Office Telegraph system, and in the following numbers:—

Daniell	87,221 cells.
Leclanché	56,420 ,,
Bichromate	21,846 ,,

Every administration has its own adopted form, differing in design, but based on one or other of these types. Magneto-electricity is employed for some forms of apparatus, and dynamo-machines are sometimes used to supplement batteries. Experiments are now being made with secondary batteries. The various terms employed—electromotive force, resistance, induction, and current—though measurable in definite units, have not yet become household words; but, being admitted into commercial, legal, and Parliamentary lore, they will soon be as familiar as feet, gallons, or pounds.

Electric currents are conveyed from place to place either overground, underground, or submarine.

Overground.—Wooden poles preserved in creosote are employed in England, but iron poles are extensively used in the colonies. The conducting wire is almost universally of iron, but copper wire is much used through smoky places where iron is liable to rapid decay. Phosphor-bronze wire is under trial, and is a very promising material, as it possesses the conductivity of copper with the strength of iron. The improvements made in the quality of iron wire have been very great, and it conducts now fully 50 per cent. better than it did a few years ago. Electric tests have had a marvellous effect upon the production of pure metallic conductors; copper has improved in even greater ratio than iron; samples have been produced better even than the standard of purity. The insulators remain principally of porcelain, and their forms vary nearly with the number of individuals who use them; the only improvement of any value recently made is one which facilitates the very necessary process of cleaning.

Underground.—Wires are almost invariably carried underground through towns. Copper wire, insulated with gutta-percha, incased in iron pipes, is the material used. There are 12,000 miles of underground wire in the United Kingdom. There is a great outcry for more underground work in England, owing to the destruction to open lines by gales and snowstorms; but underground telegraphs, wire for wire, cost at present about four times as much as overground lines, and their capacity for the conveyance of messages is only one-fourth; so that overground are, commercially, sixteen times better than underground wires. To lay the whole of the Post-Office system underground would mean an expenditure of about 20,000,000*l.* Hence there is no desire to put wires underground except in towns. Besides snowstorms are few and far between, and their effects are much exaggerated. Of the numerous materials and compounds that have been used for insulating purposes, gutta-percha remains the oldest and the best for underground purposes. It, like all other materials used for telegraphy, has been improved vastly through the searching power that the current gives the engineer.

Submarine.—The past ten years has seen the globe covered with a network of cables. Submarine telegraphs have become a solid property. They are laid with facility and recovered with certainty, even in the deepest oceans. Thanks to such expeditions as that of H.M.S. *Challenger*, the floor of the ocean is becoming more familiar than the surface of many continents. There are at present 80,000 miles of cable at work, and 30,000,000*l.* have been embarked in their establishment. A fleet of twenty-nine ships is employed in laying, watching, and repairing the cables. The Atlantic is spanned by nine cables in working order. The type of cable used has been but very little varied from that first made and laid between Dover and Calais; but the character of the materials, the quality of the copper and the gutta-percha, the breaking strain of the homogeneous iron wire, which has reached 90 tons to the square inch, and the machinery for laying, have received such great advances, that the last cable laid across the Atlantic, by the Telegraph Construction and Maintenance Company, was done in twelve days without a hitch or stoppage.

Ideas are conveyed to the mind by electric signals, and in telegraphy these signals are produced at distant places by using two simple electrical effects: (1) that a magnetic needle tends to place itself at right angles to a wire when an electric current passes through it; and (2) that a piece of iron becomes a magnet when a current of electricity circulates around it. An innumerable quantity of tunes can be played on these two strings. Various companies were established at different times to work certain systems, but when the telegraphs were absorbed by the State the fittest were selected to survive, and their number consequently declined.

The A B C instrument is the simplest to read, for it indicates the letters of the alphabet by causing a pointer to dwell opposite the desired letter. There are 4398 in use. Its mechanism is, however, complicated and expensive, and it is being rapidly supplanted by the telephone. The needle instrument is the simplest in con-

struction, but it requires training to work it. There are 3791 in use in the Post Office, and 15,702 among different railway companies. As a railway instrument it is the simplest, cheapest, and most efficient ever devised. The Morse instrument, of which there are 1330 in use in the Post Office and 40,000 on the Continent, records its letters in ink, in dots and dashes on paper tape, and, like the needle and A B C, appeals to the consciousness through the eye; it also indicates the letters of the alphabet by sound, and thus utilises the organ of hearing. Sound-reading is gaining ground in England with great rapidity. There are now 2000 sounders in use: in 1869 there were none. In America scarcely any other instrument is used. On the Continent there is scarcely one.

Acoustic reading attains great perfection in Bright's bell instrument, where beats of different sound replace the dot and dash of the Morse alphabet. Sound-reading is more rapid and more accurate than any system of visual signals or permanent record. In fact no record is kept in England, for the paper tape is now destroyed as soon as it has been read. Errors are of course inherent in all systems of telegraphy. A telegraphist cannot see what he writes, or hear what he says, and who is there that does not make mistakes whose eye follows his pen, or whose ear takes in his own words? The Hughes type-instrument, which prints messages in bold Roman characters, is much used on the Continent; it is, in fact, recognised as the international instrument, but it has had to give way in England to a more rapid system of telegraphy. It is, however, solely used for the Continental circuits by the Submarine Telegraph Company. All long cables are worked by Sir William Thomson's beautiful siphon-recorder.

In ordinary working only one message can be sent in one direction at one time; but by a simple and ingenious contrivance, by which the neutrality of opposite currents is utilised to convey signals, duplex telegraphy is rendered possible, so that two messages can be sent on the same wire at the same time; and by a still further improvement, where currents of different strength are utilised, four messages are sent on one wire—two simultaneously in opposite directions—at the same time. There are in England 319 duplex and 13 quadruplex circuits at work.

The acme of efficiency in telegraphy is attained in the automatic system, in which manual labour is supplanted by mechanism in transmitting the messages. There are 71 circuits worked by these instruments, and 224 instruments in use, and a speed of working of 200 words per minute is easily maintained upon them. When the hand alone is used, from 30 to 40 words per minute is the maximum rate attained, but by automatic means the limit is scarcely known. Since this system can be duplexed, and in many cases is so, 400 words per minute on one wire are easily sent. By the use of high-speed repeaters, the length of circuit for automatic working is scarcely limited; it would be easy to send 100 words per minute to India.

The growth of business since the telegraphs have been acquired by the State is enormous: 126,000 messages per week have grown to an average of 603,000; but the mileage of wire has not increased in anything like the same proportion, the excess of traffic having been provided for by the great improvements made in the working capacity of the apparatus. In 1873, the average number of messages per mile of wire was 147, it is now 256. It is in press work that the greatest increase has taken place: 5000 words per day at the time of the Companies have grown to 934,154 words per day now. 340,966,344 words of press matter were delivered in the year ending March 31, 1882.

The development of railways has necessitated a corresponding increase in the telegraphs required to insure the safety of the travelling public, and while 27,000 miles of wire in England, Scotland, and Wales were used for that purpose in 1869, at the end of December, 1882, the total had increased to 69,000 miles, equipped with 43,176 instruments, against 8678 in 1869.

The growth of business is equally discernible in the great cable companies. In 1871 the number of messages dealt with by the Eastern Telegraph Company was 186,000; in 1881, it was 720,000. This growth is equally striking in all civilised countries, and even in Japan 2,223,214 messages were despatched last year, of which 98 per cent. were in the native tongue. The mode of transacting the trade of the world has been revolutionised, and while wars have been rendered less possible, their conduct has been expedited, and their penalties alleviated.

CENTRAL AND WEST AFRICA[1]

THE brilliant journey of Major Serpa Pinto across Africa from Loanda, by the Zambesi to Natal, must be fresh in the recollection of our readers. The present narrative may be regarded as complementary of the major's exciting story. Captains Capello and Ivens were members of the original expedition along with Major Pinto, and for the first part of the journey the three companions worked together. The object of the expedition, which was organised by the Portuguese Government, was to thoroughly survey the great artery which—a tributary of the Congo—runs from south to north between 17° and 19° E. of Greenwich, and is known as the Cuango, as also to determine all the geographical bearings between that river and the west coast, and make a comparative survey of the hydrographical basins of the Congo and Zambesi. The three travellers started from Benguella in November, 1877, but had not proceeded far on their journey, when a difference of opinion arose as to the future route of the expedition. Messrs. Capello and Ivens did not feel at liberty to depart from the original letter of their instructions, while the bold Major Pinto conceived that he would be carrying out the spirit of their instructions by making a dash across the continent. We have nothing to do with the quarrels of the travellers; experience proves that in such an expedition there should be one supreme head, and that the best exploring work has often been done by a white traveller single-handed. Major Pinto's presence with the other two was really unnecessary, and it was certainly to the advancement of geographical knowledge that he took an entirely different route. Messrs. Capello and Ivens are evidently two pleasant and agreeable gentlemen, though we have some doubts if exploration is exactly the *métier* to which they are best adapted. At all events they have written a narrative that contains much pleasant reading, and some additions to our knowledge of the geography and natural history of the limited region which they traversed. Their real work lasted for about two years, during which they traced the Cuango northwards to about 5° S. lat., when they were compelled to turn back, partly owing to the exhaustion of their supplies, and partly to the arid nature of the country beyond their farthest point. During their journey they crossed innumerable streams, some of them adding their waters to the Cuango and others joining the Cuanza, which discharges into the Atlantic south of Loanda. The sources of the Cunene, Cuanza, and Cuango were visited and determined, and a pretty careful survey of the region all along the route made. The country traversed is mostly mountainous, cut up by innumerable streams and valleys, rich in many parts in vegetation, and even in metals, and having a considerable population clustered in villages, each of which is ruled by its chief. With each of these chiefs much diplomacy had to be used in order that the

[1] "From Benguella to the Territory of Yacca: description of a journey into Central and West Africa." By H. Capello and R. Ivens. Translated by Alfred Elwes, Ph.D. Two vols. (London: Sampson Low and Co., 1882.)

explorers and their followers might obtain provisions and be allowed to pass ; but the repetition of the same story of petty troubles and difficulties becomes ere long somewhat tiresome. The habits and dwellings, the implements and weapons, the dispositions and superstitions of the people in this region are pretty much the same as those of the other Bantu tribes with which Pinto, Stanley, Cameron, and other recent explorers have made us familiar. Among the Ganguella we find considerable



Fig. 1.—A Muata of the T'chiboco.

manufactures of iron, while Bihé is rich and fertile, and its inhabitant the greatest native travellers in Africa. In reference to the Bihenos the authors have some curious remarks on the well-known African prefix in its varying forms *ma*, *ba*, &c. They seriously lament the ignorance of ethnologists who call the Kafirs "Bantu," a word, they tell us, which simply means "persons." This is in strict analogy with the customs of nearly all peoples, who almost invariably refer to themselves by terms which mean *the people*, *the men*, &c. Bantu has come to have a well-defined ethnological significance, and is not likely to be displaced by the not too well-informed criticisms of our travellers.

Among the people of this region we find the same elaborate methods of dressing the hair, so common in Central and Western Africa, and with which readers of recent African travel must be familiar. We have some interesting details as to the history of some of the leading tribes of the region, from which it is evident that for centuries the various African peoples have been in a state of almost constant migration, that the so-called states are exceedingly unstable, and that even here it would be hazardous to regard any one race as unmixed.



Fig. 2.—Woman of Cangombe.

We give here two types : Fig. 1, a Muata, or ruler, of the T'chiboco ; and Fig. 2, a woman of Cangombe.

The sources of the Cuango were found at a height of 4756 feet, at about 11½° S., and a little east of 19° E., in one of the most extraordinary watersheds to be met with anywhere. It is thus described :—

"An extensive tract of land, all hill and dale, marks this culminating point, a sort of St. Gothard of the African waters. On the north, running through a narrow and tortuous valley, appeared the Cuango, which, shortly after its birth, flows at the foot of the plantations of manioc and massambala, growing abundantly upon the slopes, and at that time filled with girls and women engaged in hoeing and other field labours. A bluish



Fig. 3.—Ébande (Fish of the Cuango).

streak of land was visible in a south-west direction, and on the western slope, in *Canica*, appeared the sources of the Caùcu rivulet, which constitutes the modest commencement of the great Cassai. To the north-east stretched out the T'chibungo range, on whose eastern slope were visible the sources of the T'chipaca at about twenty-five miles from the point of observation, and whose latitude was 11° 27′ and longitude 19° 11′ 30″. Finally, the eye took in at various distances, approximately determined by the compass, an infinity of spring-heads, the sources of various affluents of the T'chipaca, the Cuango, the Cassai, the Lume, and the Loando, which, glittering in all directions, poured their ever-increasing waters to the Congo-Zaire, the Cuanza and the Zambese, till they were lost to sight in the valleys and ravines, where a denser vegetation still hinted at their sinuous course. The aspect of the country was magnificent. In the east, extended as far as the eye could reach, the rich green valley of the upper Cassai, clothed with numerous senzalas of *ma-quioco* and *ma-cosa*, indicated by the white patches of manioc flour spread to dry upon the *luandos* or mats of the *mabu*."

Speaking again of the same remarkable region, the writers say:—

"In a lofty position—the mean altitude being 1531 feet—the intense heat of the tropics is far from predominant, and the breeze which is stirring during a part of the year renders the climate soft and salubrious to the European. Standing upon a granitic plateau, the region may properly be described as the *Mother of the African Waters*,

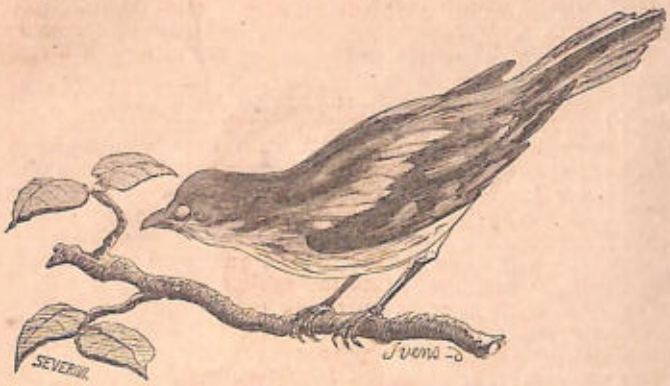


Fig. 4.—Fiscus Capelli (Cassange).

a veritable hydrographic centre whence issue, through deep gullies, the streams that flow to the two great oceans by the channels of the Congo-Zaire, the Cuanza, and the Zambese. Its mineral wealth is considerable, abounding chiefly in oligist iron; native copper exists more to the eastward, where, if we may rely upon the reports of the natives, the lodes are easily worked. The vegetable products, more especially upon the banks of the great rivers, are important. There are *Apocinaceas*, or india-rubber trees; *Burseraceas*, which yield aromatic resins such as the *Elemi*; *Herminieras*, used in the building of canoes; *Rubiacias*, or teak, mixed with *Erythrinas*, producing



Fig. 5.—Euprepes Ivensi (new species), River Cuanza.

cork; several *Euphorbias*, acacias used for dyeing purposes; *Typhas*, and a species of *Borassus*; grasses of various kinds, such as the *panicum* and *andropogon*, the



Fig. 6.—The Cuango in Yacca.

pennisetum, both smooth and barbed (massango), hemp, and a large number of *Convolvulaceas*; all these we ourselves saw. Among the variety of wild fruits of T'chiboco are distinguishable the *fungo*, not unlike a plum, but less pulpy and more sour, which grows upon a medium-sized tree; the *macolla*, of the granular species,

having the shape and size of an orange, but resembling internally the American muruenia, that produces purgative effects when taken in large doses; the *tongo*, similar in form and dimensions to the white plum; and the *tundo*, almost equal to a cherry in taste, and having black seeds. The abundance of wax is really remarkable, and towards the south and south-east it constitutes an important branch of industry."

At this point the two travellers separated in order to proceed northwards on different sides of the Cuango, and met again at Cassange, where they fell in with Dr. Max Buchner, on his way to the great Muata Yanvo. Of this famous potentate Messrs. Capello and Ivens give a fancy portrait, which contrasts markedly with that taken from the original by the German explorers who have recently done so much for a scientific knowledge of the region through which the route of the Portuguese travellers lay. Cassange may be regarded as the furthest Portuguese outpost, and a busy centre it is.

Yacca, the furthest limit of the expedition, was reached in May, 1879, and although innumerable small lakes and many streams had been passed, the region beyond was found to be an arid desert, brooded over by "the silence of the grave." Here is a summary of the travellers' observations on the course of the Cuango from its source to the limit of their journey, about 140 miles from where the river discharges into the Congo:—

"From parallel 11° 30′, approximately, where its sources are to be found, up to 5° 05′ at the Quicunji cascade, the river has a sinuous course of 580 geographic miles, and a total fall between its extreme points of about 3 feet 4 inches per mile. Rocks, stones, rapids, and cataracts interrupt the stream, and twelve of the points at which they do so are known to us, namely, the first at parallel 10° 17′, to the east of Muene-songo; the second at 10° 25′, near the Camba rivulet; the third at 10° 08′, Caxita rocks; the fourth at 10° 05′, the Louisa falls; the fifth at 10° 05′, a cataract a little above Port Muhungo; the sixth at 9° 20′, Zamba; the seventh at 19° 19′, Tuaza; the eighth at 9°, cataract Cunga-ria-Cunga; the ninth at 7° 42′, Suco-ia-Muquita or Suco-ia-n'bundi; the tenth at 7° 38′, just below the Camba; the eleventh at 7° 35′, in the midst of numerous islands; and the twelfth at 5° 05′, the Quicunji waterfall, which is only passable after the heavy rains. The greatest navigable tract, therefore, is that space which lies between the cataract at 7° 35′ and Quicunji, or about 190 geographic miles. The river there is of variable width, never less than 76½ yards, and from 5 to 20 feet in depth. The current loses a little of its speed in the upper section, where the stream in the summer season has a fall of about 3 feet 2 inches per mile. We think it well to mention that our longitudes being strictly correct, as the record, partly chronometric, was compared both on departure and arrival at the Portuguese station of Duque de Bragança, and the latter again at the terminus on the coast, it appears to us that the point of affluence of the Cuango (or Ibari-N'Kutu) as marked upon the maps, just above Stanley Pool, is erroneously placed considerably to the eastward."

Major von Mechow, who has been exploring the river urther down its course, has found it equally unnavigable, and we may say that the maps illustrating Mr. Stanley's last journey to the Congo place the mouth of the river further west than on those of his famous trans-African expedition. It was this river which Mr. Stanley ascended in his little steamer, and found it expanding into a broad lake. Messrs. Capello and Ivens came to the conclusion, confirmed by Major von Mechow, that no such lake as Aquilonda exists in this quarter. The travellers returned by a somewhat different route, staying for some time at Pungo N'Dongo, with its famous rocks, and reaching Loanda in October, 1879.

The work abounds with illustrations of the country and the people, many of them devoted to natural history. On the animal and plant life of the district traversed there are many valuable notes, and in the appendix will be found, besides tables of geographical observations and heights above sea-level, lists of additions to the fauna and flora, tables of African dialects, and a N'Bunda Vocabulary. There is a good summary of the general results in the concluding chapter, in which the authors have the following observations on the geology of the continent:—

"The physical configuration of the African continent, and more especially of the portion south of the equator, is nowadays too well known to require minute description. It may be summed up in these few words: a depressed central basin surrounded by a vast circle of high land, gradually descending to the sea, and rent by deep ravines, through which rush huge watercourses, engendered in the interior, till they overflow and seek the lower level fronting the ocean. From a very general geological point of view we may define the regions running from the littoral to the interior in the following order, viz. limestone, sandstone, and granite. But on going more minutely into the subject we shall find that these distinctions are not very exact; inasmuch as the component parts frequently run into each other and change places, while precise lines of demarcation are wanting. The geological formation on the western coast at the points observed by us between Loanda and Mossamedes, and even further to the north, exhibits generally near the sea a belt of tertiary deposits, with abundant masses of sulphate of lime and sandstone, from which they are separated by beds of white chalk alternating with primary rocks, for the most part gneiss, abounding in quartz, mica, hornblende, granite, and granulated porphyry. Towards the south large tracts of feldspar become visible. At Mossamedes whole mountains are composed of sulphate of lime; while carbonate of lime, accumulated in shells, is very frequent. Both rock-salt and nitrate of potash are found in stratification. Along the Mocambe chain, we were informed, there exists a basaltic line of great length. From that point the shifting soil may be said to commence, extremely abundant in sand, constituting true *saharas*, as in the parallel of Tiger Bay. In the transition from the lower zone towards the interior, for instance at Dondo, vast tracts of schist rock, in perfect laminæ, compose the soil; and sandstone, reddened by oxide of iron, is visible in every direction. Proceeding further into the interior we find, in a perfectly mountainous region, the ground to be composed of granite-quartzy rock, extremely hard and compact; this is the case throughout the belt crossed on the way and up to Pungo N'Dongo, the surface soil being formed by the disintegration of the granite itself. These geological characteristics will naturally be repeated to the south and north in identical parallel regions, with variations in the high table-land, where we meet occasionally with hard and tough red sandstone and rocks of feldspar as in the basin of the Lucalla."

In the same chapter will be found abundant notes on the various tribes visited, which, although the authors' ethnology appears to us by no means sound, are still a valuable contribution to a knowledge of the African peoples. As evidence of the important contributions to the natural history of West Africa, we give a few of the illustrations bearing on the subject.

ON THE AURORA BOREALIS[1]

HAVING been requested by this journal to give an account of my latest researches into the nature of the aurora borealis, I must explain that my lateness in

[1] In reference to the present interesting communication from Herr Sophus Tromholt, from his station in Ultima Thule, we ought to point out that Herr Tromholt was, at the time of writing, not aware of the important discovery as regards the nature of the aurora made by Prof. Lemström at the Finnish station of Sodankylä during December last, and of which an account appeared in NATURE, vol. xxvii. p. 322.

complying with this request arises from the fact that I had this winter changed my residence from Bergen, where the communication was directed, to this spot—Kautokeino, in Ultima Thule.

Since September last I have, for the sake of the aurora borealis, been residing here in North Finmarken (69° N. lat., 23° E. long.), in a quarter, therefore, where the auroræ attain their maxima, and where the phenomena, consequently, are so frequent and on such a scale that there cannot be a question of selecting and analysing one in particular. I therefore prefer to give briefly a description of its general appearance here, its character and occurrence.

My winter sojourn here has two objects in view—viz. firstly, to frame a pendant to the observations of the auroræ made at Bossekop, 1838-39, by the French Commission du Nord ("Voyages en Scandinavie," &c.), which, by the bye, later students of the phenomenon seem to have entirely ignored; and secondly, by means of altitudinal measurements corresponding with those now being made at the Norwegian Meteorological Station at Bossekop, to procure sufficient materials for fixing the parallax of the aurora borealis. I choose the remote Kautokeino for my observatory for several reasons—viz. that this place is situated almost exactly south of Bossekop, while the distance between the two places is very nearly a degree, a distance which is exactly suited to the opinion I have formed as to the height of the aurora, viz. 150 kilometres, and also for the reason that Kautokeino possesses a very free horizon, and that its situation, very far inland, would insure favourable weather conditions.

As previously stated, observations are made simultaneously here and at Bossekop on a common prearranged plan, and measurements made in the common vertical plane by the so-called auroral theodolite, constructed by Prof. Mohn. A similar arrangement has also been effected with the Finnish Meteorological Station at Sodankylä, which is, however, situated at a great distance from this place and in a somewhat unfavourable direction (about 45° S.E.). We shall not, of course, be able to compare notes before the spring, so I am unable at present to lay before the reader the final results; but judging from my own researches here, I feel convinced, in spite of assertions made by scientists to the contrary, that the exact height of the aurora may be ascertained by the method I advocate, and that from the observations made at these three stations we shall glean sufficient materials to solve a problem hitherto deemed an insoluble one.

Auroræ occur here, I may say without exaggeration, every night, and an evening without them would be a phenomenon as remarkable as their appearance under the equator. Unfortunately, however, unfavourable weather has during the last two months, accompanied by cloud masses unusual in these latitudes, sadly interfered with the number and completeness of my observations. Still, the magnitude of the auroræ is not the same every night. Sometimes they appear as short, faint, arc-shaped phenomena, similar to those so frequently seen in South Norway, while at others they assume an extent and grandeur which mocks every attempt at description.

In one respect my researches here have been of great moment to me, *i.e.* with regard to understanding the various types of the auroræ, their real strike and shape, and their exterior appearance, which changes in the different altitudes above the horizon; while on account of their frequency, and the circumstance that they now appear in the north, then in the south, and at last in zenith, there is a splendid opportunity to study the modifications which one particular form of aurora is subjected to when changing its position to the observer. It appears now conclusive to me that the many forms usually described in researches may be reduced to a few, almost similar, types. In most instances the aurora runs in zones, belts, in the direction of the magnetic east-west, and either as a more or less diffuse luminosity, or as thin shining bands, which I have found to be parallel with the indication of the inclination needle. But the appearance which the phenomenon assumes is entirely dependent on the relative position which the observer occupies to the same. If he is thus greatly distanced from the aurora he will only observe, a few degrees above the horizon, a continuous arc with streamers, but if he approaches nearer, he will notice several such arcs with clearly defined constituents and a greater vibratory motion, and if still closer, he will see the "belts" or bands mentioned by Weyprecht far above the horizon; and if these then travel towards *his* zenith, he will distinctly see the auroral "corona." I have just stated that the main strike of the auroræ is magnetic east-west; this is, however, only stated as a general rule, particularly with those of the luminous or "glory" type, while the "belts" may, besides their slight folds, be twisted and slung in almost any direction. I have thus seen them stretch from north to south, and even form a continuous circle, which, with zenith as centrum, has engirdled the entire heavens at an elevation of about 30°. The variable position of these luminous belts is the cause of the many peculiarities and the deviations from the normal which are so *frequently* observed with the arcs, as, for instance, their unsymmetrical position in relation to the magnetic meridian, and their uneven shape, viz. that they are often bent ecliptically back at the points, or even take the appearance of regular eclipses. I ought, however, to point out that the faint retrograding bend which great arcs assume near the horizon is due simply to optical causes. The study of the auroral corona here is very instructive. When a belt of streamers travels towards the magnetic zenith, the radiations seem to become shorter and shorter, caused by the circumstance that they are seen obliquely, and when the belt passes the magnetic zenith, its lower rim only is seen, which makes it appear as a bent and folded luminous belt. In this position one may observe that every individual streamer has only a very limited depth, but that the belt consists of several, sometimes of a great number, of luminous "sheets" in a parallel position to one another.

Besides this form of aurora, which thus embraces two kinds, viz. the continuous and the radiating, I know only one more of a character distinctly differing from the same. I do not thus consider the individual knots of ray-auroræ, or the streamers, as anything but incomplete belts; while the luminous gatherings I consider are merely remnants, so to say, of previously radiant auroræ. I may also here state that the large purple auroral clouds peculiar to this phenomenon, when observed during considerable electrical disturbances in southern climes, I have never seen at Kautokeino.

Of quite a different nature is, however, the phenomenon which I have named "coruscation." This phase of the aurora, which almost without exception belongs to the earliest hours of the morning, and after large and extended oscillations of the aurora, is developed, I believe, by the luminous clouds. But while these remain quiet, or show at least subdued oscillations, the "coruscation," as I term it, is so violent and of such a peculiar nature, that I have not even yet succeeded in ascertaining whether the motion is horizontal or vertical, or whether it is the luminous clouds themselves which flood the heavens, or their merely momentary "blazing up" under the influence of some passing waves of energy. The entire heaven is sometimes for hours a bath of liquid fire by this force, which seems, by the bye, to possess the same remarkable rapidity around zenith as at lower elevations.

As regard the colours of the auroræ, I have only noticed, when the substance of light is great, and when the oscillations are very rapid, two well-known forms, viz. green

and red. These are, however, only seen in the arcs as their lower rim, and by the forward movement one part assumes a red, another a green tint. The red colour sometimes changes into violet or ochre.

The spectroscope I have not had much opportunity of using here, but the well-known auroral "line" I can always see; any others I have not observed.

With regard to the height of the aurora I have, judging from observation, come to the conclusion that it does not appear at a lower elevation here than it does in the south of Norway, while I am convinced that its plane is to be found far above that of the clouds. There has often enough been an opportunity of observing auroræ and clouds simultaneously, but never has there been the slightest indication of the auroræ having descended to the sphere of the clouds, not even under the most violent oscillations and the most intense luminosity and play of colour. In fact I have come to the conclusion that the auroræ which I have watched at Kautokeino are identical with those I have studied in southern latitudes, while their plane is at the elevation which I estimated when choosing Kautokeino as my station of observation.

I may in conclusion state that I have never myself heard the slightest approach to any auroral "noise," and this in spite of my most earnest attention to this so-much-disputed question. Still if I ask the native people (Lapps) about here as to the "noise" there is not a single one who doubts its existence, while several even assert that they have heard it.

I have several times attempted to photograph the aurora borealis, but without success. Thus even by using the most sensitive English "dry" plates, and exposing them from five to seven minutes, I have not obtained a trace of a negative. The cause of this is, I believe, the exceedingly limited substance of light possessed by the auroræ: were thus even the entire heavens flooded by the most intense auroræ, their aggregate lighting capacity would not equal that of the moon when full. I may therefore assume that photographing the aurora borealis is an impossibility. SOPHUS TROMHOLT

Kautokeino, Finmarken, Norway, January 28

PROFESSOR HUXLEY ON EDUCATION

ON the 16th inst., Prof. Huxley gave an address in connection with the distribution of prizes at the Liverpool Institute, a revised report of which will appear in the next number of the *Journal of Education*. By the courtesy of the editor of that journal, we are enabled to give a few extracts from Professor Huxley's address. He began by referring to certain propositions which he laid down in the address he gave in Liverpool fourteen years ago as to the practical value of instruction in physical science, its superiority to any other study as a mental discipline, and the certainty that in the future physical science would occupy a much larger share in the time allotted to teaching than had been the case previously. He also laid special stress upon the fact that he was no advocate of the exclusion of other forms of culture from education, but, on the contrary, insisted that it would be a serious mistake to cripple them for the sake of science. He had no sympathy, he said, with a kind of sect or horde of scientific Goths or Vandals who think that it would be proper and desirable to sweep away all other forms of culture and instruction except those in physical science. After referring to the great variety of his past experiences, his familiarity with every form of society, from the uncivilised savage of Papua and Australia, to the occasionally somewhat over-civilised members of our upper ten thousand, and to his interest in every branch of knowledge and form of art, Prof. Huxley insisted on the vast importance of science in education, when properly taught. He pointed out, however, that unless the knowledge conveyed in the teaching of science or in the teaching of history were actually realised to themselves by the learners, it would be worse than useless.

"Make it as little as you like, but unless that which is taught is based on actual observation and familiarity with facts it is better left alone. There are a great many people who imagine that elementary teaching might be properly carried out by teachers provided with only elementary knowledge. Let me assure you that that is the profoundest mistake in the world. There is nothing so difficult to do as to write a good elementary book, and there is nobody so hard to teach properly and well as people who know nothing about a subject; and I will tell you why. If I address an audience of persons who are occupied in the same line of work as myself I can assume that they know a vast deal, and that they can find out the blunders I make. If they don't, it is their fault and not mine; but when I appear before a body of people who know nothing about the matter, who take for gospel whatever I say, surely it becomes needful that I consider what I say, make sure that it will bear examination, and that I do not impose upon the credulity of those who have faith in me. In the second place, it involves that difficult process of knowing what you know so well that you can talk about it as you can talk about your ordinary business. A man can always talk about his own business. He can always make it plain; but if his knowledge is hearsay he is afraid to go beyond what he has recollected and put it before those that are ignorant in such a shape that they shall comprehend it. That is why, to be a good elementary teacher, to teach the elements of any subject, requires most careful consideration if you are a master of the subject; and if you are not a master of it it is needful you should familiarise yourself with so much as you are called upon to teach—soak yourself in it, so to speak—until you know it as part of your daily life and daily knowledge, and then you will be able to teach anybody. That is what I mean by practical teachers, and although the deficiency is being remedied to a large extent, I think it is one which has long existed, and which has existed from no fault of those who undertook to teach, but because until within the last score years it absolutely was not possible for any one in a great many branches of science, whatever his desire might be, to get instruction which would enable him to be a good teacher of elementary things. All that is being rapidly altered, and I hope it will soon become a thing of the past."

Then as to the important question of time, Prof. Huxley said that all he asked for was that scientific teaching should be put into what politicians and statesmen call the condition of the "most favoured nation"; that is to say, that it shall have as large a share of the time given to education as any other principal subject. On the important question as to what should be regarded as "principal subjects," Prof. Huxley remarked as follows:—

"I take it that the whole object of education is, in the first place, to train the faculties of the young in such a manner as to give their possessors the best chance of being happy and useful in their generation; and, in the second place, to furnish them with the most important portions of that immense capitalised experience of the human race which we call knowledge of various kinds. I am using the term knowledge in its widest possible sense, and the question is what subjects to select, by training and discipline in which the object I have just defined may be best attained. I must call your attention further to this fact, that all the subjects of our thoughts, feelings, and propositions, leaving aside the mere materials and occasions of thinking and feeling—our sensations as all our mental furniture—may be classified under one of two heads: as either within the province of the intellect, something that can be put into proposition and affirmed or denied,

or as within the province of feeling, or that which, before the name was defiled, was called the æsthetic side of our nature, and which can neither be affirmed nor denied, but only felt and known. According to the classification which I have put before you then, the subjects of all knowledge are divisible into two groups, matters of science and matters of art; for all things with which the reasoning faculty alone is occupied come under the province of science, and, in the broadest sense, and not in the narrow and technical sense in which we are now accustomed to use the word art, all things feelable, all things which stir our emotions, come under the term of art, in the sense of subject matter of the æsthetic province. So that we are shut up to this,—that the business of education is, in the first place, to provide the young with the means and the habit of observation; and, secondly, to supply the subject matters of knowledge, either in the shape of science or of art, or of both combined. Now, it is a very remarkable fact—but it is true of most things in this world—that there is hardly anything one-sided or of one nature, and it is not immediately obvious what, of the things that interest us, may be regarded as pure science, and what may be regarded as pure art. It may be that there are some peculiarly constituted persons, who, before they have advanced far into the depths of geometry, find artistic beauty about it, but, taking the generality of mankind, I think it may be said that when they begin to learn mathematics their whole souls are absorbed in tracing the connection between the premisses and the conclusions, and that to them, geometry is pure science. So I think it may be said that mechanics and osteology are pure science. On the other hand, melody in music is pure art. You cannot reason about it; there is no proposition involved in it. So, again, in the pictorial art, an arabesque, or a 'harmony in grey,' touch none but the æsthetic faculty. But a great mathematician, and even many persons who are not great mathematicians, will tell you that they derive intense pleasure from geometrical reasonings. Everybody knows that mathematicians speak of solutions of problems as 'elegant,' and they tell you that a certain mass of mystic symbols is 'beautiful, quite lovely.' Well, you do not see it. They do see it, because the intellectual process, the process of comprehending the reasons symbolised by these figures and these signs, confers upon them a sort of pleasure, such as an artist has in visual symmetry. Take a science of which I may speak with more confidence, and which is the most attractive of those I am concerned with. It is what we call morphology, which consists in tracing out the unity in variety of the infinitely diversified structure of animals and plants. I cannot give you any example of a thoroughly æsthetic pleasure more intensely real than a pleasure of this kind—the pleasure which arises in one's mind when a whole mass of different structures runs into one harmony as the expression of a central law. That is where the province of art overlaps and embraces the province of intellect. And if I may venture to express an opinion on such a subject, the great majority of forms of art are not in the sense what I just now defined them to be—pure art; but they derive much of their quality from simultaneous and even unconscious excitement of the intellect. When I was a boy I was very fond of music, and I am so now; and it so happened that I had the opportunity of hearing much good music. Among other things, I had abundant opportunities of hearing that great old master, Sebastian Bach. I remember perfectly well—though I knew nothing about music then, and I may add know nothing whatever about it now—the intense satisfaction and delight which I had in listening by the hour together to Bach's fugues. It is a pleasure which remains with me, I am glad to think; but of late years I have tried to find out the why and wherefore, and it has often occurred to me that the pleasure, in musical compositions of this kind, is essentially of the same nature as that which is derived from pursuits which are commonly regarded as purely intellectual. I mean that the source of pleasure is exactly the same as in most of my problems in morphology—that you have the theme in one of the old masters' works followed out in all its endless variations, always appearing and always reminding you of unity in variety. So in painting; what is called truth to nature is the intellectual element coming in, and truth to nature depends entirely upon the intellectual culture of the person to whom art is addressed. If you are in Australia, you may get credit for being a good artist—I mean among the natives—if you can draw a kangaroo after a fashion. But among men of higher civilisation the intellectual knowledge we possess brings its criticism into our appreciation of works of art, and we are obliged to satisfy it as well as the mere sense of beauty in colour and in outline. And so the higher the culture and information of those whom art addresses, the more exact and precise must be what we call its 'Truth to nature.' If we turn to literature, the same thing is true, and you find works of literature which may be said to be pure art. A little song of Shakespeare or of Goethe is pure art, although its intellectual content may be nothing. A series of pictures is made to pass before your mind by the meaning of words, and the effect is a melody of ideas. Nevertheless the great mass of the literature we esteem is valued not merely because of having artistic form, but because of its intellectual content, and the value is the higher the more precise, distinct, and true is that intellectual content. And if you will let me for a moment speak of the very highest forms of literature, do we not regard them as highest simply because the more we know the truer they seem; and the more competent we are to appreciate beauty, the more beautiful they are? No man ever understands Shakespeare until he is old, though the youngest may admire him; the reason being that he satisfies the artistic instinct of the youngest and harmonises with the ripest and richest experience of the oldest. I have said this much to draw your attention to what, to my mind, lies at the root of all this matter, and at the understanding of one another by the men of science on the one hand, and the men of literature and history and art on the other. It is not a question whether one order of study should predominate or that another should. It is a question of what topics of education you shall select which will combine all the needful elements in such due proportion as to give the greatest amount of food and support and encouragement to those faculties which enable us to appreciate truth, and to profit by those sources of innocent happiness which are open to us, and at the same time to avoid that which is bad and coarse and ugly, and to keep clear of the multitude of pitfalls and dangers which beset those who break through the natural or moral laws."

Professor Huxley then went on to point out the worthlessness of the kind of literary education that used to prevail in English schools, and gave his idea of what a literary education ought to be. If, he said, he could make a clean sweep of everything, and start afresh, he would in the first place secure the training of the young in reading and writing, and in the habit of attention and observation both to that which is told them and that which they see; and he would make it absolutely necessary for everybody, for a longer or shorter period, to learn to draw, and there is nobody who cannot be made to draw more or less well.

"Then we come to the subject-matter, whether scientific or æsthetic, of education, and I should naturally have no question at all about teaching the elements of physical science of the kind I have sketched in a practical manner; but among scientific topics, using the word 'scientific' in the broadest sense, I would also include the elements of the theory of morals and

of that of political and social life, which, strangely enough, it never seems to occur to anybody to teach a child. I would have the history of our own country and of all the influences which have been brought to bear upon it, with incidental geography, not as a mere chronicle of reigns and battles, but as a chapter in the development of the race and the history of civilisation. Then with respect to æsthetic knowledge and discipline, we have happily in the English language one of the most magnificent storehouses of artistic beauty and of models in literary excellence which exists in the world at the present time. I have said before, and I repeat it here, that if a man cannot get literary culture of the highest kind out of his Bible, and Chaucer, and Shakespeare, and Milton, and Hobbes, and Bishop Berkeley, to mention only a few of our illustrious writers—I say if he cannot get it out of those writers, he cannot get it out of anything; and I would assuredly devote a very large portion of the time of every English child to the careful study of the models of English writing of such varied and wonderful kind as we possess, and what is still more important and still more neglected, the habit of using that language with precision and with force and with art. I fancy we are almost the only nation in the world who seem to think that composition comes by nature. The French attend to their own language, the Germans study theirs; but Englishmen do not seem to think it is worth their while. Nor would I fail to include in the course of study I am sketching translations of all the best works of antiquity or of the modern world. It is a very desirable thing to read Homer in Greek; but if you don't happen to know Greek, the next best thing is to read as good a translation of it as we have recently been furnished with in prose. You won't get all you would get from the original, but you may get a great deal, and to refuse to know this great deal because you cannot get all seems to be as sensible as for a hungry man to refuse bread because he cannot get partridge. Finally, I would add instruction in either music or painting, or if the child should be so unhappy, as sometimes happens, to have no faculty for either of these, and no possibility of doing anything in an artistic sense with them, then I would see what could be done with literature alone; but I would provide in the fullest sense for the development of the æsthetic side of the mind. In my judgment these are all the essentials of education for an English child." Prof. Huxley concluded by saying that if the educational time permitted, there were one or two things he should be inclined to add to these essentials (which fitted an Englishman to go anywhere or to enter on any career); among these additional subjects he mentioned Latin and German. Beyond that, let each man take up his special line.

NOTES

THE Emperor of Germany has raised Prof. Helmholtz to noble rank.

THE two English observers, Messrs. Lawrence and Woods, detailed to secure photographs of the total eclipse of the sun on May 6, left Southampton for Panama on Saturday last. The operations will be exclusively photographic. The Treasury only determined to grant the necessary funds some fifteen days before the last date on which the observers could sail; the instruments sent out, therefore, were most hurriedly put together; and the greatest praise is due to Messrs. Hilger and Meagher for their work against time. Detailed instructions and a time table stating the work to be done for every second from ten minutes before totality till ten minutes afterwards, have been sent with the observers. If all goes well more than fifty photographs will be secured.

IN reply to the Memorial addressed to the Council of the British Association on the subject of the proposed meeting of the Association in Canada in 1884, signed by 144 members of the General Committee, Mr. Bonney states that the Council of the British Association are fully alive to the difficulties which will attend the visit to Canada decided upon by the General Committee at Southampton in August last. As this decision was obtained in accordance with the usual forms and does not appear to contravene the express wording of the rules of the Association, the Council feel bound to recognise it as a valid one, and believe that they would not be justified in summoning a special meeting of the General Committee to reconsider the question. They have, however, in effect already taken steps to ascertain the general feeling of the members of the Association. In the month of November last, after a consultation with Sir A. T. Galt, the High Commissioner for Canada in this country, the officers of the Association addressed to their intending hosts in Montreal a number of questions, upon the answers to which the success of the projected visit must greatly depend. To these questions they are now daily expecting a reply. As soon as this is received, information will be given to the Members of the Association, and inquiries made as to their willingness to visit Canada. The replies will enable the Council to judge whether it will be possible to hold a successful and fairly representative meeting at Montreal.

M. RAOUL PICTET has recently tried, on the Lake of Geneva, a specimen of his "rapid vessel," the general idea of which was indicated a short time ago. The vessel is figured in *Archives des Sciences* for January, and M. Pictet gives details of the theory and working. With a length of about 67 feet, and a width of 13 feet, this vessel is peculiar chiefly in having a bottom that is of parabolic form lengthwise, the concavity downwards; transversely the bottom is nearly straight; the sides are vertical. A keel reaching from about the middle of the length, incloses a screw shaft. Among other results M. Pictet shows that the force of traction of this vessel is always less than that of an ordinary vessel of the same general form and going at the same rate. The advantages of the parabolic curve only become apparent at a certain speed, depending on the width, length, and tonnage, and the parameters of the parabolic curve. The force of traction passes through a maximum, at a certain velocity for each vessel; beyond that point, the work of the motor, and so the expenditure of fuel, diminishes, though the speed increases. Experiment has yet to decide the limits of this second period. The emergence of the vessel, very small for small velocity, grows very quickly when a speed of 5 metres (say 17 feet) per second has been reached; and it converges rapidly towards an upper limit. The recoil of the screw for different velocities increases to a maximum, then constantly diminishes and tends to become *nil* for an infinite velocity. For other features of the action we must refer to the original. The engine we note proved faulty, and in several of the experiments the vessel was towed by a steamer, at velocities rising to 27 kilometres (say 17 miles); when this last is reached, an economy of one-half is realised (growing from 16 kilometres).

THE recent death of the Rev. Titus Coan, an aged and much-esteemed missionary at Hilo, Hawaii (where he laboured nearly forty-eight years), has been announced (*Am. Journ. Sci.*). He took a deep interest in the volcanic mountain at whose foot he lived, and at each eruption was generally the first on the ground to observe and report on the movements. Three times he ascended to the scenes of the eruptions connected with the summit crater. Though not a geologist, his accounts (many of them in the journal named) have always been of geological value. He was the principal historian of the great eruption of Kilauea in 1840, and the summit eruption of 1843, when the flow was uninterrupted for twenty-five miles and continued six weeks. It was after the latter eruption that he made the very important observation (since confirmed) that

Kilauea, though 10,000 feet lower in level than the summit crater, showed no change, no signs of sympathy whatever.

A DEPUTATION from a number of the Scientific Societies of London had an interview with Sir John Lubbock on Tuesday, for the purpose of asking him to oppose the Bill to authorise the construction of a railway through Epping Forest. It was stated that the line would greatly destroy the natural beauty of the Forest, and that the existing means of access to it were abundant. Sir John Lubbock said he would be prepared to assist in opposing the Bill; but it was pointed out that, as the Corporation and the Verderers had given their sanction to the scheme, it would be difficult to secure its rejection.

THE death is announced of Herr Thomas Dickert, well known by his geographical relief-maps. He died on January 11 at Poppelsdorf, near Bonn, aged eighty-two. Also of Dr. Bohdalek, formerly Professor of Descriptive Anatomy at Prague University, who died at Leitmeritz on February 2.

AT a public meeting held in Glasgow last week, called at the suggestion of Sir William Thomson and Mr. John Burns of Castle Wemyss, it was agreed to collect the money to establish a permanent and efficient observatory on Ben Nevis. The building will cost 2000*l.*, the instruments 1000*l.* In all 5000*l.* are required, and of that sum 1400*l.* has already been subscribed. The Government has refused to assist in the matter.

M. TRESCA read before the Paris Academy of Sciences on Monday his report on the experiments of M. Marcel Deprez; the distance being exactly 17,000 metres instead of 20,000 as at first asserted, and the motive-power 6·21 horse instead of 5, the percentage is exactly 0·326, a little less than one-third. It may be supposed that the percentage of primary engines, telegraph wires, and secondary engines is 0·70, so that the result obtained is just $(0·70)^3 = 0·343$, almost exactly the real value. The measurements have been taken with accuracy, and no error can be adduced. The number of revolutions of the primary machine was 588 in a minute. Others were tried on Monday with 814 revolutions, but it is too soon to judge of the result. M. Tresca having declined to do so, an Academical Commission has been appointed to report upon M. Deprez's theories. M. Tresca praised Mr. Hutchinson who made the electrical measurement with apparatus brought from London for measuring differences of potential and number of amperes. The electrical measures were verified with dynanometers.

DR. WARREN DE LA RUE has been elected by the Committee a Member of the Athenæum Club under Rule 2, which provides for the admission of persons eminent in literature, science, or the arts, or for public services.

THE usual sitting of the Congrès des Sociétés Savants will take place in Paris on March 27, 28, and 29 next. The Minister of Public Instruction will preside over the concluding meeting on the 30th. For the first time the Academy of Aërostation has been summoned to send delegates.

FROM the beginning of the next financial year Kew Gardens will be opened an hour earlier than at present, viz. at 12 o'clock instead of 1.

AT the Technical College, Finsbury, the introductory address was given by Mr. Philip Magnus, Director and Secretary of the Institute, on Monday evening last. Sir Frederick J. Bramwell, F.R.S., was in the chair.

WE are glad to see that the new and spirited Scottish quarterly, the *Scottish Review*, does not neglect science. In the February number, which is just out, there appears an article on Medical Reform and an appreciative estimate of the late James Clerk Maxwell.

THE *Journal Télégraphique du Bureau Central de Berne*, summarising the principal lacunæ in the universal system of telegraphy, notes as one the construction of a line to Iceland for recording the principal atmospherical events observed in the Polar regions.

A DISTINGUISHED Swedish entomologist, Gustaf Wilhelm Belfrage, has recently died in Texas, where he had been for some years residing. The deceased had collected and forwarded a number of entomological specimens to the Swedish Academy of Sciences in Stockholm, for which he had received a State grant.

AN International Exhibition of Garden Produce and a Botanical Congress will be held in St. Petersburg this summer.

REPORTS from Lower Bavaria announce the discovery of auriferous and argentiferous sand deposits. They are confined to a layer of gneiss which occurs in the granitic rocks for a length of about fifteen or eighteen miles, between the villages of Innernzell and Zenting. It appears that 100 kilogrammes of the sand contain about 10 to 15 grammes of pure silver and between 2 and 10 grammes of pure gold; the sand from 4-6 metres depth is even richer. The weathered gneiss partly carries gold and silver and partly gold only; no special form is marked in the occurrence of the auriferous sand; there are deposits that seem to be alluvial, others which occur in the firm rocks, others again in distinct veins of mica slate, and still others in exposed gneiss which is many yards high.

IN a recent communication to the Vienna Academy, Prof. Graber, of Czernowitz, describes a long series of experiments with regard to the "skin-vision" of animals; affording exact proof that certain animals, without the aid of visual organs proper, can make not only quantitative but qualitative distinctions of light. These experiments relate chiefly to the earthworm, as representing the eyeless (or "dermatoptic") lower animals, and to the *Triton cristatus*, as representative of the higher ("ophthalmoptic") eyed animals. In a table Prof. Graber presents columns of numerical "coefficients of reaction," indicating how many times more strongly frequented a space illuminated with bright red, green, or white without ultra-violet, is, than one illuminated dark blue, green, or white with ultra-violet respectively, the conditions being the same as regards light-intensity, radiant heat, &c. In one set of experiments, the animals were in the normal state; in another, the anterior end of the worm, and the eyes of the triton were removed.

"CATALOGUES of the New Zealand *Diptera*, *Orthoptera*, and *Hymenoptera*, with Descriptions of the Species," by F. W. Hutton, F.G.S., Professor of Biology at Canterbury College, N.Z., have been published by the Colonial Museum and Geological Survey of the Colony. They consist of reprints of the original descriptions of such species in the orders named as have been described from New Zealand, without, as a rule, critical remarks, and form an amplification of Lists already published in the *Trans. N.Z. Institute*. Only 227 species for the three orders are enumerated. Although this publication is dated 1881, it has only just been received in England. In some respects it is already obsolete, especially in *Hymenoptera*. Mr. Kirby in 1881 enumerated 81 species in this order, Prof. Hutton enumerates about 71, which should be still further reduced from synonymic considerations.

THE Belgian Academy offers a prize of 3000 francs (120*l.*) for the best treatise on the destruction of fishes by the pollution of rivers. Several points are to be treated of which relate to the impurities which find their way into rivers from the principal branches of trade and the manufactures, and also to the practical means for rendering these impurities harmless. The treatises

competing for this prize are to be sent in before October 1, 1885.

EARTHQUAKES are reported from Silesia and North-Eastern Bohemia. Two shocks were noticed on January 31, at 2.40 p.m., at Trautenau. Their direction was from south-west to north-east. They were also felt at Braunau, Jungbuch, Freiheit, Marschendorf, Grossaupa, Spindelmühle, and Johannisbad, and also at Görbersdorf and Landeshut. The motion was undulatory and lasted from three to five seconds. No damage was done.

THE Paris papers report the extraordinary run of a small hydrogen gas balloon, capacity about two gallons, which, having been liberated at Bercy, was discovered at Grodno in Poland, having travelled more than two thousand miles; it is the longest air journey on record for so small an object.

THE French gas companies have instituted at their common expense a laboratory for testing the several inventions reported in electric lighting, and proving whether they are valuable or not. After alluding to this foundation, and the much-spoken-of experiments tried at the French Great Northern Railway Station, a French scientific periodical says: "Mieux vaut un sage enemi qu'an imprudent ami."

THE additions to the Zoological Society's Gardens during the past week include a Green Monkey (*Cercopithecus callitrichus* ♂) from West Africa, presented by Mr. J. F. Williams; a Punjaub Wild Sheep (*Ovis cycloceros* ♂) from North-West India, presented by Lieut.-Col. C. S. Sturt, C.M.Z.S.; a Thar (*Capra jemlaica*) from the Himalayas, presented by Lieut.-Col. Alex. A. A. Kinloch, A.Q.M.G., C.M.Z.S.; a Blyth's Tragopan (*Ceriornis blythi* ♂) from Upper Assam, a Fytbch's Partridge (*Bambusicola fytbchi*) from Upper Assam, presented by Capt. Brydon; a Small Hill Mynah (*Gracula religiosa*) from South India, presented by Dr. Rogers W. Taylor; a Macaque Monkey (*Macacus cynomolgus* ♂) from India, a Common Cormorant (*Phalacrocorax carbo*), British, deposited; three Stump-tailed Lizards) *Trachydosaurus rugosus*) from Australia, purchased.

OUR ASTRONOMICAL COLUMN

THE GREAT COMET OF 1882.—The following places for Berlin midnight are derived from Dr. Kreutz's ellipse:—

1883.	R.A. h. m. s.	Decl. ° ′	Log. Distance from Earth.	Sun.
February 26	5 52 10	−15 43·3	0·4551	0·5122
28	5 51 43	15 17·1	0·4629	0·5158
March 2	5 51 23	14 51·5	0·4705	0·5193
4	5 51 8	14 26·5	0·4781	0·5227
6	5 51 0	14 2·1	0·4856	0·5261
8	5 50 57	13 38·4	0·4930	0·5295
10	5 51 0	−13 15·4	0·5003	0·5329

Mr. E. E. Barnard, of Nashfield, U.S., reports that on the morning of October 14 he found to the south of the comet a large, distinct cometary mass, fully 15′ in diameter, and a similar but less bright object close beside this, their borders touching, and on the opposite side of the first a third fainter mass: the three were almost in a line, east and west. More of these cometary masses were found towards the south-east: there were at least six or eight within about 6° south by west of the head of the great comet. Their appearance was that of distinct comets with very slightly brighter centres, several being in the field at once. They were not seen again after being obscured by daylight on the morning of October 14.

Dr. Julius Schmidt's observations of a cometary mass near the head of the great comet are already published in No. 2468 of the *Astronomische Nachrichten*.

On the 5th inst., with the large refractor at Strasburg, the comet had two stellar nuclei, the fainter of the two on an angle of 246°, and 38″ distant from the brighter, which was observed for position. On January 27, Mr. Ainslie Common, of Ealing, with his large reflector, saw the nuclear part of the comet larger but less bright than previously, and resolved into a string of brightish points, the second and third of which were much the brightest. The position-angle was 240° 20′, and the distance between the brighter points was 31″·5, so that they doubtless correspond to the two "fixternartige Kerne" observed at Strasburg. In a sketch with which Mr. Common has favoured us, five points of condensation are shown; it was made at 9 P.M. on January 27.

VARIABLE STARS.—Dr. Julius Schmidt has published his usual summary of results of observations of variable stars, made at Athens in 1882. Minima of Ceraski's variable U Cephei occurred on November 25 at 8h. 57·2m. mean time at Athens, and on November 30 at 8h. 36·5m. Minima of Algol on November 29 at 11h. 30·4m., and December 2 at 8h. 7·1m., the first determined from observations extending over 5·4h., and the second from an interval of 7·5h. R Hydræ was at maximum on March 8, when it attained 4·3m. Mira Ceti at minimum on February 4, magnitude 9·5; the statement in some of our popular treatises on astronomy, that this star disappears at minimum is erroneous; its average brightness at that time is about 9m. on Argelander's scale, according to the most experienced observers. χ Cygni was at maximum September 1·5, the predicted date being August 25. The variations of α Herculis during the year were small, but well fixed by numerous observations; the period, as usual, irregular; the same may be said of *g* Herculis. T Cephei at maximum on January 11, 6·7m., the increase of light much quicker than the decrease; V Coronæ at maximum September 15·6; the fine variable R Leonis was at maximum on May 20, 6·5m., and at minimum on November 6, 9m.; R. Piscium at maximum on December 5·3, the increase of light slower than previously; Palisa's variable in Scorpio at maximum July 9·7, 12m.; of R Scuti, a maximum occurred October 11, well-determined minima, on June 21 and December 6; Harding's variable R Virginis was at maximum April 16·6, and at minimum June 30·5, the limits of brightness being 7m. and 11·7m.

It is much to be desired that the number of observers of these interesting objects should be largely increased; their observation opens up a field of useful work, even to an amateur with the most modest of optical appliances. At present our knowledge of the subject is mainly due to the systematic labours of the indefatigable director of the Observatory at Athens.

A NEW NEBULA.—Mr. Barnard notifies his discovery of a new nebula 1° 48′ north, and 5m. 39ˢ. west of φ Virginis. It was observed with the 15-inch refractor at Harvard College by Mr. Wendell, and described as "rather diffuse and faint, but gradually a little brighter in the middle"; its position for the beginning of 1882 is in R.A. 14h. 16m. 19·6s., Decl. +0° 9′ 14″. This nebula is not found in the Harvard Zones, Nos. 53 and 54, observed on May 9 and 11, 1853, and which would overlap its place, though three new and faint nebulæ were first detected in those Zones, viz. Nos. 33-35 of Prof. Auwer's Catalogue of new nebulæ in the Königsberg observations. This object may be worth watching, on the score of possible variability.

GEOGRAPHICAL NOTES

IN NATURE last week we announced that an Arctic expedition this summer had been decided on in Sweden. This expedition, which has been promoted by the well-known Swedish Mæcenas, Dr. Oscar Dickson, will be in command of Baron Nordenskjöld, whose intention it is on this occasion to explore the east and north-east coast of Greenland. It was originally his intention to have proceeded this summer into the Siberian seas, but seeing the delay caused to the Danish Polar Expedition, which will now be there during the summer, this idea was abandoned and Greenland decided on instead. Baron Nordenskjöld, having formerly visited the country, is of the opinion that some kind of "break," or oasis, is to be found in the interior of Greenland. He purposes to proceed along the east coast of Greenland, as far as the ice will allow, and then to penetrate into the interior, some 300 miles across the inland ice. The country inland is nearly the whole year covered by ice and snow, which, during the summer months, render it almost entirely one bog. The enormous stretch of inland ice has also always been a barrier to exploration. Another object in view by Baron Nordenskjöld is to attempt to find traces of the Norse colonies, which existed in Greenland

from about the year 1000 until the end of the 14th century. The ultimate fate of the Norse settlers in Greenland is shrouded in mystery, as there is no authentic record of their existence after the end of the fourteenth century. There has also in later days been great diversity of opinion where to seek for the settlements; thus the Danish explorer Graah, who, in the years 1828-31, searched for remains of the same, sought them west of Cape Farewell, but without success. Baron Nordenskjöld is, however, of the opinion that the Österbygd and the Norse settlements were situated east of the Cape, and it is here that he intends to search for them. It is hardly necessary to enlarge on the interesting and important results to science which would accrue from the discovery of these "dead cities" on the shores of the Arctic Ocean. Baron Nordenskjöld will start on his journey early in May next, and although the general expenses of the expedition, no doubt, will be defrayed by King Oscar and Dr. Oscar Dickson, it is the intention of the latter to apply to the Swedish Parliament for the use of one of the vessels of the Navy for the voyage.

More details have now reached us concerning the expedition of the African travellers, Lieut. Wissmann and Pogge. The travellers proceeded along the Kassai River during the autumn of 1881, passed through Kimbunda and reached Kidimba, the residence of Chingenge, the chief of the Tooshilange tribe, in November. Then they proceeded northwards. They reached the frontier of the West African savannah-forests and entered upon the densely populated prairies of Central Africa. In the middle of December they reached the Mukamba Lake. Now they traversed the well-populated country of the Bashilange and reached the Lubi, a magnificent river bordered by rich tropical vegetation, and which is a tributary of the Lubilash river. The opposite shore of the Lubi is inhabited by the Bassonge, a handsome and powerful tribe, which possesses numerous clean and cheerful villages adorned by palm and banana trees. On January 14, 1882, the travellers reached the capital on the left bank of the Lubilash, in 5° 7′ 18″ lat. S. Kachich, the chief of the Kotto district, whose power is based upon his reputation of fetishero (high priest), caused many obstacles to be thrown into their way. At last, on January 29, the expedition crossed the Lubilash, which is identical with the Sankura, and which flows into the Congo. This was in 5° 13′ lat. S. Then they passed through well-watered prairies, inhabited by the warlike Bassonges, by the Beneckis, who have villages 17 kilometres in length, and the Kalebues, reaching and crossing the Lomami River on March 8. All these tribes are cannibals. Between the Lubi and Lake Tanganyika, Wissmann found remains of what must once have been the natives of these parts, viz. the Batuas, little, undergrown, slender, dirty, and savage-looking people, who live only by the chase and on wild fruit, speak a curious language, and whose arms and implements indicate a very low state of civilisation. The Lomami was crossed in 5° 42½′ lat. S. The direction towards Nyangwe was now taken through flooded prairies and marshes, alternating with parts where the grass had grown to a perfect carpet resembling felt. The Lufubu River was crossed on April 2. By April 11 two canoes had been made. On April 16 the expedition reached the Lualaba River, and Nyangwe on the 17th, where they were well received by the Arabs. Here they resolved to separate. Pogge was to return to the Mukenge Station with the caravan, and Wissmann to the east. On May 5 Pogge left. Wissman started on June 1 with only a few companions, and eventually reached Cassongo and then Lake Tanganyika. At Manyema he had gone south of Stanley's and Cameron's route, and afterwards crossed it at Ca=Bambarre, passing northward into the land of the Wasi-Malungo and Ubngwe tribes towards Uguhla. On the shores of Lake Tanganyika Wissmann rested for fourteen days, staying at the missionary station of Ruande. He made an excursion to the Lukuga River and crossed the lake to Ujiji. On August 9 he left the caravan track, proceeding in a northerly direction to Uhha, to visit the renowned chief, Mirambo. Passing through many great dangers he reached Mirambo's residence, and was most hospitably received. On September 3 Wissmann reached the French mission-station at Tabora, from whence he made an excursion to the German African Society's station at Gonza. There he considered his geographical work as completed, inasmuch as Dr. Kaiser had proceeded to Gonza from the east coast. Wissmann found Dr. Boehm and Reichard both in good health, Dr. Kaiser having left a few days before. On November 18 Wissmann reached the east coast near Saadani.

It is announced by the hon. secretaries of the Egyptian Exploration Fund that Sir Erasmus Wilson, LL.D., F.R.S., has accepted the office of President of the Society, and has headed the subscription list with a donation of 500*l*. Thus launched, the Society has commenced excavations at Tel-el-Maskhuta, in the Wady Tumilat—this mound being the supposed site of Raamses, one of the two cities specified in the first chapter of Exodus as built by the forced labour of the Hebrews. M. Edouard Naville, the eminent Swiss Egyptologist, in co-operation with Prof. Maspero, has undertaken the direction of the excavation on this important site, where he is now at work, aided by an experienced engineer, and a gang of eighty labourers. The results to be anticipated from discoveries at Tel-el-Maskhuta are inscriptions which shall enable Egyptologists to identify the Pharaoh of Moses, to assign a dynastic date to the period of the oppression, and to settle the much-disputed question regarding the route of the Exodus. More funds are needed for the prosecution of the work already begun, and it is hoped that the public will liberally support the action of Sir Erasmus Wilson. Pending the election of a treasurer, subscriptions will be received by the hon. secretaries, Mr. Reginald Stuart Poole, British Museum, and Miss Amelia B. Edwards, the Larches, Westbury-on-Trym, Bristol.

In the March number of *Petermann's Mittheilungen* the principal paper is an account of Herr Fr. von Schenck's journey in the United States of Columbia in 1880, an important contribution to the physical geography of a country on which we have no very recent information. Dr. Capus gives some interesting information on the valley of Yagnan and its inhabitants, about 170 versts east of Samarcand. There is a brief sketch of Herr Schaver's journey to the sources of the Tumat, Jabus, and Jal, in the region lying between the Upper Bahr-el-Azrek and Bahr-el-Abiad. This number contains the Necrology for 1882.—In Nos. 10, 11, and 12 (in one) Band xxv. of the *Mittheilungen* of the Vienna Geographical Society is a paper, with map, by Dr. J. Morstadt on the mountain structure of South Tyrol. An important work in ten vols. on the peoples of Austria-Hungary, by many authors (Vienna, Prochaska), is reviewed by Dr. Paulitschke.—Nearly the whole of the *Compte Rendu* of the Paris Geographical Society for December 15 is occupied by M. Desiré Charnay's account of his explorations in Yucatan.

ON THE PRESENT CONDITION OF THE SODA INDUSTRY

An interesting and important paper with the above title was read by Mr. Walter Weldon, F.R.S., at a meeting of the Society of Chemical Industry held at Burlington House on January 8. The following abstract is condensed from this paper as published in the *Journal* of the Society:—

A few years ago there were twenty-five alkali-works in the neighbourhood of Newcastle-on-Tyne; now there are only thirteen. Seven or eight works are standing idle in Lancashire; in Belgium the manufacture of soda by the Leblanc process has entirely ceased. The following table represents the

Present Soda Production of the World in tons

	Leblanc soda.	Ammonia soda.	Totals.	Ammonia soda per cent. of total soda.
Great Britain ...	380,000 ...	52,000 ...	432,000 ...	12·0
France	70,000 ...	57,125 ...	127,125 ...	44·9
Germany	56,500 ...	44,000 ...	100,500 ...	43·8
Austria	39,000 ...	1,000 ...	40,000 ...	2·5
Belgium	— ...	8,000 ...	8,000 ...	100·0
United States ...	— ...	1,100 ...	1,100 ...	100·0
Totals ...	545,500	163,225	708,725	23·0

The ammonia process for making soda dates, as a practical manufacturing method, from 1866, in which year M. Solvay of Brussels established works at Couillet, near Charleroi. M. Solvay is now manufacturing soda by the ammonia process at the rate of about 75,000 tons per annum.

The production of soda has very rapidly increased on the Continent within the last five years; the greater part, but not the whole, of this increase is due to the introduction of the ammonia process. The production of soda by this process in England is entirely in the hands of one firm—Messrs. Brunner and Mond: in 1875 this firm produced 2500 tons of soda, in

1880 they produced 18,800 tons, and their output is now at the rate of 52,000 tons per annum. The new works now in course of construction in this country and on the Continent, when completed, will at once increase the production of ammonia soda by 65,000 to 70,000 tons annually.

What then can the manufacturer of Leblanc soda expect save utter collapse? But the state of the alkali-maker threatens to become even worse than it is. The source of the sulphur which is used in the Leblanc process is pyrites; the pyrites employed in this country is almost exclusively imported by three large companies from Spain and Portugal; it contains from 2 to 3 per cent. of copper, and very small quantities of silver and gold. When the soda manufacturer has burnt off the sulphur, he sends the residual ore to the copper extractor, who is able to sell the iron oxide which remains when he has taken out the copper at about 12*s.* per ton. Now the French soda-manufacturers make use of pyrites of their own, which contains little or no copper; one of the large companies which supplies the English market purposes, therefore, to start works in France, which shall employ Spanish pyrites, but which shall depend for their profits, not on the soda which they manufacture, but on the copper and iron oxides remaining after the sulphur has been burnt off from the pyrites. This company, which starts with a capital of over a million sterling, speaks of building five large works in France, and one in the neighbourhood of Antwerp.

The Leblanc soda manufacturers have tried to persuade themselves that the price of ammonia must rise considerably, and that thus they may be able to compete with the ammonia soda-makers on more equal terms than at present. But in place of ammonia becoming dearer, its price is steadily falling. New sources of ammonia are being found; a process for collecting ammonia and other volatile products from coke-ovens, which is easily applied to existing ovens, has recently been patented by Mr. J. Jameson of Newcastle-on-Tyne. If this method should be generally applied to the coke-ovens in this country, a quantity of ammonia corresponding to 180,000 tons of ammonium sulphate, worth about three and a half millions sterling, would be annually saved.

Mr. Ferrie—a member of the great iron firm of William Baird and Co.—has also contrived a method whereby the ammonia and tarry matters which are present in the gases of the blast furnace may be condensed; this process has been at work for some time at Gartsherrie, and by its help about 20 lbs. of ammonium sulphate are obtained per ton of coal burnt in the blast furnaces.

Another difficulty which presses heavily on the manufacturer of soda by the Leblanc process consists in the want of an outlet for the great quantities of hydrochloric acid which accumulate during the soda manufacture.

This difficulty is not felt by the Continental manufacturer because he finds a ready market for the chlorine which can be extracted from hydrochloric acid; but in England the supply of chlorine at present much exceeds the demand. But Mr. Weldon holds out hopes to the English chlorine-maker; he says: "I think that our English manufacturers of Leblanc soda will have to cease to devote their hydrochloric acid—when they do not throw it away—exclusively to chlorine making; . . . the difficulty hitherto has been how to turn it to account otherwise. I believe that difficulty is about to disappear. I am not free to enter into that matter now; . . . but I have very great confidence that new applications of hydrochloric acid, admitting of being applied very extensively, at comparatively small expense, are among the things of the immediate future."

Mr. Weldon then considers the ways in which the English manufacturer of Leblanc soda may hope to recover himself and again make soda at a reasonable profit. First of all, he must get his pyrites about 50 per cent. cheaper than the price he now pays for it; the present combination between the pyrites companies will expire at the end of next year; after that time the price of pyrites must, in Mr. Weldon's opinion, fall very considerably.

Secondly, the soda-manufacturer must recover all the sulphur in his alkali waste; if he can recover the sulphur at a cost not exceeding 2*l.* per ton, he will become master of the sulphur market, as the actual cost of Sicilian sulphur delivered at Marseilles is now about 5*l.* per ton.

The third thing which the soda-manufacturer must do is to distil the coal which he now uses as fuel, condense and sell the volatile products, including tar, oils, and ammonia, and employ the residual coke as fuel; he will thus get his fuel for nothing, and at the same time will confer an inestimable boon on the towns where coal is now largely used as fuel.

These three courses, says Mr. Weldon, must be all adopted by the English soda-maker. If, in addition to doing this, the strictest economy in manufacture is practised and the purest and best product that can be made is always turned out, the manufacturer of soda by the old Leblanc method may yet hope to hold his own against the new and wonderfully successful ammonia process. M. M. P. M.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—The following persons have been elected Members of the Committee for the nomination of examiners in the Natural Science Schools: Prof. R. B. Clifton; Prof. W. Odling; and Prof. H. N. Moseley. The Vice-Chancellor and Proctors complete the Committee. Up till this term the nomination of examiners lay with the Vice-Chancellor and Proctors, who appointed in turn.

The Examiners for the Burdett-Coutts Geological Scholarship have recommended Mr. F. W. Andrews, of Christ Church, for election.

Magdalen College advertises a demyship in Natural Science to be competed for in June.

CAMBRIDGE.—The following further appointments of Boards of Electors to Professorships have been made:—

Mineralogy:—Prof. Story-Maskelyne (Oxford), Dr. H. C. Sorby, Profs. Stokes, Warrington Smyth, and Liveing, Dr. Phear, Dr. Percy, and Mr. Glazebrook.

Mental Philosophy and Logic:—Prof. Croom Robertson (Univ. Coll. Lond.), J. B. Mayor (King's Coll. Lond.), and Adamson (Owens College), Messrs. H. Sidgwick, J. Ward, J. Todhunter, Shadworth H. Hodgson, and the Master of Trinity College.

Music:—Sir F. Ouseley, Messrs. Pole, T. P. Hudson, G. Grove, Sedley Taylor, G. F. Cobb, R. Pendlebury, and E. S. Thompson.

MR. ALBERT SCHAFER, F.R.S., Fullerian Professor of Physiology at the Royal Institution, has been appointed Jodrell Professor of Physiology at University College, London, in the vacancy occasioned by the resignation of Dr. J. Burdon Sanderson, LL.D., F.R.S., appointed Waynefiete Professor of Physiology in the University of Oxford.

SOCIETIES AND ACADEMIES

LONDON

Chemical Society, February 1.—Dr. Gilbert, president, in the chair.—The following were elected Foreign Members:—F. Beilstein, P. T. Cléve, H. Debray, E. Erlenmeyer, R. Fittig, H. Helmholtz, D. Mendeleeff, Victor Meyer, Lothar Meyer. The following were elected ordinary Fellows:—H. C. Bond, G. C. Basu, J. Brock, A. M. Chance, J. T. Donald, H. C. Foote, W. Fox, W. R. Flett, J. A. M. Fallon, E. C. Gill, F. Gothard, J. Hunter, H. Jones, B. R. Lee, A. H. Jackson, Joowansinghi, T. Jenner, J. E. Johnson, W. W. J. Nicol, F. W. Richardson, E. S. Spencer, C. A. Serré, T. Turner, J. E. Tuit.—The following papers were read:—On derivatives of fluorene, by W. R. E. Hodgkinson and F. E. Matthews. The fluorene was crystallised five or six times from alcohol; it melted at 113°; when pure, it does not fluoresce. A dibrom and monobrom derivative were obtained, and a fluorene sulphonic acid; by the action of caustic potash on the potassium sulphonate, a trihydroxy-diphenyl was formed; and by dropping the hydrocarbon into fused caustic potash, a dihydroxy-diphenyl was procured.—On the action of chlorine on certain metals, by R. Cowper. As observed by Wanklyn, dry chlorine has no action upon melted sodium; the author finds that dry chlorine has no action upon Dutch metal, zinc, or magnesium; it acts very slowly upon silver and bismuth; tin, arsenic, and antimony are attacked rapidly, with evolution of heat.—Some notes on hydrated ferric oxide, and its behaviour with sulphuretted hydrogen, by L. T. Wright. The author found great difficulty in obtaining ferric hydrate, by precipitating the chloride with ammonia, free from basic chloride. Having poured some ferric chloride into an excess of ammonia, he evaporated to dryness at 100°. The residue, when treated with water, gave a reddish solution which would not yield a clear filtrate, some of the

iron being probably in the so-called "colloidal" condition. Such ferric hydrate is not turned black by sulphuretted hydrogen; ordinary ferric hydrate is turned black at once, and the sulphide of iron dissolves in excess of potassium cyanide, forming potassium sulphide and ferrocyanide.—On alpha cyanonaphthalene sulphonic acid, by W. K. Dntt. The author first prepared the naphthalene sulphonic acid, then distilled the potassium salt with dry potassium ferrocyanide, and converted the cyanonaphthalene by sulphuric hydrochloride into the above substance.

The Institution of Civil Engineers.—February 13, Mr. Brunlees, president, in the chair. The paper read was on "The Design and Construction of Repairing-Slipways for Ships," by Mr. T. B. Lightfoot, M. Inst. C.E., and Mr. John Thompson.

EDINBURGH

Royal Society, January 29.—Mr. Thomas Stevenson, M.Inst.C.E., vice-president, in the chair.—Dr. Knott read a paper by Mr. H. R. Mill on the rainband, being a description of the author's observations during the last six months of 1882. The observations were all made with Mr. Hilger's smallest size of pocket spectroscope, in which the presence of the rainband is indicated only by an apparent broadening of the D line. Mr. Mill measured the varying intensities of the rainband by comparing D with the other evident lines in the spectrum—E, *b*, F. The distinctness of the fine lines in the green was also found to be an additional factor in prognosticating the weather; the less distinct these lines the greater the chance of rain. An analysis of the observations showed that of the "rain" predictions 78 per cent. came true; of the "no rain" predictions 64 per cent. —The Rev. J. L. Blake read his third communication on the theory of monopressures applied to rhythm, accent, and quantity. —Mr. John Aitken read a paper on the effect of oil on a stormy sea, in which it was proved by experiment that the presence of the oil film did not calm the waves, but merely prevented them from breaking. The reason given was that the wind had no power to produce wavelets on the oil-surface, since in virtue of the action of surface-tension any forward motion of a portion of the oil-film necessitated the forward motion of the whole. In the case of a clean water surface, again, the wind acting strongly upon any small surface portion would push it *over* the contiguous surface, and so give rise to a wavelet. Some beautiful experiments on the effect of surface-tension were shown as bearing upon the subject.—A note was read from the Astronomer-Royal for Scotland calling attention to the remarkably high temperature maximum which had occurred some time during the preceding night.

CAMBRIDGE

Philosophical Society, February 12.—The following communications were made to the Society:—On the isochromatic curves of polarised light seen in a uniaxal crystal cut at right angles to the optic axis, by Mr. R. T. Glazebrook.—On a spectrophotometer, by Mr. R. T. Glazebrook. The paper describes an arrangement for viewing simultaneously the spectra formed by the light from two different sources after traversing the same set of direct-vision prisms. These two spectra are polarised in two planes at right angles and their relative intensity is determined by the position of a Nicol in the eye-piece through which they are observed.—On a common defect of lenses, by Mr. R. T. Glazebrook. The author exhibited some lenses which, when placed between two crossed Nicol's prisms, showed strong elliptic polarisation.—On the motion of a mass of liquid under its own attraction, when the initial form is an ellipsoid, by Mr. W. M. Hicks.—On functions of more than two variables analogous to Tesseral Harmonics, by Mr. M. J. M. Hill.—Observations of the transit of Venus across the sun, taken near Kingston, Jamaica, December 6, 1882, by Dr. J. B. Pearson. In this paper the author described observations taken by himself of the late transit of Venus. He unfortunately missed seeing the first external contact, and only first saw Venus when she had intruded about one-third of her sphere on the sun's disc. On the internal contact he noticed no kind of black drop, or sympathetic attraction or assimilation between the limb of the planet and that of the sun. It seemed to him that when the planet was actually projected on the sun's disc, about 20" before the time he assigned for actual contact, the black surface of the planet adjoining the atmosphere seemed to begin to be picked out with little white dots commencing very probably from either side. He could not say that he actually saw two horns of light gradually advancing until their points touched, but rather that the segment of the planet nearest the sun's limb, and still obscure, began to be speckled with white dots which in not more than twenty seconds, or twenty-five at the outside, developed into a white line. He saw nothing like an atmosphere around Venus, though he looked carefully for it; it was possible that his telescope, considerably smaller than what might be called the authorised size, was not large enough to show it.

BERLIN

Physiological Society, January 26.—Prof. du Bois-Reymond in the chair.—Prof. Fritsch, who, in his study of the torpedo at the zoological stations of Naples and Villafranca, has discovered, in addition to the facts already published, a series of new facts in reference to the development of this electric fish, combined these facts with those already discovered by previous investigators, and thus produced a general sketch of the development of this remarkable animal before the Society, illustrated by numerous preparations. The torpedo exhibits so many different forms in its ontological development that already de Sanctis distinguished a squaliform stage, a raiform stage, and a torpediform stage; and in fact the different stages, as the lecturer demonstrated in his series of preparations, first resemble shark-embryos, afterwards pass over into the form of rays, and finally change into that of torpedoes by the development of the electric organ. The first embryonic beginnings of the electric organ have the greatest resemblance to embryonic muscular fibres. Upon longitudinal section, there are to be seen in the interior of sheaths consisting of connective-tissue-cells very distinct longitudinal fibrous striæ, with traces of transverse striation and many oval nuclei. In a later stage, on making a longitudinal section, the longitudinal fibration and transverse striæ are seen to have entirely disappeared; the nuclei have become much more numerous and circular, and in the interspaces the disc-like elements of the pillars that are to be developed are already to be seen as transverse striæ. The whole represents, in a sheath of connective-tissue, a granular mass of protoplasm with numerous nuclei. On making a transverse section, we see in the first stage, in which the organ resembles embryonic muscular-tissue, the cut ends of the longitudinal fibres as circular contours in an homogeneous connective-tissue. When the electric organ is further developed, there is seen, on making a transverse section, a polygonal net of connective-tissue, in whose meshes the round pillars lie, being separated from the walls by cellular masses. Hence Prof. Fritsch believes that the histological development of the electrical organ is analogous to the transformation of normal muscle in myomata, and that it would not be incorrect to call the electric organ a normal myoma. The phylogenetical development of the torpedo has already been described in the account of its ontogenetical development. The electric organ is developed from muscle, and indeed from the outer gill-muscles of the fifth gill-arch. The gill-arch muscle, which develops in rays and sharks into the extraordinarily powerful lower-jaw muscle, is wanting in the torpedo, and in its place we find the electrical organ, which is, comparatively speaking, a more serviceable weapon of offence and defence to the small animal than the lower-jaw muscle of the related predatory-fishes. The lecture was illustrated by a great number of microscopic and macroscopic preparations.

Physical Society, February 2.—Prof. v. Helmholtz in the chair.—Dr. Hertz described a series of peculiar light-phenomena which he had observed in the case of electric discharges. When, in a moderately rarefied space (pressure about 20 to 30 mm. of mercury), the electric discharge takes place between electrodes, one of which is fixed in a tube that is closed at one end and drawn out to a small opening at the other end, while the second electrode is placed laterally near the opening of the tube, the spark of discharge springs from the opening, laterally, to the second electrode; at the same time, however, one sees a ray of yellow-brown light break forth from the tube, reaching out a few centimetres in the prolongation of it. With stronger or with weaker pressure, the ray is shorter and less luminous; and if a Leyden jar be inserted, the ray is also shorter, but it is more luminous. The form of this ray (which broadens at the end) is very varied; and if it impinges on the wall of the vessel inclosing the rarefied space, it produces whirling there. The colour of the ray is different according to the gas: yellow with air and oxygen, blue with hydrogen, &c., and spectrum analysis shows that it is the respective gases that glow. If a small

mica-disc be introduced into the luminous ray, it enters into oscillation; and a small mill is set in rotation by the ray. This proves that real material particles, glowing masses of gas, are driven forth in the discharge from the tube. The wall on which the ray impinges is strongly warmed, and a thermometer put into the ray rises 10° to 20°. If the ray, which to the naked eye seems quite continuous, be looked at through a slit in a rotating disc, so arranged that the slit, in different, very short intervals of time after each opening of the primary current of the induction-coil, passes before the eye, one sees in the first moment a small ray at the opening, then, at a later moment, a small cloud above the opening, and finally a larger luminous cloud floating at a greater distance from the opening. The light-ray is thus discontinuous; and at each spark-discharge separate clouds of glowing gases are driven out from the tube, which are ever enlarging. Even at atmospheric pressure these light-phenomena may, with careful observation, be perceived. They occur mostly in the air as yellow sheaths about the aureoles of the sparks, and with different electrodes present manifold forms: sheaths, swellings, whirls, and the like. In moist air the phenomenon is quite absent, and in hydrogen it soon ceases. The great variety of the appearances have not yet been brought under one common standpoint.—Dr. Goldstein had observed similar phenomena to those just described by Dr. Hertz, and made a number of experiments regarding them. In spectral tubes he saw the yellow light appear at the places of passage from the thin to the wider parts, in cylindrical tubes, on the other hand, the yellow light always surrounded the red discharge-light as an envelope, which in the neighbourhood of the cathode gradually widened, and from there progressively filled the tube. If evacuation be effected during the discharge, one sees that the yellow light, with the air, is driven out of the tube. This glowing of the gas Dr. Goldstein connects with the long-known after-luminosity of Geissler tubes, which he has sometimes found to last many seconds, and even some minutes, after discharge. The essential thing in the case of phosphorescent Geissler tubes is the change between wider and narrower parts, because only at the places of transition does the after-luminosity develop that light—yellow in air, blue in hydrogen, and other colours in other gases.

PARIS

Academy of Sciences, February 12.—M. Blanchard in the chair.—The following papers were read:—On the difference of barometric pressures at two points of a given vertical, by M. Jamin. He shows from records of the double observatory at the base and at the top of the Puy de Dôme, for 1880, that the difference of pressures varies very regularly every day and throughout the year, diminishing till 3 p.m., then increasing till sunrise, also increasing from the summer to the winter solstice. Kaemitz, in 1832, proved such variation with the season in Switzerland. Similar effects, due to temperature, doubtless occur everywhere. We have to conceive an atmospheric enlargement, a kind of air-tide, moving round with the sun. The resulting phenomena are complex. M. Jamin shows how the variations of the difference of pressures in a given vertical, with changes of temperature, pressure, and hygrometric state, may be calculated.—Researches on chromates, by M. Berthelot.—On the groupings of the animal world in primary times (second note), by M. Gaudry. Each of the epochs seems to have had special expansions, beings that began with it and ended with it. The irregularities met with do not favour the idea of a struggle for life in which the victory was to the strongest and best-endowed. There are many striking personalities, *rois de passage* (so to speak), giving the epochs a character of their own, so that as we speak of the age of Charlemagne, &c., we may say the age of *Paradoxides*, of *Pterichthys*, &c. But it is often the most specialised and perfect beings that have disappeared. Other types, representing the just mean, have persisted.—On the numbers of unequal ordinary fractions which may be expressed by using figures which do not exceed a given number, by Mr. Sylvester.—Refutation of a second critique by M. Zeuner, &c. (continued), by M. Hirn.—Researches on the *rôle* of inhibition in a special kind of sudden death, and with regard to the loss of consciousness in epilepsy, by M. Brown-Séquard. The losses of function and activity of the brain, in certain cases, are pure effects of inhibition, arising from irritation more or less distant.—Influence of subterranean humidity and of capillarity of the soil on the vegetation of vines, by M. Barral. The fruitfulness of the vine on the sandy soil of Aigues-Mortes is due to abundant water in the subsoil (from 1 m. depth) rising to the roots by capillarity. The author describes several laboratory experiments.—On treatment of the vine with sulphur in Greece, by M. Gennadius. This treatment (for oïdium) is thought successful only if carried out on a day without wind, rain, or clouds, and with a burning sun. This fine weather must last twenty-four hours. It is the sulphurous vapour, and not the sulphur powder, that kills the spores in the air and on the vine, though the powder may act mechanically (and other fine powders will do the same) by protecting tender parts from contact with spores.—On germinated wheat, by M. Ballard. The gluten is profoundly altered; there is more acidity and more sugar and lignin; less fatty matter.—On the relations that exist between covariants and invariants of binary forms, by M. Perrin.—On the theory and experiments of MM. Mercadier and Vaschy tending to establish the non-influence of the di-electric on electro-dynamic actions, by M. Lévy.—General method for strengthening telephonic currents, by Mr. Moser. He introduces more induced coils.—On chlorides of lead and of ammonia, and oxychlorides of lead, by M. André.—Preparation of ethers of trichloracetic acid, by M. Clermont.—Contribution to the study of isomerism in the pyridic series, by M. Œchsner de Coninck.—On the relative toxical power of metallic salts, by Mr. Blake. His tabulated data of experiments show why he cannot accept the law formulated by M. Rabuteau (that metals are more active the greater their atomic weight and the smaller their specific heat).—Penetration of actinic radiations into the eye of man and of vertebrate animals, by M. de Chardonnet. He finds that no medium of the eye is transparent for the ultra-solar radiations, that is, for waves shorter than T or U, the limits of the ultra-violet solar spectrum. The mitilating membrane in sparrow-hawks and fowls is translucid for part of the ultra-violet spectrum (up to O and Q). The absorbing power of the vitreous humour, cornea, and crystalline lens varies in different species. The general fluorescence corresponds to actinic absorption, but there are exceptions.—New researches on the production of monsters in the hen's egg by the effect of late incubation, by M. Dareste. This takes place more slowly in winter than in summer. Also eggs of the same age grow old more or less quickly.—On the tonic and inhibitory *rôle* of the sympathetic ganglions, and their relation to vaso-motor nerves, by MM. Dastre and Morat.—The mode of fixation of the suckers of the leech studied by the graphic method, by M. Carlet. The movements of the animal on smoked paper were observed. It has been received that the oval sucker is attached first by the centre, then by the borders, but the author finds that the borders are fixed first. Detachment, too (which does not seem to have attracted attention), begins at the borders.—On a new fixed Crinoid, *Democrinus parfaiti*, obtained in dredging from the *Travailleur*, by M. Perrier. This makes only the fifteenth species known. It is distinguished by a long funnel-like cup, formed of five basal pieces.—Geological and chemical researches on the saliferous formations of the Swiss Alps, and especially on that of Bex, by M. Dieulafait. These beds the author regards as products of evaporation of ancient seas.

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THURSDAY, MARCH 1, 1883

RECENT ARMOUR-PLATE EXPERIMENTS

AT the conclusion of their labours the "Iron Plate Committee" reported, in 1865, that the best material for the armour of war-ships was wrought iron of the softest and toughest nature. Steel, or steely iron, or combinations of iron and steel were all pronounced unsuitable for the purpose, after a long course of careful experiments. Accepting this verdict the designers of armoured ships continued to specify for soft iron armour, the makers of guns and projectiles aimed at the perforation of this kind of armour, and the manufacturers sought to secure the desired qualities of softness and toughness in the thicker and heavier plates which they were constantly being called upon to produce. All the armoured ships built from 1860 to 1876 were "ironclads," and in that time the thicknesses of armour plates carried on the sides or batteries of completed ships had advanced from 4½ inches to 14 inches, while the weights had risen from 4 or 5 tons to 20 or 25 tons. Greater aggregate thicknesses of iron had been arranged for prior to 1876. For example, the *Inflexible* had been designed to carry 24 inches of iron on her sides, but this was in two layers of 12-inch plates. The adoption of the so-called "sandwich-fashion" of armour plating was based upon experiments made at Shoeburyness, and it had certain advantages of a constructive character; it also enabled broader and longer plates to be produced within the fixed limits of weights with which the manufacturers could deal, and enabled them to insure excellence of quality which might not have been so certain of attainment in plates of 20 inches or upwards in thickness.

While the two great Sheffield firms and their rivals in France were thus developing the manufacture of iron armour plating, the Creusot Company, of which M. Schneider was the head, were attempting to reverse the verdict against steel armour, and to produce specimens which could hold their own against the best iron armour of equal thickness. The Italian Admiralty brought the claims of the rival materials to the test of experiment at Spezia in October, 1876. In order to decide on the kind of armour to be used on the *Duilio* and *Dandolo*, specimen targets were erected and a series of firing trials made against them on a scale of unprecedented magnitude. A gun weighing 100 tons, manufactured at Elswick, was brought to bear upon targets protected by iron or steel plates 22 inches thick, backed by great masses of timber and strong supports. Other guns of considerable weight and power were also used, but their performances were overshadowed by those of the monster weapon. The results of these trials may be briefly summarised. Against the 10-inch and 11-inch guns the 22-inch iron plates had a decided advantage over the steel plate of equal thickness. The penetration was somewhat greater in the iron plates, but the steel plate cracked badly. On the other hand, when the 100-ton gun was brought against the targets the iron plates and their backings were completely perforated as well as broken up: whereas the steel plate, although smashed to pieces, prevented the shot from passing through the backing. Various opinions were formed as to the deductions which should be made from these trials. On the one side it was urged that as steel plates of great thickness could be gradually cracked and destroyed by guns incapable of perforating them, steel ought not to be used instead of iron, which could be battered by a great number of projectiles from such guns, and be neither perforated nor cracked. On the other side it was maintained that there was small probability of any single armour plate on a ship's side being struck repeatedly in action; and consequently that the material should be preferred which could best resist perforation by a single projectile from the most powerful gun, even if the resistance to perforation involved the partial destruction of the plate struck. The Italian authorities adopted the latter view, and the *Duilio* and *Dandolo* have steel armour, being the first ships protected in that manner.

Although these steel armour plates were made in France, the French authorities did not follow the Italian lead and abandon iron armour. Nor was a similar course followed in England. Change was seen to be inevitable, but it was endeavoured to make the change in a direction which should combine the high resistance to perforation of steel with the power to resist cracking and disintegration possessed by tough rolled iron. To Messrs. Cammell and Co. of Sheffield belongs the honour of taking the lead in this direction; Messrs. Brown speedily followed, and the Admiralty gave substantial assistance in the conduct of the necessary experiments. In the earlier stages many failures and disappointments were experienced; but eventually better results were obtained, and "steel-faced armour" became recognised as the substitute for iron on English war-ships. Steel-faced armour, as the name implies, consists of a rolled iron back-plate, on the face of which is welded a layer of steel. The hard steel face resists perforation, and breaks up or deforms the projectiles, while the intimate union of the tough iron back with the hard steel face prevents the serious cracking which occurs in steel alone. Curiously enough the idea was not merely an old one, but a small plate made on this principle, 4½ inches thick, had been fired at in 1863. This early steel-faced plate was broken into two pieces at the first shot of a light gun, and was condemned by the Iron Plate Committee. Fourteen years later plates of a similar character, so far as the combination of steel and iron is concerned, but of improved manufacture, were successfully resisting three shots, either of which would have perforated an iron plate of equal thickness.

The first steel-faced plates were used on the *Inflexible's* turrets: they were 9 inches thick, worked "sandwich-fashion" outside 7-inch iron armour. It was part of the contract that a test-piece from each steel-faced plate should be fired at with a 12-ton gun, and should receive three shots without being broken up or perforated. This was considered to be a very severe test at the time, and undoubtedly was so when the novel conditions of the manufacture are considered. It was successfully met, however, and from that time onwards the manufacture has steadily improved. As an indication of what has been done, it may be stated that steel-faced plates 11 inches thick have received no less than eight shots from the 12-ton and 18-ton muzzle-loading guns, with battering charges and at 10 yards' range, without perforation or

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very serious cracking, this enormous "punishment" having been sustained by an area of 48 square feet only. Most of the trials made against steel-faced armour have been against plates from 10 to 12 inches in thickness. For thicknesses up to 12 inches it is probably within the truth to say that for *normal impact* the steel-faced plates of recent manufacture have been equal in their resistance to perforation to iron plates 25 to 30 per cent. thicker and heavier. For oblique impact the hard armour is probably still more superior to iron, glancing the projectiles at angles of obliquity when they would have "bitten" into the iron. A few experiments have been made in this country and abroad on much thicker steel-faced plates, ranging up to 18 or 19 inches in thickness, and of these the most recent and important are the trials made at Spezia in November, 1882. Three targets were constructed for these trials, the armour plate on each being nearly 11 feet long, 8½ feet wide, and 19 inches thick. One of the targets was covered by a steel-faced plate made by Messrs. Cammell, another by a steel-faced plate made by Messrs. Brown, and the third by a steel plate made at Creusot. All three plates were similarly backed and supported by 4 feet of oak; the Creusot plate was fastened by no less than 20 bolts, and the Sheffield plates had only 6 bolts each. Against these targets the 100-ton muzzle-loading gun was brought into action. At first the powder-charge used (329 lbs.) was that which gave such a velocity to the chilled cast-iron projectiles—2000 lbs. in weight—as would have perforated a 19-inch iron armour plate. The actual penetrations were from 3½ to 5 inches in the steel-faced plates, and 8¼ inches in the steel plate, showing that the actual superiority of all the plates over iron considerably exceeded the estimate. The steel plate did not crack at the first shot: the steel-faced plates did, but not to any serious extent. Next followed a more severe attack, the powder charge being increased to 480 lbs., giving the projectiles a velocity estimated to be capable of perforating about 24 inches of iron armour. The total energy of the projectile moving at this velocity exceeded 33,000 foot-tons. All the plates were broken into pieces by this terrific blow. The steel plate was split into six pieces, but the numerous bolts held these pieces in position, and still preserved the defensive power of the target. Each of the steel-faced plates was broken into five pieces, and on account of the fewness of the bolts these pieces fell to the ground, leaving the targets uncovered. The whole of the chilled cast-iron shots were broken up on impact, and the penetration into the steel-faced plates was less than that in the steel plate. At this stage the comparative tests ended. A third round was fired, with the heavier charge and a steel projectile against the steel plate. The shot was stopped, the penetration was only 7 inches, but the plate was broken up, and the backing seriously splintered. A fourth round was fired at this target and completely wrecked it.

On a review of all the circumstances of the experiments, it must be admitted that the greatest success was attained by the steel plate, although this must be attributed rather to the number and excellence of its fastenings than to superiority in quality of the plate over the steel-faced plates. The latter proved themselves less penetrable than the steel plate, and had rather the advantage as regards fracture at the end of the first two series of rounds; but they were insufficiently secured. One definite lesson to be learned from these experiments is, therefore, that a larger number of bolts is needed for a given area of steel or steel-faced armour than has been commonly used. Another lesson taught by these trials is that the steel armour plates of Creusot manufacture in 1882 are far superior to those made six years earlier. It is not at all probable that light guns such as broke the 22-inch steel plate to pieces in 1876 would have been equally effective against the 19-inch plate recently tested. In both cases the plates were made specially for the firing tests, and they may not have been "merchantable articles" in the sense of representing large quantities of steel armour. But nevertheless this 19-inch plate shows what can be done with steel, if cost is of secondary importance. Authoritative statements are wanting of the actual processes of manufacture, or of the cost of production. It is reported that the 19-inch plate was hammered down from an ingot three or four times as thick as the finished plate, and that the face was oil tempered. If this is correct the cost must be high, and probably as great as, if not greater than, that of steel-faced plates. Moreover if such an amount of "work" has to be put into steel plates in their conversion from ingots into the finished forms, then no great economy or advantage can result from the power which the maker has to cast steel ingots in special shapes or sectional forms. The Creusot Company use a soft steel containing perhaps three-tenths to four-tenths per cent. of carbon, give it toughness by means of a large amount of hammering, and harden the face by oil tempering. On the contrary, the Sheffield firms, as the result of numerous experiments, use a hard steel for the face, the percentage of carbon amounting to about twice that in the Creusot plate, and support this by a tough iron back. With this hard steel, oil tempering does not appear to be beneficial, although with softer steel it undoubtedly is an advantage. These steel-faced plates which were tested at Spezia were really samples of large quantities made at Sheffield in the same manner. Probably equally good results would have been obtained if any one of the batch of plates represented had been selected for test. In this respect, therefore, there is a marked difference between the test to which the two manufactures were subjected.

As between the steel and steel-faced plates tried at Spezia we may assume that there is no notable difference in resistance to perforation or to fracture. Possibly, with equally good and equally numerous fastenings the steel-faced plates would have had some slight advantage, and in other trials mentioned later on steel-faced plates have had a decided advantage. Supposing no important difference to exist, then the choice between the two kinds of armour will be governed by their relative prices, and how these compare, we have no means of judging, but it seems probable that the steel-faced plates would be at least as cheap as steel plates made in the manner described above for the steel test-plate.

It may be convenient in this connection to briefly describe the mode of manufacture of steel-faced plates. Messrs. Cammell prefer to pour the molten steel on to the face of a wrought-iron plate which has been brought to a good welding heat. The layer of molten steel is surrounded by a frame of wrought-iron which has

previously been attached to the iron plate; and it is pressed against the surface of the iron plate by a cover carried by an hydraulic ram, until the welding is complete and the steel has solidified. Messrs. Brown prefer first to roll a steel face-plate as well as an iron back-plate, and then to raise both to a welding heat; the molten steel is afterwards poured into a space left between the two, and hydraulic pressure is applied until the solidification has taken place. The remaining processes are similar in the practice of both firms. After welding has been completed, the whole mass is reheated and rolled down to the finished thickness of the armour plate. The steel face is usually about one-half the thickness of the iron back, and it is a curious fact that the iron and steel maintain their relative thicknesses as the rolling proceeds, even when the reduction in thickness during rolling is very considerable. This reduction varies from one-half for thin armour-plates, up to 10 or 11 inches in finished thickness, to one-third with 18 to 20 inches of finished thickness. Some competent authorities consider that too little work is done in the rolls on the thicker plates, but there is a need for further experiment to show whether this view is correct. Whatever may be the cause, it would seem that the best results so far have been obtained with steel-faced plates below 12 inches in thickness.

Simultaneously with the Spezia experiments another competition was proceeding, near St. Petersburg, between steel-faced and steel armour. The plates tested were 12 inches thick, 8 feet long, and 7 feet wide. They were first fired at with the 11-inch breech-loading gun, throwing a 550-lb. chilled cast-iron projectile, with a powder charge of 132 lbs. The velocity of the shot was 1500 feet per second. Messrs. Schneider supplied the steel plate, which was fastened with twelve bolts. Messrs. Cammell made the steel-faced plate, which had only four bolts in it. The first blow on the steel plate broke it into five pieces; the projectile was destroyed, but it penetrated 13 inches into the target. A blow of equal energy on the steel-faced plate produced only a few unimportant cracks in the steel, and the penetration was about 5 inches only. Three out of the four bolts were, however, broken. A second shot was then fired at each plate with 81 lbs. charge. The steel plate was broken into nine pieces, and the penetration was 16 inches: whereas on the steel-faced plate the principal effect produced was to break the only remaining bolt and to let the plate fall to the ground, face downwards. The back of this plate was perfect, and the target behind the plate was uninjured. In this trial the steel-faced plate proved greatly superior to the steel, but had insufficient fastenings. It is proposed to increase the bolts in number, re-erect the plate, and continue the trial, of which the further results cannot fail to be interesting.

This contest between steel and steel-faced armour must not be allowed to withdraw attention from the great superiority of both, in certain respects, to iron armour. Even as matters stand, either of these modern defences is greatly to be preferred to their predecessor. Against this hard armour chilled cast-iron projectiles break up in a manner never seen with soft iron. Projectiles of this kind are virtually impotent, and must be replaced by more expensive, harder projectiles, if steel or steel-faced armour is to be attacked. Even with steel projectiles results cannot be obtained such as were possible with iron armour. Perforation of armour by shells carrying relatively large bursting charges is no longer a possibility: and the heaviest gun yet made cannot drive its projectiles through a thickness of hard armour only three-fourths as great as the thickness of iron which it could perforate.

The use of steel and steel-faced armour will involve many experiments to determine not merely what descriptions of projectiles are best adapted to damage or penetrate it, but what are the laws of the resistance of such armour to penetration and disintegration. All the formulæ based on experiments with soft iron armour and chilled cast-iron projectiles are inapplicable under the new conditions. Perforation is no longer to be feared as the most serious damage likely to happen to armour plates: more moderate thicknesses of hard armour suffice to stop the projectiles from the heaviest guns than would have been considered possible a short time ago. Instead of perforating 19 inches of steel or steel-faced armour, the projectile of the 100-ton gun with a given velocity only penetrates 8 inches into the plates. But, on the other hand, the possible disintegration and fracture of the armour plates are becoming important matters. Makers of armour plates have to endeavour to produce materials which shall resist fracture as well as penetration, and the only proof of their success or failure is to be found in the results of actual trials. Experiments are equally essential to progress in the manufacture of guns and projectiles. The example set by Italy must be followed; the necessary experiments must be on a large and costly scale, and they may lead to many departures from former practice. But if real progress is to be made in the armour and armament of ships, it must be prefaced by experiments beside which those of the former Iron Plate Committee will appear insignificant.

In conclusion it may be stated that although iron armour has been practically superseded for the sides and batteries of war ships, it is still preferred for decks. Experiments have shown that for angles of incidence below 20 degrees, and for such thicknesses—not exceeding 3 or 4 inches—as are used on decks, good wrought-iron is superior to both steel and steel-faced plating. The explanation of this departure from the laws which hold good for thicker plates and greater angles of incidence cannot be given here, but the fact has been established by elaborate trials made in this country and abroad.

SMOKE ABATEMENT

Report of the Committee of the Smoke Abatement Exhibition. (London: Smith & Elder, 1883.)

THIS volume, which has just been issued, presents many points of interest, as it is the outcome of the labours of a Committee formed in 1881 with a view to ascertain what means could be adopted to check the growing evils arising from the evolution of smoke which attends the combustion of bituminous coal. It may be said to be the continuation of work undertaken by the several Parliamentary Committees which met in 1819, 1843, and in 1845. In the previous efforts attention appears to have been mainly directed to lessening the

nuisance arising from smoke from factory and other furnaces, but in the present movement it is evident that the importance of the domestic fireplace as a foe, if not the chief one, to the purity of the air of cities, has been generally recognised and has been the main object of attack.

It is not a little remarkable that, although elaborate experiments have been made from time to time with a view to ascertain the nature and composition of the gases generated in furnaces, but little attention has been devoted to the gases given off from stoves and grates. On the Committee of the recent Smoke Abatement Exhibition chemists were well represented, and this brief notice will mainly refer to the general chemical results that have been obtained.

The examination of the gases withdrawn from flues to which stoves and grates were attached, was intrusted to Prof. Chandler Roberts, who at first considered that the analysis of representative samples might best be made by the aid of the rapid methods of gas analysis arranged by Orsat. In view, however, of the peculiar conditions under which the tests had to be made, and bearing in mind that more than one hundred appliances were submitted for testing in the limited time during which the Exhibition was open, Prof. Roberts submitted a plan to the Committee which received its approval.

He points out in his report that the first researches on chimney gases are due to Péclet, who published some results of analysis in 1828, but Péclet's results and those of different experimenters who followed him were open to the objection that the samples submitted to analysis were only small fractions of the total gases in the flues, and as the samples were not taken with sufficient frequency they could not represent the mean composition of the gaseous mixture passing up the chimney. This grave defect was, however, remedied by Scheurer-Kestner in an elaborate research on the composition of the flue-gases of boiler furnaces, which will always be the basis of future experiments in this direction, and to which frequent allusion is made in the Report. The details of the method adopted are given in the Report itself; it will be sufficient to say here that the gases were withdrawn through a fine slit in a tube extending across the flue, an arrangement which rendered it possible to draw the gases uniformly from the entire diameter of the ascending current of gas in the flues. The effluent gases were withdrawn by aspiration through a tube loosely filled with asbestos to retain the solid particles of carbon and soot; they then passed through a U-tube filled with chloride of calcium to absorb water, and thence through three U-tubes filled with soda-lime to absorb carbonic anhydride; the gases were then led to a tube of porcelain filled with cupric oxide and heated to redness by means of a small furnace. The complete combustion of the remaining gases was thus effected, the carbonic oxide being burnt to carbonic anhydride, and the hydrocarbons and free hydrogen to aqueous vapour and carbonic anhydride; the water was retained in a U-tube filled with chloride of calcium, and the carbonic anhydride in two other soda-lime tubes; the residual gases (unconsumed oxygen and nitrogen) then passed to the aspirator, a chloride of calcium tube being interposed to prevent any moisture from the aspirator from penetrating the system of tubes.

It will be evident that this plan renders it possible to compare the relative proportion of the completely burnt products of combustion with those in which combustion has been imperfect. With regard to the proportion of carbon lost as soot, the evidence afforded by the results of the tests made at the Exhibition, although they do not unfortunately render it possible to give a clear and precise answer to the question, are sufficiently definite to show that the amount probably does not exceed 1 per cent. of the total carbon in the fuel, and is in many cases far less.

The coal used in testing the grates and stoves was either 'Wallsend,' which yielded 67·1 per cent. of coke, or Anthracite, giving 94 per cent. on distillation in a closed vessel.

With regard to the completeness of the combustion, the carbon present in the form of carbonic anhydride varied in relation to that present as carbonic oxide and as hydrocarbons, C_xH_y, within the limits of 1,000 to 4 and 1,000 to 375, but of the whole eighty-six tests in only three was the number indicating imperfect combustion below 10, and in only nine cases was it above 200, and six of these nine cases (three grates and three stoves) were worked purposely for "slow combustion."

The total amount of carbon present in the gases ascending the flue (either in the free state or combined with carbon) bore a relation to the hydrogen present which varied between the limits of 1,000 to 8 and 1,000 to 259, the latter probably being due to the fact that the grates and stoves were tested whilst the mortar in which they were set was still wet.

The mean of the results of the tests of the seventeen best grates shows that the loss of carbon in the form of carbonic anhydride and hydrocarbons is about 3·4 per cent. of the carbon in the fuel used (in the case both of Anthracite and Wallsend), the mean for the whole of the grates being about 9 per cent. of the total carbon.

The comparative imperfection of the combustion shown in some of the tests is hardly to be wondered at when it is remembered that the bituminous coal employed yielded on distillation no less than 32 per cent. of volatile matter, and that in the case of many of the appliances the cold fuel was simply charged on to the top of a mass of coal already in the state of incandescence.

Professor Roberts cautiously points out that all that has hitherto been done in this series of tests "merely renders it possible to select certain typical appliances which deserve more detailed examination." He appears, however, to have spared no pains to render this very laborious investigation as complete as the circumstances allowed, and the Chemical section of this Report is certainly one of the most important contributions ever made to our knowledge of the combustion of fuel.

E. FRANKLAND

NORTH AFRICAN ETHNOLOGY

Sahara und Sudan: Ergebnisse Sechsjähriger Reisen in Afrika. Von Dr. Gustav Nachtigal. Part II. (Berlin: 1881.)

NEARLY a decade has elapsed since Dr. Nachtigal's return to Europe after his travels in East Sahara and Central Sudan during the years 1869-74. Most of the geographical and ethnological results of his researches

in that region have already appeared at various times in the memoirs of the *Gesellschaft für Erdkunde* and of other learned societies. But the issue of the monumental work embodying all the details in a permanent form is proceeding at a very slow rate. The first part, covering the years 1869-70, did not appear till 1879, and an interval of two years elapsed before the publication of this second instalment, which, although forming a bulky volume of 790 pages, gets no further than the first days of September, 1872. In the preface the delay is attributed mainly to the time occupied in the tedious process of sifting the ethnological and especially the linguistic materials brought home by the traveller. The help afforded by Rudolf Prietze in arranging these materials is handsomely acknowledged in the preface, where occasion is also taken to express regret for omitting to give the source of the familiar mailclad, mounted Bornu warrior borrowed in Part I. from Denham and Clapperton's work, attention to which oversight had been called in our review of that volume (see NATURE, vol. xxi. p. 198).

The three books forming the present volume embrace the trips made to Kanem and Borku north of, to Baghirmi south of, Lake Chad, and to the islands in the lake itself. Separate chapters of great permanent value are devoted to the main geographical features of these regions, and to the history and complex ethnical relations of their inhabitants. Here special attention is naturally claimed by the mysterious Tubu people of the East Sahara, and a serious attempt is made to explain their relations on the one hand to the Hamitic Tuariks (Berbers) of the West Sahara, on the other to the Negro races of Sudan.

The Tubu, that is, "people of Tu" or Tibesti,[1] are by Lepsius[2] with great probability identified with the Garamantes of Herodotus (iv. 183), whose capital was Garama (Edrisi's Germa) in Phazania (Fezzan). Ptolemy, who places the Garamantes in the same region, that is, in the Libyan Desert (Sahara) south of the Syrtis Major (Gulf of Sidra), already speaks doubtfully of their ethnical affinities, and seems disposed to affiliate them rather to the Ethiopian (Negro) stock.[3] Later on this position is disturbed by Leo Africanus, whose fifth great division of the Berbers are the Gumeri (Garamantes?), whom he elsewhere calls Bardæi (Bardoa). These Bardæi, whose name appears to survive in the *Bardai* oasis of Tibesti, are accordingly identified by Vater with the Tubu, and by him grouped with the Berbers.[4] Now comes Lepsius, who again removes the Tubu from the Libyan (Berber) connection, and with Ptolemy transfers them to the Negro group. He admits a strong modification of the original dark, and a corresponding assimilation to the Libyan, type. But this is attributed to their position along the great historical trade route across the Sahara between North Africa and the Chad basin, while their language is regarded as decisive proof of their Negro relationship.[5]

And thus this interesting, if somewhat troublesome, nomad race has continued throughout the historic period to occupy a dubious ethnological position between the surrounding Hamitic and Negro peoples. That Dr. Nachtigal should attempt to grapple with the problem was inevitable, and although his own inferences are vague and hesitating, he at all events supplies ample material for a satisfactory solution. As in so many other anthropological fields, the difficulty turns, so to say, mainly on the collision between ethnical and philological interests The present physical resemblance of the Tubu to their western neighbours, the Berber Tuariks, admits of no doubt, and this resemblance increases as we proceed from the Dasa, or southern, to the Teda,[1] or northern, division of the race. In fact there is here the same gradual transition between the Hamites of the Sahara and the Negroes of Sudan, which is found further west all along the borderlands from Bornu to the Atlantic, and which is conspicuous especially in the more or less mixed Sonrhay, Pul, Hausa, and Toucouleur nations of the Chad, Niger, and Senegal basins. To these correspond in Central Sudan the Negroid Kanuri, Kanembu, Baele, and Zoghawa peoples,[2] while the same complexity is presented in the Nilotic regions, where the Nuba family merges imperceptibly north and south in the Egyptians and Negroes of the White Nile.

But Lepsius (*op. cit.*) now holds that the two elements have become interpenetrated throughout the whole of the Sudan, which he consequently regards as an intermediate zone of transition between the intruding Hamites from Asia and the autochthonous Negroes, whose original domain is relegated to the southern half of the continent. In this scheme, which thus recognises in Africa only two fundamental racial and linguistic types, what place can be assigned to the Tubu? We have seen that Lepsius himself disposes of the question by regarding them as originally Negroes assimilated physically to the Berbers, while retaining their primitive Negro speech. If this view could be accepted, we should have an instance of the linguistic surviving the ethnical type, a theory in which anthropologists would in any case be slow to acquiesce. But Dr. Nachtigal's researches, while confirming Barth and Koelle's conclusions regarding the intimate relation of Tubu to Kanem and Kanuri, also show that this group is fundamentally distinct from the Bantu, that is, from the typical Negro linguistic stock of Lepsius. If it could be affiliated to the Hamitic family, there would be no further difficulty as to the ethnical position of Tubu. But Nachtigal also shows that it differs quite as radically from Hamitic as it does from Bantu. His inquiries have in fact resulted in the discovery of an independent and widespread linguistic family corresponding in the East Sahara and Central Sudan to the northern Hamitic and southern Bantu groups. The source, or at least the most archaic known form, of this family is the Teda, or northern Tubu, whose direct offshoots are the more highly developed Dasa, or southern Tubu, the Kanem north of Lake Chad, the Kanuri of Bornu, the Baele of Ennedi and Wanyanga, and the Zoghawa of North Dar-Fur. More distant members appear to be the Hausa, Fulu, and Sonrhay of

[1] Cf. *Kanem-bu* = people of Kanem, where *bu* is the pl. of the personal postfix *ma*, answering to the personal prefix *m*, pl. *ba*, *wa*, of the Bantu races, as in M'Ganda, Waganda; and to the *be* of *Ful-be* = "Pul people."
[2] *Nubische Grammatik. Einleitung.*
[3] "Οσων δὲ καὶ αὐτῶν ὅδε μάλιστ' ἀθρόοι", i. 8.
[4] "Mithridates II.." p. 45 of Berlin ed. 1812
[5] "Ursprünglich ein Negervolk," *op. cit., Einleitung*, xlviii.

[1] In this word *Teda* we have apparently the root of the *Tedamensii*, a branch of the Garamantes placed by Ptolemy south of the Samamycii in Tripolitana. If my identification is correct, it gives us a fresh proof of the identity of the Garamantes with the Tubu.
[2] See my paper "On the Races and Tribes of the Chad Basin," NATURE, vol xv p. 559.

West Sudan, the Logon, Bagrimma (Baghirmi), and Mandara (Wandela) of the Shary basin, and the Maba of Wadai. But the actual relationship of these and other outlying branches to the main trunk can only be determined by future research. Meantime, Dr. Nachtigal rests satisfied with having demonstrated the existence of this widely-ramifying family and its radical difference both from the Hamitic and Bantu groups. "How far the relationship may extend will be made more and more evident by a further study of the Sudan languages, especially the Hausa, Masa, Bagrimma, Maba. For the present it is enough for me to have established the relations of the Tubu dialects to each other, and of both to the Kanuri and Baele" (p. 209).

But when he comes to the consequences of his premisses he speaks with singular hesitation, as if overweighted or hampered by the brilliant generalisations of Lepsius. The fact is, this theory of the three zones leaves no room in Africa for the great linguistic family which Nachtigal has nevertheless discovered there. But instead of boldly giving up the theory, he timidly suggests alternatives, in order somehow to reconcile it with the actual conditions. After clearly showing the independent position of Tubu, he leaves the reader to choose between a possible "extremely remote connection with the Negro languages, or, if it be preferred, to regard it as a distinct species, which has held its ground between the Negro and Hamitic linguistic types" (p. 201). Most ethnologists will probably be prepared to accept the latter alternative, even at the risk of adding one more to the two "linguistic types" which alone Lepsius will tolerate. The only point of contact between Tubu and Bantu seems to be the absence of grammatical gender, a negative feature which both share with a thousand other languages in the Old and New Worlds. Yet apparently in order to save Lepsius's scheme, Nachtigal is content on this weak ground to allow a connection between Teda and Negro, adding, still more inconsequently, "in which case, considering the vagueness of the concept 'Negro' (bei der Unbestimmtheit des Begriffes 'Negro'), there can certainly be no objection to group the Tubu themselves with the Negroes, although, taking the word in its ordinary sense, in other respects they essentially differ from them" (p. 209). So the Negro—that is, the most marked of all human varieties—is frittered away to a "vague concept," because Tubu is a no-gender language, or because Lepsius will allow only two linguistic types in Africa.

But by getting rid of this theory, an easier exploit than getting rid of the Negro, everything will fall into its place. The consideration that the centre of evolution of the Tubu group lies, not in Sudan, but in the Sahara, far north of the original Negro domain, placed by Lepsius south of the equator, would almost alone suffice to separate it from that connection. Dr. Nachtigal himself shows that Teda, or northern Tubu, represents the germ, of which the southern Dasa, Kanuri, Baele, &c., are later developments. He also shows that the migrations, as was natural to expect, were always from the arid plains and uplands of the Sahara to the fertile region of Sudan. Except under the lash of the slave-driver, the Blacks seem never to have moved northwards. But we have seen that the roving nomads, Tuariks in the west, Tubu in the east, have everywhere, all along the line, penetrated from their desert homes into the "Black Zone." The inference seems obvious. Nachtigal himself regards the Tubu as "a thoroughly pure, homogeneous people (*eine durchaus reine, homogene Bevölkerung*), unmodified by any changes from remote times" (p. 190). He also shows their close physical resemblance to the Tuariks (Berbers) of the western Sahara, and their essential difference from the Negro type. The anthropologist will not hesitate to remove them from the latter and group them with the former race. The Tubu and Berbers are thus ethnically two slightly differentiated branches of the Hamitic section of the great Mediterranean (Caucasic) division of mankind.

From this standpoint the Tubu speech, although as radically distinct from the Hamitic as it is from the Bantu, will no longer present any difficulty. In Europe the Mediterannean races have developed at least one radical form of speech, the Basque; in Asia several, the Aryan, Semitic, Georgian, and others in Caucasia. Why should they not have developed two in Africa, the Hamitic and Tubu? Elsewhere I have endeavoured to account for this remarkable phenomenon of specific diversity of speech within the same ethnical group.[1] Here it will suffice to note the fact, and if the no-gender character of Tubu be urged as a difficulty, the reply is twofold. First, no-gender languages occur also in other Caucasic groups, as in Basque, Georgian, Lesghian; secondly, although gender has not been developed in Tubu, nevertheless it contains the raw material, so to say, which has been elaborated into a system by the more cultured Hamitic peoples. After admitting that, but for the absence of this feature, there would be no scruple (*Bedenken*) in affiliating Tubu to the Hamitic order, Dr. Nachtigal adds: "Tubu also certainly seems to possess the elements by which gender is indicated in the Hamitic—*o* and *ṕ* for the masculine, *t* for the feminine, as in *o-mi't*, man; *mi*, son, by the side of *ddi*, woman; *dô*, daughter; *dê*, mother; *edi*, female" (p. 200). Here *d* of course answers to *t*, the universal mark of the feminine gender in Hamitic, and in the Berber group often both prefixed and postfixed, as in *akli*, negro; *taklit*, negress.

Room must therefore be made in Lepsius's scheme for a third linguistic family, the honour of having determined which belongs to Dr. Nachtigal. This Tubu family must be assumed to have been independently evolved in remote ages by the Garamantes, ancestors of the Tubu nomads, during long isolation in Kafara, Kawár, Tibesti, and the other oases of the eastern Sahara and Fezzan. Lastly, the Tubu themselves must be absolutely separated from the Negro ethnical connection, and grouped with the Hamites in the Mediterranean division of mankind.

A. H. KEANE

OUR BOOK SHELF

The Electric Lighting Act, 1882, *the Acts incorporated therewith, the Board of Trade Rules, together with numerous Notes and Cases.* By Clement Higgins, M.A., Recorder of Birkenhead, and E. W. W. Edwards, B.A., Barrister-at Law. (London: W. Clowes and Sons, 1883.)

PRACTICAL electricians unversed in law, and lawyers unversed in the practical applications of electricity, will

[1] See Appendix to my "Asia" (Stanford Series), p. 695.

find much useful matter in this volume. The authors are thoroughly competent to deal with the legal aspect of the case, whilst their judicious comments show that they appreciate at least many of the technical difficulties necessarily presented by the subject. The contents deal with the various sections of the Electric Lighting Act, adding copious notes and comments, and references to legal precedents and decisions. Quotations are given from the evidence collected by the Select Committee on Electric Lighting, and from the Rules and Regulations recommended by the Society of Telegraph Engineers and Electricians concerning the prevention of fire-risks. One or two minor slips in the science are to be regretted, as for example where the authors state that a current of unit strength will decompose ·09378 grammes of water per second. It is a pity, moreover, that they have departed from customary usage in speaking of the "strength" of a current as its "intensity." That term has been and is still so much abused, that so long as it is liable to mislead its use should be avoided. One of the authors describes himself as "Fellow of the Physical Society of London." We were not aware that the Physical Society of London recognised any such grade amongst its members.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Ben Nevis Observatory

IN NATURE, vol. xxvii. p. 399, there is among its notes of scientific intelligence, a paragraph mentioning that at a public meeting in Glasgow last week, called at the suggestion of Sir William Thomson and Mr. John Burns of Castle Wemyss, it was agreed to collect money for a permanent observatory on Ben Nevis.

AS NATURE has always kindly encouraged this project of the Scotch Meteorological Society, perhaps you will permit me, as Chairman of the Society's Council, to add a little to this brief notice.

A requisition was presented to the Lord Provost of Glasgow, which was signed, not only by the eminent physicist and the extensive ship-owner mentioned in your notice, but also by Dr. Grant of the Glasgow Observatory, suggesting that a meeting of the merchants and ship-owners should be called to aid the Society in raising the necessary funds.

The Lord Provost in compliance called a meeting for the the 14th inst., which was well attended, and at which very able speeches were made, not only by the three requisitionists, but by the Lord Provost and by other influential citizens. The result of the meeting was a resolution expressing approval of the Society's proposal, and appointing a Committee to obtain subscriptions in aid of it.

It is expected that the amount of the funds required will be obtained from a community so wealthy and so public-spirited as that of Glasgow. But if we are mistaken in this, the Society's Council intend to appeal to other communities also for help, being resolved to resort to every legitimate means of attaining an object allowed on all hands to be of national importance.

The Council began with Glasgow, not only because it is the richest community in Scotland, but because the Scotch Meteorological Society *originated* there. The late Sir John G. Forbes of Pitsligo, and I, being both of us interested in meteorology, applied to the British Association for the Advancement of Science, when it met in Glasgow in September, 1855, under the presidency of the Duke of Argyll, to see whether it would approve of the formation of a Meteorological Society for Scotland. The result of our application was the following resolution by the General Council:—

"*Resolved, that the British Association express their satisfaction at the proposed establishment of a Scotch Meteorological Society, and their willingness to afford the Society any assistance which can be yielded by the establishment of the Association at Kew.*

"*That a letter to this effect be addressed to the Meteorological Society by the General Secretary.*"

On the basis of this testimonial by so influential a body, Sir John Forbes and I proceeded at once with the organisation of a Society, the Duke of Argyll being our first President, and assisting us greatly by his patronage.

When the Society resolved on attempting the formidable undertaking of establishing an observatory on Ben Nevis, at a cost of at least 5000*l.*, the first movement for funds was made among its own members and friends, the result of which was a promise of 1400*l.* provided the full sum of 5000*l.* was raised. In order to be enabled to fulfil this condition, the Society's Council not unnaturally went first to the town where it originated, and which more than any other town would be supposed to take an interest in the Society and its operations.

There was this further reason: that the Observatory being intended to be on the west coast, its proximity to Glasgow would add to that interest, and the more so as, on account of the vast shipping and commerce of the Firth of Clyde, no district of Scotland could be so deeply concerned in obtaining additional data for storm warnings.

The British Association, by way of encouraging the formation of the Meteorological Society, expressed in the resolution before quoted a willingness to afford to it assistance from its establishment at Kew.

This promise, unfortunately, the Association was unable to fulfil. But this disappointment to our Society has now been so far compensated by a handsome donation of 100*l.* towards the Ben Nevis fund from Dr. Siemens, the present President of the Association.

The Scotch Meteorological Society is one out of many proofs of the usefulness of the British Association in encouraging researches in particular branches of science, and the recent recognition of the Society's work in this Ben Nevis enterprise by so eminent a man as the present President of the Association is very gratifying to the Council. DAVID MILNE HOME

Milne Graden, Coldstream, February 26

Indian Archegosaurus

THE skull and part of the vertebral column of a large labyrinthodont, allied to *Archegosaurus*, was obtained in 1864 from the Bijori-group of the trias-jura of India, and presented to the Asiatic Society of Bengal. It was soon after sent to England for determination. All traces of this unique and important specimen, which should now belong to the Government of India, are now lost, and I write in the hope that some of your readers may be able to afford us a clue to its present position. The specimen can hardly have been mislaid, as it is some two feet in length. RICHARD LYDEKKER

The Lodge, Harpenden, Herts, February 21

The "Vampire Bat"

KINDLY permit me to ask for a further explanation from Mr. Geo. J. Romanes about the vampire bat, in regard to which he says in his criticism of "Zoological Sketches" (Oswald): "Mr. Bates says (I presume it is a clerical error giving Mr. Belt as the authority) the vampire, however, is the most harmless of all bats." Yet he, Mr. Bates, would lead us to believe that a species of the same genus, Phyllostoma, is a blood-sucker, and had even attacked himself (see p. 91 of the fifth edition of his "Naturalist on the Amazon").

Is there a species of Phyllostoma that lives on fruits, the vampire, and another species of the same genus that Mr. Bates calls "the little grey blood-sucking Phyllostoma," that may possibly attack human beings?

The late Chas. Waterton seems to have had no doubt that the vampire attacks persons asleep, and gives an instance.

The common name vampire may not be in South America confined to the species *Phyllostoma spectrum*. Mr. Romanes' remarks would lead one to believe that he considered there was no species of bat that attacked human beings.

THOS. WORKMAN

4, Bedford Street, Belfast, February 15

DR. ROMANES, in criticising a book ("Zoological Sketches"), in NATURE, vol. xxvii. p. 333, says: "The writer speaks of

vampire bats as those which suck the blood of sleeping persons, whereas the truth is, as Belt has remarked, 'the vampire is the most harmless of bats.'"

In Charles Darwin's "Voyage of the *Beagle*," we find an account of a vampire bat (*Desmodus d'orbignyi*) sucking the withers of horses during repose. We also have Charles Waterton's most circumstantial account of the sucking of the blood of human sleepers. Waterton says there are two species, only one of which attacks man. The Rev. J. G. Wood tells us in his notes to "Waterton's Wanderings" that the bat is *Vampirus spectrum*, on what authority he does not say, but quotes C. Kingsley in confirmation of the blood-sucking habit. Again, Prof. Mivart has an article in the *Popular Science Review* for July, 1876, on bats, in which he not only quotes Darwin's account, but speaks of the modification of the teeth and stomach of *Desmodus* as specially suited to this habit. What I wish to ask in all humility, as a mere onlooker, is, How are we to reconcile the above statement with all this authority?

94, Jacob Street, Liverpool, February 12 A. W. AUDEN

I INADVERTENTLY wrote the name of Belt while quoting from the work of Bates. The answer to the question which your correspondents ask is sufficiently simple, and has, in fact, been furnished by one of them, viz., that while the vampire bat itself does not suck blood, the name is popularly extended to other kinds of bats which do. These other kinds—or at any rate some of them—belong indeed to the same sub-family as the vampire (viz., genera *Phyllostoma* and *Desmodus*); but that the large and repulsive-looking vampire is innocent of the habit in question may briefly be made evident by citing again, and a little more fully, the authority of Mr. Bates, who writes: "The vampire was here by far the most abundant of the family of leaf-nosed bats. . . . . No wonder that imaginative people have inferred diabolical instincts on the part of so ugly an animal. The vampire, however, is the most harmless of bats, and its inoffensive character is well known to residents on the banks of the Amazons" ("Naturalist on the Amazon," p. 337). Again, Mr. G. E. Dobson writes: "This species (*Vampirus spectrum*), believed by the older naturalists to be thoroughly sanguivorous in its habits, and named accordingly by Geoffroy, has been shown by the observations of modern travellers to be mainly frugivorous, and is considered by the inhabitants of the countries in which it is found perfectly harmless" ("Catalogue of the Chiroptera, &c." p. 471).

In conclusion, I cannot quite understand why my remarks should have led any one to believe, as one of your correspondents says, that I consider there is no species of bat which attacks human beings. I stated that the author whom I was reviewing was wrong in speaking "of vampire bats as those which suck the blood of sleeping persons," a statement which appears to me plainly enough to imply that there are certain other bats which do suck the blood of sleeping persons.

GEORGE J. ROMANES

Hovering (? Poising) of Birds

LET me entreat the Duke of Argyll not to confuse the issue between us. I made bold to ask his Grace to draw a diagram showing by what balance of forces he thought a bird could be sustained in mid-air, motionless on motionless wings, in a perfectly horizontal wind; and he refers me to a beautiful drawing of a kestrel hovering, *with fluttering wings, in still air*. (See note at foot of page 161 of the "Reign of Law," 5th edition, 1868: "Mr. Wolf's illustration of a kestrel hovering shows accurately the position of the bird when the action is performed in still air.")

This is quite beside the mark. The problem to be solved is not, How does a bird remain at rest in mid-air on fluttering wings? That question is admirably answered in the "Reign of Law" (p. 160). But the problem before us—the same that was discussed in NATURE in 1873-74—is simply this, How does a bird remain at rest in mid-air on perfectly motionless wings?

Does the Duke deny that this ever takes place? Has he forgotten the letters of Prof. Guthrie and Major Herschel (NATURE, vol. viii. pp. 86 and 324) in which the phenomenon was so graphically described? The Duke himself says (NATURE, vol. x. p. 262), "that under certain conditions of strength of air-current a kestrel can maintain the hovering position with no visible muscular motion whatever;" and compares the action to that of a rope-dancer "standing still in some tiptoe attitude." At that time he appears to have recognised the peculiar features of motionless hovering; but now he denies that he has ever "seen a kestrel's wings motionless when hovering," except for a moment or two, and even then he "could detect the quivering of the quills."

I am really at a loss to know whether the Duke maintains his former position; or whether by shifting his ground he admits that it is untenable; or, lastly, whether he has not partly misapprehended the problem under discussion.

In instancing the "hovering of a boy's kite" the Duke curiously parodies the mistake which he made in his last letter, which required for its correction the tilting of gravity through a certain angle. So here, when he says, "the element of weight is here represented by the string, held at the surface of the ground," he forgets the all-important angle between the direction of gravity and the direction of the string at its point of attachment to the kite. HUBERT AIRY

February 26

HAVING all my life given some attention to the flight of birds, I may mention that I have frequently noticed both hawks and gulls stationary in the air, without flapping, for five or six seconds over the Cornish cliffs when the wind has been blowing off the sea, but never under the circumstances mentioned by Dr. Rae. I totally fail to see why Mr. Airy should be, as the Duke of Argyll states (NATURE, vol. xxvii. p. 387), "mistaken in his description of the facts," it having been plain throughout that Mr. Airy employs the term "hovering" as equivalent to "hanging in motionless poise." Mr. Wolf's kestrel in the "Reign of Law," p. 160, is shown as moving its wings through an angle of about 30°.

Although I believe there is nothing in the etymology of the word "hover" which implies movement, yet its similarity to such words as "quiver," "shiver," &c., may have caused the idea of movement to be associated with it; but whether this be a "disease of language" or not, Mr. Airy seems to have most accurately described what is surely not an uncommon fact of observation. W. CLEMENT LEY

The Auroral "Meteoric Phenomena" of November 17, 1882

IF Dr. Groneman has established the fact that the spindle-shaped beam from every point of observation appeared moving in a straight line, that is an important point gained; but I fail to gather from his letter on p. 388 that there is clear evidence of this. He cites S. H. Saxby as one observer in favour of this, but his description appears to me very ambiguous. When he says, "Its trajectory was much flatter than that of the stars," what stars does he mean? If he means the stars at the same declination as that of the beam, viz. about 10° S., then a great circle undoubtedly would be flatter, but still more would a small circle having its centre at the magnetic pole. On the other hand, H. D. Taylor writing from near York describes the path of the beam as from south-east to south-west, thus making it a small circle curved in the wrong direction for an auroral arch.

It must be remembered that it is very difficult to judge whether a trajectory is a straight line when it covers a great extent in azimuth. T. W. BACKHOUSE

Sunderland, February 26

IT is much to be desired that the increasing interest concerning this great phenomenon should supply the only way of obviating the paucity and incompleteness of observations, by having a meeting of observers and advanced nature-students either at London or Bristol. The Utrecht observation says: "When this arch had obtained the length of 90° (which lasted only a few seconds), a separation was made in the middle of its length," &c. I think this accounts for many of the discrepancies.

Mr. Groneman writes: "The Dutch observations confirm the English, only the phenomenon seems to have been of greater apparent size and therefore nearer." I used to think this for the same reason he gives, but I now think it probable that it was further from the earth when it first approached.

From Bordeaux I learn the sky was cloudy, but the aurora was well seen from Rome, Spezia, and Florence, and I have hopes of observations from the north of Italy.

The logical position is that we must lay aside all preconceived

opinions; that we must be prepared to receive fresh ideas from our new views of the action of intense heat on gases and meteorites.

I have only one point to add to my own observation (at two, not ten, minutes past six, as misprinted), that the object, when nearest, presented through its length (but rather below than above) a remarkable "*boiling*" appearance (as seeds in a capsule), while the edges appeared smooth and quiet.

The Rookery, Ramsbury, February 20 ALFRED BATSON

Aurora

A NEWSPAPER paragraph that has come under my notice describes "a strange phenomenon" seen at Brixham on Thursday morning at 1.30—the 15th instant is to be inferred from the date of the paper. It would seem to have been an aurora—yet another example of exceptional auroral activity attendant on the passage of large sun-spots, as there was a spot of importance approaching the sun's central meridian at the time. Any definite information concerning this particular manifestation, or indeed aurora generally near the date in question, appears worthy of a place in your journal. The sun-spot maximum is passing—perhaps past—and such opportunities should not be lost.

February 24 F. B. E.

DIURNAL VARIATION OF THE VELOCITY OF THE WIND ON THE OPEN SEA, AND NEAR AND ON LAND[1]

DURING the three-and-a-half years' cruise of the *Challenger*, ending with May, 1876, observations of the force and direction of the wind were made on 1202 days, at least twelve times each day, of which 650 days were on the open sea, and 552 days near land. The observations of force were made on Beaufort's Scale, (0—12) being the scale of wind-force observed at sea. The five oceans have been examined separately, viz., the North and South Atlantic, the North and South Pacific, and the Southern Ocean, and thereafter the results grouped together. The mean diurnal periodicity in the force of the wind on the open sea and near land respectively is shown on Fig. 1, where the figures on the left are Beaufort's Scale, and those on the right their equivalents in miles per hour. The solid line represents the mean force on the opea sea, and the dotted line the mean force near land.

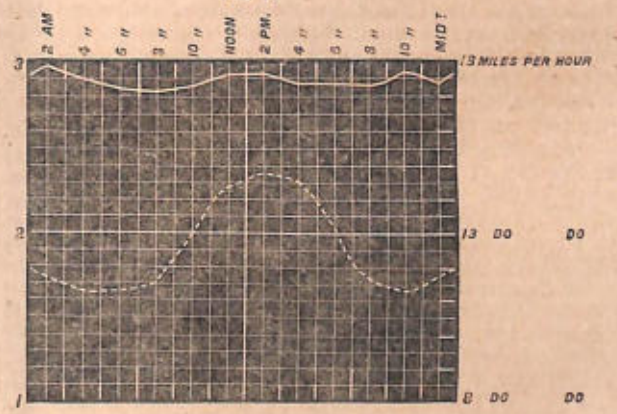
As regards the open sea, it is seen that the diurnal variation is exceedingly small, showing only two faintly-marked maxima about midday and 2 a.m. respectively. On examining, however, the separate means for the five oceans, no uniform agreement whatever is observable among their curves. The slight variations which are met with are different in each case, not one of the maxima or minima being repeated at the same hours in more than two of the five oceans. It follows, therefore, that the force of the winds on the open sea is subject to no distinct and uniform diurnal variation. The difference between the hour of least and that of greatest mean force is less than a mile per hour.

Quite different is it with the winds encountered by the *Challenger* near land, where the observations of the force of the wind give a curve as pronouncedly marked as the ordinary diurnal curve of temperature. The minimum occurs at 2 to 4 a.m., and the maximum from noon to 4 p.m., the absolute highest being at 2 p.m. The curve constructed for each of the five oceans, from the observations near land, gives one and the same result, viz., a curve closely agreeing with the curve of diurnal temperature.

The 650 daily observations on the open sea give a mean velocity of 17½ miles per hour, but the 552 near land give a velocity of only 12½ miles per hour. The difference is greatest at 4 a.m., when it amounts to upwards of 6 miles an hour, but is diminished as the temperature rises, till at 2 p.m. it is less than 3 miles an hour.

At Mauritius, which is situated within the south-east trades, the minimum velocity of the wind is 9·7 miles per hour, occurring from 2 to 3 a.m., from which hour it rises to the maximum 18·5 miles from 1 to 2 p.m., the influence of the sun being thus to double the wind's velocity. At Batavia, situated in a region where the mean barometric gradient is much smaller, the differences are still more decided. From 1 to 6 a.m., 85 per cent. of the whole of the observations are calms, whereas from noon to 2 p.m. only 1 per cent. are calms. In all months the minimum velocity occurs in the early morning, when the temperature is lowest, and the maximum from 1 to 3 p.m., when the temperature is highest. At Coimbra, the mean maximum hourly velocity in summer is five times greater than the minimum velocity, whereas in winter it is only about a half more. At Valencia, in the south-west of Ireland, one of the stormiest situations in western Europe, the three summer months of 1878 gave a mean hourly velocity of 13·3 miles per hour, the minimum oscillating from 10 to 11 miles an hour from 9 p.m. to 6 a.m., and the maximum exceeding 16 miles an hour from 11 a.m. to 5 p.m. The absolutely lowest hourly mean was 10 miles at 11 p.m., and the highest 18 miles at 1 p.m., the velocity about midday being thus nearly double that of the night. The results of observations at many other places might be added to these, including those published by Wild, Hann, Köppen, Hamberg, and others, which go to establish the fact that the curves of the diurnal variation of the velocity of the wind generally conform to the diurnal curves of temperature. The curves of the diurnal variation are most strongly marked during the hottest months. The maximum velocity occurs at 1 p.m., or shortly thereafter, being thus before the maximum temperature of the day (occurring therefore at the time when insolation is strongest); and the minimum in the early morning, when the temperature falls to the lowest, or when the effects of terrestrial radiation are at the maximum. The rule appears to hold good with all winds, whatever be their direction, as shown by Hamberg. The exceptions to this rule are so few, and of such a nature, that they are in all probability attributable to causes more or less strictly local.

FIG 1.



With respect to cloud, Hann has pointed out that for a number of places the mean maximum hourly velocity is 102 per cent. above that of the minimum with clear skies; 77 per cent. with skies half covered with clouds; and 50 per cent. with skies wholly covered. At Vienna, however, these rates of increase are, for clear skies, 101, and half-covered skies, 66 per cent., whereas when the sky is overcast the variation becomes irregular and but faintly marked. Hann has also examined the Vienna observations of the wind on those days when the velocity

[1] Part of this article is abridged from a forthcoming volume of the "Reports" of H.M.S. *Challenger*, by permission of the Lords Commissioners of H.M. Treasury.

did not exceed 30 kilometres per hour and on the days when this rate was exceeded, and finds the diurnal periodicity well marked with light and moderate winds, but irregularly and only slightly marked with strong winds and stormy weather.

In inquiring into the remarkable facts regarding the variation in the diurnal velocity of the wind observed in all climates, attention is first drawn to the two curves of Fig. 1, showing the observations of wind-force made on board the *Challenger* during the cruise. As regards the open sea, the diurnal curve shows practically no variation. The whole of the observations of the surface temperature of the North Atlantic made by the *Challenger* have been discussed, with the result that the daily range is only $0^{\circ}.7$. Hence the statement may be regarded as substantially correct, that over the ocean the atmosphere rests on a floor the temperature of which is all but constant day and night; and, so far as concerns the generation of ascending aërial currents from a heated surface, practically constant.

On approaching the land, however, the daily range of the temperature of the air over the sea becomes materially augmented, the daily range being $4^{\circ}.3$, and, as all observation shows, the temperature over land still more so. Now, bearing in mind that the temperature has risen above its daily mean at 10 a.m., and fallen below it at 10 p.m., an examination of the curve of velocity near land in Fig. 1 reveals the fact that the increase in the diurnal velocity of the wind is entirely restricted to those hours of the day when the temperature is above the daily mean, and the maximum velocity is reached at the hour when insolation, or the sun's heating power, is strongest. The phenomenon of the diurnal variation in the wind's velocity is thus associated in the closest manner with the temperature of the surface on which the air rests. Where there is practically no variation, as in the temperature of the surface of the sea, there is no variation in the velocity; but where, as on land, the temperature of the air has a strongly-marked daily period, the wind-force also is strongly marked, and the increase rises and falls with the degree of insolation on the surface. Further, the velocity increases, not with the increase in the temperature of the air, but with the heating of the surface; in other words, with the conditions on which ascending aërial currents depend.

It is also to be observed, as regards the curves of the five oceans, that they show in each case and at all hours of the day a greater velocity of the wind on the open sea than near land.

The following are the mean and extreme hourly velocities, in miles per hour, for the five oceans:—

	North Atlantic.	South Atlantic.	North Pacific.	South Pacific.	Southern Ocean.
	Miles.	Miles.	Miles.	Miles.	Miles.
Mean hourly velocity on open sea	18·0	18·1	14·5	16·2	23·5
Mean hourly velocity near land	15·0	14·7	9·6	11·0	17·6
Difference	3·0	3·4	4·9	5·2	5·9
Highest mean hourly velocity near land	17·0	16·4	11·6	13·7	20·8
Lowest mean hourly velocity near land	13·1	13·0	10·0	9·3	14·3
Diurnal variation near land	3·9	3·4	1·6	4·4	6·5

Thus the winds are lightest on the North Pacific, and strongest on the Southern Ocean, and these oceans show respectively the least and the greatest diurnal variation in the force of the wind on nearing land.

From the number and character of the two sets of observations, it may be assumed, without risk of error, that the open-sea and the near-land winds, summarised and represented in Fig. 1, were atmospheric movements resulting from mean barometric gradients substantially equal to each other. From the above table it is seen that in each of the oceans the mean velocity near land is less than that on the open sea, the two extremes being the North Atlantic, with a difference of 3·0 miles, and the Southern Ocean with a difference of 5·9 miles; and that even the maximum velocity during the day is always less than the velocity on the open sea. The slight rise in the near-land curve during night is probably wholly caused by the land-breezes felt on board the *Challenger* when near land. In strictly inland places, tolerably well situated for making observations of the wind, this feature does not appear in the curve, and there the velocity falls to the diurnal minimum during the period of lowest temperature, or when the effects of terrestrial radiation are most felt on the surface of the ground.

From these results it follows that, so far as concerns any direct influence on the air itself, considered apart from the floor or surface on which it rests, solar and terrestrial radiation do not exercise any influence in causing the diurnal increase of the velocity of the wind with the increase of the temperature of the air; or if there be any influence at all, such influence is altogether insignificant, as the observations of the *Challenger* on the five great oceans of the globe conclusively prove. The same observations show that on nearing land the wind is everywhere greatly reduced in force, the retardation being due chiefly to friction, and to the viscosity and inertia of the air in relation to the obstructions offered by the land to the onward course of the wind. The retardation is greatest when the daily temperature is at the minimum, and it is particularly to be noted that though the temperature rises considerably, yet no marked increase in the velocity sets in till about 9 a.m., when the temperature has begun to rise above the daily mean. From this time the increase is rapid. The maximum velocity is reached immediately after the time of strongest insolation, and falls a little, but only a little, during the next three to five hours, according to season, latitude, and position. The velocity is low during the hours when the temperature is lower than the daily mean, and the least velocity occurs early in the morning. Even the maximum near land falls considerably short of the velocity which is steadily maintained over the open sea by night as well as by day.

The period of the day when the wind's velocity is increased is thus practically limited to the time when the temperature is above the daily mean, and the surface superheated, and the influence of this higher temperature is to counteract to some extent the retardation of the wind's velocity resulting from the causes already stated. The results show that the increase in the diurnal velocity of the wind is due to the superheating of the surface of the ground, and to the ascensional movement of the air consequent thereon, which tend to reduce the effects of friction and viscosity of the air. It is of importance in this connection to keep in view the fact, shown by hourly observations made at the instance of the Marquis of Tweeddale in 1867 on the temperature of the soil and air, that in cloudy weather a temperature much higher than that of the air near the ground was radiated from the clouds down upon the earth's surface (*Journal Scottish Meteorological Society*, vol. ii. p. 280). Hence in cloudy weather the superheating of the surface-layer of the ground will often take place, the greatest degree of heating being under an overcast sky, where the cloud-covering is of no great thickness, and the temperature of the clouds themselves is much higher than that of the surface of the earth. On the other hand, little or rather no heating will take place, when the cloud-screen which overspreads the sky is of great thickness, and the

temperature of the clouds is not greater than that of the surface; and when the temperature of the cloud-screen is lower than that of the surface, the temperature of the latter will fall. It is scarcely necessary to remark that in discussing the influence of cloud on the diurnal periodicity of the wind's velocity, only such means are of real value as are calculated from a very large number of observations.

During the night, when terrestrial radiation is proceeding, the temperature of the surface falls greatly, and instead of an ascensional movement in the lowermost stratum of the air, there is, on the contrary, a tendency towards, and, if the wind be light, an actual descensional movement down the slopes of the land. The effects of friction being thus intensified, the velocity of the wind falls to the daily minimum during these hours.

ALEXANDER BUCHAN

EPHEMERIS OF THE GREAT COMET, b 1882

(Communicated by Vice-Admiral Rowan, Superintendent U.S. Naval Observatory)[1]

GREENWICH MEAN NOON

1883		R.A. h. m. s.	Decl. ° ′ ″	Log. *r*.	Log. Δ.
Feb.	10·0,	6 0 37·8 ...	−19 41 17 ...	0·48137 ...	0·38891
	14·0,	5 57 40·4 ...	18 40 13 ...	0·48909 ...	0·40520
	18·0,	5 55 19·7 ...	17 41 17 ...	0·49669 ...	0·42132
	22·0,	5 53 32·7 ...	16 44 35 ...	0·50413 ...	0·43723
	26·0,	5 52 14·7 ...	15 50 14 ...	0·51133 ...	0·45282
March	2·0,	5 51 24·4 ...	14 58 16 ...	0·51841 ...	0·46817
	6·0,	5 50 58·7 ...	14 8 43 ...	0·52532 ...	0·48322
	10·0,	5 50 54·8 ...	13 21 37 ...	0·53200 ...	0·49790
	14·0,	5 51 12·3 ...	12 37 0 ...	0·53861 ...	0·51231
	18·0,	5 51 47·9 ...	11 54 52 ...	0·54508 ...	0·52635
	22·0,	5 52 39·5 ...	11 15 10 ...	0·55135 ...	0·53995
	26·0,	5 53 46·1 ...	10 37 56 ...	0·55751 ...	0·55316
	30·0,	5 55 6·1 ...	10 3 6 ...	0·56354 ...	0·56594
April	3·0,	5 56 38·1 ...	9 30 34 ...	0·56944 ...	0·57828
	7·0,	5 58 20·9 ...	9 0 19 ...	0·57520 ...	0·59015
	11·0,	6 0 13·9 ...	−8 32 21 ...	0·58090 ...	0·60158

Note.—In the published elements ϕ should be 89° 13′ 42″·70 instead of 89° 7′ 42″·70.

Washington, February 10

E. FRISBY,
Prof. Math., U.S.N.

ILLUSTRATIONS OF NEW OR RARE ANIMALS IN THE ZOOLOGICAL SOCIETY'S LIVING COLLECTION[2]

XI.

29. THE CAPE SEA-LION (*Otaria pusilla*).—It is a singular and as yet unexplained fact in geographical distribution, that while the Sea-lions amongst Mammals and the Albatrosses amongst Birds are confined to the South Atlantic Ocean, both these groups reach up to high northern latitudes in the Pacific. In the Atlantic, no Albatross is seen "north of the line," whereas these birds are familiar objects on the coasts of both California and Japan. No Sea-lion is met with in the Atlantic until we get to the Cape on one side and the La Plata on the other, but these animals are well-known objects at San Francisco, and the great supply of their much-valued furs comes from the far northern territory of Alaska.

The Sea-lion first became an inhabitant of our Zoological Gardens, and thus known to Europe in a living state, in 1866, when a French seaman, François Lecomte, brought to this country an example of the Patagonian species (*Otaria jubata*), and exhibited it to the public. The remarkable form of this animal, its extreme docility, and its agile movements attracted great attention, and led to its acquisition by the Zoological Society, in whose Gardens it quickly became an established favourite. Upon the death of this individual in the autumn of the same year, the Council of the Society determined to send out Lecomte, who had entered their service in charge of it, to the Falkland Islands, in order to obtain other specimens. Lecomte returned to this country in August, 1867, but owing to various unforeseen circumstances only succeeded in landing alive one of the four Sea-lions with which he had started from Port Stanley. This animal, young and small on its arrival, throve well under Lecomte's careful management, and soon supplied the void occasioned by the death of the original specimen. Like its predecessor, it exhibits extraordinary agility in the water, and catches the fishes thrown to it for food both above and below the surface with unerring aim.

Four years subsequently, in 1871, the Society received from Sir Henry Barkly, then Governor of the Cape Colony, a present of a young specimen of the Cape Sea-lion, of which we now give an illustration (Fig. 29). Like its Patagonian relative, the Cape Sea-lion is a female, and although quite adult, does not attain the dimensions of the male sex of these animals In general appearance, shape, and form, the two species are very similar, and present little obvious differences to the casual observer, except that the ear-lobe is longer in the Cape animal. To the two females has recently been added a young male of the Patagonian form, and the three individuals now live together in the narrow limits of their basin in the greatest harmony, forming one of the most attractive groups in the Regent's Park Gardens. Little has been recorded of the mode of life of the Sea-lion in a state of nature, but Mr. E. L. Layard in his "Catalogue of the South African Museum," tells us that it "is abundant along the whole of the coasts of the colony, and has given its name to numerous bays, islands, and capes, of which 'Robben' Islands near Cape Town is perhaps the best known.

"It resorts to these places in great numbers for breeding purposes, and is sought for and slain for the sake of its fur and oil. The male is said to be maned, and to much exceed the female in size, but though double the market value of the skin has been offered by the Museum for a skin of the male of this common animal, as it is not *the custom* of the sealers to take the skin off, leaving in the head and feet, we have been unable to procure one."

As regards the habits of some of the other members of this genus, which are of the most extraordinary character, we have now ample details concerning the North Pacific species in a very interesting and well illustrated work prepared by Mr. Henry W. Elliott on the Seal Islands of Alaska and their productions.[1]

Soon after the Sea-lions were established in the Zoological Gardens in this country, specimens of these animals were obtained by the principal Gardens on the Continent, and basins built for the exhibition of their aquatic evolutions. But the examples on the Continent, as well as those in the Aquarium at Brighton, all belong to one of the North Pacific species of Sea-lion (*Otaria californiana*), which is found in enormous multitudes upon the Pacific coast. Of the South African species now figured, the example in our Zoological Society's Gardens is the only one yet brought alive to Europe.

30. BLANFORD'S SHEEP (*Ovis blanfordi*).—Every high mountain-tract in Northern and Central Asia appears to be occupied by a distinct form of Wild Sheep (*Ovis*), while single outliers of the same genus are found far to the west in Sardinia and to the east in North America. Some of these animals, such as the celebrated "Ammon" of Ladakh (*Ovis hodgsoni*) and the Snow-sheep of Kamschatka (*O. nivicola*), attain a magnificent size and

[1] Computed from elements (NATURE, vol. xxvii. p. 225) and reduced to the mean equinox 1883·0.

[2] Continued from p. 154.

[1] "A Monograph of the Seal Islands of Alaska. By Henry W. Elliott Reprinted, with additions, from the Report of the Fishery Industries of the Tenth Census. 4to. Washington, 1882.

development, whilst others, like the Sardinian species (*Ovis musimon*), are more nearly of the dimensions of the ordinary domestic animal.

The Wild Sheep best known to the sportsmen of British India is the "Koch" or "Gudd" of the Punjaub, also called the "Oorial." The Oorial frequents the

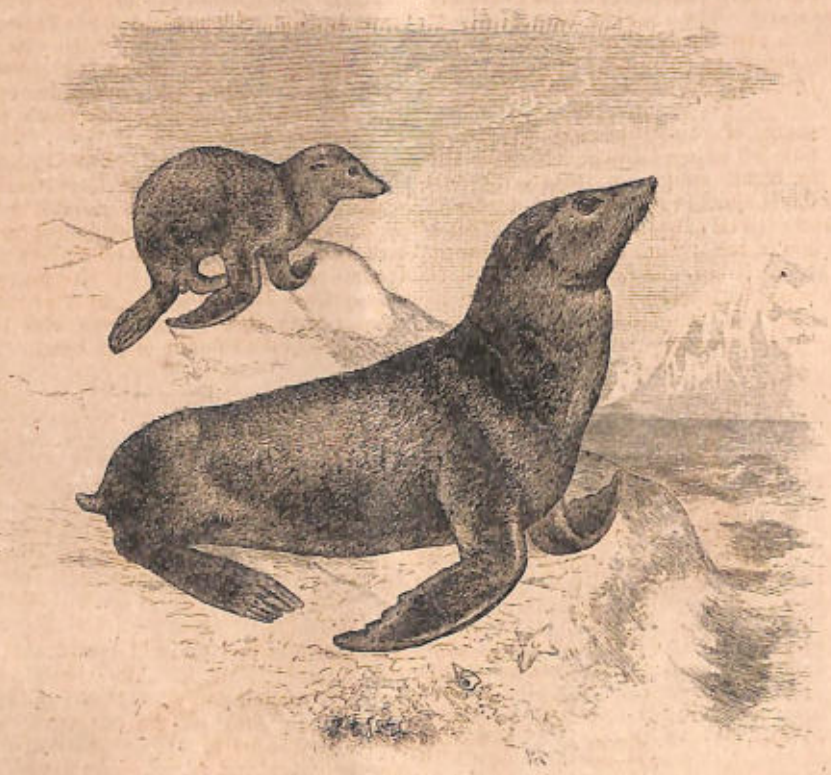


FIG. 29.—The Cape Sea-lion



FIG. 30.—Blanford's Sheep.

rocky and stony hills of the Punjaub, as also the Sulimani range on the other side of the Indus and the hills of Hazara and Peshawur. In these districts, Dr. Jerdon tells us, it occurs at very low elevations—from 800 to

2000 feet above the sea-level—and is capable of enduring great heat.

The Punjaub Sheep was introduced into the Zoological Society's series nearly thirty years ago, and has frequently bred in their menagerie. The adults of both sexes and the lambs, of which two are generally produced at a birth, are correctly figured in Wolf and Sclater's "Zoological Sketches" from specimens living in the Society's Gardens.

The recent extension of British influence into Khelat and Afghanistan has led to our acquaintance with the Wild Sheep of the higher ranges of these territories, which, although closely allied to that of the Punjaub, is perhaps distinct and entitled to specific separation. Such at least is the opinion of the Indian naturalist, Mr. A. O. Hume, who in 1877 described and figured the horns of this form, from a specimen received from Major Sandiman, under the name *Ovis blanfordi*, in honour of Mr. W. T. Blanford, a well-known Indian zoologist and geologist. Mr. Hume's example of Blanford's Sheep was obtained in the hills above the Bolan Pass. The specimen from which our drawing has been prepared (Fig. 30) was captured in Afghanistan during the recent campaign in that country, and presented to the Zoological Society by Capt. W. Cotton in February, 1881. It is a young male animal with the horns not yet fully developed, and has been placed in company with a female of the better-known Sheep of the Punjaub hills, with which, there is no doubt, it will readily cross.

31. The Uvæan Parrakeet (*Nymphicus uvæensis*).—In the second volume of Capt. Cook's "Voyage towards the South Pole and round the World" will be found (at p. 110) a large, double, copper-plate engraving entitled a "View in the Island of New Caledonia," taken from a sketch prepared by Hodges, the artist of the expedition. The left-hand corner of this engraving contains a rude figure of a parrot with two feathers projecting from the summit of its head. This is doubtless



Fig. 31.—The Uvæan Parrakeet.

the first representation ever given of the celebrated "Horned Parrot" of New Caledonia, of which a single specimen was obtained by the great circumnavigator when the island itself was first discovered (in September, 1774), and brought home for the collection of Sir Joseph Banks.[1]

Since the time of Cook until recent days little more has been known of this singular parrot. In the posthumous works of Forster, who accompanied Cook as naturalist, it was described as "*Psittacus bisetis*," and Wagler, in 1832, made it the type of a new genus "*Nymphicus*." But specimens remained very scarce, and Dr. Finsch in his excellent history of the Parrot-tribe, published in 1868, tells us that it was then still one of the rarest of the whole family in the museums of Europe.

In 1879 however two living examples of this parrot were brought to London in a vessel coming from Sydney, and secured by the Zoological Society for its parrot-house. There was thus for the first time for those who do not wish to make a voyage to New Caledonia, an opportunity of seeing this lovely species in its full beauty. What, however, followed was still more singular. In April, last year, while the two Horned Parrots were still alive, there arrived in the London market a second pair of birds of the same peculiar structure, but presenting ample distinctions for their recognition by naturalists as a distinct species. These birds, it need hardly be said, were quickly secured for the Zoological Society's Aviary, so that the two forms might be exhibited side by side. At the same time was received a communication from Mr. E. L. Layard, H.B.M. Consul at Nouméa, in New Caledonia, a Fellow of the Zoological Society, and a well-known naturalist, describing the new species under the name *Nymphicus uvæensis* (see *P. Z. S.*, 1882, p. 408, Pl. XXVI.).

[1] See Latham's "Synopsis of Birds," vol. i. p. 248.

It would appear that the Uvæan Parrakeet, of which we now give an illustration (Fig. 31), taken from one of the Zoological Society's living specimens, is a kind of satellite of the New Caledonian Parrakeet, as is the small island of Uvéa, in which it is found, of the larger island of New Caledonia. Mr. Layard had a living pair of the Uvean bird for some time in his possession before he noticed their difference from the New Caledonian bird, of which he had regarded them as the immature form. But in the first place the crest of the two birds is totally different. In *Nymphicus cornutus* the crest is composed of two elongated feathers, which are black, faintly tinged with green, and broadly tipped with red. In *N. uvæensis*, as will be seen in our figure, the crest consists of a bunch of about six short, upturned, entirely green feathers, springing from the end of a small spot of red which occupies the centre of the forehead. In *N. cornutus* the two long crest-feathers rise from the centre of the broad red cap which covers the whole top of the head. Besides this difference the former bird does not present the broad orange nuchal collar which ornaments *N. cornutus*, and exhibits only the faintest trace of orange on the rump.

The small island of Uvéa, one of the Loyalty group, to which the new species is confined, consists, as Mr. Layard tells us, of a series of small islets joined together by a connecting reef with a lagoon in the centre. It is very singular that this distinct form should be found only in so restricted a locality, while its near relative, the "Horned Parrot" of Cook, appears to be distributed all over the large island of New Caledonia.

THE ELECTRIC LIGHT AT THE SAVOY THEATRE[1]

MR. D'OYLY CARTE, having determined to light the Savoy Theatre by the Swan incandescence electric light, intrusted the work of installation to Messrs. Siemens Brothers and Co. The theatre is lighted by no less than 1194 Swan lights of the improved form introduced by Mr. C. H. Gimingham, of the Swan United Electric Light Company. Of these 1194 electric lights, the auditorium is lighted by 150 lamps attached in groups of three, supported on threefold brackets projecting from the different tiers and balconies, each lamp being inclosed within a ground, or opaloid, shade, by which arrangement a soft and pleasant light is produced. These bracket lamp-holders have been designed and constructed by Messrs. Faraday and Son, of Berners Street, London.

Two hundred and twenty lamps are employed for the illumination of the numerous dressing-rooms, corridors, and passages belonging to the theatre, while no less than 824 Swan lamps are employed for the lighting of the stage.

The stage lights are distributed as follows:—

6	rows of	100	lamps each	above the stage	...	600
1	,,	60	,,	,, ,,	...	60
4	,,	14	,,	fixed upright ...	...	56
2	,,	18	,,	,,	...	36
5	,,	10	,,	ground lights	...	50
2	,,	11	,,	,,	...	22
						824

In addition to the above-mentioned lights within the theatre, there are eight pilot lights in the engine-room, which, being in the same circuit with some of the lights in the theatre, serve the purpose not only of illuminating the machinery, but also of indicating to the engineer in charge of the machines, by the changing of their illuminating power, when the lights in the building are turned up or down.

The lamps are at present worked in parallel circuit in six groups, five of which comprise 200 lamps each, and the sixth 202 lamps. The current of each group is produced by one of Messrs. Siemens Brothers and Co.'s W_1 alternate current machines, the field magnets of which are excited by a separate dynamo electric machine of the Siemens type, known as D_7. The machines and engines are fixed in a shed erected on a piece of waste land adjacent to the Victoria Embankment, the current being conveyed to the theatre by means of insulated cables laid underground.

The six alternate or W_1 machines are driven at a speed of 700 revolutions per minute, and the six exciting or D_7 machines at 1150 revolutions, by three steam-engines, that is to say, a portable 20-horse engine by Garrett, a 12-horse power portable by Marshall, and a 20-horse semi-portable engine by Robey, but the power actually utilised, as measured by a "von Hefner Alteneck" dynanometer, is between 120 and 130 horse-power. We must not, however, omit to state that, in addition to the six pairs of machines for working the 1202 incandescent lamps, there is also a D_9 Siemens dynamo machine for producing the powerful arc electric light suspended outside the theatre, and over the principal entrance in Beaufort Buildings, and that the power to drive this machine is included in the above-mentioned horse-power employed, as well as that necessary for driving the shunt machine used to charge the secondary batteries for the fairy lamps.

The most interesting feature, however, from a scientific point of view, of this most interesting installation, is the method by which the lights in all parts of the establishment are under control, for any of the series of lights can in an instant be turned up to their full power or gradually lowered to a dull red heat as easily as if they were gas lamps, by the simple turning of a small handle. There are six of these regulating handles, corresponding to the number of the machines and circuits—arranged side by side against the wall of a small room on the left of the stage, and each handle being a six-way switch, can, by throwing into its corresponding magnet-circuit greater or less resistance (according to its six stages), lessen or increase the strength of the current passing through the lamps by as many grades. The special interest of this part of the installation, however, is the fact that the turning down of the lights is accompanied by a corresponding saving of motive power in the engine, for the variable resistance which is controlled by the regulators is not thrown into the external or lamp circuit of the alternate current machines, but into the circuit by which their field magnets are excited.

The fittings of the lamps in the passages, staircases, &c., have, up to the present, been of a temporary nature, but, as the electric lighting has worked to the entire satisfaction of all concerned, these temporary fittings will now be replaced by permanent brackets, quite independent of the gas brackets.

All risk of fire is avoided by the leading wires being thoroughly insulated, and small pieces of lead wire being inserted into the circuit wherever a branch wire leaves the mains. These "safety-pieces" of lead are chosen of such size that they will melt before the conductors themselves become sufficiently heated to cause any danger, and by their melting the current is at once interrupted.

The small lamps worn by the fairies, and which have been specially made by the Swan United Electric Light Company, are rendered incandescent by the current produced from a small "secondary" battery, which is carried on the back like a small knapsack. These secondary batteries have been made by Messrs. Siemens Brothers and Co. on a new plan, and are charged by a shunt-wound Siemens' dynamo in the engine-shed. Each battery is provided with a switch, by means of which the light can be turned on or off by the wearer at pleasure.

The system of electric lighting has now been working

[1] Communicated.

at this theatre for about a year and a half without any accidents, and has proved to possess many advantages over gas as applied to the illumination of buildings of this description. Not the least amongst these are the total absence of heat and vitiated air in the house, and the length of time during which the decorations will retain their freshness and colour instead of becoming quickly faded and tarnished, as would be the case were the old system of gas adopted.

ON THE NATURE OF INHIBITION, AND THE ACTION OF DRUGS UPON IT

BY inhibition we mean the arrest of the functions of a structure or organ, by the action upon it of another, while its power to execute those functions is still retained, and can be manifested as soon as the restraining power is removed.

It is thus distinguished from paralysis, in which the function is abolished, and not merely restrained.

Inhibition is one of the most perplexing problems in physiology, and we have at present no satisfactory hypothesis regarding it. It plays, however, such a very important part in pharmacology, that we cannot pass it over; and as it is through the action of drugs upon the various functions of the body that we have already arrived at a knowledge of inhibitory actions, which would otherwise have been impossible—as, in fact, pharmacology has here quite outstripped physiology—we are obliged to enter into some hypothetical considerations, in order to be able to form some kind of idea regarding the mode of action of many drugs.

Hypotheses serve as "pegs on which to hang facts," and by their aid the isolated facts which few memories could carry may be arranged, and their relation to each more readily perceived. A hypothesis serves also as a guide for further experiments, by which it may be either disproved or supported. Should facts be against it, so much the worse for the hypothesis; it must be discarded, and another tried in its place; but if facts agree with it we obtain a means of predicting phenomena, and make another step in knowledge. Like other useful things, hypotheses are not without danger, and sometimes do harm by satisfying people and stopping further inquiry. Thus Sultzer noticed the peculiar taste produced by the contact of two dissimilar metals with each other and with the tongue forty years before Galvani; but at that time the doctrine of vibrations was employed to explain all natural phenomena, and he concluded that some peculiar vibration occurred from the contact of the metals, which produced the peculiar sensation on the tongue. All the world were satisfied with the explanation, and thus a prominent fact slept in obscurity from the time of Sultzer to that of Galvani, no further attempts being made to determine the nature of the vibrations or the laws which governed them.[1] Yet in their proper place hypotheses are most useful, and but for the hypothesis that light, heat, and sound are due to waves, our knowledge of their phenomena would be much less than it is.

The cases of inhibition, as we may term them, which we meet with in the study of physics, are the production of complete silence by the interference of two sounds, and of darkness by the interference of two rays of light.

When two sounds or two rays of light are combined, so that the crests of the waves of which they consist coincide, the sound becomes louder and the light brighter. If they are thrown together, so that the crests of the waves in the one sound or ray coincide with the sinuses or hollows of the other, they completely counteract each other, and silence or darkness is produced.

When the waves are of different rhythms, the crests and hollows of the two sounds or rays, which at one time coincide, will gradually interfere, and again gradually coincide, so that rhythmical alternations of loud sound and silence, of bright light and darkness, are produced.

A good example of interference or physical inhibition, and one that affords an illustration well suited to our purpose, is that of Newton's rings. When a lens of small curvature is placed on a plane surface of glass, a series of rings is observed, starting from the centre of the lens and passing concentrically outwards. If monochromatic light is used, such as pure yellow light, pure red light, &c., these rings are alternately bright and dark; but if white light is used, they appear as a number of circular bands of different rainbow colours. The cause of these rings is, that though the surface of the lens appears to the eye to be in contact with the plate of glass over a considerable area, it is not really so; a very fine film of air of varying thickness being interposed between them.

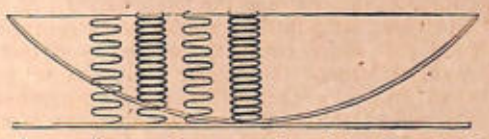


FIG. 1.—A very diagrammatic representation of interference in Newton's rings.

When a ray of light passes through the lens on to the glass, part of it is reflected back from the lower surface of the lens, and part of it from the upper surface of the glass plate. Between those two points there is a very minute film of air: *one ray has therefore to travel somewhat further than the other.* The distance which it has to travel is only through the extremely thin layer of air lying between the surface of the lens and the glass and back again; but this distance at some places is just sufficient to throw the waves in the one beam *half a wave-length* behind those in the other, and to produce darkness by their interference.

As we recede from the point of most complete contact between the lens and the glass, the thickness of air increases, the ray has somewhat further to travel, and the distance is then just sufficient to throw it *a whole wave-length* behind the other ray; no interference is produced, and we get a ring of bright light.

Further outwards the increased thickness of the film of air is again sufficient to throw one ray *a wave-length and a half* behind its fellow; interference is again produced, and darkness is the result.

With rays, then, of one colour, or of one wave-length, we get alternately light and darkness by interference.

But it is evident that the extra distance which the waves have to travel in order to produce interference will not be the same for long and short waves; and thus it is found that when white light, which contains rays of different wave-lengths, is used, the rings, instead of being alternately light and dark, are coloured.

The very distance which was sufficient to throw the red rays half a wave-length behind the other, and to produce interference, will throw, let us say, the violet rays a whole wave-length behind, and thus there will be no interference and *vice versâ*; the distance which causes interference of the violet rays does not cause interference of the red, and so on with other colours.

Thus the spaces which would have been perfectly dark when rays of pure red or pure violet, or more correctly ultra-violet, were used, would be filled up by the other if used together, and when white light is used, the various waves interfere at different places, and so we get a series of rainbow colours.

The extra distance which one beam has to travel in order to produce interference with another is *not absolute*, but relative to the wave-length. This relation differs for different wave-lengths, and therefore if the relative distances remain constant, the effect of the beams on each other will vary if their wave-lengths be changed.

It is obvious that if both the wave-lengths and the

[1] Ree's Cyclopedia. Article "Galvanism."

distances they have to travel remain the same, the effect of the beams on each other will be altered by any change in their rate of travel such as would be effected by altering the media through which they pass.

This is a most important point in regard to the hypothesis of the causation of inhibition by interference of vibrations in the nervous system. It may therefore be useful to illustrate this further, and probably it could not be done better than by using, with a little modification, the example given by Sir J. Herschel in his article on Light in the Encyclopædia Metropolitana. "Let R be a reservoir of water, from which the channels A and B pro-

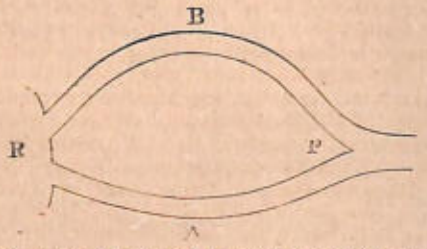


FIG. 2.—Diagram to illustrate Sir J. Herschel's observations on interference. Adapted from his article on "Absorption of Light," Phil. Mag. 1833, p. 405.

ceed, to join each other at P; they are supposed to be equal in every respect except that B is longer than A. If a wave from the reservoir enters the openings of A and B at the same time and travels at the same rate along them, the wave which passes through A will reach P sooner than the one which passes through B, so that the water at that point will be agitated by two waves in succession. But let the original cause of undulation be continually repeated so as to produce an indefinite series of equal and similar waves. Then if the difference of lengths of the two canals A and B be just equal to half the interval between the summits of two consecutive waves, it is evident that when the summit of any wave propagated along A has reached the point of intersection P, the depression between two consecutive summits (viz., that corresponding to the wave propagated along A and that of the wave immediately preceding it) will arrive at the intersection P by the course B. Thus in virtue of the wave along A the water will be raised as much above its natural level as it will be depressed below it by that along B. Its level will therefore be unchanged. Now as the wave propagated along A passes the intersection P, it subsides from its maximum by precisely the same gradations as that along B, passing it with equal velocity, rises from its minimum, so that the level will be preserved at the point of intersection P undisturbed so long as the original cause of undulation continues to act regularly.[1] So soon as it ceases, however, the last half-wave which runs along B will have no corresponding portion of a wave along A to interfere with, and will therefore create a single fluctuation at the point of concourse P."

It is obvious that if everything else remains the same, the effect which the waves have upon each other at P will be altered if the rate at which they travel is increased or diminished.

The more the speed is increased the less effect comparatively will the greater length of B have in retarding the wave which flows along it, so that its crest will no longer coincide with the trough or sinus of the waves in A, but will, on the contrary, coincide more nearly with the crest of one of the waves in A.

The more the speed is diminished, the more will the wave in B lag behind that in A, so that its crest, instead of coinciding with the trough between two crests of the waves from A will gradually come to coincide with the crest succeeding the trough, and thus double its magnitude instead of destroying it.

We see, then, that under the conditions we have supposed either increase or diminution in the rapidity of their transmission may convert the interference of waves into more or less complete coincidence, and the effect of the two waves may thus be doubled instead of neutralised by their superposition.

The alteration which is produced in the mutual effect of two waves by increase or diminution of their rate of transmission along channels of constant length supplies us I think with a test by which we may ascertain the truth of the hypothesis that inhibitory phenomena in the animal body are due to interference. For if it be true we ought to find that a nerve which produces inhibitory phenomena when excited under normal conditions will gradually lose this power when the rate of transmission along it is increased or diminished, as, for example, by the influence of heat or cold, and will gradually acquire an exactly contrary or stimulating action. This, I think, is shown to be the case by our experimental data so far as they go.

Several authors have pointed out the analogy between inhibitory phenomena in the animal body and the effects of interference of waves of light or sound. This has been done with special precision by Bernard[1] and Romanes.[2] The tendency to do away with the idea of distinct inhibitory centres is gradually spreading, but hitherto no attempt has been made to bring all the phenomena of inhibition under one general rule or to explain the mode in which they are affected by the action of drugs. The object of the present paper is to gather together some instances of inhibition which we find in the body, and to see whether by the theory of interference it is not possible to explain both the curiously perplexing exceptions which we meet with in physiological experiments, and the still more perplexing action of drugs on inhibitory phenomena.

One of the most striking examples of reflex action and of inhibition, is the effect of a slight touch or touches, and of firm pressure upon the palms of the hands, the soles of the feet, or the axillæ, and in some persons also the knees. In many persons a very slight touch or succession of touches upon these parts is sufficient to throw first the respiratory muscles, and then the whole body into violent convulsions. Indeed, it is stated that during the persecution of the Albigenses by Simon de Montfort, several people were tortured to death by tickling the soles of their feet with a feather. The stimulus here applied, and the consequences it produces, appear to be out of all proportion to one another; the stimulus being almost infinitesimal, and the consequences enormous.

In the case of Newton's rings it might be possible with much trouble to throw a different beam into such a condition that it would interfere with one of the beams in the rings and produce darkness, but in the rings a similar effect is produced in a very much simpler way by alteration of part of the same beam. A similar occurrence is to be observed in the inhibition of the reflex action on tickling.

By a very powerful effort of the will we may completely arrest the reflex movement which would otherwise occur, and allow the limb to remain perfectly passive. But the same effect is produced in a much simpler way by applying a firm pressure instead of a slight touch. The firm pressure neutralises the effect of the touch in regard to motion, and not only are no reflex convulsive actions produced, but no tendency whatever to them is felt.

But while the pressure has neutralised the tendency to motion, and has altered the character of the sensation, it has not neutralised sensation. On the contrary, it has rendered it more definite, so that one can distinguish with much greater certainty the particular point of the

[1] This actually happens in the harbour of Batsha, into which the waves pass from the open sea through two channels of unequal length.

[1] Bernard, La Chaleur Animale, Paris, 1876, p. 371.
[2] Romanes, Phil. Trans. 1877, p. 730.

surface which has been touched. Increased pressure has thus inhibited motion but increased sensation.

In a paper on "Inhibition, peripheral and central," which I wrote in the West Riding Asylum Reports in 1874, I tried to explain these phenomena in the following manner: "It appears to me to be in all probability due to there being two sets of ganglia in the cord itself, one motor and one inhibitory. The motor is more readily excited than the inhibitory, and causes violent movements, which the inhibitory centres of the brain cannot restrain without the greatest difficulty, though they are readily controlled by the inhibitory ganglia in the spinal cord. A slight titillation excites the motor, but not the inhibitory spinal ganglia; a stronger pressure stimulates the inhibitory centres also, and thus arrests the movements without any action being required on the part of the inhibitory centres in the brain. We may try to explain this, by supposing that there are two distinct sets of nerves proceeding from the skin to the cord, one of them having the power to excite inhibitory, and the other to excite motor centres. Further, we must suppose that these sets of fibres are endued with different degrees of excitability, the motorial ones being stimulated by a slight touch, but the inhibitory ones only by a stronger impression.

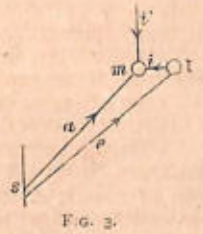


FIG. 3.

"This is represented in Fig. 3, where *s* is the skin; *a*, the fibres proceeding from it to the motor ganglion, *m* and *e*, those going to the inhibitory ganglion, I; *i* is the fibre by which I arrests the action of *m*, and *i* that by which the brain exerts a similar action. The different fibres by which *m* acts on the muscles have not been introduced into the diagram.

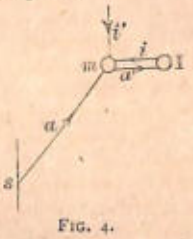


FIG. 4.

"This hypothesis, however, is a very clumsy one, and we explain the facts quite as well by supposing that there is only one set of afferent nerves (*a*, Fig. 4) from the skin to the cord, which transmit a slight impression only to the motor ganglia, *m*, but convey a stronger one along *a'* to the inhibitory ganglia I, also, which then react through *i* upon the motor ones. This latter supposition renders intelligible the fact that it is only when something is drawn quickly and lightly across the skin, so as to make a slight and transient impression on the ends of many sensory nerves, that tickling is felt. If the pressure on the skin is heavier, or if the motion over it is slow, the effect is quite different, and this is just what we might expect if a short and slight impression travels only to the motor ganglia, and a stronger or more lasting one goes to the inhibitory beyond them."

These diagrams themselves are suggestive of interference; but I did not in that paper say anything regarding it, contenting myself only with the term inhibition. One reason that prevented me from considering inhibition in animals as corresponding closely to the interference of light, was that the rapidity of transmission of nervous impulses was differently given by different observers, and indeed, according to Munk, it varies along the course of the same nerve.[1]

Unless the rate of transmission of impulses is constant, one cannot expect interference to produce inhibition. But in his observations on Medusæ, Mr. Romanes found that when the circumference of the bell in a medusa was cut into a long spiral strip, leaving only the centre of the bell uninjured, stimuli applied to the extreme end of the strip passed along it, and were delivered to the centre of the bell, just as if they had been applied to the central part itself—all passing at the same rate they did not interfere with one another. But when the strip was pressed upon or stretched, the passage of impulses was interfered with.

This seems to show that the rate of transmission of a stimulus along a conducting structure is a definite one, provided the structure remain under the same conditions. But still more instructive on this point are the experiments with the Ton-inductorium, invented by my friend Prof. Hugo Kronecker. Other observers have found that when a muscle is irritated by an interrupted current applied to its nerve, the tetanic contraction into which it would be thrown by twenty interruptions per second ceased when the interruptions became as frequent as 250 per second. By using an interrupted current induced by the vibrations of a magnetic rod, which gave out a definite tone, Kronecker and Stirling were able to throw the muscle into tetanus with no less than 22,000 interruptions per second. This success is probably to be attributed to the regularity and equality of the stimuli applied by Kronecker's method, while the fact that their predecessors got no tetanus with more than 250 interruptions per second is probably due to interference of the stimuli they applied.[2] Kronecker's observations show, I think, how definite must be the rate of transmission of stimuli along a nerve so long as it remains under the same conditions and give us a basis for extending the theory of interference from waves of light and sound to vibrations in nervous and muscular tissues.[3]

We are justified, I think, by these experiments in considering that interference may occur in the nervous system, and that one part may exercise an interfering or inhibitory effect upon the other, which is constant under normal conditions, but will be modified when these conditions are altered.

Let us now try to apply this hypothesis to the reflex action which we have just been discussing.

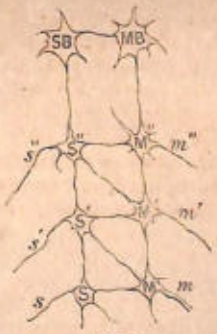


FIG. 5.

Let S, S' and S'' be three sensory cells in the spinal cord, M, M' and M'' motor cells, *s* and *s'* sensory nerves, and *m m'* motor nerves. S B is a sensory and M B a

[1] Archiv. f. Anat. u. Physiol. 1860, p. 798.

[2] It must be borne in mind, however, that the overtones of such a vibrating rod are in the ratio of *n*, 3 *n*, 5 *n*, &c., and not in that of *n*, 2 *n*, 4 *n*, like those of a vibrating string or pipe. Quincke (Poggendorff's Annalen, 1866, vol. viii. p. 182) failed to silence the sounds of such a rod by means of an interference apparatus.

[3] *Vide* Hermann's Handbuch d. Physiol. Bd. i. Th. i. p. 44, and Bd. ii. Th. i. p. 39.

motor cell in the brain. When *s* is stimulated by a slight touch, the stimulus is transmitted up to S, thence to M, and down *m* to muscles, thus causing reflex contraction. This is increased when a number of slight touches are made over a limited surface, as in tickling, because then S and S′ are both stimulated, and more motor impulses are produced. But when harder pressure is made on *s* the stimulus, instead of being confined to S, is transmitted to S′ and thence to M, as well as direct from *s* to M. Thus two impulses are sent to M, which, starting at the same time from *s*, have had a different length to travel round. This different length, we suppose, is just sufficient to allow the impulses to interfere with one another in M and thus destroy each other's action in regard to motion. When *s*′ is also irritated at the same time as *s*, the same interference is produced by a stimulus passing from S′ to S″, and then to M′. But at the same time that the relation of S′ S″ to M and M′ is such as to produce interference and inhibition in regard to motor impulses, the relation to each other is such that the impulses mutually strengthen one another on their way up to the brain, and thus the sensation which we perceive on firm pressure is more definite and better localised.

On this hypothesis each successive layer of sensory and motor cells in the spinal cord may have several different functions: (1) Each cell may exercise its own sensory or motor functions in relation to the sensory or motor nerves connected with it; (2) it may exercise an inhibitory function on the sensory and motor cells above or below it, and also on other sensory or motor cells on the same plane with itself; (3) it may have a stimulating function on other cells above, below, or on the same plane as itself, increasing instead of abolishing their action.

The effect that any sensory or motor cell produces when stimulated is not determined then simply by the *properties* of the cell itself, but by its *relations* to other cells or fibres.

Motion, sensation, inhibition, or stimulation are not positive, but simply relative terms, and stimulating or inhibitory functions may be exercised by the same cell according to the relation which subsists between the wave-lengths of the impulses travelling to or from it, the distance over which they travel, and the rapidity with which they are propagated.

T. LAUDER BRUNTON

(*To be continued.*)

NOTES

M. JANSSEN was present at the sitting of the Academy of Sciences on Monday, for the last time before his departure from Paris. He is very busy preparing his apparatus.

BARON NORDENSKJÖLD so very carefully considers every step he takes that we may be sure he has satisfactory reasons for claiming the reward of 25,000 guilders (about 2000*l.*) offered by the Dutch three centuries ago to the discoverer of the North-east Passage. Some surprise is expressed at the Baron's claiming a reward which lapse of time may be considered as having rendered obsolete. At the time it was offered the North-east Passage was regarded as a sea-route of the highest commercial importance, though this idea has been long exploded. Still to some extent Baron Nordenskjöld has shown that the old conception was not without justification, and although the passage is now of no value as a route to China and India, still the Swedish explorer has proved that as a trade-route it may be rendered of considerable value. Moreover as he is so disinterested, ardent, and successful a pioneer of science, we should be glad if the Dutch Government cheerfully admitted the claim. It may be remembered that the much larger reward offered by our own Government to the discoverer of the North Pole was withdrawn many years ago.

AT the last meeting of the Royal Swedish Geographical Society, on the proposal of Baron Nordenskjöld, the greatest honour at the disposal of the Society, the *Vega* gold medal, was conferred on Mr. Stanley. The medal, struck *in memoriam* of the *Vega* expedition "for geographical discovery," has only been twice before conferred—viz. in 1881, on Baron Nordenskjöld, and in 1882, on Capt. Palander.

IN 1880 the Belgian Academy proposed, as a prize-subject, the relations between physical and chemical properties of simple and compound bodies (completion of the knowledge of these by new experiments). The prize (a gold medal valued at 1000 francs) has been awarded to M. De Heen, engineer at Louvain. His memoir is an extension of one previously sent in, which gained high approbation for original work and results, but was thought badly-proportioned, so that the subject was re-proposed. The work is in five sections, dealing successively with specific heats, dilatability of solids and liquids by heat, changes of state in relation to chemical composition, capillarity, and (here without original researches) molecular volumes, refraction, spectral analysis, and absorbent power of bodies for heat. The ample *résumé* M. Spring gives of this memoir (*Bull. Belg. Acad.* No. 12) indicates matter that must be of much interest and value to the physicist and the chemist.

PERHAPS never in the history of science, the *Lancet* says, has a distinguished career equalled in its length that of M. Chevreul, whose name is best known in this country in connection with his investigations on colour; and it is probably altogether unique for a *savant* to be able, at one of the most distinguished scientific societies in the world, to refer to remarks which he made before the same society more than seventy years previously. A few days ago M. Chevreul made a communication to the Académie des Sciences, and at its close he observed: "Moreover, gentlemen, the observation is not a new one to me. I had the honour to mention it here, at the meeting of the Académie des Sciences, on the 10th of May, 1812"!

THE death is announced of the Silesian botanist, Herr Johann Spatzier, aged seventy-seven; also of Herr Josef Knörlein, the entomologist, at Linz, on February 12, aged seventy-seven.

MOUNT ETNA is very active and ejects red-hot lava. At night the glare is constantly visible. A violent shock occurred on February 15.

WE find in the last number of the *Izvestia* of the Russian Geographical Society a note, by Prof. Lenz, on the cosmical dust collected by M. Marx at the meteorological station of Yeniseisk. After having vainly searched for traces of cosmical matter, as he was advised to do by Baron Nordenskjöld, he discovered it finally on October 31, 1881. The wind was blowing in the evening with great force from the west, and during the night it turned into a strong gale, with some snow and rain. When M. Marx measured next morning the amount of water in his pluviometer, he remarked that it had a considerable quantity of suspended matter of a brick-red colour. After careful analysis this matter proved to consist of iron, nickel, and cobalt. Prof. Lenz does not doubt that the red dust found by M. Marx had a cosmical origin, and points out that it was observed on a day very near to the appearance of the November meteors.

AT Monday's meeting of the Paris Academy of Sciences, M. Tresca read a paper full of facts on the experiments tried at the Gare du Nord. Deducting certain work for the mechanical transmission to the generator, the result was 42 per cent. of energy conveyed instead of 35 per cent. with a smaller

velocity, but without deducting this work the alteration was very slight, 33 per cent. instead of 32 per cent.

THE second annual general meeting of the members of the London Sanitary Protection Association was held on Saturday at the rooms of the Society of Arts, under the presidency of Prof. Huxley. From the report of the council, presented to the meeting, it appeared that 368 new members had joined the Association during the year, and there was a total of 533 members. The total number of houses inspected was 362, and in the greater number of these serious errors in the sanitary arrangements of the houses were found and corrected. Twenty-one of them, or 6 per cent., were found to have the drains choked up, and no communication whatever with the sewer; all the foul matter sent down the sinks and soil-pipes simply soaking into the ground under the basement of the houses. In 117 houses, or 32 per cent., the soil-pipes were found to be leaky, allowing sewer-gas, and in many cases liquid sewage, to escape into the house. In 137, or 37 per cent., the overflow pipes from the cisterns were led direct into the drains or soil-pipes, allowing sewer gas to pass up them, and contaminate the water in the cisterns, and in most cases to pass freely into the house. In 263, or nearly three-fourths of the houses inspected, the waste-pipes from baths and sinks were found to be led direct into the drain or soil-pipes, thus allowing the possibility of sewer gas passing passing up them instead of being led outside the house, and made to discharge over trapped gullies in the open air as they should be. Prof. Huxley moved the adoption of the report, and stated that he had found himself unable longer to act as president of the association, owing to the increasing demands upon his time and energies. He was glad however to say that the Duke of Argyll had consented to succeed him in that post. The second annual meeting of the Sanitary Assurance Association was held at the office, Argyll Place, Regent Street, W., on Thursday. In the absence of Sir Joseph Fayrer, Prof. T. Hayter Lewis, F.R.I.B.A., was elected to preside. The secretary read the report of the council for the year 1882, from which it appeared that the inspection of houses, supervision of work, and issue of certificates had been continued on the plan initiated by the association in 1881. The financial statement showed that considerable progress had been made since the issue of the first report. The increase during 1882 had been nearly double that of 1881.

PART 2 of vol. ii. of "The Encyclopædic Dictionary," published by Messrs. Cassell, extends to the word Destructionist. The present instalment seems quite up to the standard of those already published, though for a work of such extent we think the account of the corona of the sun inadequate. On the other hand, to illustrate the term Darwinism, we have half a column biography of Charles Darwin.

THE last number of the *Izvestia* of the East Siberian Geographical Society, which has just reached us, contains a letter from M. Yurgens, chief of the meteorological station at the mouth of the Lena. When leaving Yakutsk with his companions, Dr. Bunge and M. Eigner, he took with him, besides provisions for eighteen months, a wooden house 42 feet long and 21 feet wide, 40 cwts. of petroleum, two cows with a calf, plenty of hay, bricks, lime, and even moss and clay, as there is no clay in the delta of the Lena. As is unfortunately too often the case with such expeditions, the barometers went out of order, and the observers found great difficulty in filling and boiling them again, so that the new meteorological station at Olekminsk has remained without a barometer. On this subject a correspondent writes: "A new portable barometer would be really an immense benefit for countries like Siberia, but in the meantime would it not be advisable for second-rank meteorological stations to make use of the aneroid? Of course the correction of each aneroid changes slowly but continuously, so that an uncontrolled aneroid has no value at all; but would it not be possible to control it, say every fortnight, by means of a hypsothermometer—a most reliable instrument if the observer follows the advice of Dr. Wild—and, after having boiled the water, leave the thermometer to cool, and make use only of a second reading, which is made when boiling the water for a second time. The observations of Dr. Wild, repeated by M. Kratzkin at the St. Petersburg Physical Observatory on five hypsothermometers taken from an optician's shop, proved that they were most reliable if the above-mentioned precaution were used. Might it not be useful to repeat these observations on hypsothermometers on a larger scale, in order to ascertain the degree of accuracy that might be expected from these instruments, which highly recommend themselves to travellers, and especially for small meteorological stations, by their portability?"

AN avalanche, or rather a landslip, took place at Gudvangsören, in the remote and narrow Nærö valley, in Norway, at the end of January last. The quantities of earth and stone precipitated into the valley destroyed several farms, and killed two women. Landslips have previously occurred in this valley.

IT is remarkable that a disease like leprosy should flourish in Norway. From the returns just published this appears however to be the case, although we are happy to say that the number of afflicted is decreasing. At the end of 1875 there were 2008 patients reported in the country. At the end of 1880 the number had fallen to 1582. The disease is stated to be due to the consumption of food in an unwholesome condition, particularly fish, and also to uncleanliness.

ON February 5, at 6.45 p.m., a meteor of unusual size and appearance was observed near Arvika, in Sweden. An observer who happened at the time to be passing a lake—Glasfjorden—states that he first observed the meteor high on the horizon, going from south-east to north-west, when, after about eighteen seconds, it suddenly changed its course to south-east. During its progress to north-west, calculated at eighteen seconds, the meteor made several digressions from its plane, while its size varied from that of an ordinary star to that of the sun, sometimes emitting a white, at others a yellow light, and at times discharging showers of sparks. At the point of changing its direction, when it was so near the surface of the lake that its path was reflected therein, it possessed a distinct tail, and with this adjunct it passed out of the range of sight in a south-easterly direction, after being observed for nearly fifty seconds.

AT Iserlohn (Rhenish Prussia) the fall of a meteorite was observed by several persons on the evening of February 1. Next morning the meteorite was found, having penetrated deeply into the hard-frozen soil of a neighbouring garden. Its weight is 165 grammes, its size that of a goose's egg. The surface is of a glistening black, and the point seems broken off.

A NEW substance, remarkable for its intense sweetness, being much sweeter than cane-sugar, has been lately found by Dr. Fahlberg in the course of some investigations on coal-tar derivatives (*Journ. Frank Inst.*). He designates it *benzoic sulphinide*, or *anhydrosulphamine benzoic acid*.

MR. H. HEATHCOTE STATHAM will give the first of two lectures, at the Royal Institution, on "Music as a Form of Artistic Expression," on Saturday, March 10. The subject of Prof. Tyndall's discourse on Friday evening, March 16, is "Thoughts on Radiation, Theoretical and Practical."

ON February 11, at 9.50 a.m., an earthquake was noticed at Szigeth (Hungary). It lasted four seconds. It was also felt in the Bosnian village of Looskrupa and its neighbourhood.

An Electro-technical Exhibition will be opened at Königsberg on April 15 next.

The additions to the Zoological Society's Gardens during the past week include two Common Marmosets (*Hapale jacchus*) from Brazil, two Brazilian Caracaras (*Polyborus brasiliensis*) from Uruguay, presented by Mr. Donald F. Mackenzie; a Rook (*Corvus frugilegus*), a Common Magpie (*Pica caudata*), British, presented by Mr. C. L. Sutherland; a Lump Fish (*Cyclopterus lumpus*), British Seas, presented by Mr. W. K. Stanley; a Bonnet Monkey (*Macacus radiatus*) from India, deposited; a Humboldt's Saki (*Pithecia monachus*) from the Amazons, a Squirrel Monkey (*Chrysothrix sciurea*) from Guiana, two Red-vented Bulbuls (*Pycnonotus hæmorrhous*) from India, a Crested Black Eagle (*Lophoaëtus occipitalis*) from West Africa, a Cirl Bunting (*Emberiza cirlus*), British, purchased; a Zebu (*Bos indicus* ♂), five Brown-tailed Gerbilles (*Gerbillus erythrurus*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

Ceraski's Variable Star, U Cephei.—On comparing Dr. Julius Schmidt's observations of this star in 1882, with minima determined by the same observer in the autumn of 1880, there results a period of 2·49289 days, or 2d. 11h. 49m. 46s., on the assumption that it is regular or equable. Dr. Schmidt suspected a marked variation in the period, each successive period being 5·25 seconds longer than the preceding one. The following are the times of minima in March, which will be observable here:—

	h. m.		h. m.		h. m.
March 2 ...	12 43	March 12 ...	12 2	March 22 ...	11 21
7 ...	12 22	17 ...	11 41	27 ...	11 0

The Total Solar Eclipse of 1901, May 17.—The ensuing return of the solar eclipse in May next, for the observation of which this country with France and the United States have despatched observers to the Pacific, will take place on May 17, 1901, when the duration of totality will be even longer than in the present year, and the part of our globe where observations will be most advantageously made will be rather more accessible than in the approaching eclipse. The following are approximate elements of the phenomenon:—

G.M.T. of conjunction in R.A. 1901, May 17, 17h. 28m. 14s.

Right ascension		54 15 36	
Moon's hourly motion in R.A.		39 30	
Sun's ,, ,,		2 29	
Moon's declination		19 1 34	N.
Sun's ,,		19 23 49	N.
Moon's hourly motion in decl.		5 14	N.
Sun's ,, ,,		0 34	N.
Moon's horizontal parallax		60 57	
Sun's ,, ,,		0 9	
Moon's true semi-diameter		16 36·5	
Sun's ,, ,,		15 48·9	

Hence the middle of the general eclipse occurs at 17h. 33m. 25s. G.M.T. The central phase commences in longitude 39° 57′ E., latitude 27° 21′ S; the eclipse is central and total with the sun on the meridian in longitude 97° 0′ E., latitude 2° 7′ S., and the central phase ends in longitude 157° 8′ E., latitude 13° 0′ S. If we calculate directly for a point in 100° 59′ E. and 1° 14′ S., which is close upon the central line and to the west coast of Sumatra, we find—

	h. m. s.	
Beginning of totality, May 18, at	0 22 11	Local mean
Ending ,, ,,	0 28 35	time.

Hence the duration of total phase is 6m. 24s. The sun's altitude is 68°.

The Variable Star, S Virginis.—This object, which varies between 5·7m. and 12·5m., appears to have escaped observation during the last few years. Prof. Schönfeld assigns a period of 374 days, according to which, reckoning from his tabular maximum, the last would have occurred on October 25, 1882, and if the minimum falls about 119 days before maximum, as stated by the Bonn astronomer, one will be due about July 8. This star has an intense reddish-yellow light: its position for 1883 is in R.A. 13h. 26m. 54s., N.P.D. 96° 36′.

There is a suspicion that another star in the vicinity varies through about two magnitudes, 8–10. Its place for 1883 is in R.A. 13h. 24m. 26s., N.P.D. 98° 58′.

The Binary Star, ζ Cancri.—Among several orbits recently calculated for this star by Dr. H. Seeliger, in an interesting memoir communicated to the Academy of Sciences at Vienna, the following is perhaps the most satisfactory:—

Passage of Peri-astron	1870·393
Node	71 32
Node to Peri-astron on orbit	113 52
Inclination	10 53
Eccentricity	0·34327
Mean motion	−5°·8867
Semi-axis major	0″·8515
Period of revolution	61·154

This orbit gives for 1883·0, position 71°·1, distance 0″·83; and for 1885·0, position 61°·4, distance 0″·93.

GEOGRAPHICAL NOTES

As there seems to be some misunderstanding as to the route to be followed by Baron Nordenskjöld in his Greenland Expedition, we may say that we have good reason to believe that there is no intention to proceed along the west coast to Cape York. An attempt will certainly be made to add to our knowledge of the old Danish settlements on the south and south-east coast, but the chief purpose of the expedition is to further explore the east coast, and to penetrate the interior. Baron Nordenskjöld will be accompanied by a complete scientific staff; but we believe he does not intend to divulge the details of his plan till after the expedition sails. He has made a thorough study of all that is known of Greenland; among other things he has published an elaborate investigation of the voyage of the Zeni. A Danish expedition, under Lieut. Holm, will also be sent to Greenland this year; it will be away two years.

The Russian Geographical Society announces the early publication of the following works which it has already received:—A large work, by M. Mayeff, being a statistical and economical description of the Khanate of Bokhara; the report of M. Polakoff on his explorations in Sakhalin, with several maps, including the eastern coast; and a work, by M. Adrianoff, on the antiquities of the Altay and Sayan, with numerous drawings. These works will be published in the *Memoirs* of the Society, but each of them will appear separately, as soon as printed, without awaiting the completion of a volume, as was formerly the case, which caused great delay in the appearance of interesting papers.

At the last meeting of the Caucasian Geographical Society, General Stebnitsky exhibited his new orographical map of Asia Minor and adjacent countries. The map is based on measurements of heights of about 1500 places. Dr. Radde made a further communication on his great work, "Ornis Caucasica," which is the result of his many years' travels in the Caucasus, and of the description of the collection of the Tiflis Museum, which contains no less than 4000 specimens of birds.

M. Polakoff, who was sent by the Academy of Sciences for the exploration of Sakhalin Island and of the coasts of the Pacific, spent last winter and spring at Taranka, in the Gulf of Patience, and has returned to Korsakovo with rich scientific collections. He will now begin the exploration of the coasts of Russian Manchuria. One part of his report has already reached the Geographical Society.

News has been received at the Paris Geographical Society that the French had reached the banks of the Niger, Colonel Desborde having been obliged to cut his way through the Beledegou region. He fought a battle with the chief of Daba, after having crossed a stream called Baoulé. The victory was won by artillery, and the chief of Daba was killed, as well as a large number of his followers.

The Danish Ministry received on February 24 a despatch from their representative in St. Petersburg, to the effect that the Samoyedes sent out to look for the *Dijmphna* and the *Varna*, had returned on January 6 to Liapine in the Obi basin, and reported that "neither had they seen any vessel at sea, nor heard of any shipwrecked crew."

In the last part of the *Bulletin* of the Paris Geographical Society for 1882, Dr. J. Montano describes his excursion into the interior and along the coast of Mindanao; Commander Gallieni gives a detailed narration of his mission to the Upper Niger and Segou; M. Aymonier describes the result of his excursion to Central Cambodia; a paper by the late Dr. Crevaux gives the leading results of his exploration of the Yary, Paron, Içá, and Yapura; and M. Dutreuil de Rhins has a paper on the observations of the transits of Venus.

In the new number (102) of the *Zeitschrift* of the Berlin Geographical Society we have the usual annual systematic list of new works, papers, and maps in all departments of geography published during the past year, a list indispensable to geographers, and which will be found useful by students of the many departments of science related to geography. In the *Verhandlungen* (No. 1, for 1883) Prof. Foerster has a paper on the expeditions for the observation of the recent transit of Venus, and Prof. Brauns a paper on the Island of Yezo. Interesting news from the various German expeditions in Africa will be found in Heft 4 of Band iii. of the *Mittheilungen* of the German African Society, including a detailed account of Dr. Wissmann's journey across the continent, to which we referred last week. There are four letters from Herr Flegel on the progress of his Niger explorations, and several communications of great importance from the party stationed at Gonda, in East Africa, who are accumulating material of great value. They were arranging for a visit to Lake Moero according to the latest intelligence.

In a paper on the Gulf Stream in the *Bulletin* of the American Geographical Society (No. ii. 1882), Commander Bartlett gives some of the results of the examination of that current by the party in the *Blake* in the summer of 1881.

The principal paper in the February number of the *Bollettino* of the Italian Geographical Society is a narrative, with illustrations, by Lieut. Bove, of his mission to South America.

THE OPENING OF THE FINSBURY TECHNICAL COLLEGE

We have already given in our issue of February 1 (p. 318) a brief outline of the curriculum of study to be pursued at the Finsbury Technical College, in our review of the programme of instruction recently published. The new college was opened on Monday, February 19, with an address by Mr. Philip Magnus, the Principal of the College, and Director of the Institute. The address was delivered in the hall of the Cowper Street School, none of the lecture-rooms of the new college being large enough for the purpose. There were present about 1200 persons, chiefly artisans. Sir Frederick Bramwell occupied the chair, and among those on the platform were Sir Sydney Waterlow, Dr. Siemens, Professors Roscoe, Abel, Carey Foster, Adams, Ayrton, Huntington, Armstrong, and Perry, Dr. Gladstone, Mr. H. T. Wood, Mr. J. G. Fitch, Mr. Swire Smith, Mr. Matthey, Mr. Owen Roberts, Mr. John Watney.

Mr. Magnus commenced by indicating some of the incorrect ideas still prevalent on the subject of technical education. He considered that any definition ought to be expressed in very wide terms, so as to be referable to the different kinds of training to which the term technical education applies. He himself proposed to call that education, training, or instruction technical which had a direct reference to the career of the student who received it. Thus considered, technical education was no new thing, except in its reference to careers called into existence by recent developments of science. It was because the system of education to which we had been accustomed was no longer the best preparation for actual work, and not because no relation hitherto existed between the boy's training and the man's career that such colleges were needed. The necessity of technical education he attributed to the invention of the steam-engine and the breaking-up of the apprenticeship system, and the tide which was pushing it forward would not subside until it had influenced the educational institutions of the country from the primary school to the university. The Council had been guided by the desire to supplement, and not to duplicate, existing educational machinery. The college consisted really of a day school for pupils entering between the ages of fourteen and seventeen, and an evening school for apprentices, workmen, &c. The former would give preparatory training to students for practical work in the factory or engineer's shop, and the evening department was intended to help those already at work to understand the principles underlying processes they saw exemplified in their daily work. The college was therefore a technical school of the third grade, and whilst the majority of the pupils would complete within it their instruction, some would proceed to the technical high school or central institution in course of erection at South Kensington. The college might claim to represent a new grade of school. It was not an institution in which any particular trade would be taught, except it were some art industry, nor would it teach the excellence, precision, and rapidity of execution which could only be acquired in the workshop or factory, where, under the severe strain of competition, salable goods were being manufactured. Proceeding to indicate the course of instruction to be given, Mr. Magnus explained that on entering the institution, the student would generally declare whether he wished to be trained as a mechanical engineer, an electrical engineer, or with a view to some branch of chemical industry, or whether he wished to study applied art, and the subjects would be taught with special reference to the career of the student. The teacher would keep steadily in view the purpose to which the student would apply his knowledge. The work would be essentially practical, and more would be done in the laboratory than in the lecture-room, the lectures forming rather a commentary on the practical work than the practical work an illustration of the teaching of the lecture-room. The main purpose was not to turn out scientists, but to explain to those preparing for industrial work the principles that had a direct bearing on their occupation, so that they might be able to trace back the principles they saw to their causes, and thus substitute scientific method for mere rule of thumb. Of the four departments of the College—electrical engineering, mechanical engineering, chemistry, and applied art—that of electrical engineering promised to be the most attractive to students. But there was an intimate connection between the different branches of science not to be lost sight of in the training of a student in any one department. In the course of his remarks on the evening school and the curricula arranged for artisans engaged in various industries, Mr. Magnus referred very pointedly to the narrow view which adult workmen generally take of their own educational requirements. He impressed upon this class of students the necessity of acquainting themselves with branches of industry cognate to their own, and suggested that one of the objects of technical education was to correct the cramping and narrowing influences of extreme division of labour. He referred to a fact told him by a medical friend, that a student refused to dissect the abdominal cavity because, as a surgeon, he intended to occupy himself exclusively with diseases of the eye, and stated that this view of technical instruction needed to be strenuously resisted. He also insisted very strongly upon the importance of artisan students gaining a knowledge of the principles of science, as helping them to deal with unexpected and exceptional cases of difficulty certain to arise in their ordinary work. Mr. Magnus referred at some length to the methods of teaching to be adopted in the college, showing that there was no real opposition, as sometimes stated, between technical instruction, properly understood, and mental culture—that science might be so taught as to yield mental discipline, and yet at the same time have a direct reference to the career or occupation of the student. Mr. Magnus further explained the exact position which the Finsbury Technical College is intended to occupy in the Institute's general scheme of technical education. He illustrated this part of his address by a diagram showing the Bavarian school system, which he said was pronounced by many educational authorities to be the best in Germany, and the technical part of which was in many respects similar to the series of schools which the Institute is engaged in establishing. Mr. Magnus attached great importance to the Central Institution, now being erected in South Kensington, as crowning the educational ladder which pupils from the primary schools should have the opportunity of ascending, and as influencing, in the same way as the Universities at present influence, the entire system of education pursued in the series of schools leading up to them. The speaker did not omit to refer to the Applied Art Department which has recently been added to the College, and in which the instruction he said would be specialised according to the particular occupation of the student. In conclusion Mr. Magnus hoped the college would do much to wipe away the reproach of the neglect of technical education under which the country had hitherto lain compared with other countries. On

the motion of Dr. Siemens, seconded by Prof. Abel, Mr. Magnus was thanked for his address. In seconding a vote of thanks to the Chairman, Alderman Sir Sydney Waterlow said their success was attributable to the generous aid of the Livery Companies, and he appealed to them to render permanent those grants hitherto given at their pleasure.

SCIENTIFIC SERIALS

The Journal of Anatomy and Physiology, vol. xvii. Part 2, January, 1883, contains:—On a method for the estimation of urea in the blood, Part 1, by Dr. J. B. Haycraft.—On the homologies of the long flexor-muscles of the feet of Mammalia, with remarks on the value of their leading modifications in classification, by Dr. G. E. Dobson (Plates 4-6).—On obliterative endarteritis and the inflammatory changes in the coats of the small vessels, by Dr. R. Saundby (Pl. 7).—The presence of a tympanum in the genus Raia, by G. B. Howes (Pl. 8).—The ligamentum teres, by J. B. Sutton (Pl. 8).—Fibrinous coagula in the left ventricle, by Dr. A. M'Aldowie (Pl. 9).—A simple method of demonstrating the nerves of the epiglottis; the trachealis muscle of man and animals; the sulphocyanides of ammonium and potassium as histological reagents, by Dr. Wm. Stirling.—A new theory as to the functions of the semicircular canals, by Dr. P. M'Bride.—Some points on the myology of the common pigeon, by W. A. Haswell, M.A.—The action of saline cathartics, by Dr. M. Hay (Pl. 10).—Some variations in the bones of the human carpus; a first dorsal vertebra with a foramen at the root of the transverse process, by Prof. W. Turner, M.B.—Multiple renal arteries, by Dr. Macalister.—Division of the scaphoid bone of the carpus, with notes on other varieties of the carpal bones, by Dr. R. J. Anderson.

Journal of the Royal Microscopical Society, December, 1882, contains:—On some organisms found in the excrements of the domestic goat and the goose, by Dr. R. L. Maddox (Pl. 7).—On a further improvement in the Groves-Williams ether-freezing microtome, by J. W. Groves.—Summary of current researches relating to zoology and botany (principally Invertebrata and Cryptogamia), microscopy, &c., including original communications from Fellows and others.—The proceedings of the Society.

February, 1883, contains:—Observations on the anatomy of the Oribatidæ, by Dr. A. D. Michel (plates 1 and 2).—On a minute form of parasitical Protophyte, by G. F. Dowdeswell, M.A.—On the use of incandescence lamps, as accessories to the microscope, by H. C. Stearn, with figure—and the usual summary of current researches relating to botany and zoology.

Revue internationale des Sciences, December 15, 1882, contains:—On the Nofoures of New Guinea, by Élie Reclus.—On movements and sensibility in plants (finis), by J. L. de Lanessan.—Reviews.—Notices of learned Societies: the Academy of Sciences, Paris; the Academy of Sciences, Amsterdam.

January 15, 1883, contains:—On the localisation of the cerebral functions in the cerebral hemispheres in man and animals, by Julius Nathan.—On the development of colours in flowers, by H. Müller.—On cell-division or cytodieresis, by L. F. Henneguy.—On the vaginal stopper in rodents, by Dr. Lataste.—On the adulterations in provisions in Paris, by M. Egasse.

Zeitschrift für wissenschaftliche Zoologie, Bd. 37, Heft 4, December 22, 1882, contains:—On the Coelenterata of the South Sea, No. 1.—On *Cyanea annaskala*, nov. sp., by Dr. R. v. Lendenfeld, of Melbourne (Plates 27 to 33; Pl. 27, a coloured representation of the new species).—Contribution to the anatomy, developmental history, and general biology of *Trombidium fuliginosum*, Herm., by H. Henking (Plates 34-36).—On some facts in the life-history of freshwater polyps, and on a new form of *Hydra viridis*, by Wm. Marshall, of Leipsig (Pl. 37).—Supplementary remarks on Dinophilus, by Dr. E. Korschelt.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, February 22.—"On the Effects of Temperature on the Electromotive Force and Resistance of Batteries," by W. H. Preece, F.R.S.

That heat has considerable influence on the condition of galvanic elements is well known, and it has been investigated by De la Rive, Faraday, Daniell, and many others. Some attribute the result to increased chemical affinity, and others to increased conductivity of the liquid, but no one has eliminated the effect on electromotive force from that on internal resistance with the view of expressing each in definite measurement. This the author has done. Special apparatus was made, so as to vary the temperature, and a very careful series of experiments were made upon Daniell, Leclanché, and bichromate of potash cells, measuring the electromotive force and resistance at each change of temperature in rising and falling between 0° and 100° C. The results are tabulated and plotted out as diagrams.

The conclusions are (1) that the E.M.F. is not materially affected by changes of temperature; (2) that the internal resistance is affected very materially according to a fixed law that apparently varies with every cell. A Daniell's cell at 100° C. has only one-third the resistance it has at 0° C. Between 10° and 20° C. it falls one half. Bichromate and Leclanché cells, though much reduced, are not reduced to the same extent; (3) when a liquid is warmed up, its resistance at the same temperature in cooling is greater than when it was being warmed up, and it takes a very long time (fifty hours) to recover its normal condition.

Chemical Society, February 15.—Dr. Gilbert, president, in the chair.—It was announced that a ballot for the election of Fellows would take place at the next meeting (March 1).—The following proposed changes in the list of officers were also announced:—Prof. G. D. Liveing and Dr. A. Voelcker as vice-presidents instead of Professors J. Dewar and A. V. Harcourt; Prof. Dittmar, Dr. W. R. E. Hodgkinson, Messrs. P. D. Howard, and R. Meldola as members of Council instead of Dr. T. E. Thorpe, and Messrs. F. D. Brown, J. M. Thomson, and W. Thorp.—The following papers were read:—On some derivatives of diphenylene ketone oxide, by A. G. Perkin. During the preparation of this substance from salicylic acid and acetic anhydride, a body was noticed which was separated out as transparent, satiny plates containing 75·2 per cent. carbon, and 4 per cent. hydrogen. The author has also investigated the action of nitric acid, of bromine, and of sulphuric acid on the above substance.—On α-ethyl valerolacton, α-ethyl β-methyl valerolacton, and on a remarkable decomposition of β-ethyl aceto-succinic ether, by S. Young.

Anthropological Institute, February 13.—Prof. W. H. Flower, F.R.S., president, in the chair.—Mr. Colquhoun read a paper on the aboriginal and other tribes of Yünnan and the Shan country. Mr. Colquhoun first dwelt upon the races of the South China borderlands. Between Canton and Nan-ning (one of the important towns on the Si-Kiang in Kwang-si), the inhabitants met with were pure Chinese. West of that to the Yünnan frontier, a mixed population on the river and aboriginal tribes in the interior were found. Throughout Yünnan the chief population consisted of Shans disguised under a great variety of tribal names. Lo-lo and Miao-tzŭ aborigines were met with, as well as Thibetans under the name of Kutsung. On the west side of Yünnan Mahomedans are numerous, presumably the remains of the armies of Ginghis Khan. The costumes are most varied and picturesque, and the Shans and all the aboriginal people were kind, frank, and hospitable, and in these respects and in their feet being uncrushed offer a great contrast to the Chinese. Besides the tribes met with, Mr. Colquhoun pointed out that there were in the north and north-west Yünnan, as well as in Ssŭ-chuan, four divisions, namely Li-ssŭ, Moro, Sifan, and Mantzŭ. A great similarity of language exists between the Lo-lo, Li-ssŭ, Sifan, and Burmese. The large area over which the Shan population is distributed was pointed out, and the habitat of the Karens and Lawas. The paper was illustrated by part of a collection of admirable photographs and sketches made during Mr. Colquhoun's late exploration, exhibited by means of the oxhydrogen light. These form a portion of the illustrations which will appear in Mr. Colquhoun's forthcoming account of his late journey.

Geological Society, February 16th, Annual General Meeting.—J. W. Hulke, F.R.S., president, in the Chair.—The Secretaries read the Reports of the Council and of the Library and Museum Committee for the year 1882. The Council expressed their regret that, owing probably to the same causes as last year, they could announce no material advance in the prosperity of the Society, although its financial position was well maintained, the balance at the close of 1882 showing an increase over that of the previous year, notwithstanding a large expenditure upon the *Quarterly Journal*. The total number of Fellows was diminished by one, but there was an increase of nine in the number of contributing

Fellows. The Council stated that Mr. Ormerod had furnished a second Supplement to his Classified Index to the publications of the Society, bringing that work down to the end of 1882. The Council's Report further announced the awards of the various Medals and of the proceeds of the Donation Funds in the gift of the Society.

In presenting the Wollaston Gold Medal to Mr. W. T. Blanford, F.R.S., F.G.S., the President addre sed him as follows: "Mr. Blanford,—The Council has awarded you its highest distinction, the Wollaston Medal, in recognition of your services to geology in Abyssinia, in Persia, and on the Geological Survey of the Indian Empire. They are so well and so generally known that it is not necessary for me to enlarge upon them here. Your writings, which treat of a not inconsiderable portion of the Eastern Hemisphere, comprise, in addition to geology, much information respecting zoology and the climates of the countries in which you served. Stamped with thoroughness and comprehensiveness, they constitute important additions to our knowledge of those regions. In conferring upon you this distinction, the Council of the Geological Society desires to mark its sense of their great value."

The President then handed the balance of the proceeds of the Wollaston Donation Fund to Prof. J. W. Judd, F.R.S., for transmission to Prof. John Milne, F.G.S., of Tokio, Japan, and addressed him as follows: "Prof. Judd,—The Council, in bestowing upon Mr. Milne the balance of the proceeds of the Wollaston Fund, wishes to mark its appreciation of the importance of his investigations into the phenomena of earthquakes, to which he has devoted so much time and attention during his residence in Japan. In handing to you this cheque for transmission to him, I would ask you to convey to him the hopes of the Council that this award may assist him in continuing those inquiries in Seismology which he has proved himself so well able to undertake."

In handing the Murchison Medal to Mr. Warington W. Smyth, F.R.S., for transmission to Prof. Heinrich Robert Göppert, F.M.G.S., of Breslau, the President said: "Mr. Warington Smyth,—The Council of the Geological Society has awarded one of its high distinctions, the Murchison Medal and a part of the proceeds of the Murchison Fund, to Prof. H. R. Göppert of Breslau, one of our Foreign Members, in recognition of his labours in fossil botany. The very large number of papers, 245, recorded in the Scientific List of the Royal Society under Prof. Göppert's name, testifies to the zeal and success with which he has cultivated this branch of biology during half a century. In asking you to transmit to him this Medal, I would desire you to express to him the high estimation in which this Society holds his work."

The President then handed to Prof. Morris, F.G.S., for transmission to Mr. John Young, F.G.S., the balance of the proceeds of the Murchison Donation Fund, and said: "Professor Morris,—The Council of the Geological Society, in awarding to Mr. John Young, of the Hunterian Museum, Glasgow, the balance of the proceeds of the Murchison Donation Fund, wishes to mark its appreciation of the value of his long-continued researches on the fossil polyzoa, especially those of the western part of Scotland, and of his investigations into the structure of the shells of the Carboniferous Brachiopoda. In his absence, I have much pleasure in placing the amount in your hands for transmission to him."

The President next presented the Lyell Medal to Dr. W. B. Carpenter, F.R.S., and addressed him in the following words: "Dr. Carpenter,—The Council of the Geological Society has awarded to you the Lyell Medal with (in compliance with the terms of the bequest) a portion of the proceeds of the Lyell Fund, in recognition of the great value of your investigations into the minute structure of invertebrate fossils and your deep-sea researches. Your contributions 'On the Structure and Affinities of the Eozoon Canadense,' 'On the Microscopic Structure of Nummulina, Orbitolites, and Orbitoides,' published in our *Journal*, your numerous papers on the intimate structure of shells, communicated to the Royal Society, and others published in the 'Annals and Magazine of Natural History,' your long-continued work on Foraminifera, your communications on Oceanic Circulation and on Abyssal Life-forms, all testify to a life-long devotion to branches of natural knowledge bearing on that department of science, the cultivation of which is the *raison d'être* of this Society. I count it a pleasure, Dr. Carpenter, that it has devolved upon me to hand you this Medal."

In presenting one moiety of balance of the Lyell Donation Fund to Mr. P. Herbert Carpenter, the President addressed him as follows: "Mr. P. Herbert Carpenter,—The Council of the Geological Society, in awarding to you a portion of the balance of the proceeds of the Lyell Donation Fund, desires to express its sense of the great value of your researches into the structure and relationship of several families of fossil Echinodermata. Your papers 'On some little-known Jurassic Crinoids,' 'On the Cretaceous Comatulæ,' 'On the Crinoids from the Upper Chalk,' and that read last session, 'On Hybocrinus, Baerocrinus, and Hybocystites,' are models of clearness and an excellent earnest of future work. The Council hopes that this award may aid you in continuing those lines of research in which you have already achieved signal success."

The President then handed the second moiety of the balance of the Lyell Donation Fund to Prof. Seeley, F.R.S., for transmission to M. E. Rigaux of Boulogne, and said: "Professor Seeley,—In conferring upon M. Rigaux a portion of the balance of the proceeds of the Lyell Donation Fund, the Council of the Geological Society desires to signify its estimation of the value it places on his researches in the Jurassic formations of the Boulonnais and their contained fossils. In asking you to transmit to him this cheque, I would desire you to convey to him with it our hopes that he may continue those lines of inquiry in prosecuting which he has attained so great success."

The President finally presented the Bigsby Gold Medal to Dr. Henry Hicks, F.G.S., and addressed him in the following words: "Dr. Hicks,—The Council, in conferring on you the Bigsby Medal as a mark of their appreciation of your labours amongst the oldest fossiliferous and the Archæan rocks of Great Britain and Ireland, feels, in your community of interests, a peculiar fitness in associating you with the memory of the founder of this distinction. Your numerous communications, beginning with one 'On the genus *Anopolenus*,' written in 1865, and culminating in that which you read at our last meeting, show to what good purpose you have employed the *horæ subsecivæ* of a busy professional life in prosecuting those researches which have had a distinct effect on geological thought. In handing to you this Medal, I would express the wish that you will continue to prosecute the line of inquiry to which you have so long and so successfully devoted your leisure hours."

The President then read his Anniversary Address, in which he passed in review the work done by the Geological Society during the past year, and discussed at considerable length a question arising out of this review, namely, the structural characters presented by the sternal framework and the limbs of Enaliosaurians, and the classificational value which they possess. He also referred to the discoveries which have been lately made in America of numerous remains of Pterosaurians, often of gigantic size; adverted to the proceedings of the International Geological Congress, held in 1881, at Bologna, and noticed, as one gratifying result of the latter, the establishment of an Italian Geological Society.

The ballot for the Council and Officers was taken, and the following were duly elected for the ensuing year:—President: J. W. Hulke, F.R.S. Vice-Presidents: Prof. P. M. Duncan, F.R.S.; R. Etheridge, F.R.S.; J. Gwyn Jeffreys, F.R.S.; Prof. J. Prestwich, F.R.S. Secretaries: Prof. T. G. Bonney, F.R.S.; Prof. J. W. Judd, F.R.S. Foreign Secretary: Warington W. Smyth, F.R.S. Treasurer: Prof. T. Wiltshire, F.L.S. Council: H. Bauerman; W. T. Blandford, F.R.S.; Prof. T. G. Bonney, F.R.S.; W. Carruthers, F.R.S.; Prof. P. M. Duncan, F.R.S.; R. Etheridge, F.R.S.; John Evans, F.R.S.; A. Geikie, F.R.S.; Rev. Edwin Hill, M.A.; G. J. Hinde, Ph.D.; Prof. T. M'Kenny Hughes, M.A.; J. W. Hulke, F.R.S.; J. Gwyn Jeffreys, F.R.S.; Prof. T. Rupert Jones, F.R.S.; Prof. J. W. Judd, F.R.S.; S. R. Pattison; J. A. Phillips, F.R.S.; Prof. J. Prestwich, F.R.S.; F. W. Rudler; Prof. H. G. Seeley, F.R.S.; Warington W. Smyth, F.R.S.; W. Topley; Prof. T. Wiltshire, F.L.S.

Physical Society, February 10.—Prof. Fuller in the chair. Annual general meeting.—New officers elected for the year:—President: Prof. R. B. Clifton, M.A., F.R.S. Vice-presidents: Sir W. Thomson, Prof. G. C. Foster, F.R.S., Dr. J. Hopkinson, F.R.S., Lord Rayleigh, F.R.S., Prof. W. C. Roberts, F.R.S. Secretaries: Prof. A. W. Reinold, M.A., Mr. Walter Baily, M.A. Treasurer: Dr. E. Atkinson. Demonstrator: Prof. F. Guthrie, F.R.S. Other Members of Council: Prof. W. G. Adams, M.A., F.R.S., Prof. W. E. Ayrton, F.R.S., Mr. Shellford Bidwell, M.A., LL.B., Mr. W. H. M. Christie, M.A.,

F.R.S., Prof. F. Fuller, M.A., Mr. R. T. Glazebrook, M.A., F.R.S., Mr. R. J. Lecky, F.R.A.S., Prof. O. J. Lodge, D.Sc., Mr. Hugo Müller, Ph.D., F.R.S., Prof. J. Perry. New Member: Prof. Blyth of Anderson College, Glasgow.—Prof. Sylvanus P. Thomson explained his new graphical method of showing Jacobi's law of maximum rate of working, and Siemens's law of efficiency for dynamo-electric machines. This has been fully explained in the *Philosophical Magazine* and in the Cantor lectures on Dynamo electric Machinery, delivered by Prof. Thomson. Prof. W. G. Adams pointed out the advantages of a graphic system of the kind.

The Institution of Civil Engineers.—February 20, Mr. Brunlees, president, in the chair. The paper read was on "Covered Service-Reservoirs," by Mr. William Morris, M. Inst. C.E. (of Deptford).

EDINBURGH

Royal Society, February 5.—Prof. Jenkin, F.R.S., vice-president, in the chair.—Emeritus Professor Blackie, in a paper on scientific method in the study of language, maintained that the true way to learn a foreign language was to learn it in the way a child learns its native language—conversationally; and that this method should be adopted for the teaching of the dead languages as well as for modern ones. Simple sentences expressing facts with which the pupil is in direct contact, the grammatical rule for construction being given after the construction is practically mastered by repetition, should lead by insensible gradations to more complicated sentences and ideas. The paper finished with some characteristic remarks about quantity and accent in Latin and Greek, which called forth criticism from Prof. Butcher and Mr. Marshall, Rector of the High School.—Prof. Tait, in a short note on the mirage problem, mentioned that he had come across a paper in Gorgonne's *Annalen* criticising Biot's great paper upon the subject. Thinking that possibly he might have been forestalled in some of his theorems, he had looked into the paper, the author of which, however, in attacking Biot, had given a construction which, if applied to the case of ordinary desert-mirage, would give a direct instead of an inverted image. Mr. Sang, in his criticisms on the paper, maintained that such mirage as was said to have been observed by Vince was impossible.

PARIS

Academy of Sciences, February 19.—M. Blanchard in the chair.—The following papers were read:—Observations of small planets, made with the large meridian instrument of the Paris Observatory, during the fourth quarter of 1882, by M. Mouchez.—Results of experiments made in the workshops of the Chemin de fer du Nord, on M. Deprez's electric transport of work to a great distance, by M. Tresca (See p. 399).—Note on the theorem of Legendre cited in a note inserted in *Comptes rendus*, by Prof. Sylvester.—Report on a memoir of M. Rosenstiehl, entitled "Researches on the Colouring-matters of Madder," by M. Wurtz. *Inter alia*, M. Rosenstiehl has found a new mode of formation of purpurine (decomposition of pseudopurpurine by heating with alcohol at 40°), and his discovery of the composition of pseudopurpurine (which is really a trioxycarboxyl-anthraquinone) throws much light on several facts that were obscure. Madder contains only three glucosides, giving respectively pseudopurpurine, carboxyl-alizaric acid, and munjistine, or carboxyl-xanthopurpuric acid.—M. Hospitalier presented a note on the influence of the mode of coupling of dynamo-electric machines in experiments on transport of force to a distance.—Observations of the new planet (232) Palisa, made at the Paris Observatory, by M. Bigourdan.—Observations of the great comet *b* 1882, made with the Brunner equatorial of Toulouse Observatory, by M. Baillaud.—On a curious modification of the nucleus of the great comet, by M. de Oliveira Lacaille. On the evening of January 8 the nucleus was seen to be much elongated and subdivided into four small nebulosities in a line, with centres like stars of the 12th magnitude. At 9.30 a.m. next day there was a change in the relative position: the first nebulosity being more separated, and the second having taken its place, &c.—On the observation of the transit of Venus of 1882 at the Lick Observatory on Mount Hamilton, California, by Mr. Todd. He got 147 photographs, of which 125 are well fitted for micrometric measurement.—On the uniform functions of a variable connected by an algebraic relation, by M. Picard.—On the relations between co-variants, &c. (continued), by M. Perrin.—On the functions of several imaginary variables (continued), by M. Combescure.—On a question of divisibility, by M. de Polignac.—On the equilibrium of the elastic cylinder, by M. Schiff.—On crystals observed in the interior of a bar of cemented Swedish iron, by M. Stoltzer. These crystals of steel are not regular octahedra like those of pig-iron and iron.—On the immediate analysis of pozzuolanas, and on a rapid process for testing their hydraulic properties, by M. Landrin. The rapid process is attack with hydrochloric acid, and trial of the insolubles with lime-water. There is no possible comparison between the action of pozzuolanas and of their insolubles on lime-water.—On sulphocyanopropiophine, by MM. Tcherniac and Norton.—On allotropic arsenic, by M. Engel. When arsenic is isolated by the wet or dry way under about 360°, it is amorphous, dark grey, brown, or black, and unalterable in moist air; and its density is between 4·6 and 4·7. Heated to 360°, it changes into arsenic with a density of 5·7,—the steel-grey arsenic of laboratories, which crystallises when formed from condensation of arsenic vapour about 360° or more.—On benzoyl-mesitylene, by M. Louise.—Researches on mesitylene, by M. Robinet.—Toxical power of quinine and of cinchonine, by M. Bochefontaine. The former has more active physiological properties than the latter. Both are convulsive, cinchonine more than quinine, and quinine is distinguished by its vomitive effects and depressing action on the central nervous system.—On the value of intercrossing of the movements of cerebral origin, by M. Couty. This intercrossing is not constant, and has not the value that has been attributed to it.—Vision of ultra-violet radiations, by M. de Chardonnet. The spectrum of the crystalline lens corresponds exactly to that of the visible spectrum. From observations of persons with the lens removed, the author concludes that the retina is sensitive to ultra-violet radiations that come to it (as well as visible radiations), at least to about the line S. Thus the crystalline lens alone limits the visible spectrum. The absorption of the long ultra-solar spectrum of the electric arc probably fatigues the eye.—Researches on the production of monstrosities by shocks imparted to hens' eggs, by M. Dareste. He produced tremors, and so monstrosities, by means of a beating apparatus used by chocolate-makers.—On the generation of cells of renewal of the epidermis and of epithelial products, by M. Retterer.—On M. Merejkowski's Suctociliates (second note), by M. Maupas.—On the structure of simple subterranean branches of adult Psilotum, by M. Bertrand.—On the conservation of solar energy, by M. Duponchel. He infers from peculiar circumstances of our epoch that the sun-spot period which has varied in the neighbourhood of ten years for 130 years, will be extended to fourteen for the present and the two following periods. The first maximum will be in 1885.—Imitation of diffraction-spectra by dispersion, by M. Zenger.—The second part of M. Grüner's geological description of the coal basin of the Loire was presented (with analysis) by M. Daubrée.

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THURSDAY, MARCH 8, 1883

THE ORIGIN OF CULTIVATED PLANTS

Origine des Plantes cultivées. Par Alph. de Candolle (Paris: Germer, Baillière et Cie., 1883.)

Les Plantes potagères, Description et Culture des principaux Légumes des Climats tempérés. Par Vilmorin-Andrieux et Cie. (Paris: Vilmorin-Andrieux et Cie., 1883.)

ALPHONSE DE CANDOLLE occupies a position in the botanical world which in its way is unique. He is in a manner the *doyen* amongst the heads of the botanical establishments of different countries which have for their especial object the study of the earth's vegetation in its taxonomic aspect. There is a special appropriateness in his being so; the Geneva botanical school, though in filiation related to the French, has always seemed to belong more to Europe than to Switzerland. The effect of this circumstance has doubtless operated indirectly on a mind naturally inclined to wide and general views. Accordingly as the invaluable "Prodromus"—the only modern work which has attempted to describe all known species of flowering plants—drew near the point at which it was decided to conclude it after occupying two generations of botanists, we find De Candolle himself more and more engaged with works dealing with general questions—works which both temperament and point of view peculiarly fitted him to undertake. Such were his "Histoire des Sciences et des Savants depuis deux Siècles," published in 1873, and his "Phytographie; ou, L'Art de décrire les Végétaux," published more recently (1880).

Long ago however, in 1855, he had published his classical "Géographie botanique raisonnée," and in this he had stated the theory, sufficiently novel then, though now a commonplace, that the present distribution of the earth's flora cannot be accounted for by any possibility as the result of the existing configuration of its surface, but is the gradual result of long antecedent geological changes. De Candolle's conclusions are now seen to form a particular case in the general theory of evolution. But we must not forget that they were the independent result of a long and laborious induction.

The study of geographical distribution requires the elimination from the facts of all disturbing elements. It is necessary to ascertain the precise nature of the flora of any given district undisturbed by artificial modifications. The slow action of natural forces is one thing: the changes brought about by man are another. De Candolle was therefore obliged to devote no small attention to the question of introduced and cultivated plants. They must clearly be eliminated from the enumeration of feral productions. But the question then arises, What is to be done with them, and to the flora of what country are they to be relegated? The result is not merely one of disembarrassment to the botanist; it has its interest no less for anthropology in the widest sense. Different races have taken advantage of plants susceptible of cultivation in the places where nature had originally planted them; and as these races have migrated they have taken their cultures with them. If the botanist then does his work properly in tracking them back to their original cradle, he is tracking back the migratory race as well, and doing the work of the anthropologist. Plants often take their old names with them, and these may and frequently have persisted when the race that brought them has passed away, been dispersed, or changed its language. All the various names, for example, given to hemp by the descendants of the Aryan race go back to the same root.

These considerations will be sufficient to establish the utility of the study which De Candolle has had in hand for some thirty years, and with regard to which he has now given us, in a singularly succinct form, probably as much as we are ever likely to know. Hitherto we have been badly off for a handy synopsis of the subject. It is true we have the chapter in De Candolle's work already referred to, and Mr. Darwin brought together a considerable body of information in his "Variation of Animals and Plants under Domestication." The former book has, however, long been out of print, and Mr. Darwin's purpose only led him to deal with those species which have largely varied under cultivation. For my own part I have generally used for reference the two admirable articles in the ninth volume of the *Journal* of the Horticultural Society—a body which unhappily, while taking the title of Royal seems to have lost its taste for such studies. These articles—in form a review of a little work by Targioni-Tozzetti, of which I have never seen but a single copy—are really an extremely useful examination of the whole subject; and as it is an open secret that they are from the pen of Mr. Bentham, the critical opinions they contain as to the origin of all our more important cultivated plants may be relied upon with considerable confidence.

"An English vegetable garden," says Mr. Tylor, "is a curious study for the botanist, who assigns to each plant its proper home; and to the philologist, who traces its name." But De Candolle, not confining himself to our temperate pot-herbs, has included in his studies the cultivated plants of all countries. Accurate knowledge in this matter is a thing of comparatively recent growth. Linnæus bestowed no pains upon it. Humboldt in 1807 dismissed it as an impenetrable secret. De Candolle has now discussed no less than 247 species. It is curious—perhaps significant—to note that 199 of these trace back to the Old World; only 45 are American, and 3 doubtful. Neither the tropical nor the southern regions of either hemisphere have any of these species in common. The northern have five which are so, but it goes with the rest of the facts that the domestication of these belongs to the Old World, and to this De Candolle has accordingly credited them. Some things no doubt have escaped him, although the list is remarkably complete. Perhaps the most curious omission is rhubarb, the use of which for the table seems pretty much confined to England and Holland.

It is rather to be regretted that De Candolle has abandoned the attempt to indicate the points on the earth's surface from which the maximum number of cultivated plants appear to have sprung. He contents himself with saying that the original distribution of the stocks of cultivated plants is most irregular. "It had no relation with the needs of man supplied nor with the area of origin." I have a decided suspicion that the facts might be made to yield a different result. There does not seem any *a priori* reason why plants susceptible of useful develop-

ment under cultivation should be so arbitrarily distributed. The number of species domesticated in a given area would, other things being equal, seem to be related to the intelligence of the races working on them. North America has only given us the vegetable marrow and the Jerusalem artichoke; and neither deserve more than a *succès d'estime*. But our best domesticated plants have developed their merits *pari passu* with the races that educated them. If we stumbled *now* against the primitive stocks they might seem as little susceptible of development as the plants of the United States, whose capabilities we rank so low. But had the Old World races been but early enough on the New World soil to work out their progress to civilisation, possibly the balance in the proportion of domesticated plants would have been redressed. If the gardens of the United States are filled with Old World vegetables, the houses are inhabited by an Old World stock. The two things seem to me to go together; the indigenous races could neither develop their latent vegetables nor hold their own against an Old World human invasion.

The circumstances of domestication, however, impose certain conditions which the flora drawn upon must fulfil The early stages of civilisation were probably unsuited to any fixity of abode. Tylor, it is true, remarks that "even very rude people mostly plant a little." But they will plant only what will give a quick return, and the qualities of foresight as well as a permanent social structure must be developed before men would have the disposition to plant fruit trees, which perhaps only their descendants would gather from. The first domesticated plants must have been those that were in themselves succulent, or would in the course of a single season yield some desired product. We find then that out of the 44 species, the cultivation of which in the Old World goes back to the dawn of civilisation, half are annuals; and these are just what the great temperate flora of the northern hemisphere would supply. On the other hand, Patagonia and South Africa have not yielded a single domesticated plant. Australia only contributes the overrated *Eucalyptus globulus*, and New Zealand a wretched spinach (*Tetragonia*). But then, as De Candolle remarks, their floras are destitute of the types of *Gramineæ*, *Leguminosæ*, and *Cruciferæ*, which were available in the northern hemisphere, and predominate in the list of the 44 most anciently cultivated plants. As between the north and the south I think this argument is valid. But as between the east and the west in the north hemisphere, since the main features of the flora are radically the same, any similar explanation does not hold.

With regard to such of these primitive cultures as belong to the temperate regions of the Old World, it will be interesting to give De Candolle's conclusions. The turnip and rapeseed (not however sustainable as distinct species) originated in Northern Europe. The cabbage was derived from the western coasts of Europe, where its wild stock may still be found; it was first gathered and then cultivated by pre-Aryan races. Purslane is wild from the Western Himalayas to Greece. The onion was brought from Western Asia. As to textiles, the origin of flax is somewhat complicated. The inhabitants of the Swiss lake-dwellings of the Stone Age did not use our present annual flax but a subperennial sort indigenous to Southern Europe (*Linum angustifolium*). This was displaced by *Linum usitatissimum*, a native of countries south of the Caspian, which was introduced into Europe and India by Aryan races. The knowledge of hemp seems to have been brought into Europe by the Scythians about 1500 B.C.; there is no trace of it in the Swiss lake-dwellings. The vine is indigenous in Western Asia, whence its use was carried to various countries by both Aryan and Semitic races; but it did not reach China before 122 B.C.

The almond, although so characteristic of Mediterranean countries, seems to be a native of Western Asia, and perhaps Greece. As late as the time of Pliny the fruits were known to the Romans as *Nuces græcæ*. The wild stocks of our pears and apples seem to have been indigenous to Southern Europe and Western Asia before the Aryan invasion; their remains abound in the Swiss lake-dwellings. The quince is a native of North Persia, but seems to have been introduced into Eastern Europe in pre-Hellenic times. Remains of a form of the pomegranate have been found in strata of the Pleiocene age in Southern France by Saporta; but it died out and was reintroduced from countries adjoining Persia in prehistoric times into the Mediterranean region of which it is now so characteristic a feature. The primitive home of the olive was apparently the eastern shores of the Mediterranean, where the Greeks discovered its useful qualities the Romans learning them later. The fig has left its remains in quaternary rocks in France along with the teeth of *Elephas primigenius*, but its prehistoric home must be sought in the Southern Mediterranean shores and lands, where it survived after probably perishing in France. The common bean (*Faba vulgaris*) seems to have become extinct in a wild state; it may have originated south of the Caspian, and was introduced into Europe by the Aryans. The remains of lentils have been found in lake-dwellings of the Bronze Age, and it was probably indigenous in Western Asia, Greece, and Italy before its cultivation in these countries; subsequently it was introduced into Egypt. The chick-pea was carried from the south of the Caucasus by the Aryans to India and Europe. The carob is indigenous to the Eastern Mediterranean, whence the Greeks introduced it into Italy and the Arabs into Western Europe. De Candolle regards all the various kinds of wheat as derivatives of the small-grained kind found in the most ancient lake-dwellings of Western Switzerland. He inclines to the belief that the wild stock of this originated in Mesopotamia, where it may still exist. The origin of spelt is very doubtful, and it may possibly be an ancient cultivated derivative from the wheat stock. As to barley, the inhabitants of the Swiss lake-dwellings cultivated both the two-rowed and the six-rowed kinds. The former is found spontaneously in the area between the Red Sea and the Caspian; but nothing is known of the spontaneous occurrence of the latter or of the four-rowed kind. Either then both were derivatives in prehistoric times of the two-rowed variety, or they are the cultivated representatives of species which have since become extinct. As to rye, probability points to an origin in South-Eastern Europe. The lake-dwellers even of the age of Bronze did not know it, but Pliny mentions its cultivation near Turin. De Candolle supposes that the Aryan migrations

westward met with it in Europe and carried it onward. Oats seem also to have originated in Eastern Europe; they are found not earlier than the Bronze Age in Switzerland. From Pliny's mention that the Germans used oatmeal, it is concluded that it was not cultivated by the Romans.

Space will not allow of my giving an idea of the method by which these results are arrived at. But they seem to me to take advantage of every line of evidence and to be as near the truth as we are at present likely to approach. De Candolle sums up with great pains the philological evidence which he has collected from the best quarters, and though, as he is prepared to admit, a professed philologist might handle the evidence in a different way, he claims that the inferences he draws are such as are fairly within the competence of instructed common sense. And controlled as they are by other lines of inquiry they do not seem to me to be pushed to a point where, except in the hands of an expert, they would be likely to prove treacherous. It is obvious that the philological evidence alone might make the most careful go astray. The two instances given by Tylor are, in a way, a case in point. "Sometimes," he remarks, "this (the name) tells its story fairly, as where damson and peach describe these fruits as brought from Damascus and Persia." This is true perhaps as far as it goes. The cultivated plums of Damascus had a reputation in the time of Pliny; but the wild stock does not extend to the Lebanon, and its home was probably far to the north in Anatolia and Northern Persia. As to the peach, De Candolle points out that its having no Sanscrit or Hebrew name is against an origin in Western Asia, and he gives a considerable body of evidence pointing to China as its true native country.

It is in fact the indirect evidence given by such names through their origin and history which is of use, not the actual information they imply. The Jerusalem artichoke is a well-known instance. As De Candolle says, it is not an artichoke, and being a North American plant can have nothing to do with the Holy Land. The plant is technically a sun-flower (*Helianthus*), though in our climate it rarely betrays its affinity by flowering. And the ordinary explanation is that Jerusalem is a corruption of *Girasole*. But this seems to be a wanton piece of euhemerism; there is no evidence that the Italians ever used such a name for it, and the real explanation seems to be that Jerusalem was applied in a vague way, like Indian or Welsh, simply to indicate a foreign origin. Thus an old-fashioned garden plant (*Phlomis fruticosa*) is called sage of Jerusalem with about as little reason. And in France the term Jerusalem artichoke is applied to a species of gourd.

The second work which I have cited at the head of this article deserves a more extended notice than it is possible to give it. It is safe to say that only in France could such a book be produced, either as regards the share of authors or publishers. It is a sort of complement to De Candolle's treatise, describing, from the cultural point of view (but with botanical references apparently carefully accurate), the various esculent vegetables which are known, with all their typical varieties, including even such sorts as are rarely seen in the gardens of cool countries. It is a book which any botanist will find a useful addition to his library, if only for the delicate illustrations, which contrast so strikingly with the coarse ostentation of the ordinary trade catalogue.

W. T. T. D.

OUR BOOK SHELF

Useful Rules and Tables relating to Mensuration, Engineering, Structures, and Materials. By William John Macquorn Rankine, C.E., F.R.S., &c. Sixth Edition, thoroughly Revised by W. J. Millar, C.E. With Appendix, Tables, Tests, and Formulæ for the use of Electrical Engineers, by Andrew Jamieson, C.E., F.R.S.E. (London: Charles Griffin and Co., 1883.)

We learn from the title-page of this edition of Rankine's well-known book of Rules and Tables that it has been thoroughly revised, but we are sorry to find so little evidence of this in the work itself. Many of the rules given in this book are only applicable in particular cases, and generally no explanation as to this is given. Take for example Rule XXV., p. 211, which gives the collapsing pressure of tubes as $\frac{9672000 \times \text{thickness}^3}{\text{length} \times \text{diameter}}$.

This rule is evidently based on Fairbairn's experiments, which were made on tubes with closed ends and of lengths in no case exceeding ten times the diameter. The rule as it stands is simply absurd, for it gives zero collapsing pressure for infinite length. The addendum to Part II. p. 367, referring to springs, may be taken as another example. Straight springs cannot be treated by the formulæ given for beams unless they are only slightly bent, and again, the ratio of the force to the elongation produced by it in a spiral spring is that given by the formula only when the spires have small inclination to a plane at right angles to the axis. This formula is not even accurate, as the r^3 in the denominator should be r^2. Some estimate of the care bestowed on the work by the editor may be gathered from the following slovenly sentences taken from p. 374:—"From experiments by Major Morant, R.E., India, it appears that only one-half the quantity of dynamite and one-third the number of bore-holes is required to remove the same quantity of rock as gunpowder." "The area of the fire-grate being about 14½ square feet."

The Appendix on Tables, Tests, and Formulæ, for the use of electrical engineers, is a real addition to the book. Beginning with a table of the "Formulæ of the Absolute Units," Mr. Jamieson goes on to the definition of the different practical units now in use; and then gives a large amount of useful information in the form of tables and rules for making electrical tests. Much of the information here given is published for the first time, and on this account will be the more valuable to engineers.

Cable work has received special attention, and although not fully treated, absorbs, we think, more than its due share of the sixty-four pages devoted to the appendix. The space taken up by some of the less useful tables might, it seems to us, have been saved, and a fuller treatment of the different methods of testing electromotive force, battery- and other resistances, &c., given. Mr. Jamieson gives only a few rules for such tests, and refers to other books for more, but we think that a book of rules should be full if it be anything. In order to measure the resistance of thick wire, Mr. Jamieson suggests that a piece of it may be drawn down to a fine gauge, and then tested in the Wheatstone bridge. This is neither convenient nor satisfactory, and should be replaced by one of the well-known methods of testing such wires. Again on page 361, Mr. Jamieson proposes to measure the work done in charging a secondary battery by joining up a suitable voltameter as a shunt to it. This method is worse than useless.

Although we should like to see an absence of such faults as the above, and considerably more space devoted

to the subject of general laboratory and engineering work, Mr. Jamieson has made good use of the space at his disposal, and we have much pleasure in recommending his appendix as likely to prove exceedingly useful to electricians.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Mr. Stevenson's Observations on the Increase of the Velocity of the Wind with the Altitude

A HEAVY pressure of professional work has prevented me till now from noticing Mr. Archibald's remarks on my paper on simultaneous observations of the wind at different elevations.

I fear I have not been sufficiently explicit as to the object of my observations. All of them have reference strictly to the retarding influence of the friction caused by the earth's surface, and are not so much of a meteorologic as an engineering character. This I thought would have been understood from my statement that I believed they were approximately correct "*for practical purposes.*" The formulæ are intended to be applicable only to heights within the limits of my observations, and the idea of applying them to the higher regions of the atmosphere, such as from 3800 to 23,000 feet above the earth, never for one moment crossed my mind, and I think the following facts observed by Mr. Glaisher in his balloon ascents prove the futility of attempting to deduce from experiments made near the surface of the earth what the velocity of the wind may be at such great elevations.

"In almost all the ascents the balloon was under the influence of currents of air in different directions." "The direction of the wind on the earth was sometimes that of the whole mass of air up to 20,000 feet, whilst at other times the direction changed within 500 feet of the earth. Sometimes directly opposite currents were met with at different heights in the same ascent, and three or four streams of air were encountered moving in different directions."

"On January 12, 1862, the balloon left Woolwich at 2h. 8m. p.m., and descended at Lakenheath, seventy miles distant, at 4h. 19m. p.m. At Greenwich Observatory, by Robinson's anemometer, during this time the motion of air was six miles only."

On June 26, 1863, "at 9000 feet the sighing and moaning of the wind were heard, and Mr. Glaisher satisfied himself that this was due, not to the cordage of the balloon, but to opposing currents." On the descent, "a fall of rain was passed through, and then *below* it a snow-storm, the flakes being entirely composed of spiculæ of ice and innumerable snow crystals."

On July, 1862, the temperature of the air at starting was 59° Fahr., at 4000, 45°, at 10,000, 26°, at 13,000, 26°, at 15,500, 31°, at 19,500, 42°, at 26,000, 16°. On descending, it was found to be 37°·8 at 10,000, while on the ascent at the same height, it was only 26°.

Mr. Buchan states (article "Atmosphere," "Ency. Britt.," 9th edition): "observations of the winds cannot be conducted, and the results discussed, on the supposition that the general movement of the winds felt on the earth's surface is horizontal, it being evident that the circulation of the atmosphere is affected largely through systems of ascending and descending currents." The observations in the higher regions of the atmosphere quoted by Mr. Archibald confirms this irregularity of the atmospheric currents; as, for example, the velocity at an elevation of 1600 feet is greater than at an elevation of 7200 feet, showing that no satisfactory results can be deduced from them.

My observations on the 50-feet pole are only applicable to "small heights" above the ground, and they have proved the absolute necessity of all anemometers being placed at one uniform height above the ground, and are mainly useful in enabling us to reduce anemometric observations obtained by instruments at different heights to the same standard level—a matter which, as a meteorologist, I deem of great importance.

I believe that the formula for small heights will be useful, because I consider it applicable to such engineering works, for example, as the Tay and Forth Bridges.

As regards the other observations with pressure anemometers at comparatively great heights, the highest observed being 1600 feet at the Pentland Hills, the simple formulæ which I proposed were made only to cover the observations which I actually obtained, and they do agree nearly with these results. As to the assertion that I supposed that the force of the wind ought to vary as its velocity, the contrary is the fact, as Mr. Archibald might have seen by my statements that the only hypothesis on which I could account for the paradoxical result of the same formula being *practically* applicable both to force and velocity was the decreased density of the air as we ascend. I

Observations on Velocity at Arthur's Seat

Velocity recorded at high elevation.	Velocities computed for lower station			Velocity recorded at low elevation.
	By Mr. Stevenson's 1st formula.	By Mr. Stevenson's 2nd formula.	By Mr. Archibald's formula.	
775 feet above sea-level.	$v = V\sqrt{\frac{h}{H}}$	$v = \frac{V h}{H}$	$v = V\sqrt[4]{\frac{h}{H}}$	550 feet above sea-level.
885	703	592	766	720
1,698	1,430	1,205	1,558	1,364
2,620	2,206	1,859	2,405	2,133
3,416	2,876	2,424	3,132	2,718
4,328	3,646	3,071	3,973	3,465
5,575	4,697	3,957	5,117	4,592
6,763	5,698	4,800	6,208	5,640
8,035	6,765	5,702	7,376	6,782
9,368	7,893	6,648	8,600	7,862
10,820	9,115	7,679	9,933	8,765
12,410	10,455	8,807	11,392	9,789
13,700	11,542	9,722	12,576	10,639
15,058	12,687	10,686	13,833	11,680
÷ 13	79,713	67,152	86,869	76,149
Mean results	6,132	5,165	6,682	5,857

Height of stations above sea-level in feet.	Observations		Calculations for lower station		
	At higher station.	At lower station.	By Mr. Stevenson's 1st formula. $f = F\sqrt{\frac{h}{H}}$	By Mr. Stevenson's 2nd formula. $f = \frac{F h}{H}$	By Mr. Archibald's formula. $f = F\sqrt[4]{\frac{h}{H}}$
1617	5·656	—	—	—	—
1500	—	5·250	5·424	5·246	5·491
1500	8·542	—	—	—	—
915	—	5·000	6·671	5·208	7·516
438	5·077	—	—	—	—
371	—	4·259	4·675	4·301	4·873
371	4·577	—	—	—	—
276	—	4·181	3·945	3·405	4·247
Average for lower stations		4·672	5·179	4·540	5·532

am not prepared to admit that the velocity at 100 feet above the sea will, as Mr. Archibald supposes, be much greater than at sea-level, for my simultaneous observations of wind passing over the sea, over sand and over grass (*Min. Cir. Eng.*) render it doubtful. If for example a wind passes over the surface of the sea with a given velocity, which will depend to a certain extent on the comparatively small amount of friction due to passing over water, that velocity will be at once reduced when the current meets the shore and begins to pass over the more retarding surface of solid land. At a height of 100 feet above sea-level, it may not therefore have attained the initial velocity which it had at sea. But as to the whole subject, which

is one of great difficulty, I will only repeat what I formerly said, that "additional observations are much wanted at high levels," and I might have added, at small elevations also.

I have tabulated as above the results given by my two formulæ and by that of Mr. Archibald, from which it appears that my first formula, viz. $v = V \sqrt{\frac{h}{H}}$, agrees more nearly with the recorded results of velocity at Arthur's Seat than any of the others, while my second formula, $f = \frac{Fh}{H}$, agrees best with my first observations of pressure.

Edinburgh, February 17 THOMAS STEVENSON

The Supposed Coral-eating Habits of Holothurians

IN glancing through my back numbers of NATURE my attention has been drawn to a letter on the above subject by Mr. H. B. Guppy, published in the issue dated November 2, 1882. Quoting the late Mr. Charles Darwin's famous work on "Coral Reefs," where it is stated at p. 14, on the authority of Dr. J. Allan, that Holothuriæ subsist on living corals, he recounts the results of his investigations made on the reefs of Santa Anna and Cristoral, with the object of putting such statement to the test. As the upshot of his experiences he writes that he has by no means satisfied himself that Holothuriæ do subsist on living coral. In no instance did he meet with a single individual browsing on the patches of living zoophytes, the two species observed being indeed found living only in the plots of detritus or dead coral matter that flanked the growing masses. Mr. Guppy gives an approximate estimate of the amount of coral sand daily voided by an individual Holothurian, but adduces no evidence as to the manner in which such hard matter is taken into its body. This phenomenon indeed he apparently did not witness; nor, so far as I am able to ascertain, has any other investigator brought forward any positive testimony in this direction.

Through my cultivation of Holothuriæ in company with various other Echinoderms a few years since in the tanks of the Manchester Aquarium, and also more recently in the Channel Islands, I find myself in a position to supply this hiatus in our knowledge of their life economy. The two species that were more particularly the subject of my observations included the large, dark purple *Cucumaris communis*, derived from the Cornish coast, attaining, in its fully extended condition, a length of from eight to twelve inches, and the white or dirty yellow variety of *Cucumaria pentactes*, that rarely exceeds half these dimensions. The oral tentacula in both of these species are largely developed, taking the form of ten extensively ramifying pedunculate plumose or dendriform tufts stationed at equal distances around the oral opening. It is with these organs that the food substances are seized and conveyed to the alimentary system, though in a manner totally distinct from what obtains in other tentaculiferous animals, such as a sea-anemone, tubiculous annelid, or cuttle-fish. When on the full feed it was observed indeed that the tentacles of the Holothurian were in constant motion, each separate dendritic plume in turn, after a brief extension, being distally inverted and thrust bodily nearly to its base into the cavity of the pharynx, bearing along with it such fragments of sand and shelly matter as it had succeeded in laying hold of. No consecutive order was followed in the inversion of the separate tentacles, that which at the moment had secured the most appetising morsel gaining seemingly the earliest *entrée*. But little time was lost in this feeding process, for no sooner was one tentacle everted than another was thrust into the gullet, and so the meal continued, as not unfrequently observed, for several hours together. To furnish a fitting simile for this anomalous phenomenon of ingestion, one might imagine a child provided with ten arms, after the manner of ancient Buddha, grasping its food with every hand and thrusting it in a quick and continuous stream down its throat, the hands and arms with every successive mouthful, not stopping at the mouth, but disappearing up to or above the elbow within the visceral cavity. That the Holothuriæ are not devourers of living corals is shown not only in connection with the data just recorded, but from the fact also that several of these animals were kept in a tank containing sea-anemones and corals (*Balanophyllia verrucosa*) without their interfering with them in any way, or manifesting alimentative functions other than those just described. All that they require for their nutrition is evidently derived then from the coral or shell debris with which they are customarily associated. At first sight this material would appear to be in the last degree adapted for the sustenance of such highly-organised animals, but, as may be confirmed at any time by the investigation of like conditions in aquaria, it will be found that shell-sand, gravel, and all other debris forming the superficial layer at the bottom of the water, when exposed to the light, is more or less completely invested with a thin pellicle of Infusoria, Diatoms, and other microscopic animal and vegetable growths. It is upon these minute organisms that the Holothuriæ feed, swallowing both them and the shelly or other matter upon which they grow, much in the same way as we might subsist on cherries, swallowing stones and all—the nutritious matter in the case of the cherries being in much greater ratio—and the Echinodermata having the advantage over us that they have no vermiform appendage to their alimentary system to jeopardise their safe indulgence in such stone-swallowing propensities. Most probably, but this as a fact I did not at the time take steps to determine, the shell or coral debris, with its investing organisms conveyed to the mouth, is triturated by the characteristic teeth that arm the pharynx into one homogeneous mass, which, after the extraction of all nutritious substances, is discharged in the form of sandy pellets at the opposite extremity. At all events, the phenomenon of food ingestion as witnessed and here described amply accounts for the relatively prodigious quantities of shell- or coral-sand that the Holothuriæ have been observed to void by Mr. H. B. Guppy and other writers.

Data of interest concerning the feeding processes of various other Echinodermata were noted by me at the Manchester Aquarium. Two species of Echini—*E. miliaris* and *E. lividus*—throve well, and devoured large quantities of the seaweed, *Ulva latissima*, thus demonstrating their essentially herbivorous tastes, while the common Sand-star, *Ophiura texturata*, exhibited peculiarly interesting habits. These were kept in a shallow tank with a sandy bottom, and, except at feeding-time, were but rarely visible. No sooner, however, had a few small pieces of chopped fish been dropped into the water and settled to the bottom than their snake-like arms appeared above the sand in all directions, next their entire bodies, then a general scramble ensued for the provided food. This was conveyed to the creatures' mouths with the aid of the flexile arms, one of which was dexterously twisted round the selected fragment—as an elephant might use its proboscis—and the morsel then dragged beneath the body with its central oral aperture, or rather the body dragged on top of it. Phenomena of corresponding interest, did space permit, might be related of numerous other species, but the foregoing will suffice to illustrate the amount of knowledge that may be gained, and gained in no other way, concerning the habits and life-histories of marine organisms by their intelligent study in the tanks of an aquarium.

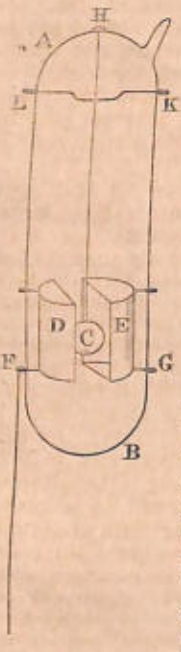
Apropos of this subject of aquaria, it will interest all biologists to know that the long-hoped for opportunity has at last arrived, or is about to arrive, of securing to the nation an aquarium suited in all ways, in both position and equipments, for the conduct of scientific pisciculture and biological research. Included in the buildings now in course of erection for the forthcoming International Fisheries Exhibition is a fine series of marine and freshwater tanks, having such substantial construction and perfect mechanical arrangements, that it is proposed to leave them standing after the close of the Exhibition, their final destiny having alone to be decided on. The close contiguity of these tanks in the Western Arcade of the Horticultural Gardens to the existing Museum of Economic Pisciculture immediately suggests the appropriateness with which they might be incorporated and more fully developed in conjunction with that Museum, or with the biological department of the Science Schools on the opposite side of the road. As an appanage of the last-named institution, it is indeed almost impossible to prognosticate the important rôle that aquarium might be adapted to fulfil. Leaving a sufficiency of tanks for public exhibition, the remainder might form an efficient depot or inland zoological station for both supplying the class-rooms and for placing at the disposal of original investigators facilities hitherto unprecedented in this country for making themselves acquainted with the structure, habits, and developmental histories of organisms pertaining to every branch of marine biology. It is at all events most earnestly to be desired that steps will be taken at the right time by the proper authorities to turn to good account so magnificent and rarely recurring an opportunity. W. SAVILLE KENT

Buckland Fish Museum, South Kensington, February 21

Influence of a Vacuum on Electricity

THE theory of Prof. Edlund that a perfect vacuum is a perfect conductor of electricity, but that a discharge across such a vacuum between two electrodes is prevented by an electromotive force at the surface of the electrode, involves our attributing to the vacuum the property of screening from electrical influence any body which it envelops. If the vacuum be a conductor, what we call induction cannot take place through it.

Not having been able to find any record of an experiment which conclusively proved that a vacuum so perfect as to offer considerable resistance to the passage of a current nevertheless permitted induction to take place through it, I have tested the matter by means of the apparatus shown in the figure.



AB is a glass tube about 15 cm. long; C is a light hollow platinum ball, 1 cm. in diameter, hung by a fine platinum wire from the top of the tube between D and E the two separated halves of a cylindrical platinum box, which are insulated from each other and held in position by platinum connections sealed into the sides of the tube, and projecting to the outside at F and G.

It is of importance to mention that the upper terminal H, from which the sphere hung, does not reach more than about 3 millimetres above the inner surface of the tube. The two halves of the cylindrical box are sufficiently near together to prevent the ball coming in contact with the sides of the glass.

This tube was exhausted until an induction current, would give a 12-millimetre spark in air, rather than pass between two terminals, K L, sealed in the upper part of the tube with their opposed ends about half a centimetre apart.

A wire about 30 cm. long was then hung from F, and an electrified body presented to the lower end. On the approach of this body to the wire the sphere was at once attracted towards D, and when a discharge was permitted between the electrified object and the wire, the sphere was violently attracted, and a minute spark was seen when the wire holding it touched the cap of the box D. The sphere was then repelled by the similarly charged box.

It thus appears that the phenomena of electric induction take place across a discharge-resisting vacuum, and that the sphere hung in it is not screened from electrical influence as it would be if surrounded by a conductor.

A. M. WORTHINGTON

Clifton College, Bristol, February 22

The Meteoroid of November 17, 1882

THERE has already been much discussion on this subject, but I do not think that such exceptional phenomena lose any of their interest by having happened a few months ago; and so I write partly to correct a misapprehension on the part of Mr. Backhouse and Mr. Groneman as to the bearings of the positions of appearance and disappearance of the meteoroid as seen by myself. It seemed to me to appear in the S.E.E. and disappear S.W. by S., but these are not the directions of those points where the trajectory and the horizon would intersect. By mentally continuing the apparent path down to the east and west horizons, points would be reached, nearly, but I think not quite, 180° apart, the former about 20° N. of E. and the latter nearly opposite, so that I scarcely think that it was a great circle, but it is very uncertain. Mr. Saxby states that a similar cloud was observed to cross the zenith of Brussels by M. Montigny. Now there are two accounts—one by M. Zeeman of Zierickzee and the other from near Rye (Sussex)—which seem to consistently apply to one and the same thing, for the latter place is W. by 20° S. from Zierickzee, and from both places the same elevation of about 50° was reached. These accounts, if combined with that from Brussels, indicate a height of about 70 miles; but then how does such a height agree with some of the English observations? On the supposition of the above height, the altitudes of culmination as seen from Woodbridge and Windsor would be about 29°, from Bristol 25°, and from York 10° only, which last angle is directly at variance with the actual one. For my part, I will give up the reconciling of such contradictory evidence to those who have an aptitude for conundrums. The evidence is in favour of this being an auroral manifestation, but the spectrum of the cloud does not prove this, for as yet it is not known whether the extremely rarefied upper atmosphere may not be excited to such incandescence as will yield the so-called "auroral" spectrum by other means than the electric discharge, as, for instance, by the passage of a cloud of meteorites. Mr. Petrie upholds the latter hypothesis, but I think that there is a simple but weighty objection to it; for it is difficult to see how a cloud of meteoric dust of such closeness and defined form as the appearance of this cloud would imply, could travel through space for any length of time without coalescing into one granular lump, owing to the mutual gravitation of its particles. Of course this objection will have the less weight the smaller we suppose the individual particles to be. This argument will scarcely apply to the well-known meteor streams, whose individual particles are really so very far apart. If this "flying arch" was subject to gravity, it certainly had more than sufficient velocity to prevent it being appropriated by our earth as a satellite, for the tangential speed necessary to a circular orbit of 4100 miles radius round our earth would only be about 4¾ miles per second, with a period of 1½ hours. All interested in this phenomenon will no doubt pay more attention to the southern sky during future auroras, in hopes of noting something more of a similar nature, and they will also look forward to seeing a full account of Prof. Lemström's remarkable experiments on the nature of the aurora, which he has been conducting at Sodankylä with such unlooked-for results.

Heworth Green, York H. DENNIS TAYLOR

A Meteor

LAST evening at 9.35 p.m. a remarkably large and brilliant meteor was seen from here, appearing at a point about 10° east of η Canis Majoris, passing slowly over that star in a south-west direction, and vanishing a few degrees above the horizon; time about three seconds. Its light had a pale green tint, and in brightness and apparent diameter it far exceeded Sirius (which was particularly bright all the evening), so much so that my companions, though not looking in that direction, were instantly attracted by the light, and saw it in its splendour.

R. W. S. GRIFFITH

Eyeworth Lodge, Lyndhurst, Hants, March 3

Aurora

LAST night at about ten o'clock there were two beautiful white auroral streamers, like the tails of enormous comets, near the Pleiades. They were nearly vertical, and slowly moved, in a direction parallel to the horizon, towards Orion, after which they gradually vanished. There was little wind, and the night was bright starlight, after a cloudy day. There was an auroral glow like twilight over the northern horizon. The barometer rose during yesterday and last night, and stands high.

JOSEPH JOHN MURPHY

Old Forge, Dunmurry, Co. Antrim, February 28

Hovering of Birds

WITH regard to Mr. W. Clement Ley's remarks, I have already been permitted to explain in NATURE (vol. xxvii. p. 366) how I had accidentally misunderstood Mr. Airy's meaning. I do not believe that any bird having a greater specific gravity than the air can retain a perfectly fixed position in a calm without some wing-motion. Mr. Ley "believes that there is nothing in the etymology of the word 'hover' that implies movement." This has induced me to look up a somewhat voluminous and recent dictionary, in which I find "Hover, *v.i.* (W. hoviaw, to hang over, to fluctuate, to hover). To flap the wings, fluttering or flapping the wings with short irregular flights"; and more to the same effect, all indicating movement.

J. RAE

AMATEURS AND ASTRONOMICAL OBSERVATION

THE labour done by astronomical amateurs has had no little influence upon the progress of the science. The work achieved by them has indeed often been of the

utmost value, and a long list of names might be adduced of those who in past years attained a most honourable position either as discoverers, as systematic observers, or as both. Seeing therefore that amateurs, whose efforts are purely disinterested and the natural outcome of a love for the subject, have contributed so largely to place our knowledge of astronomy in its present high place, their efforts should be encouraged and utilised by their contemporaries, who hold official positions, and who may find it convenient to assist them by some of that practical advice and instruction which they are eminently qualified to afford.

It seems a thing to be deplored that in this country there are no establishments where astronomy is made a special subject for teaching, and where those who early evince a taste in this direction may be educated in conformity with inclination. We think that an institution giving special facilities to astronomical students, and affording instruction both in observation and computation, must prove a most efficient means of advancing the interests of the science. It cannot be denied that the work of many amateurs is rendered far less valuable than it would otherwise be by its approximate character, that is to say, by its lack of critical exactness—both as regards practice and theory. This cannot be avoided under present circumstances. A man on first becoming imbued with the desire to study astronomy as a hobby is generally in a measure isolated; he has to rely entirely upon his own exertions and what he can get out of the popular treatises upon the subject. It must, however, be conceded that he has many difficulties to encounter, both imaginary and real, before he proceeds very far; and these impediments are of such force as either to deter him altogether from advancing further, or check him so effectually that more than ordinary enthusiasm is required to surmount them. Now this could be obviated by a little timely assistance from some practical astronomer. Treatises, however exhaustive and felicitous in explanation, can never be as effective as personal instruction and example, and hence it seems a desideratum that some establishment should be arranged to afford assistance to such amateurs as are anxious to qualify themselves as practical astronomers. It is certain that could such instruction be imparted on reasonable terms, there are many amateurs who would gladly avail themselves of the opportunity. The main purpose might be to train observers to the use of equatoreals, transit instruments, micrometer work, photography, &c., and in the proper reduction of observations and computation of orbits.

The fault with amateurs seems to be that they are devoid of organisation, and generally of proper education to the work in hand. Labouring independently and intermittently they have, as a rule, no definite purpose in view other than the mere gratification of curiosity. It is obvious that some means should be adopted to attract them to suitable channels for systematic work, so that they may be enabled not only to find pleasure, as hitherto, in seeing objects of interest, but also more effectively to aid the progress of the science by making their observations of practical utility. For it cannot be doubted that the means of determining exact positions and the capacity to reduce them will naturally increase the ardour and interest of observers, and must introduce a new and powerful element to the further advancement of astronomy. The number of amateurs is steadily increasing year by year, and there are now in this country a very large assortment of efficient telescopes which are lying comparatively idle or so misdirected as to be of little service. Under these circumstances it seems desirable to make some attempt to organise the labours of amateurs in directions suitable to their means and inclinations, and to utilise such results for the benefit of astronomy.

It is generally the case that amateurs employ their instruments in spasmodic fashion, and do not tenaciously follow up important observations even when such are well within their grasp. For instance, an interesting marking on a planet may be once seen and recorded as a feature of peculiar interest, but it is then allowed to escape subsequent observation, and thus the value of the record is lost. It is not sufficient to see a thing; we must hold it as long as possible, watching its variations of motion and form, and thus possibly arriving at something definite as to its behaviour and physical character. We cannot, it is true, expect amateurs, who generally are much pressed with other engagements, to work for long periods and at inconvenient hours, because this directly means a sacrifice of other interests which it is imperative should not be neglected. But by the exercise of discretion, and by the utilisation of favourable opportunities, we think that observers, though their time may be much restricted and their instrumental means very limited, may yet contrive to do valuable work in one or other of the many attractive departments of astronomy.

The fact sometimes forces itself upon us that astronomical *work* is not nearly commensurate with the *means*. The large number of powerful instruments now in use might surely be expected to yield a most abundant harvest of results; but we cannot deny that this is far from being the case. It is sometimes the boast of the fortunate possessors of a 10-inch refractor or 12-inch reflector that their instruments are comparable, as regards performance and reach, with those employed by the first Herschel; and this being granted, how comes it that there is such a manifest lack of new discoveries and of that unwearying enthusiasm exhibited by the earlier observers? Possibly some of our best instruments are merely erected as playthings serving to gratify popular curiosity. The possessor of a "big" telescope is always courted to a certain degree by people who, though knowing little and caring nothing about the science, yet profess great interest in order to be permitted to view some of the most interesting wonders in the sky. These ordinary sightseers love novelties of any kind; moreover a view of such objects and an explanation by the "astronomer" himself is a thing to be desired, because one acquires self-importance and can dilate upon the subject to one's open-mouthed friends who have never been honoured with such marked distinction. It is needless to say that such exhibitions are mere waste of time; valuable opportunities—and they are few enough in this climate!—are lost never to return.

Many fine telescopes, though occasionally in use, are not directed to the attainment of any important ends. Year after year they are kept in splendid adjustment; a speck of dust on the lens is removed with scrupulous care; a spot of dirt on the circles is rubbed off with anxious energy, and the owner stands off a few paces to view his noble instrument with intense pleasure. How grand it looks! How massive! Surely this splendid machine is able to reveal the most crucial tests of observational astronomy? The knowledge that he has the means to see great things is in itself a sufficient satisfaction without any practical application. Besides, how can he think of departing from his invariable custom of going to bed at 10.30 p.m. and risk catching a slight cold into the bargain? His intention certainly had been to make a prolonged vigil to-night, but that was decided on in the sunny afternoon before the frosty air came on and before the fog began to rise up from the valley, and so he decides with some show of reluctance to leave it all to another night! Here is the hour, but not the man.

It is a fact to be regretted that many promising amateurs have had to relinquish, prematurely, all astronomical work on account of circumstances. A man on first experiencing the desire to do something to astronomy buys a few books, and then, when he finds it indispensable, a telescope, thus expending it may be the hard-earned savings of a few years. He becomes more interested with new facilities, and devotes much time to the subject. Ultimately the fact is realised that his business affairs

are suffering from want of proper attention, and what is of even more importance his health is failing with over-application to work. There is no alternative but to relinquish his favourite hobby, and he parts with his books and instruments for what little they will fetch. How many are there who have had this experience? How many promising observers have left the science because it offers no pecuniary rewards or benefits such as other work commands? "Life is real, life is earnest"; the telescope must be neglected for the ploughshare, and the solitary though withal happy hours of vigil must be given over to Morpheus! Many have realised all this, and though their names will never be known as astronomers, they have deserved as much credit for their disinterested efforts as many others who have from more fortunate circumstances achieved eminence.

It must be admitted that observers of the present day have many advantages over their predecessors, owing to the greater perfection and size of instruments and the conspicuous advances in the serial literature of the science. The latter has developed wonderfully during the last few years with such publications as *The Observatory*, *Copernicus*, *L'Astronomie*, *Sirius*, *Ciel et Terre*, *The Sidereal Messenger*, &c. Formerly we had but the *Astronomische Nachrichten*, *Wochenschrift für Astronomie*, and *Astronomical Register*. This leads us to hope for a corresponding increase in the number of astronomical workers.

It cannot be questioned that the essential direction of labour on the part of amateurs should be more of a systematic or methodical character than hitherto. A certain department or definite work should be taken in hand and followed up persistently. Little good is likely to accrue from erratic work or from the hasty and necessarily incomplete examination of many different objects. Every observer has a leaning towards a speciality, and he should pursue this exclusively even to the absolute neglect of other departments. Astronomy offers such a large number and variety of objects that to attempt an investigation of more than a mere fragment will tax more than the energies of a lifetime. We would therefore recommend amateurs to apply themselves sedulously to such special branches as they may individually select, for the indiscriminate use of a telescope is to be deprecated on many grounds. W. F. DENNING

ON THE NATURE OF INHIBITION, AND THE ACTION OF DRUGS UPON IT[1]

II.

M. VULPIAN has observed that the excitability of the lower parts of the spinal cord increases as the upper part is gradually shaved away, so that each layer of the cord appears to exercise an inhibitory action on the one below it. M. Brown-Séquard supposes that in each layer of the cerebro-spinal system there are both dynamogenic elements and inhibitory elements for the subjacent segments.

We are, in fact, almost obliged to assume that each nerve-cell has two others connected with it, one of which has the function of increasing, and the other that of restraining the function of the nerve-cell itself.

Applying this same hypothesis to Newton's rings, we would say that certain parts of the lens or of the glass plate possessed the property of interfering with the rays of light, or were inhibitory centres for them. Others again had the property of increasing the brightness, or were stimulating centres for them; and, moreover, that different parts of the lens or of the glass plate contained each its stimulating and inhibitory centres for different coloured rays.

The multiplication of centres in the lens and glass plate soon becomes more than the imagination can well take in; and we are at present almost precisely in the same condition regarding inhibitory and stimulating centres in the nervous system.

As soon as we get rid of the idea that the darkness caused by the interference of the rays of light at certain points is due to some peculiar property inherent in the glass, and attribute the interference simply to the relationship between the waves of light and the distance they have to travel, the whole thing becomes perfectly simple, and the same is, I think, the case in regard to inhibition in the nervous system.

Let us now take a few more examples of inhibition.

We find in experiments with the frog's foot exactly the same as on our own hand. Thus, when a little turpentine is placed upon the toes it excites a violent reflex, but if a little turpentine be injected under the skin of the same foot, the reflex is abolished.[1] We find also that irritation applied to a limited region of the skin usually causes marked reflex, but if the same stimulus be applied to the sensory nerve supplying that region, the reflex is very much less.[2] In the cases just mentioned the irritation is applied to sensory nerves of the same part of the body, and close together, and the explanation of its different results is the same as that already given for the different effects of tickling and pressure. Different sensory nerves on the same side of the body, but at some distance from each other, will also cause inhibition of motor reflexes; thus it has been shown by Schlosser[3] that simultaneous irritation of the skin over flexor and extensor surfaces will lessen reflex action.

Some years ago I observed that frogs suspended by the fore-arms with cords, or tied with their bodies against a board, reacted less perfectly to stimulation of the foot by acid than a frog suspended by a single point, as in Türck's method. Tarchanoff[4] has also observed that frogs held in the hand also respond less perfectly than when hung up; the gentle stimulation of the sensory nerves in the skin of the body appearing to exercise an inhibitory action over the reflex from the foot.

The injection of acids or irritating solutions into the mouth[5] or dorsal lymph sac[6] also exercises an inhibitory action on reflexes from the foot.

A similar effect is produced by irritating the sciatic nerve on one side by a Faradaic current, and applying a stimulus to the other foot. So long as the irritating current is passed through the sciatic nerve, no reflex movement can be elicited by stimulation of the other foot; but so soon as the Faradaic current stops, the reflex excitability again appears in the other foot.[7] As this phenomenon occurs when the influence of the brain and upper part of the spinal cord has been destroyed by a section through the cord itself, the inhibition which occurs must be due to an action which takes place in the lower portion of the spinal cord.

Stimulation of the nerves of special sense has also an inhibitory action on reflex movements. This we can readily see in ourselves, by observing our actions in the dark. If we touch something cold or wet, or if something suddenly comes against our face, we give an involuntary start, sometimes almost a convulsive one. If, however, we were able to see, we should not give a start in the least when we touched a piece of wet soap, or when the end of a curtain suddenly came against our cheek.

Without entering into the nervous mechanism through which sight effects this change in our actions, but only reducing it to its simplest form of expression, as we would

[1] Continued from p. 422.

[1] Richet, Muscles et Nerfs, Paris, 1882, p. 710.
[2] Marshall Hall, Memoirs on the Nervous System, London, 1837, p. 48.
[3] Arch. of Physiol. 1880, p. 303, quoted by Richet, *op. cit.* 709.
[4] Quoted by Richet, *op. cit.* p. 709.
[5] Setschenow, Physiologische Studien über die Hemmungsmechanismen für die Reflexthätigkeit des Rückenmarks im Gehirn des Frosches, Berlin, 1863, p. 33.
[6] Brunton and Pardington, St. Bartholomew's Hospital Reports, 1876 p. 155.
[7] Nothnagel, Centralblatt d. med. Wiss. 1869, p. 211.

in talking of animals, we say that the stimulus to the sensory nerves of the hand or cheek, by contact with the wet soap or with the curtain, caused in us a reflex spasm, which was inhibited by the stimulus applied to our optic nerves. A similar occurrence is observed in frogs, and the reflex actions produced by stimuli applied to the feet are much stronger when the inhibitory effect of the optic nerves upon them is removed by covering up or destroying the eyes, or by removal of the optic lobes.[1]

Regarding the optic lobes, we will have a good deal more to say presently, for they have been considered to be special inhibitory centres, and are often known by the name of Setschenow's centres.

If we try to explain all those instances of inhibition by the assumption of special inhibitory centres for each action, we must suppose, in connexion with every sensory nerve, that centres exist which lessen or abolish the ordinary reflexes produced by stronger or weaker stimulation applied to the nerve. Besides this, we must suppose other centres which inhibit motor actions in other parts of the body: as for example, when irritation of the extensor lessens reflex excited by irritation of the flexor surfaces, or *vice versâ*, or when the irritation of one sciatic stops reflex action from mechanical irritation of the other foot. A special inhibitory centre must be placed also in the optic lobes in connection with the optic nerves. This complication reminds us of the multitude of inhibitory centres which one must imagine in glass, in order to explain the occurrence of Newton's rings by them, but it seems to me that all these cases are readily explained on the hypothesis that the motor and sensory cells concerned in them are so placed with relation to each other that the stimuli passing from them produce interference *under normal or nearly normal conditions of the organism*.

A spot of light may be caused to disappear by throwing another ray upon it, so as to interfere with it, but it may be also made to disappear from the place where it was, by simply reflecting it somewhere else.

A similar occurrence to this takes place in the body, and although two stimuli may interfere with and destroy each other, we not unfrequently find that the apparent abolition of the effect of a stimulus is simply due to its diversion into some other than the usual channel. In very many cases, where we have inhibition we have also diversion; and it is not at all improbable that when the stimulus is very strong complete inhibition may be impossible by interference alone, and can only be effected by diversion of part of the stimulus. We have already said that two waves of sound will neutralise each other and produce silence, but this only occurs when the waves are not too powerful. When they reach a certain intensity they produce secondary waves which give resultant tones, and several facts seem to point to an analogous condition in animal organisms.

We have hitherto considered cases in which the inhibition was probably brought about by interference of two stimuli, so that the one counteracts the other in much the same way as two rays of light interfered with one another in Newton's rings. In one case which we have mentioned, the movement of the hand when it is tickled is entirely arrested by a strong effort of the will, and the hand is allowed to remain perfectly passive and limp. Here we suppose the impulse sent down from the motor centres in the brain to interfere with that which has originated in the cord by irritation of the sensory nerves, and to counteract it so that no muscle whatever is put in action. But very frequently we find that a result apparently similar is produced by a different mechanism, viz. by diversion of the stimulus into other channels. In the former case the arm is felt to be quite limp, but in the latter though it is quite quiet, it is perfectly rigid—all the muscles being intensely on the stretch. Here the stimulus which would usually have excited convulsive movements of the arm, and probably of the body, resulting in a convulsive start, have been diverted from the body into other muscles of the same limb.

A similar power of diverting a stimulus is seen in the instinctive muscular efforts which any one makes when in pain. One of the most common of these is clenching the teeth, and it used to be a common practice in the army and navy for men to put a bullet between the teeth when they were being flogged, and at the end of the punishment this was usually completely flattened. A patient seated in a dentist's chair usually grasps convulsively the arms of the chair, or anything which may be put into his hand; and there can be little doubt that pain is better borne, and appears to be less felt, when the sensory stimulus occasioning it can thus be diverted into motor channels. In children the motor channels into which diversion usually takes place are those connected with the respiratory system, and the sensory stimulus works itself off in loud yells. At a later age the stimulus is often diverted into those motor channels through which reaction occurs between the individual and his surroundings. Thus most people probably remember how a kick in the shins at football often served simply to accelerate their speed; and during the heat of battle the pain of a wound is often but little felt, the stimulus having been diverted into motor channels.

Many more instances might be given of the effects of diversion of stimuli, but having discussed this subject at length in a former paper,[2] I shall not pursue it further here.

Sensory stimuli are also capable of inhibition by interference. Hippocrates[3] noticed, and it is a matter of general observation, that pain in one part of the body may be lessened or removed by the occurrence of pain in another. In many instances, the removal of the pain to one part may be indirect, through the action exerted on the vessels by the pain in the other part. But in some instances it may be, and probably is, due to the direct interference of sensory impressions.

This question of the removal of pain by the interference of waves in the sensory nerves or nerve-centres has been very fully and clearly discussed by Dr. Mortimer Granville.[3] Starting from the hypothesis of interference, he has also devised a plan of treatment which appears to give satisfactory results. By means of a small hammer moved by clockwork or electricity, he percusses over the painful nerve in order to induce in it vibrations of a different rhythm to those which are already present and which give rise to the pain. Thus he percusses rapidly over a nerve when the pain is dull or grinding, and percusses slowly when the pain is acute, in order to produce interference if possible. In many instances the treatment is successful, and its success affords additional support to the hypothesis on which it is based.

We have hitherto spoken of reflex inhibition in the cerebro-spinal axis alone, but we find also reflex inhibition of motor actions produced by irritation of sympathetic nerves; and, *vice versâ*, we find inhibition of the movements of internal viscera produced by irritation of cerebro-spinal nerves. Thus strong irritation of the sensory nerves of the liver, intestine, uterus, kidney, or bladder, occasionally abolishes the power of walking or standing. Irritation of a sensory nerve will frequently arrest the movements of the heart.

The phenomena which occur in swallowing afford an excellent example, not only of inhibition occurring in parts innervated by the sympathetic system, but also of

[1] Langendorff, Arch. f. Anat. u. Physiol. 1877; Von Boetticher, Ueber Reflexhemmung, Inaug. Diss., Jena, 1878, p. 12.

[1] Brunton, "Inhibition, Peripheral and Central," West Riding Asylum Reports, 1874.

[2] Hippocrates, Aphorisms, sec. i. 46; Sydenham Soc. Ed. vol. ii. p. 713.

[3] Mortimer Granville, Nerve Vibration and Excitation. (London: Churchill, 1883.)

partial diversion of stimuli. Kronecker has found that when we swallow, the food or water is sent down at once into the stomach by the contraction of the muscles of the pharynx, and that afterwards a peristaltic contraction of the œsophagus occurs. When several attempts to swallow are made one after the other, however, the œsophagus remains quiet until they are ended, and then it occurs at the same interval of time after the last, that it would have done after a single act of swallowing.

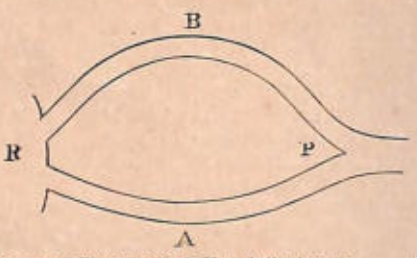


FIG. 2.—Diagram to illustrate Sir J. Herschel's observations on interference. Adapted from his article on "Absorption of Light," Phil. Mag. 1883, p. 405.

If we now refer again to our diagram (Fig. 2, which for convenience we repeat here) we will see that it answers just as well for the contractions of the œsophagus as for the tides at Batsha by simply giving a different meaning to the letters. Let R now instead of representing a reservoir or the open sea represent the ganglia of the pharynx, A and B the nerve fibres which conduct nervous impulses from these ganglia to P, and let P be the ganglia of the œsophagus which stimulate its muscular fibres to peristaltic action. A single wave passing from R causes two waves at P, one succeeding the other, but a number of waves from R under the conditions supposed also cause only two waves: one at the beginning and one at the end, for during all the intermediate period they neutralise each other.

It might perhaps seem that the two stimuli should cause two contractions of the muscular fibres of the œsophagus. But it frequently happens that a single stimulus is unable to produce muscular contraction. It only increases the excitability of the contractile tissue to a second stimulus, and when this is applied contraction ensues. The effect of the first wave then would be to increase excitability, that of the second wave to cause contraction. This is well shown in the accompanying tracing from the contractile tissue of medusæ, which I owe to the kindness of my friend, Mr. Romanes. He has found that when very slight stimuli, such as from weak Faradaic shocks, are applied, the first has no apparent action, but the effect of each successive stimulus is added to that of the preceding ones, until contraction is produced. Two shocks were applied before the first small contraction shown in the tracing occurred, and the shocks are all of the same strength, although the last ones produce the maximal contraction of which the tissue is capable, and the first had apparently no effect at all. This relation of the contractile tissue to stimuli is usually expressed by saying that the tissue has the power of summation.

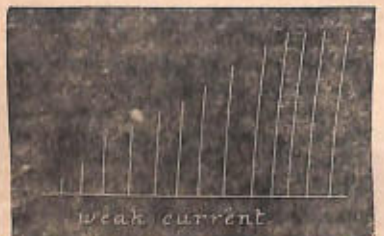


FIG. 6.—Showing the increasing contractions of the tissue of medusa when stimulated by repeated weak induction shocks of the same intensity.

At the same time that a stimulus is sent down from the pharynx to the œsophageal ganglia, which has an inhibitory action, there appears to be another sent to the medulla oblongata, which acts on the roots of the vagus nerve. This latter stimulus has a very curious effect, viz. inhibition of inhibition. The vagus usually exercises an inhibitory action on the heart, rendering its beats less rapid than they would otherwise be, but during swallowing this inhibitory action is removed and the heart pulsates at nearly double its normal rate.[1] Here we seem to have a stimulus one part of which passes along one path, while another part is diverted and passes along another. Each part interferes with the nervous actions which would occur in its absence, but one part interferes so as to prevent, and the other so as to increase muscular activity in the œsophagus and heart respectively.

The same diversion of a stimulus which we find in the case of the œsophagus seems to occur frequently throughout the body. Thus we find it almost invariably in relation to the vascular changes which occur on stimulation of a sensory nerve. When a sensory nerve going to any part of the body is irritated, the vessels of the district which it supplies usually dilate, while those of the other parts of the body contract.[2] The stimulus in this case passes to the vasomotor centre, and thence is reflected as an inhibitory stimulus in one direction and as a motor stimulus in another.

Some results of the greatest interest have recently been obtained by Dastre and Morat, in some experiments which they have made on the subject of vascular dilatation or inhibition.

In many cases the stimulation and inhibition of vascular nerves take place in the medulla oblongata, or in the spinal cord, and the inhibitory and motor centres are close to each other; but in other cases, such as those experimented on by Dastre and Morat,[3] we find the inhibitory and motor centres separated from one another, some of the motor centres being in the cord and some of the inhibitory in a ganglion situated nearer the periphery.

It was previously known that in some cases, as in the dilatation of the vessels of the submaxillary gland on irritation of the chorda tympani, small ganglionic structures were situated at the terminal branches of the nerve, and it was supposed that these ganglia, by their interposition between the nerve and the structure on which it was to act, converted its motor power into an inhibitory one. The experiments of Dastre and Morat are much more definite on this point. Excitation of the cervical sympathetic nerve has the effect of causing the vessels of the ear to contract very greatly in the rabbit, but irritation of the same nerve causes in the dog enormous dilatation of the vessels of the mouth. Moreover, in the rabbit this constricting action on the vessels of the ear is exerted only when the nerve is irritated between the first cervical ganglion and the ear. When the nerve is irritated beyond the cervical ganglion, instead of causing constriction, it produces dilatation.

In order to explain this action, the authors suppose that the fibres of the sympathetic, in passing through the ganglion, end in the ganglionic cells, and thus suspend the tonic action which they exert on the constricting fibres which issue from the ganglion and pass to the ear. It seems to me, however, that a more satisfactory explanation of this fact also is afforded by the hypothesis of interference.

In the cerebro-spinal system, cells being ranged above, below, and around one another with free communication between them, we have ample provision for the passage of two stimuli along paths of such different length, as to enable them to interfere with and inhibit each other.

[1] In my own case the proportion is 120 to 76.
[2] Ludwig and Loven, Ludwig's Arbeiten, 1866, p. 17.
[3] Archives de Physiologie, 1882, tom. x. p. 326.

But in peripheral nervous mechanisms, such as those in the heart of the frog, where we have no such provision, and the cells are not only few in number,

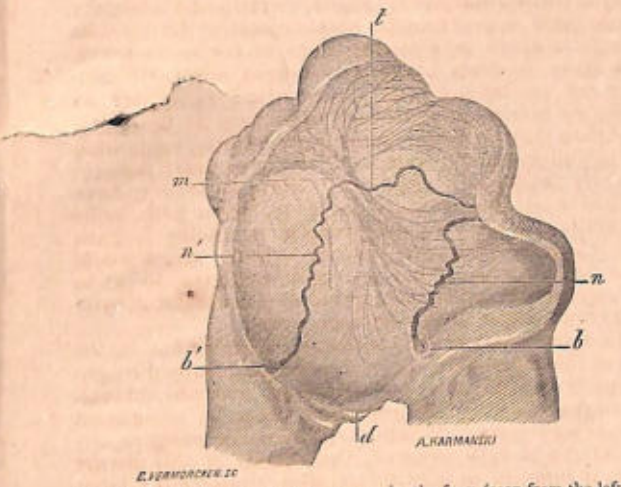


FIG. 7.—View of the auricular septum in the frog (seen from the left side). *n* is the posterior, and *n'* the anterior cardiac nerve. *l* is a horizontal portion of the latter nerve; *b* is the posterior, and *b'* the anterior auricular ventricular ganglion; *m* is a projecting muscular fold. This figure is taken by the kind permission of my friend M. Ranvier, from his Leçons d'Anatomie Générale. Année 1877-8.—Appareils nerveux terminaux, t. 6. p. 79. (Paris: J. B. Baillière et Fils, Rue Hautefeuille 19.)

but not arranged in strata, we find a special form of ganglion cell which seems constructed for this very purpose. This is the spiral cell described by Beale,

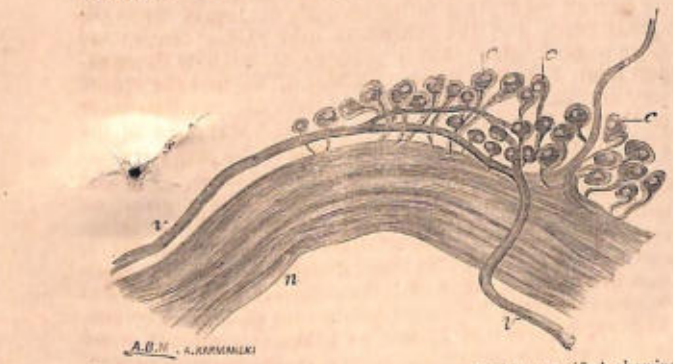


FIG. 8.—Part of the posterior cardiac nerve more highly magnified, showing the ganglia (Ranvier, *op. cit.* p. 106).

in which we find one nerve-fibre twisted round and round in a way which reminds us of a resistance coil in a galvanic circuit. The object of this peculiar arrangement has, so far as I know, not been discovered; but it seems to afford the exact mechanism which is wanted, in order to alter the distance two stimuli have to travel, and thus allow them to interfere with and inhibit each other. The occurrence of these ganglia in the heart and other viscera seems to afford in itself some support to the



FIG. 9.—Spiral ganglion cell from the pneumogastric of the frog. This figure is not taken from the cells in the cardiac nerves, as in them the connection between the spiral and straight fibres has not been clearly made out, but it is probable that these cells have a structure similar to the one figured (Ranvier, *op. cit.* pp. 114-20). *a* is the cell body, *n* the nucleus, *r* the nucleolus, *d* nucleus of the capsule, *f* the straight fibre, *g* Henle's sheath, *gf* spiral fibre, *g'* its gaine, *n'* nucleus of Henle's sheath (Ranvier, *op. cit.* p. 114).

hypothesis here advanced; but we will defer the consideration of the mode in which inhibition occurs in the heart and other internal viscera, and pass on at present to the effect of various parts of the central cerebro-spinal system upon each other.

T. LAUDER BRUNTON

(To be continued.)

THE SHAPES OF LEAVES

I.—*General Principles*

THE leaf is the essential and really active part of the ordinary vegetal organism: it is at once the mouth, the stomach, the heart, the lungs, and the whole vital mechanism of the entire plant. Indeed, from the strictest biological point of view every leaf must be regarded as to some extent an individual organism by itself, and the tree or the herb must be looked upon as an aggregate or colony of such separate units bound together much in the same way as a group of coral polypes or the separate parts of a sponge in the animal world. It is curious, therefore, that so little attention, comparatively speaking, should have been given to the shapes of the foliage in various plants. "The causes which have led to the different forms of leaves," says Sir John Lubbock, "have been, so far as I know, explained in very few cases." Yet the origin of so many beautiful and varied natural shapes is surely worth a little consideration from the evolutionary botanist at the present day, the more so as the main principles which must guide him in his search after their causes are simple and patent to every inquirer.

The great function of a leaf is the absorption of carbonic acid from the air, and its deoxidation under the influence of sunlight. From the free carbon thus obtained, together with the hydrogen liberated from the water in the sap, the plant manufactures the hydro-carbons which form the mass of its various tissues. Vegetal life in the true or green plant consists merely in such deoxidation of carbonic acid and water, and rearrangement of their atoms in new forms, implying the reception of external energy; and this external energy is supplied by sunlight. We have thus two main conditions affecting

the shape and size of leaves: first, the nature and amount of the supply of carbonic acid; and second, the nature and amount of the supply of sunshine. But as leaves also aid and supplement the roots as absorbers of water, or even under certain circumstances perform that function almost entirely alone, a third and subordinate element also comes into play in many cases, namely, the nature and amount of the supply of watery vapour in the air.



FIG. 1.

This last element, however, we may leave out of consideration for the present, confining our attention at the outset to the first two.

Carbonic acid is the true food of plants: water, one may say, is only their drink. The roots can almost always obtain a sufficient amount of moisture; and though no doubt there is sometimes a fierce struggle for



FIG. 2.

this material between young plants, yet its effects are not usually so obvious or so lasting on the shape of the parts concerned. But for the carbon of which their tissues must be built up there exists a competition between plants as great and as evident as the competition between carnivores for the prey they pursue, or between herbivores for the grasses and fruits on which they subsist. The plant endeavours to get for itself as much as it can of this fundamental food stuff; and all its neighbours endeavour to frustrate and to forestall it in the struggle for aërial nutriment. Again, the carbon is of no use without a supply of sunlight in the right place to deoxidise it and render it available for the use of the plant. Hence these two points between them mainly govern the shapes of leaves. Natural selection insures in the long run the survival of those types of foliage which are best fitted

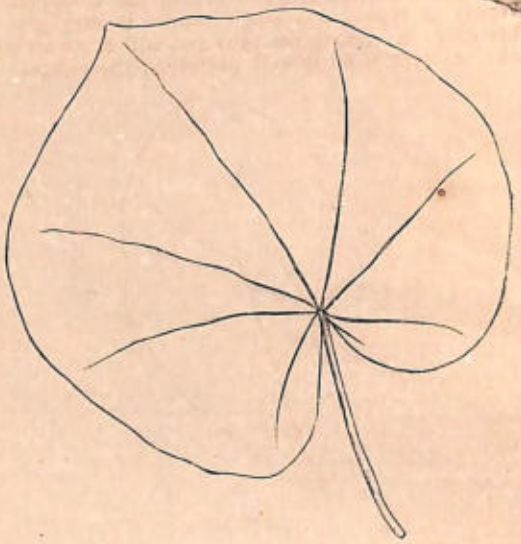


FIG. 3.

for the performance of their functions as mouths and stomachs in the particular environments that each species affects. Accordingly, in the final result each plant tends to have its chlorophyll disposed in the most economical position for catching such sunlight as it can secure; and it tends to have its whole absorbent surface disposed in the most advantageous position for drinking in such particles of carbonic acid as may pass its way. The im-port-

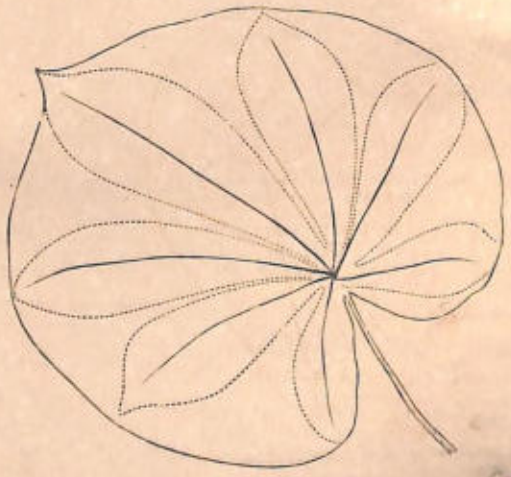


FIG. 4.

ance of the first element has always been fully recognised by botanists; but the importance of the second appears hitherto to have been too frequently overlooked.

At the same time, the shape of the leaf in each species is not entirely determined by abstract considerations of fitness to the function to be performed: as elsewhere in the organic world, evolution is largely bound by hereditary forms and ancestral tendencies. Each plant inherits a certain general type of foliage from its ancestors; and

it modifies that type so far as possible to suit the exigencies of its altered conditions. It cannot remake the leaf *de novo* at each change of habit or habitat: it can only remodel it in accordance with certain relatively fixed ancestral patterns. Hence, as a rule, each great group of plants—family, tribe, or genus—has a common type of leaf to which all its members more or less closely approximate. Occasionally, as among the composites, the diversity of types in a single family is very great; at other times, as among the peas and still more among the pinks, the type is fairly well preserved throughout. But,



FIG. 5.

in spite of all apparent exceptions, and of numerous very divergent cases, there is a general tendency in most allied plants to conform more or less markedly to a certain general central and ideal form of leaf—the form from which all alike are hereditarily descended with various modifications. The actual shape in each case is not the ideally-best shape for the particular conditions; it is only the best possible adaptive modification of a pre-existing hereditary type.

The point that is most common to leaves of different



FIG. 6.

sorts in the same group is their vascular framework or ground-plan; in other words, their venation. This is the typical thing which tends most of all to reproduce itself, under all varieties of external configuration. The plant seems to build up first, as it were, its ancestral skeleton, and then, if it can afford material, to flesh it out with the intervening cellular tissue (not, of course, literally, for all the leaf buds out at once from a single knob). A glance at the accompanying diagrams will show how easily, by failure of growth in the intervals between the principal ribs, a simple primitive rounded leaf may be converted during the course of evolution into a lobed or compound one. In Fig. 1 we have such an ovate leaf, with digitate venation: the dotted line marks the chief intervals between the ribs, mainly filled by cellular tissue. In Fig. 2 we have the leaf of a sycamore, with the same venation, but with the intervals between the ribs unfilled. Here it will be noticed that the apex of the five main lobes corresponds in each case with the termination of a main rib; and the largest lobe answers to the midrib. Similarly, the apex of each minor serration answers to the termination of a secondary riblet. The type remains the



FIG. 7.

same throughout; only in Fig. 1, material has been supplied to fill it all in, and in Fig. 2, only enough has been supplied to cover the immediate neighbourhood of the main veins.

In Figs. 3 and 4 we get a further modification of a similar type. Here the cutting of the lobes goes so deep as to divide the entire blade into separate leaflets; and the result is the compound leaf of the horse-chestnut.

The same thing may also occur with pinnately-veined leaves. In Fig. 5 we get a typical leaf of this character, where the supply of carbonic acid and sunshine under the



FIG. 8.



FIG. 9.

average circumstances of the plant is sufficient to allow of its having assumed a full and rounded specific form. Fig. 6 shows the less fully-veined tracts in such a type of foliage; and in Fig. 7, where the ordinary conditions do not favour full development, we get the familiar irregularly-lobed blade of the English oak. The diagrammatic representation in Fig. 8 suggests the steps by which a regularly pinnately-veined leaf, such as that of the common olive, may pass into a pinnatified and pinnatisect form by non-development of the mainly cellular tracts. We may thus get either a lobed leaf like the hawthorn, as

adumbrated at the summit of the diagram, or a compound leaf with pinnate leaflets like the commonest papilionaceous type, as shown in the lower portion. These examples will at once make clear the principle that with very slight changes in the real structural composition of a leaf we may have very great differences in the resulting outline. How the various underlying types of venation themselves are acquired or modified we must consider at a later stage; for the present we must take them for granted as relatively fixed generic or tribal characteristics.

It may be necessary to warn the reader in passing that comparatively little importance must be attached to the particular circumstances of each individual leaf. It is the average circumstances of the species which give rise to the specific type. True, each particular blade cannot grow at all except in so far as material is supplied to it during its growth from the older and more settled members of the complex plant-commonwealth; but even when such material is supplied to it, it will only grow to the extent and into the shape which natural selection has shown to be the best on the average for all its predecessors. For example, no plethora of available material would make the sycamore or the oak produce leaves like those represented in Figs. 1 and 5; it would only make them produce a greater number of normal leaves like those represented in Figs. 2 and 7, since these embody the final result of all the past experience of the race—the residuum of countless generations of unsparing selection.

A single illustration of the way in which these general principles work can best be found, as a first example, in the foliage of the water-crowfoot (*Ranunculus aquatilis*, Fig. 9). This well-known plant, growing as it does in streams or pools, has two forms of leaf on the self-same branch, strikingly different from one another. The lower or submerged leaves, which wave freely to and fro in the water, are minutely subdivided into long, almost hair-like, filaments; the upper or floating leaves, which loll upon the surface of the stream, are full and rounded, though more or less indented at the edge into from three to six obovate lobes. Familiar as is this curious little English plant, the causes which give it its two types of leaves admirably illustrate the laws which we must employ as the general key to all the shapes of foliage throughout the vegetal kingdom.

First, as to the submerged leaves. These organs, growing in the water under the surface, have not nearly so free access to carbonic acid as those which grow in the open air. For the proportion of carbonic acid held in solution by water is very small; and for this small amount there is a great competition among the various aquatic plants. As a rule, the cryptogamic flora of fresh waters consists of long streaming algæ or characeæ, which assume filamentous shapes, and wave about in the water so as to catch every passing particle of the precious gas. When flowering-plants, like the water-crowfoot, take to inhabiting similar spots, their submerged leaves also tend to assume somewhat the same forms, and to move freely with every current in the pond or stream, so as to catch whatever fragments of carbon may happen to pass their way. In this case, there is no dearth of sunshine, no interference of other plants with the incidence of the light; the waving thread-like form depends solely upon the comparative want of carbon in the surrounding medium. The leaves have acquired the shape which enables them best to lay hold on whatever carbon there may be in their neighbourhood; any other arrangement would involve a waste of chlorophyll—a misplacing of it in an unadvantageous position. Full round leaves would be useless under water, because there would not be work enough for them to do there.

On the other hand, when the leaves reach the surface, they have room to spread out unmolested into an area singularly free from competing foliage. Here, then, they plim out at once into a larger rounded type, as they can obtain abundant carbonic acid from the air around, and can catch the unimpeded sunlight on the surface of their pond. The two cases, as Lamarck long since remarked, are somewhat analogous to those of gills and lungs; for though in the one case it is oxygen that is required, and in the other case carbonic acid, yet inasmuch as both are gases dissolved in water, the parallelism on the whole is very close.

It is to be noted, however, that in both cases the central ranunculaceous type of leaf is faithfully preserved in the ground-plan or framework. This central type of leaf is found in a rounded form in the lesser celandine (*R. ficaria*), and in the radical leaves of the goldilocks (*R. auricomus*). It is more divided and cut, or (to put the same thing conversely) less filled out between the ribs in the common meadow buttercup (*R. acris*). But in the water-crowfoot, the floating leaves remain very close to the rounded form of lesser celandine, though a little more lobed at the edge; while in the submerged leaves, we get hardly anything more than an attenuated skeleton of the venation, still essentially keeping up the typical form, though in a somewhat exaggerated and minutely subdivided manner. When one compares these submerged leaves with the equally filiform and minutely dissected submerged foliage of the water-violet (*Hottonia palustris*) and the water-milfoil (*Myriophyllum spicatum*), one sees at once that the same effect has been obtained in the various cases by like modification of wholly unlike ancestral forms. While assuming extremely similar outer appearances, all these plants retain essentially diverse underlying ground-plans.

Furthermore, there are various minor forms or varieties of the water-crowfoot in which minor peculiarities of like import may be observed. The form known as *R. fluitans* lives chiefly in rapidly-running streams, where none of its leaves can reach the surface; hence all its foliage is submerged, and deeply cut into very long, thin, parallel segments, which wave up and down in the rapids, and are admirably adapted to catch the floating particles of carbonic acid carried down by the water in its course. The variety known as *R. circinatus* grows mainly in deep still pools, where also its leaves cannot reach the top; and it has likewise submerged foliage with finely-cut segments, but the separate pieces are "shorter and more spreading," because this form is best adapted to catch the stray dispersed particles of carbonic acid in the quiet waters. The common type (*vulgaris* of Bentham) has both forms of leaves, floating and submerged, and grows mostly in shallow pools or slow streams. The type known as ivy-leaved crowfoot (*R. hederaceus*) creeps on mud or ooze, and has only the full three-lobed leaves. Finally, it may be noted that even the particular position of individual leaves here counts for something; since nothing is commoner than to find one of the finely-cut submerged leaves with a few upper segments floating on the surface; and these upper segments begin to fill out at once into broader green tips, thus giving the end of the leaf an odd, swollen, and bloated appearance.

GRANT ALLEN

(To be continued.)

HERRING AND SALMON FISHERIES

AT a meeting of the Executive Committee of the Edinburgh International Fisheries Exhibition of 1882, which proved so successful, held on Wednesday, February 28, it was resolved, on the motion of Mr. John Murray, seconded by Sheriff Irvine, that the funds at the disposal of the Executive Committee be granted to the Council of the Scottish Meteorological Society to carry out the proposed investigations with reference to the herring, salmon, and other fisheries which are described

in the Circular submitted by the Council with their letter of application to the Committee of May 23, with power to arrange for a zoological station, and with a recommendation that an application be made to Government for assistance. The sum granted is upwards of 1500*l.*

The results already obtained by the Scottish Meteorological Society in connection with the herring fishery show a close relation between the fluctuations of the catches and changes of temperature, wind, sunshine, cloud, thunder, and other weather phenomena. Thus the observations show, for the six years ending with 1878, that a low temperature is attended with large catches, and a high temperature with small catches. Good catches are also had when the temperature fluctuates about the average, and high temperatures, if short continued, scarcely diminish the catches. So far as the discussion of the observations has gone, it appears that the maximum catches are made when the temperature of the sea is about 55°·5, but this point requires further investigation. Thunderstorms, if widespread, are followed for some days with small catches over the region covered by them.

The Council has hitherto been unable, from want of funds, to complete the discussion of the observations already made; to inspect the fishing districts and confer with the fishermen, and thereby secure observations of the fulness and exactness which are required; and to carry on certain investigations in physics and in natural history which are essential to this inquiry. Of the physical investigations may be mentioned the heating power of the sun's rays at different depths of the sea, which appears to have important bearings, directly and indirectly, on the depth at which the herrings are caught. The inquiries in natural history are mainly those which concern the food of the herring and also the food of the animals on which the herrings prey, together with the influence of weather and season on the distribution of these animals in the sea. In carrying out the latter inquiries, the fishermen would be invited to assist, by entering, in schedules prepared for the purpose, observations as to the colour and appearances of the sea-water, due to the presence of minute organisms. As regards the discussion, it will be necessary to make weather maps of Scotland for each day of the fishing seasons—say upwards of 500—in which special prominence is given to charting the temperature, wind, cloud, thunder, and the other elements of weather which affect the fishings,—together with the catch of each day entered on the positions of the maps where they were severally made round the coast. From these maps some of the causes which tend to localise the shoals will become apparent.

The desiderata at present requiring to be supplied in carrying on the investigation of sea and river fishing are these:—1. Fuller and more exact observations of the temperature of the sea at the surface, and at different depths, by the fishermen at the fishing grounds. 2. The resumption of continuous maximum and minimum temperature observations at Peterhead, and the establishment of similar observations at other points round the coast. 3. The observation of maximum and minimum temperatures in other of the more important salmon rivers. 4. Daily temperature of the sea, by boat at some distance from land, at about six selected places. 5. The discussion of past observations, particularly of the herring fishings as described above. 6. Assistance of specialists in carrying on investigations into the food of the herrings, and into the heating power of the sun's rays at different depths.

We are glad to think that with the surplus funds of the Edinburgh Fisheries Exhibition, so wisely disposed of, the Scottish Meteorological Society will be able to prosecute their researches on these points with some hope of a satisfactory result.

NOTES

THE mathematical papers and memoirs of the late Prof. Henry Smith are, we believe, to be collected, and published in two volumes quarto by the Press of his own University. Miss Smith will contribute a biographical introduction; and the general editorship of the work, which will include a considerable quantity of hitherto unpublished material, will be intrusted to Mr. J. W. L. Glaisher.

IN NATURE for February 1 we gave a brief account of the remarkable results obtained by Prof. Lemström with his network of wires arranged up the face of the mountain at his station at Sodankylä, in North Finland. By this means he succeeded in procuring an appearance exactly similar to that of the aurora borealis. In connection with these experiments Mr. G. A. Rowell, assistant in the Natural History Department at Oxford, has issued a circular calling attention to the suggestion made by him forty years ago in reference to similar experiments. "My views on the cause of auroræ," Mr. Rowell states, "are that they result from electricity carried over with vapour by the superior trade-winds, from tropical to polar regions, and its occasional accumulation in the latter to such a degree as to flash back to lower latitudes, through the atmosphere at a reduced density, but still within the regions at which vapour is flotable although in a frozen condition. The directive properties of the magnetic needle I attribute to the return current of electricity from polar to tropical regions. The following is the concluding paragraph of the report on my paper on this subject:—'The author supports his opinion by general reference to the observations on the aurora, &c., in the appendix to Capt. Franklin's "Journey to the Polar Seas,' and concludes with proposing the experiments of raising electrical conductors to the height of the clouds in the *frigid regions during the frosts in winter*, which in his opinion would cause the aurora to be exhibited and lead to important discoveries in the science of magnetism.'"—(*Report* of the British Association, 1840, *Transactions* of the Sections, p. 49.)

DURING the past winter, the weather in Shetland and the north has been more stormy than for a number of years. In evidence of the severity of the weather, the inhabitants of the Island of Foula, which lies about eighteen miles to the west of Shetland were only able last week for the first time this year to cross to the mainland in their boats. The large supplies of food laid in, as is usually done, were in many cases exhausted, and several families were only saved from starvation by help received from neighbours who were better supplied.

ARRANGEMENTS have been completed for an exhibition, on an important scale, of hygienic dress, sanitary appliances, and household decoration, under Royal and distinguished patronage, and under the direction of the National Health Society, at Humphreys' Hall, Knightsbridge. The exhibition will be opened on June 2 next. The exhibits will be divided into seven classes, and besides hygienic, rational, and artistic dress, will include food-products, appliances for the sick-room, home nursing and home education, industrial dwelling and cottage hygiene, the sanitation of the house and hygienic decoration, heating, lighting, and cooking apparatus, fuel, &c. The Superintendent is Mr. E. J. Powell, 44, Berners Street, W.

THE National Smoke Abatement Institution is making arrangements for opening a permanent exhibition in a central part of London in an extensive range of buildings, for the display of apparatus, fuels, and systems of heating, combining economy with the prevention of smoke, and the best methods of ventilating and lighting. The exhibition will be free to the public, and will include examples of all the most recent inventions and improved apparatus. A lecture hall for the reading of papers, and instruction classes will be provided; also testing rooms under the

supervision of experts, for the purpose of continuing the series of tests and trials commenced in connection with the South Kensington and Manchester Smoke Abatement Exhibitions of 1882. Particulars may be obtained at the offices of the National Smoke Abatement Institution, 44, Berners Street, Oxford Street, London, W.

THE Executive Committee of the International Fisheries Exhibition have come to a decision to light their galleries by electricity, and they have already made arrangements for the illumination of fully two-thirds of the area. Messrs. Davey Paxman and Co. have undertaken to supply the necessary motive power, which has been estimated at little less than 700 horse-power.

THE International Medical Congress, which, in accordance with the resolutions of the Italian Congress of last year, is to be held this year in Holland, will take place at Amsterdam, during the Colonial Exhibition, from September 6 to 8 next.

WE have on good authority the following instance of the liberality of Dr. Oscar Dickson, who has contributed so largely to the various expeditions of Baron Nordenskjöld:—An energetic Swedish botanist, Sven Berggren, was some years ago engaged in studying the flora of New Zealand, of which he gave some account in the Swedish *Aftonblad*. In one of his letters he stated, however, that his studies would have to be discontinued from want of funds. The next day a sum of 1000*l*. was received anonymously by the *Aftonblad*, with instructions to forward it to Herr Berggren. It was only many years after that it leaked out that the generous donor was Dr. Oscar Dickson.

PART IV. of Mr. Distant's "Rhopalocera Malayana" appeared this week. A complete synoptical key is given to the genera, and the geographical distribution of the genera and species is fully described. An attempt is made to allude to all biological facts which can illustrate or explain the many complexities in the distribution and economy of Malayan butterflies, and to draw attention to the different theories which have been promulgated to account for the same. The work may thus prove useful as an introduction to the study of Rhopalocera. Already it has assumed much larger proportions than estimated owing to the number of additional species recently received or found in other collections. Woodcuts have also been given, and the plates are equal to anything yet produced by chromolithography. Mr. Distant's work deserves every encouragement.

AN International Congress for the Protection of Animals is to be held at Vienna in September next. A great number of local societies, such as those of Berlin, Cologne, Munich, Dresden, Hanover, &c., besides several Spanish, Italian, and Russian, have expressed their intention of being represented at the Congress. Anti-vivisectionist societies will not be invited, as the promoters of the Congress, eminent men of science, do not consider them as societies for the protection of animals, and hold them to be generally incompetent regarding questions relating to such protection.

THE Dutch press considers the demand made by Baron Nordenskjöld perfectly legitimate and just.

THE death is announced of Dr. Bertillon, the well-known French anthropologist and statistician.

AT its January meeting, the Russian Chemical and Physical Society awarded its Sokoloff premium to Prof. Menshutkin, for his researches into the influence of isomerism of alcohols and acids on the formation of compound ethers.

IT is interesting to examine the items in the budget of Norway for the ensuing year, which has just been issued, relating to the "extraordinary" grants made in that country for the benefit of science. The following are some of the donations for this year:—To the academies of science in Christiania and Throndhjem, 600*l*.; the museums of Bergen, Stavanger, and Tromsö, 900*l*.; travels of scientific students abroad, 350*l*.; the European geodetic commission, 400*l*.; international observations of the physical condition of the polar regions, 700*l*.; *Archiv* of mathematics and natural sciences, 70*l*.; other scientific journals, 130*l*.; a new natural history journal, 70*l*.; "further," towards the publication of the works of the distinguished Norwegian mathematician, Abel, 100*l*.; a work by Herr Norman on the Arctic flora of Norway, 350*l*.; Herr Tromholt for the study of the aurora borealis, 60*l*.; the *Acta mathematica*, 60*l*.; scientific study of the Norwegian sea fisheries, 300*l*.; for the artificial hatching of salmon ova, 90*l*.; geological researches of Southern Norway, 600*l*.; the society for promoting the Norwegian fisheries in Bergen, 1600*l*.; publication of the reports of the North Atlantic expedition, 100*l*. These amounts, as well as the 3000*l*. granted towards the expenses of the Fishery Exhibition in London, are all in addition to the ordinary subsidies of the year.

THE Swedish Government has granted a sum of 60*l*., for this year, to an entomologist, whose duty it will be to advise farmers as to the best means of destroying injurious insects.

WE are informed by the secretary of the Society of Telegraph Engineers and of Electricians that the Crown Prince of Austria has consented to become patron of the Vienna Electrical Exhibition, and that the Emperor has signified his intention of devoting some highly decorated rooms for the purpose of testing the effects of incandescent lighting in connection with various styles of decoration. The time fixed for the receipt of applications for space has been extended from the 1st to the 20th inst., by which latter date they should be in the hands of the Secretary of the Society, 4, The Sanctuary, Westminster. We are also authorised to state that the Committee at Vienna are making arrangements for a reduction in the rates of transit on all goods forwarded to Vienna for exhibition.

IT is a common belief among persons who keep poultry that the shocks and tremors to which eggs are subject during transport on road or railway affect the germ contained in the egg. M. Dareste, who has been studying this matter (*Comptes Rendus*), found, a few years ago, that in eggs submitted to incubation directly after a railway journey, the embryo very generally died; but a few days' rest before incubation obviated this. He has lately inquired into the effect of shocks on the fecundated egg-germ, with the aid of a *tapoteuse*, or machine used by chocolate-makers to force the paste into the mills; it gives 120 blows a minute. Monstrosities were always the result of the tremors so caused. This teratogenic cause is the more remarkable that it acts before the evolution of the embryo; whereas the other causes M. Dareste has indicated, as elevation or lowering of temperature, diminution of porosity of the eggshell, the vertical position of the egg, and unequal heating, only modify the embryo during its evolution. The modification impressed on the germ by those shocks did not disappear after rest, as in the case mentioned above; but it is not known why. A few eggs escape the action.

THE radiometer is an instrument which may render good service in the hands of the teacher. Prof. Rovelli has been showing this (*Riv. Sci. Ind.*), and among other experiments he suggests are these:—Placing the instrument at the focus of a parabolic mirror, while a mass of snow is put at the focus of a like mirror facing the first a little way off; placing it, with sulphuric ether, under the bell-jar of an air-pump, and exhausting, afterwards letting in the air (the motion is opposite after the air is admitted); exposing the radiometer at the focus of a parabolic mirror turned towards the weak light reflected from snow, on a cloudy day, then turning the mirror away from the snow. Prof.

Rovelli finds that 8° of dark heat neutralise the effect of the weak light emitted by a common candle at the distance of 45 centimetres from the radiometer. The instrument may serve advantageously to demonstrate the relation between the absorptive and the emissive power of bodies, and to determine their respective values.

M. FERRY, the new Premier in the French Cabinet, as well as Minister for Public Instruction, will deliver the usual address to the Congrès des Sociétés Savantes at the end of this month.

M. HOUZEAU, the director of the Brussels Observatory, has returned from San José, but has obtained leave from his Government, and will spend the remaining part of the winter at Cannes. The King of Belgium is anxious to have the Observatory transferred to Laeken, to an eligible site placed in the vicinity of his castle, but nothing is decided in that respect. A temporary shed has been erected for the new meridian circle by Repsold, but the readings are taken with the old one.

M. SHULACHENKO, who managed the Russian military telegraph during the Kulja expedition, communicates to the Russian Physical Society the following results of his experiments with Siemens' telephones:—At a distance of 93 miles, music, singing, and speaking were heard quite distinctly; at 130 miles, conversation was difficult,—it was necessary to shout loudly, and those who received messages had to display a great sensibility of ear; but it was possible to have conversation even at a distance of 212 miles. When six pairs of telephones were put side by side, having each its wire, and the wires not being connected with one another, the conversation on one of them was heard on all the others. When the connecting wire of one pair of telephones was broken, the conversation on this pair was heard on the next pair of telephones the wire of which was in good state.

A COMMEMORATIVE stone has been placed on the house No. 17 in Via Dei Prefetti, Rome, to Morse, the telegraphist. The inscription was as follows, translated into English:—"Samuel Finkez Breese Morse inhabited this house from 20th February, 1830, to January, 1831, inventor of the writing electromagnetic telegraph. He was born at Charlestown 27th April, 1791; died at New York 2d April, 1872."

THE last number of the *Izvestia* of the Russian Geographical Society gives interesting particulars of the naphtha-wells in the province of Ferghana, in Turkistan. There are no less than 200 wells which are situated at the foot of both mountain ridges that inclose the valley of Ferghana. One range of wells, twenty-seven miles long, is situated on both banks of the Naryn, twenty miles north of Namangan. The other, about sixty-five miles long, is situated in the latitude of Makhram, in the districts of Marghilan and Kokan. There is a third intermediate group some thirty miles east of Andijan. The wells are situated in the limestones and slates of the "Ferghana level" of the chalk formation. The specific weight of the Ferghana naphtha is 0·950 at 17° Cels., 0·9517 at 28°, and 0·945 at 43°; it belongs therefore to the heavy mineral oils. The heavier parts remaining after the evaporation of naphtha in open air are known under the name of *khilk*, and when mixed with sand give an excellent waterproof cement, sometimes used by natives for irrigation canals. There are also mines of mountain-wax on the Kok-tube Mountain, in the district of Namangan, and a very good mine of sulphur at Karim-duvany.

M. DOMOJIROFF continues to publish in the *Izvestia* of the Russian Geographical Society his anemometric observations on board the clipper *Djighit*. In June, 1881, during the cruise from the Zond Strait to the Seychelles Islands, he met mostly with south-east winds, the velocity of which varied from 3 to 7·5 metres per second, with one exception, on June 9, when it reached 15 metres. On the cruise from the Seychelles to Aden, from June 25 to 30, the wind was mostly south-west, and varied from 5 to 12·7, reaching 14·3 metres per second on June 29. The observations are carried on in the same way as was described in a preceding number of NATURE.

THE young West Siberian branch of the Russian Geographical Society proposes to publish in its next volume of *Memoirs* a botanical description of the district of Tara, which has the interest of having an intermediate flora between the forest region and the Steppes, the Irtish being a boundary-line between the two. The same Society continues the excavation of several koorgans in the district of Yalutorovsk.

FROM various parts of the Greek Archipelago and from the Pelikon district continued volcanic phenomena are reported. The neighbourhood of Volo in Thessaly is particularly affected. Also the island of Chios seems again to be a centre of disturbance. The volcano at Santorin is very active.

ON February 16, at 8.10 a.m., a slight earthquake was noted at Bologna and the whole Southern Romagna. Mount Vesuvius increased its activity on that occasion.

A DISCOVERY, which is expected to throw some light on prehistoric times in what is now Germany, has been made near Andernach on the Rhine. Remains of prehistoric animals have been found in a pumice-stone pit, and Prof. Schaaffhausen of Bonn has investigated the spot closely. A lava-stream underlying the pumice-stone was laid bare, showing a width of only two metres. The crevices between the blocks of lava were filled with pumice-stone to a depth of one-half to one metre; below this, however, there was pure loam and clay, and in this were found numerous animal bones, apparently broken by man, as well as many stone implements. It is supposed that there was a settlement there, of which the food-remains fell into the lava-crevices *before* the whole was covered with pumice-stone.

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Miss Annie M. Davis; an Ocelot (*Felis pardalis*) from South America, presented by Mrs. A. Harley; a Grey Ichneumon (*Herpestes griseus*) from India, presented by Miss G. Gordon Clark; a Black Rat (*Mus rattus*), British, presented by Mr. H. B. Stott; a Tawny Eagle (*Aquila nævioides*) from South Africa, presented by Mr. Roland Trimen, F.Z.S.; a Slender-billed Cockatoo (*Licmetis tenuirostris*) from South Australia, presented by Mr. A. Anderson; a Common Magpie (*Pica rustica*), British, presented by Mr. Charles Davis; a Ring-necked Parrakeet (*Palæornis torquatus*) from India, presented by Miss Bibby; a Common Curlew (*Numenius arquata*), a Golden Plover (*Charadrius pluvialis*), British, purchased.

OUR ASTRONOMICAL COLUMN

THE COMET 1883 *a*.—In a circular issued from the Imperial Academy of Sciences, Vienna, are the following elements of a comet discovered at Rochester, N.Y., on the 23rd ult., founded by Dr. Hepperger upon observations on February 24, 25, and 26.

Perihelion passage, February 20·20206 M.T. at Berlin.

Longitude of perihelion	33 23 51	M.Eq. 1882·0.
,, ascending node	280 4 20	
Inclination	77 32 48	
Logarithm of perihelion distance ...	9·879124	

Motion—direct.

Prof. Millosevich kindly communicates observations made at the Collegio Romano in Rome:—

	Rome M.T.	R.A.	Decl.
	h. m. s.	h. m. s.	° ′ ″
Feb. 28 ...	7 43 12	23 43 19·58 ...	+31 37 54·5
March 1 ...	7 53 14	23 53 1·27 ...	+31 49 7·2

From Prof. A. Riccò, who writes from Palermo on February 28, we learn that he has found the spectrum to be formed of the three bands of hydrocarbons, with an extremely faint continuous spectrum of the nucleus; the sodium line (D) was not present.

The comet is receding from the earth as well as from the sun. The elements have but little similarity to those of any comet previously calculated.

THE GREAT COMET OF 1882.—Prof. Julius Schmidt has published some particulars of his observations of this remarkable body since the commencement of the present year. On Jan. 3 the tail was traced through upwards of 11° with the naked eye; on the 10th it was visible for 8°, on the 28th it had diminished to 5½°, but was readily seen without the telescope; on the 30th its length was 3°. On February 5 a tail 2° in length was perceptible to the naked eye; Prof. Schmidt obtained his last distinct glimpse of the comet without the telescope on February 7.

Dr. B. A. Gould, director of the Observatory at Cordoba, who is now in London *en route* for the United States, informs the writer, that on February 11, three days out from Rio Janeiro, he was satisfied of the visibility of the tail of the comet to the naked eye; its distance from the earth at this time was 2·48, and its distance from the sun 3·05.

THE VARIABLE STAR U CEPHEI.—Mr. G. Knott secured a good observation of the minimum of this variable, at Cuckfield, on the night of March 2. An uninterruptedly clear sky enabled him to keep a watch on the star from 7h. 24m. to 14h. 30m. G.M.T. At about 8h. 15m. it began to fade from 7·2m., and at 14h. 30m. it had risen again to 8·1m. The observed time of minimum was 12h. 36m., or seven minutes earlier than the time assigned in the ephemeris in NATURE, and the magnitude at minimum was 9·45. The star remained at minimum for nearly 2½ hours. The low magnitude attained, Mr. Knott considers, is confirmatory of a suggestion he made from his earlier observations, that at alternate minima the star touches a lower magnitude than at those which intervene.

NEW NEBULÆ.—M. Stephan, director of the Observatory at Marseilles, publishes a catalogue of fifty nebulæ observed there, forty-five of which he believes to be new. A group of four pretty bright nebulæ he gives as identical with *h*, Nos. 2352, 2356, 2358, and 2359, but their relative positions resulting from his observations are not in accordance with Sir John Herschel's Catalogue. The Marseilles places and descriptions are—

	R.A. 1880·0.			N.P.D.			
	h.	m.	s.	°	′	″	
No. 42 ...	11	9	8·45 ...	71	14	8·7	Assez belle, assez petite, ronde, condensation centrale.
,, 43 ...	11	10	28·49 ...	71	19	39·1	Assez belle, assez petite, ronde, condensation centrale.
,, 44 ...	11	10	36·52 ...	71	17	35·0	Belle, ronde, assez étendue, condensation graduelle centrale très forte.
,, 45 ...	11	10	40·73 ...	71	11	46·7	Assez belle, ronde, condensation graduelle centrale assez forte.

The catalogue is published in the *Comptes Rendus de l'Académie des Sciences* of February 26.

GEOGRAPHICAL NOTES

WE are now enabled, on the authority of Dr. Oscar Dickson, to give the following particulars of the programme of Nordenskjöld's proposed expedition:—The expedition will leave Sweden early in May next, in all probability in the Government steamer *Sophia*, and if the state of the ice is favourable to a landing on the east coast this will be effected; but as this is not expected to be the case until later in the season, Baron Nordenskjöld will proceed to the west coast, not for geographical discovery, but to study the appearance and extent of the inland ice on this side before attempting to penetrate from the eastern side. There are also known to exist on the west coast some very large blocks of ironstone, perhaps of meteoric origin, which a party of the expedition will be despatched to examine. When these researches are finished, and the state of the ice more favourable, the vessel will make her way from Cape Farewell along the eastern shore in the open channel, which is generally found between the coast and the drift-ice. With regard to the "break" or oasis, believed by Baron Nordenskjöld to exist in the interior of Greenland, to which we have previously referred, the explorer has been led to this conviction during his wanderings on the inland ice on a former occasion. He maintains that not only the constant advance of the ice-mass, but the fact that the country does not rise continually in the interior, show that the whole land is not covered with perpetual snow and ice; and this theory, he states, has been further corroborated by the studies made by him and others of the temperature and moisture of the air on the inland ice. The expedition, which will be accompanied by a complete scientific staff, will also aim at studying the conditions of the drift-ice between Iceland and Cape Farewell, the fossil remains in Greenland, as well as the appearance and quantity of the cosmic dust there. One object will also be, if possible, to discover traces of the former Norse settlements. It is expected that the party will return in September next. We understand that the reason why Baron Nordenskjöld has not issued any official programme concerning his expedition is that, being occupied with preparations for his journey and public duties, he would not be able to enter into any critical controversies as to his plans and theory.

IT appears from a letter of Dr. L. E. Regel to the Secretary of the Russian Geographical Society, that this Central-Asian traveller successfully pursued his explorations during last summer. He left Samar-land at the end of June last, and to reach Hissar he chose the shortest route, *viâ* Penja-kent. This route, by which the expedition visited the Fan River and Lake Iskander-kul, and crossed the Mur Pass, was very difficult; but the botanical collections and the geographical results were all the richer. In the centre of this region is situated a great mountain range, whose summits—the peaks of Kuli-kalan and the Chandar and Bodhan Mountains—are seen from Samarkand. To the south of this range runs the Saridagh valley, beyond which rises the Hissar range proper; to the north it has the Kul-i-kalan plateau and the valleys of a tributary of the Voron and the Pasrut River. The plateau of Kul-i-kalan has a circumference of about thirteen miles, and is dotted with five lakes 10,000 feet above the sea-level. The mountains around it have no real glaciers, but there are old moraines which can be traced also along the tributary of the Voron, which is fed by one of these lakes. We have here a separate Alpine landscape, the mountains of which are mostly fossiliferous limestones (sandstones with casts of thick fossil trees are found in the Pasrut valley), and with a vegetation not only richer than that of any other part of the basin of the Zarafshan, but also more varied as to its distribution. The forest vegetation is richest in the zone between 4000 and 8000 feet above the sea-level: M. Regel found there apple, cherry, and nut trees, together with the *Archa*. The upper zone, where the *Archa* also predominates, contains birches, willows, and an arborescent *Ephedra*; it reaches 10,500 to 11,000 feet, and the vegetation altogether goes higher up than the limit of perpetual snow. The Mur Pass—about 14,000 feet high—is very steep; the expedition had to cross snow-fields for nearly four miles, and found on the southern slope immense accumulations of snow, which probably is due to the foggy climate of Hissar,, although the amount of rain is small in this region. The vegetation of the southern slope is very rich and much like that of Karateghin. The range is composed of syenite; the next range, of the same height, between Khoja-Hassan and Hakimi, consists of granite, syenite-gneiss, and fossiliferous slates. Between Hakimi and Karatagh there is a series of lower parallel ridges, consisting of fossiliferous sandstones. The same sandstones are met with also between the two main ranges; they contain fossils at Khoja-Hassan. Changing his former plan, M. Regel proceeded further directly to Kala-i-Khumb, while his topographer was despatched to Kulab, *viâ* Hissar, the two to meet in the Darvaz. The remainder of M. Regel's letter gives several interesting topographical details, and information about different routes, as well as an enumeration of the chief questions that must be resolved as to the topography of this region.

WE announced last week that a Danish expedition would explore the east coast of Greenland during the summer. The funds required for this expedition were voted by the last Danish Parliament, and it will consist of two lieuteuants in the navy, G. Holm, and T. Garde, with two scientific men, but the remaining members will be natives of Greenland. The expedition will only employ boats for their purpose.

THE Ural Mountains are again becoming the field of exploration for Russian geologists and geographers. We learn from the *Izvestia* of the Russian Geographical Society that M. Nasi-

loff is spending a third year in the exploration of the Northern Ural. After having explored the river Lala under 59° N. lat., where he discovered layers of spherosiderites which were not yet known on the eastern slope of the Ural Mountains, he explored the banks of the Sosva—their geological structure, and the koorgans that are met with in its basin, as well as the fauna and flora of the region. In 1882 he visited the banks of the Lozva and Sosva, and the old mines of this locality, and made large geological, botanical, and ethnographical collections. He followed the Lozva to its junction with the Tavda, and went up the Sosva. The collections brought home by M. Nasiloff are now in the Mining Institute, in the St. Petersburg University, and in the Geographical Society. Another member of the Geographical Society, M. Malakhoff, continued his zoological and ethnographical researches on the Middle Urals. He explored the lake-dwellings discovered in the neighbourhood of Ekaterinburg, and, together with a member of the Mineralogical Society, explored the 3000 feet high mountain, Kachkanar, making there interesting collections of plants and insects. Later on in the summer he visited the districts of Irbit, Ekaterinburg, and Tvitsk, and discovered close by Irbit very interesting accumulations of bones, lake-dwellings on Lake Ayat, containing large implements of slate, and finally stone and bone implements in a cavern close by the Mias ironworks. At Lake Bagaryak he discovered interesting forms for casting animal and human figures during the prehistoric epoch.

HARTLEBEN of Vienna has published a unique little work by Dr. Jos. Chavanne, on "Afrika's Ströme and Flüsse," in which the author briefly surveys the hydrography of Africa as far as recent discoveries have furnished them. The book is accompanied by a well-drawn hydrographical map.

IN the March number of Hartleben's *Deutsche Rundschau* for geography and statistics, Dr. Chavanne has a sketch of the progress of discovery in Africa during 1882. There are interesting biographies, with portraits, of General Strelbitski and the late Prof. Henry Draper.

THE following papers will be read at the third German "Geographentag," which will be held at Frankfort-on-the-Maine on the 29th-31st inst:—On the importance of Polar research to geographical science, by Prof. Ratzel (Munich); on the commercial conditions of South Africa, by Dr. Buchner (Munich); on the significance of the International Colonial Exhibition at Amsterdam with regard to geographical science, by Prof. Kan (Amsterdam); on the reciprocal relations of climate and the shape of the earth's surface, by Dr. Penck (Munich); on the means of determining the geographical position at the time of great discoveries, by Dr. Brensing (Bremen); on the latest efforts made to determine more accurately the shape of the earth, by Dr. Günther (Ansbach); memoir of Emil von Sydow, by Dr. Cramer (Gebweiler); on topography as an introduction to geography, by Dr. Finger (Frankfort); on the pedagogic requirements and principles in drawing wall-maps for the use of schools, by Herr Coordes (Cassel); on the method of representing various objects on maps, by Prof. Jaroslaw Zdenek (Prague); on the Prussian teaching order and examination with reference to geographical instruction, by Dr. Kropatschek (Brandenburg); on the geographical handbooks of M. Neander, by Dr. Votsch (Gera). Three other highly interesting papers are also promised, viz. notes from his botanical journeys in Tropical America extending over five years, by F. R. Lehmann; on the Balkan Mountains, by Prof. Toula (Vienna); and a report on his great journey across Africa, by Lieut. Wissmann.

NEWS from Zanzibar, dated November 8, 1882, brings the sad announcement of the death of Dr. Kayser, who had been sent by the German African Society to their station on the shores of Lake Tanganyika, together with Drs. Boehm and Reichard, and who had left his station and was on his way to the Gold Coast.

THE CONSERVATION OF EPPING FOREST FROM THE NATURALISTS' STANDPOINT[1]

THE great expanse of primitive woodland in the immediate neighbourhood of East London declared "open" to the public on May 5, 1882, by Her Majesty the Queen, should be regarded as one of the numerous bequests to posterity marking the enlightenment of our times. The feelings leading to the agitation for the preservation of open spaces in and around the metropolis are sure indications on the part of the public of a recognition of the necessity for protecting and conserving our common lands for outdoor recreation—a recognition which must be considered as marking a decided advancement in the ideas of the British holiday-maker. If we compare a map of the environs of London of, say, twenty years ago, with the actual state of the country at the present time, it will be seen that large tracts of open land have disappeared; shady coppices and furze-clad heaths have been inclosed and built upon, and the country-loving Londoner has had to go further and further afield for his rambles. If it is obviously true that increased pressure of population demands more dwelling accommodation, it is equally true that a denser population requires more open spaces. The indifference of the public in former times to their own rights and to the wants of their successors is naturally making itself more and more seriously felt with a rapidly augmenting population and a corresponding spread of buildings. The formation of such public bodies as the Commons Preservation Society and the Epping Forest Fund was a healthy sign that people were beginning to be alive to the gravity of the situation, and we may now fairly say that rural London is on the defensive. The remarks which I am about to offer on the present occasion are based on an unpublished article written many months ago, when that wooded area in which our interest as a society centres was threatened by tramway invasion. The withdrawal of the Great Eastern Railway Company's bill for extending their line from Chingford to High Beech in 1881, and the apparent collapse of the tramway scheme had led to the hope that the "people's forest" would remain unmolested, and that the Epping Forest Act of 1878 would be carried out in spirit and in letter. But unfortunately new grounds of alarm have recently arisen, and our honorary secretaries, to whom I showed the original manuscript, did me the honour of thinking that the views which I had expressed would still be found to be in accordance with those of our own and kindred societies.

Like other open tracts in the metropolitan district, the great Waltham Forest, which comprised the forests of Epping and Hainault, was rapidly undergoing absorption. From the report of the Select Committee of the House of Commons presented in 1863, it appears that of the 9000 acres which constituted the Forest in 1793, only 6000 acres then remained uninclosed. In 1871, when the Corporation of London took up the Forest question, this area had been reduced to 3500 acres. I do not here propose to trouble you again with the now familiar history of the rescue of this picturesque remnant of primeval Britain (see Mr. J. T. Bedford's "Story of the Preservation of Epping Forest," *City Press* Office, 1882). The work—commenced more than a decade ago by the Corporation of London—received its crowning reward at the late Royal visitation. We shall the more appreciate the results of the action taken by the Corporation when we bear in mind that the total area dedicated to the public last May is very nearly equal to the expanse of 6000 acres reported upon by the Select Committee of 1863. But whilst expressing the gratitude of metropolitan field naturalists generally for the restoration of one of their largest and most accessible hunting grounds, it certainly does seem to me that the shout of triumph raised by the Conservators has been allowed to drown the smaller voices of those who had previously demonstrated to certain rapacious lords of manors by somewhat forcible means that a "neighbour's landmark" was not a movable thing. It must not be forgotten that prior to the year 1871, besides many vigorous individual protests, both the Commons Preservation Society and the Epping Forest Fund had declared war against illicit inclosure. The restoration of the Forest to the people has cost a sum of money considerably exceeding a quarter of a million pounds sterling, and it will be generally admitted that this amount has been well if not wisely spent in the public cause. There are no doubt many who have suffered by their own cupidity, or by that of former manor lords, who still feel aggrieved at the action of the Corporation, and it must indeed be conceded that many whose estates have suffered curtailment have been the unconscious receivers of illegally acquired property and are thus deserving of commiseration. The principles involved in the conflict between public rights on the one hand and manorial actions on the other are of the very deepest importance to the community at large, and it is therefore no matter of surprise that the "Forest Question" should have acquired

[1] Being a paper read before the Essex Field Club, at the meeting held on February 24, by Raphael Meldola, vice-president of the Club.

a quasi-political aspect during the last few years in this neighbourhood.

As far as I have been able to learn, the motives leading to the preservation of our Forest at the great cost specified appear to have been purely philanthropic. The main object was to secure this splendid area for the "recreation and enjoyment" of Londoners generally, and more especially for the East-End inhabitants, whose chances of holiday-making are only too often limited to an occasional day in the country. In one sense the latter class may now, thanks to the movement first set in action by Mr. J. T. Bedford, claim to have a decided advantage over their wealthier West-End brethren, for the total area of the West-End parks (including Regent's) amounts only to about 1150 acres as compared with the 5000 to 6000 acres of open country so easily accessible to East Londoners. In the face of such an obviously enormous gain to the country rambling holiday folk, it may perhaps seem ill-advised to attempt to criticise the action of the Conservators in their dealings with the Forest. It is with great reluctance on my part that I forsake the peaceful paths of scientific study to take up a question which generally appears to lead to nothing more than a manifestation of angry controversy, and I only do so now on behalf of that numerous and ever increasing scientific class of holiday-makers whose claims thus far appear to have been altogether put out of court.

Long before the question of encroachment or of preservation had been brought into its present prominence, botanists, entomologists, microscopists, and students of nature generally were in the habit of frequenting our Forest and of rambling in quest of the objects of their study through this woodland expanse so conveniently situated with respect to the great scientific centre of this country. There are records which prove that Epping Forest has been for more than a century the hunting ground of many who have gathered materials from its glades for the great storehouse of human knowledge, and who have taken a true and purely intellectual delight in studying its animal and vegetable productions. The London naturalists of the present time should surely have something to say in connection with the fate of this favourite haunt, made classic ground to them by the memories of such men as Richard Warner, the author of the "Plantæ Woodfordienses" (1771), Edward Forster, the Essex botanist, who wrote between the years 1784 and 1849, and Henry Doubleday, of Epping, our own grocer-naturalist, who died in 1877. It is time for the natural history public, by no means such an insignificant body as is generally supposed, to raise their voice on behalf of these "happy hunting grounds." The position to be taken up is not necessarily one of antagonism towards the Conservators, but it is certainly desirable that some understanding should be come to respecting the claims of those who, in pursuit of knowledge, have long been contented to bear with the pitying smile of the ignorant for "trifling away their time upon weeds, insects, and toadstools." The numerous scientific societies and field clubs of the metropolitan districts have already declared their views on former occasions, and it is chiefly with the object of attempting to define the respective attitudes of the parties concerned that I have entered the arena on the present occasion.

There are at the present time more than twenty Natural History Clubs in the environs of London, and of these many have long been in the habit of making collecting excursions to our Forest. Our own Society and our Walthamstow colleagues have their head-quarters in the Forest district. Some of the East-End clubs are entirely composed of working men, and have done excellent work in fostering a healthy taste for the study of out-door natural history among this class of the community, a matter of considerable importance to us when we so often hear that the Forest has been acquired as a recreation-ground chiefly for the working men of East London. In addition to these numerous local clubs, there are the great London Societies, which, like the Linnean, Zoological, Entomological, Royal Microscopical, and Quekett Club, are all interested in promoting the study of biology in its various branches. Now, in face of the rapid destruction of all the truly wild tracts of country in the vicinity of London, it must assuredly be of the greatest importance to the natural history public as a body to watch with a most jealous eye the dealings by those in authority with this the largest, wildest, and most accessible of all the open spaces in the metropolitan district. To naturalists generally such a tract of primitive country as that which has come under the management of the Corporation is something more than a mere picnic-ing ground—to all students of nature it is a *biological preserve*. Nay, I will even go so far as to declare that forest management is essentially a scientific subject in itself—a natural history question in the broadest sense. Now with the exception of our esteemed members, the Verderers, by whom we were invited to a conference some months ago, it appears to me that the Conservators as a body—and a confessedly unscientific body—are not aware that scientific counsel is necessary to enable them to faithfully carry out the Act of Parliament, *i.e.* to keep the area committed to their charge in its "natural aspect" as a forest. I will therefore take the present opportunity of pointing out that scientific criticisms would have been disarmed and the fears of natural history students allayed if the Epping Forest Committee had only recognised the claims of science by consulting, let us say, the Directorate of Kew Gardens, or by appealing to the Councils of some of the London Societies.

If we consider the actual work done during the period that the Forest has been under the jurisdiction of the Corporation, we may fairly say that the energies of this body have hitherto been developed in the direction of landscape gardening; *i.e.* of *artificialising* certain portions of the Forest. The great hotel at Chingford has been made the centre of convergence of a number of roads, some of which have been newly cut even at the risk of being superfluous. The aquatically-disposed holiday-maker may hire boats in which he can paddle about on an "ornamental water," or can embark on a floating machine turned by hand-paddles, and possibly constructed with a view to delude the occupants into the belief that they are on board a steamer. The exhausted East Londoner whose vitality appears to require that recuperation which seems to be derivable from swinging, steam-roundabouts, and throwing sticks at cocoa-nuts, has been amply provided for, and his wants have in every way been attended to. In 1881 the Forest was threatened by a railway; in 1882 by a tramway, and again this year another railway bill is about to be introduced into Parliament. To all these schemes the Committee, no doubt with the best motives, gave and still give their support, and one has to seriously ask what is the meaning of the word "conservator," and how far this attitude is compatible with the instruction that "the Conservators shall at all times as far as possible preserve the natural aspect of the Forest," and "shall by all lawful means prevent, resist, and abate all future inclosures, encroachments, and buildings, and all attempts to inclose, encroach, or build on any part thereof, or to appropriate or use the same, or the soil, timber, or roads thereof, or any part thereof, for any purpose inconsistent with the objects of" the Act of 1878. It must not be supposed that there is any desire on the part of naturalists to exclude the general public. I wish only to emphasise the fact that up to the present time it would appear that the Forest has fallen into the hands of those who are disposed to regard it exclusively from the point of view of excursionists and "cheap trips," and in accordance with the principle that supply and demand act and react, it may be expected that this class—which has thus far alone been catered for—will more and more frequent the Forest district. Increased accommodation for excursionists means, if we may judge from the line of action pursued by the Conservators, an extension of facilities for swinging and donkey-riding. The "improvements" that have hitherto been made have not been of such a nature as to preserve the woodland in its native beauty, but have been limited to the conversion of a portion of the Forest land into a resort for pleasure-seekers of the class indicated. To the naturalist—and I am sure I may say to the intelligent public generally—such a tract of primitive country is beautiful only so long as Nature is given full sway, and the adjustments which for long ages have been going on slowly and silently under the operation of natural laws remain unchecked and uninterfered with by man. No unscientific body of Conservators can possibly realise to the fullest extent the seriousness of the charge committed to their care.

With respect to the management of the Forest, the views of naturalists are now so well known that no excuse can be made for ignoring them. Our wants are of the simplest and most economical nature—our case is perfectly met by the trite aphorism, "let well alone." The whole Forest area at present existing may be considered to consist of primitive woodland and of tracts formerly under cultivation. The former can best be dealt with by leaving the "management" to Nature; whilst the latter should be naturalised as soon as possible. And here we cannot close our eyes to the fact that while a large amount of money has been expended in altering portions of the Forest proper, no

attempt has yet been made to plant or to restore to a natural condition those unsightly tracts which were formerly inclosed, and of which many remain as barren wastes to the present time. The cause of the naturalist is thus imperilled both by the active and by the passive position of the Committee—he is like the pitcher in the Italian proverb, which says that "whether the pitcher hits the stone or the stone hits the pitcher, it is always the worse for the pitcher."

It is now quite unnecessary to make detailed statements of the views of individual naturalists with reference to the present subject. It will be remembered that at a meeting of this Society held last year, Sir Thomas Fowell Buxton brought forward a proposal—and a very excellent one it was—that all landowners round the Forest district should agree to stop generally the destruction of all birds and animals on their estates, so that a great experiment might be carried out for some years, leading to a true "balance of nature" in the whole area comprised between the valleys of the Lea and Roding. At the discussion arising from that suggestion the preservation of the fauna and flora as a whole was advocated, and many naturalists whose opinions will carry great weight expressed their views on the question of forest management. The complete report of this meeting has not yet appeared, but I will refer you to it prospectively, and in an appendix to the part of our *Transactions* now going through the press will appear papers, drawn up at the request of the Council, by Dr. M. C. Cooke, Mr. J. E. Harting, and Prof. Boulger. The evils of deep drainage, from the naturalist's point of view, which form the text of Dr. Cooke's protest, have already been pointed out by many, and I will just call your attention to some remarks on this subject by our eminent honorary member, Mr. A. R. Wallace, in an able article published in the *Fortnightly Review* for November, 1878, wherein he says:—"It must be remembered, too, that a proportion of bog, and swamp, and damp hollows are essential parts of the 'natural aspect' of every great forest tract. It is in and around such places that many trees and shrubs grow most luxuriantly; it is such spots that will be haunted by interesting birds and rare insects; and there alone many of the gems of our native flora may still be found. Every naturalist searches for such spots as his best hunting grounds. Every lover of nature finds them interesting and enjoyable." After enumerating some of the rarer marsh plants of our Forest, Mr. Wallace continues:—"These and many other choice plants would be exterminated if by too severe drainage all such wet places were made dry. The marsh birds and rare insects which haunted them would disappear, and thus a chief source of recreation and enjoyment to that numerous and yearly increasing class who delight in wild flowers, birds, and insects, would be seriously interfered with."

It is somewhat exceptional for a society founded for the study and promotion of natural science to find itself engaged in active polemics, but in taking up the position into which we have been forced, we are simply carrying out that line of action which at our foundation I ventured to lay down as our true function with respect to the Forest. (Inaugural Address, *Transactions*, vol. i. pp. 19, 20.) It is extremely unfortunate that the claims of science should appear to be opposed to the wants of the general public—I say should *appear* to be opposed, because I am convinced that there is no real antagonism. The grievance of naturalists is not only that their claims have been ignored, but the action of the Conservators has hitherto been entirely on the destructive side, and a feeling of alarm has arisen lest the whole of the Forest should piecemeal be desecrated in the name of a fictitious philanthropy. The public wants—as interpreted by the Board of Conservators—are made to take the form of clearing of underwood, drainage, roadmaking, the intersection of the Forest by railways and tramways, and ample public-house accommodation. If these are really the fundamental requirements of holiday-seekers, then there must for ever be a strong antagonism between this class of the public and those whose cause I have taken it upon myself to advocate. At this juncture, however, we may fairly ask whether this kind of artificialised recreation-ground, *à la* Cremorne, is actually demanded by the frequenters of the Forest. I believe myself that it is not. The notion of keeping a holiday in what is only too often a bestial manner is not a fair estimate of the British excursionist. If he gives way to the temptations which have been so lavishly scattered in his path, it is, as Shakespeare puts into the mouth of King John, because "the sight of means to do ill deeds makes ill deeds done." The East Londoner who wishes to spend a day in a "people's park" is provided for elsewhere, but if we consent to the denaturalising of our Forest, the more intelligent class of excursionists—and their name is legion—will be either driven from its precincts or will suffer that degeneration which the line of action at present pursued is exclusively calculated to bring about.

In the course of these remarks I may have somewhat exaggerated the supposed antagonism between the two classes most interested in the conservation of Epping Forest, but I have done so with the object of defining as sharply as possible the position of the hitherto unconsidered naturalist. The conditions requisite for transforming the Forest into a "people's park" are fatal to its preservation as a natural history resort. Any piece of waste land can be made into a park, but a tract of wild forest once destroyed can never be restored. I would once more urge, and most emphatically, that there is not the slightest desire on the part of naturalists to exclude the "toiling million," or to prevent their full enjoyment of the Forest. I wish only to point out that my present contention is that in the long run the wants, both of the naturalist and of the ordinary excursionist, will be found to be absolutely coincident. If the neighbourhood of a railway terminus with its concomitant evils leads to the destruction of the "natural aspect" of any portion of the Forest, that portion is ruined, not only for the naturalist, but likewise for the general public who come to enjoy a day in the country far from "the busy hum of men." By judicious management the requirements of both classes can be met, and it rests entirely with the Conservators to determine whether the attitude of the respective parties is to be pacific or the reverse. It must be remembered that long before the Forest was rescued by the Corporation this district was a favourite resort of multitudes of holiday folk, and, not being interfered with to any considerable extent, was at the same time available to the naturalist. The note of alarm must be sounded, or we may find ourselves worse off than in pre-Conservatorial times. The constitution of the Epping Forest Committee is apparently prejudicial to our interests if we may judge by the standard of past and present actions. Of this Committee the Verderers, who, as representing the Commoners and as residents in the Forest district, are best qualified to advise with respect to the management of the Forest, form but four of a Committee of sixteen. However enlightened the views of these gentlemen may be—and I only wish I could say that the present Verderers were unanimously of our way of thinking—they are thus liable to be outvoted. Another evil, and a most serious one so far as we are concerned, is that the Committee is practically a secret one—its proceedings are conducted with closed doors, and the people at large, whether naturalists or excursionists, have no means of making their voices heard. Whether this action is just in a case where the funds are derived from a public source it does not enter into my province to consider.

The views which I have now put forward are offered with the best of intentions with respect to the body Conservatorial. We cannot be unmindful of our obligation to the Corporation for having saved the Forest, but we appeal to them to assist in exalting the ideas of those who frequent this place as a holiday resort instead of pandering solely to the more degraded aspect of human nature. A day spent amid the natural beauties of our sylvan glades is the beau ideal of a holiday, intellectually, morally, and physically, to those whose pursuits keep them confined to the town. Let Epping Forest be preserved for the multitudes who have for so long enjoyed it rationally. The "recreation and enjoyment of the public" will thus become possessed of a higher meaning, and the naturalist while carrying on his studies as heretofore will be doubly grateful to those who have secured these time-honoured preserves as a public space free from all fear of inclosure or destruction. The ideas which I have attempted to formulate are I know entertained by large numbers not only of working naturalists, but also by the continually growing class of lovers of the country and of nature in general. It is becoming a matter of almost national importance that the surviving tracts of open country in the neighbourhood of all large towns should be rigidly preserved, and opinions in accordance with this have from time to time been forcibly expressed both with respect to our own Forest and all the common lands in the environs of London.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—The following farther appointments to Electoral Boards have been made:—

Professorship of Botany: Sir Jos. Hooker, Dr. F. Darwin,

Dr. M. Foster, Prof. Oliver, Dr. Hort, Dr. Phear (Master of Emmanuel College), Rev. G. F. Browne, and Rev. M. J. Berkeley.

Woodwardian Professorship of Geology: Prof. Prestwich (Oxford), Rev. E. Hill, Mr. W. H. Hudleston, Mr. A. Geikie (Director of Geological Survey), Dr. Phear, Mr. R. D. Roberts, Mr. Ewbank, and Prof. A. Newton.

Professorship of Zoology and Comparative Anatomy: Prof. Flower, Prof. Moseley (Oxford), Dr. M. Foster, Prof. Huxley, Mr. J. W. Clark, Dr. F. Darwin, Prof. Humphry, and Mr. D. McAlister.

The Woodwardian Professor has been authorised to apply a sum equivalent to the late Assistant's stipend in payment of Demonstrators for this and the next term.

The regulations for the degrees of Doctor in Science and Doctor in Letters have been confirmed, with minor modifications.

The additional mathematical examination of candidates for honours in the "Little Go" is to be discontinued; Elementary Logic is to be hereafter allowed as a substitute for Paley's Evidences; Euclid is to be limited to the more useful propositions; algebra is to be increased in quantity; and the examination is to be held three times a year, the additional time being at the beginning of October.

The subject for the next Sedgwick Prize Essay, 1885, is "The Jurassic Rocks of the Neighbourhood of Cambridge."

The last Report of the Mathematical Board recommends that the Moderators and Examiners shall be the adjudicators of the Smith's Prizes, and that the Smith's Prizes be awarded on the results of Part III. of the Mathematical Tripos. This would give more distinction to the examination in the higher subjects. The concurrence of Professors Stokes, Adams, and Cayley in this recommendation is a strong point in its favour.

The report of the Moderators and Examiners in the last Mathematical Tripos, the first under the new system, gives particulars about Part III., to which only the Wranglers are admitted. Of the twenty-nine Wranglers, sixteen presented themselves for Part III., of whom two were not finally classed. In order to give opportunity to a candidate who had confined his reading mainly to one group of the higher subjects to employ his whole time in questions in that group, the examiners in the five bookwork papers gave at least four questions in each group which came into the paper, and fixed five as the limit of questions to be answered. In the fifth paper, subjects for essays were chosen from each group. The majority of candidates attempted too many subjects, and their answers as a rule were poor and meagre. The Examiners are far from satisfied with the average performance of the candidates in Part III., but they expect better results when the new system is better understood, especially the encouragement given to limiting reading in the higher subjects to one or two groups.

FREE admission to the lectures and courses of practical instruction in the Normal School of Science and Royal School of Mines at South Kensington and Jermyn Street will be granted to a limited number of Teachers and Students of Science Classes under the Science and Art Department, who intend to become Science Teachers. The selected candidates will also receive a travelling allowance, and a maintenance allowance of twenty-one shillings per week while required to be present in London. The courses given and the duration of each are as stated below:—Chemistry: Part I., October to February; Parts II. and III., October to June. Physics: Part I., October to February; Parts II. and III., October to June. Biology: October to June. Geology: Part I., February to June; Part II., October to February. Mechanics: Part I., February to June; Parts II. and III., October to June. Metallurgy: October to June. Mining: October to June. Agriculture: October to January. Attendance is required from 9 or 10 a.m. to 4 or 5 p.m. daily, in addition to the time necessary in the evening for writing up notes, &c. Students will be required to attend the Classes for Mathematics, Geometrical Drawing, and Freehand Drawing, so far as may be considered necessary. Candidates for these Studentships must send in their applications before May 31, on Science Form No. 400, copies of which may be obtained on application to the Secretary, Science and Art Department, South Kensington. When the same student is a candidate for more than one course, the order of preference should be given. It should, in all cases, be stated for which course or courses the student is a candidate.

SCIENTIFIC SERIALS

American Journal of Science, February.—Henry Draper, by G. F. B.—Fauna at the base of the Chemung group in New York, by H. S. Williams.—Geological chemistry of Yellowstone National Park.—Geyser waters and deposits, by H. Leffmann.—Rocks of the Park, by W. Beam.—Electromagnetic theory of light; general equations of monochromatic light in media of every degree of transparency, by J. W. Gibbs.—The rainfall in Middletown, Connecticut, from 1859 to 1882, by H. D. A. Ward.—Discoveries in Devonian Crustacea, by J. M. Clarke.—Observations of the transit of Venus, 1882, made at the Lick Observatory, by D. P. Todd.—The antennæ of Meloë, by F. C. Hill.—Hypersthene-Andesite, by W. Cross.—Method for determining the collimation constant of a transit circle, by M. Schæberle.

The American Naturalist, December, 1882, contains:—A pilgrimage to Teotihuacan, by R. E. Hills.—On the grey rabbit (*Lepus sylvaticus*), by Samuel Lockwood.—The Palæozoic allies of Nebalia, by A. S. Packard, jun.—American work on recent mollusca in 1881, by W. H. Dall.—The organic compounds in their relations to life, by L. F. Ward.—The reptiles of the American Eocene, by E. D. Cope.

January, 1883, contains:—The history of anthracite coal in nature and art, by Jas. L. Lippincott.—The development of the male prothallium of the field horse-tail, by D. H. Campbell.—On the geological effects of a varying rotation of the earth, by J. E. Todd.—On the bite of the North American coral snakes (*Elaps*), by F. W. True.—Achenial hairs and fibres of Compositæ, by G. Macloskie.—Instinct and memory exhibited by the flying squirrel in confinement, with a thought on the origin of wings in bats, by F. G. King.—The extinct Rodentia of North America, by E. D. Cope.

February, 1883, contains:—The Kindred of Man, by A. E. Brown.—Indian Stone Graves, by C. Rau.—On organic physics, by C. Morris.—The mining regions of Southern New Mexico, by F. M. Endlich.—The extinct Rodentia of North America, by E. D. Cope.—Spencer and Darwin.—The Beastiarians.

Annalen der Physik und Chemie, No. 2.—The electric conductivity of some cadmium and mercury salts in aqueous solutions by D. Grotrian.—On the change of the double refraction of quartz by electric forces, by W. C. Röntgen.—On the optical behaviour of quartz in the electric field, by A. Kundt.—On the function of magnetisation of steel and nickel, by H. Meyer.—Contributions to the history of recent dynamo-electric machines, with some remarks on determination of the degree of action of electromagnetic motors, by A. von Waltenhofen.—On the viscosity of salt solutions, by S. Wagner.—Researches on the absorption of gases by liquids under high pressures, by S. v. Wroblewski.—Strecker's memoirs on the specific heat of gaseous biatomic compounds of chlorine, bromine, iodine, &c., by L. Boltzmann.—On the luminosity of flames, by W. Siemens.—Distillation in vacuum, by A. Schuller.—Researches on the elasticity of crystals of the regular system, by K. R. Koch.—On absolute measures, by C. Bohn.—Correction of the method adopted by R. Kohlrausch in his researches on contact-electricity, by E. Gerland.—The volume-change of metals in melting, by F. Nies and A. Winkelmann.—Correction, by A. Guebhard.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, February 22.—"Preliminary Note on the Action of Calcium, Barium, and Potassium on Muscle." By T. Lauder Brunton, M.D., F.R.S., and Theodore Cash, M.D.

It has been shown by Ringer that calcium prolongs the contraction of the frog's heart. This prolongation is diminished by the subsequent addition of potash.

It occurred to us that calcium and potassium salts might exercise a similar action on voluntary muscle. On trying it we found this to be the case. Calcium in dilute solution prolongs the duration of the contraction in the gastrocnemius of the frog. Potassium salts subsequently applied shorten the contraction. We have been led to try the effect of barium on muscle by considerations regarding the relations of groups of elements, according to Mendelejeff's classification, to their physiological action. These considerations we purpose to develop in another paper. The effect of barium is very remarkable. It produces a curve

very much like that caused by veratria, both in its form and in the modifications produced in it by repeated stimuli. We have found that the veratria curve is restored by potash to the normal in the case of the gastrocnemius, just as Ringer found it in the case of the frog's heart. The peculiarity which barium produces in the gastrocnemius is also abolished by potash. We have tested a number of other substances belonging to allied groups, and find that some of them have a similar, though not identical, action with barium. The results of these experiments, as well as the general considerations to which we have already alluded, we purpose to discuss in another paper.

"On the Formation of Uric Acid in the Animal Economy, and its Relation to Hippuric Acid." By Alfred Baring Garrod, M.D., F.R.S.

The paper is divided into an introduction and three parts. The introduction contains the results of a series of experiments upon the solubility of uric acid and its most important salts, at the temperature of the body; and upon the effects of mixing the urates of sodium and ammonium with the phosphates and chlorides of the same bases.

Part I. contains observations upon the physical and microscopic characters of the urinary excretions of birds, reptiles, and some invertebrata, as well as chemical investigations of such excretions, and of the blood of the same classes of animals, with a view to the detection therein of uric acid. Part II. deals with the formation of uric acid in the animal economy. The rival theories are discussed, and from the consideration of the very large quantities of uric acid, in proportion to the body-weight, excreted by many of the lower animals, as well as the inability of the kidneys to excrete uric acid which has been taken by the mouth or injected into the blood, the author is led to the opinion that the uric acid is a product of changes which take place in the kidneys itself, and is not merely filtered off from the blood. This view receives further support from the fact that, whilst the kidneys excrete ammonium urate, uric acid when found in the blood is in the form of the more stable sodium urate.

It is further shown that when solutions of hippurates are mixed with solutions of urates, the salts exert an influence upon each other, and details of experiments to demonstrate this action are embodied in an appendix.

Linnean Society, February 15.—Sir John Lubbock, Bart., F.R.S., president, in the chair.—Mr. Jenner Weir exhibited a perfect hermaphrodite butterfly (*Lycæna icarus*), and a blue male and brown female of the same species for comparison. The hermaphrodite in question possesses two spotless blue wings on the left, and two spotted brown wings on the right, thus being intermediate in colour between the two sexes.—Dr. W. C. Ondaatje exhibited a collection of thirty species of Ceylon corals, of which twenty were of a stony character. The series agree in the main with those of the Indian fauna; four are new species, viz. two of *Caloria*, one of *Pavonia*, and one of *Alcyonium*, the two latter however showing most affinity to agree with those found in islands of the Pacific Ocean.—Mr. T. Christy called attention to examples of Carnauba palm leaves and to the wax of the tree; and he also showed specimens of a hybrid Primula (*P. japonica* and *P. sinensis*) with double whorls of flowers.—Mr. J. G. Baker read his third contribution to the flora of Madagascar. In this he gives descriptions of the new Incompletæ and Monocotyledons contained in the collections recently made in Madagascar by the Rev. R. Baron and Dr. G. W. Parker. The only new genus is *Cephalophyton*, a Balanophorad used in medicine, of which the material is not complete. Most of the new species belong to widely spread tropical genera, such as *Ficus*, *Loranthus*, and *Croton*. Cape types are represented by *Faurea*, *Peddiea*, *Dais*, *Kniphofia*, and *Dipcadi*, one species of each, and by four Aloes. Of *Obetia*, a genus of arborescent stinging-nettles known only in Madagascar and the neighbouring islands, there are four new species. The Bamboo common in the woods of Imerina proves to be conspecific with that of the interior of Bourbon. There is a curious *Exocarpus* with phyllocladea, nearly allied to species from Norfolk Island and the Malay archipelago.—Mr. C. B. Clarke has contributed a complete synopsis of all the species of *Cyperus* known in Madagascar and the neighbouring islands.—Mr. George Murray read a paper on the outer peridium of *Broomeia*. This gasteromycetous fungus, which is nearly related to *Geaster* consists of a mass of individuals closely seated together on a corky stroma. These individuals have been found up till now with only one peridium, and the Rev. Mr. Berkeley, who first described the plant in 1844, treated the stroma as the homologue of an outer peridium. Mr. Murray has found on some specimens recently brought from Dammara Land a true outer peridium common to all the individuals. From an examination of it he is able to throw light on the mode of development of this fungus.—A paper was read on the "Manna" or Lerp insect of South Australia, by Mr. J. G. Otto Tepper. This contained observations on the insect in question and on the peculiar saccharine substance derived from it, which is deposited on Eucalypt trees.—Mr. W. B. Hemsley read a communication on the synonymy of *Didymoplexis*, and on the elongation of the pedicle of *D. pallens*. The latter saprophyte orchid is widely scattered in tropical Asia, though apparently nowhere very common. It is remarkable for the elongation of its pedicles after flowering. At the time of flowering the pedicles are shorter than the flowers, which are less than half an inch long; but afterwards they elongate, sometimes as much as a foot. The object seems to be to carry the ripening fruit clear of the wet decaying vegetable matter in which the plant grows.

Zoological Society, February 20, W. H. Fowler, F.R.S., president, in the chair.—Prof. F. Jeffrey Bell exhibited a selection of microscopical preparations received from the Zoological Station at Naples, and made some remarks upon them.—Mr. J. J. Weir exhibited and made remarks on an apparently hermaphrodite specimen of *Lycæna icarus*.—Mr. Sclater gave an account of the birds collected by Mr. H. O. Forbes, F.Z.S., during his recent expedition to Timor Laut, and exhibited the specimens. The species were fifty-five in number, sixteen of which were described as new to science under the following names:—*Ninox forbesi*, *Strix sororcula*, *Tanygnathus subaffinis*, *Geoffroius tenimberensis*, *Monarcha castus*, *Monarcha mundus*, *Rhipidura hamadryas*, *Myiagra fulviventris*, *Microeca hemixantha*, *Grauculus unimodus*, *Lalage mœsta*, *Pachycephala arctitorquis*, *Dicæum fulgidum*, *Myzomela annabellæ*, *Calornis crassa*, and *Megapodius tenimberensis*. The general facies of the avifauna as thus indicated was stated to be decidedly Papuan, with a slight Timorese element, evidenced by the occurrence of certain species of the genera *Geocichla* and *Erythura*; while the new owl (*Strix sororcula*) was apparently a diminutive form of a peculiar Australian species.—Prof. F. Jeffrey Bell read the second of his series of papers on the Holothuroidea. The present communication contained the descriptions of some new species which the author had discovered while examining the specimens of this group contained in the collection of the British Museum.—Dr. Hans Gadow read a paper on the suctorial apparatus of the Tenuirostres, pointing out that the tubular structure of the tongue in this group is produced by the overgrowth of the horny lingual sheath, the edges of which curl upwards and inwards.—A paper was read by Mr. L. Taczanowski, C.M.Z.S., Curator of the Museum at Warsaw, in which he gave the descriptions of some new species of birds in the collection made by Dr. Raimondi during his recent explorations in Peru. The species in question were seven in number, belonging to six genera, namely, *Carenochrous seebohmi*, *C. dresseri*, *Phytotoma raimondi*, *Ochthœca jelskii Upucerthia pallida*, *Cynanthus griseiventris*, and *Psittacula crassirostris*.—Mr. Taczanowski also read a communication from Dr. Dybowski, in which the sexual differences between the skulls of *Rhytina stelleri* were pointed out.—A communication was read from Mr. G. B. Sowerby, jun., containing the descriptions of nine new species of shells and of the opercula of two known species.

Entomological Society, February 7.—J. W. Denning, M.A., F.L.S., president, in the chair.—Two Members and one subscriber were elected.—Mr. J. R. Billups exhibited a species of *Conocephalus* which was found in a greenhouse at Lee and kept alive some time.—Mr. F. P. Pascoe read some comments on a letter recently contributed to NATURE by the Duke of Argyll, respecting a moth observed by him at Cannes.—Mr. E. A. Fitch exhibited three species of *Hymenoptera* from Ambarawa, Sumatra.—M. L. Peringuey communicated notes on the habits of three species of *Paussus* observed by him at the Cape of Good Hope.

Mineralogical Society, February 15.—Mr. W. H. Hudleston, F.G.S., president, in the chair.—Prof. Church exhibited and described a specimen of siliceous matter obtained by Mr. Vicary from the Upper Greensand of Haldon, which contained 98 per cent. of silica.—The President then read a paper on a recent hypothesis with respect to the diamond rock of South Africa. A discussion ensued in which Profs. Rupert Jones, John Morris, and Church took part.—A paper from Mr. J. H.

Collins was read on the minerals of Rio Tinto. The President, Prof. Morris, and Mr. Kitto joined in the discussion.

Meteorological Society, February 21.—Mr. J. K. Laughton, F.R.A.S., president, in the chair.—Rev. W. R. C. Adamson, R. P. Coltman, W. F. Gwinnell, Capt. C. S. Hudson, T. Mann, F. G. Treharne, and W. Tyson, were elected Fellows.—The following papers were read:—Notice of a remarkable land fog bank, "the Larry," that occurred at Teignmouth on October 9, 1882, by G. W. Ormerod, M.A., F.M.S. The "Larry" is a dense mass of rolling white land fog, and is confined to the bottom of the Teign Valley, differing therein from the sea fog which rises above the tops of the hills; it appears about daybreak, and has an undulating but well-defined upper edge, which leaves the higher part of the hillsides perfectly clear. The author gives an account, illustrated by photographs, of the remarkable fog bank that occurred at Teignmouth on the morning of October 9.—Barometric depressions between the Azores and the continent of Europe, by Capt. J. C. de Brito Capello, Hon. Mem. M.S. The author gives the tracks of several depressions from the Azores to Europe, and shows that if there had been a telegraphic cable, nearly every one of them could have been foretold in England.—Weather forecasts and storm warnings on the coast of South Africa, by Capt. C. M. Hepworth, F.M.S.—Note on the reduction of barometric readings to the gravity of latitude 45°, and its effect on secular gradients, and the calculated height of the neutral plane of pressure in the tropics, by Prof. E. D. Archibald, M.A., F.M.S.

Physical Society, February 24.—Prof. Clifton in the chair.—New members: Prof. A. W. Scott, M.A., Mr. F. E. M. Page, B.Sc.—Mr. Lewis Wright read a paper on the optical combinations of crystalline films, and illustrated it by experiments. He exhibited the beautiful effects of polarisation of light and the Newtonian retardation by means of plates built up of thin mica films and Canada balsam. The wedges thus formed gave effects superior to those of the more expensive selenite and calcite crystals. The original use of such plates is due to Mr. Fox, but Mr. Wright showed many interesting varieties of them, including what he termed his "optical chromotrope," formed by superposing a concave and ¼ wave-plate on each other. Norenberg's combined mica and selenite plates were also shown. Mr. Spottiswoode praised the results very highly, and pointed out their value to the teacher and student as showing how the effects can be produced step by step. The phenomena can be shown by an addition to the ordinary microscope, costing some two guineas, as made by Messrs. Swift and Sons.—Mr. Braham then gave an experimental demonstration of the vorticle theory of the solar system by rotating a drop of castor oil and chloroform in water until it threw off other drops as planets.

EDINBURGH

Royal Society, February 19.—Mr. A. Forbes Irvine in the chair.—Mr. G. Auldjo Jamieson read a long and interesting paper on land tenure in Scotland in the olden time, in which the author, after describing in detail the various ancient systems and the survivals of them that exist even now in different parts of the country, strongly deprecated the position taken by some that a return to the old systems would be beneficial.—Prof. Rutherford, in a paper on the microscopical appearances of striped muscular fibre during relaxation and contraction, maintained that the views held generally by physiologists as to which is the contractile portion of the fibre were quite erroneous. A great deal of the inconsistency that seemed to exist was due to difference in the appearance of muscular fibre according as it was relaxed or contracted; and previous observers had been unable to explain this simply because they had not hit upon an effective method of preserving the fibre in either condition. The paper was illustrated by enlarged diagrams and by microscopical preparations of the fibres in various conditions.

BERLIN

Physiological Society, February 9.—Prof. du Bois Reymond in the chair.—Dr. Walten, who was present as a visitor, gave a detailed account of his experiments upon the power of hearing in hysterical, hemianæsthetic persons. He has determined the presence of different degrees of deafness, in cases of partial and complete hemiæsthesias, in addition to the manifold motor and sensory hyperæsthesias and anæsthesias. In all cases anæsthesia of the external auditory meatus and of the membranum tympani existed on the affected side; the lesser degrees of deafness manifested themselves in the same way that senile deafness sets in, *i.e.* in interference with the propagation of sounds through the cranial bones, while direct hearing by the ear was still normal. When a higher degree of hysterical deafness was present, high tones could not be perceived by the ear. In extreme cases deafness is absolute on the affected side. All degrees of uni-lateral hysterical deafness could, like the remainder of the manifestations of hemiæsthesia, be transferred to the healthy side through the operation of a powerful magnet. Dr. Walten was able to measure the gradual decrease of deafness on the affected side and its gradual increase on the healthy side.—Dr. Martius, reasoning by analogy from the fact that a frog's heart cannot contract unless it is bathed in a nutritive fluid, from which it takes the energy required for its work, has tried to determine by experiment if other organs, *e.g.* the brain, require a continual supply of a nutritive fluid in order to keep up their activity. He therefore replaced the blood of frogs by a neutral salt-solution of the strength of 0·6 per cent., with which he washed out the blood-vessels until the fluid ran off free from blood, and as clear as water, and he observed the functions of the central nervous system in these frogs (salt-frogs). It was, however, soon discovered that all the blood had not been removed from the body by driving salt-solution through once, because, when the process was repeated after a few hours' time, the fluid flowed off deeply coloured with blood, and this process had to be renewed very frequently before the blood was reduced to a minimum. Hence, in the experiments with "salt-frogs," the brain was supplied by blood that was more and more diluted, and it reacted as follows:—After washing out the vessels once with salt-solution, the frog behaved like a brainless frog behaves. It sat still and did not make any spontaneous movements; it breathed normally, and exhibited the croak-reflex to perfection. After the vessels had been washed out twice, the croak-reflex had disappeared, and breathing was irregular and intermittent; finally, when the blood was still further diluted, respiration entirely ceased and the general reflex-irritability was greatly increased, as in frogs whose spinal cord is separated from their medulla oblongata. The conclusion to be drawn is that the brain, as well as the heart, requires the presence of a nutritive fluid from which it abstracts the energy for its work. The frogs that were operated upon recovered perfectly from the first stage in a few days' time, but did not recover from the later stages. It is evident that they bore the transfusion of such large quantities of salt-solution very well. On the other hand, they could not be used for experiments upon blood-transfusion, because they died even after very moderate transfusions of blood from other animals.

GÖTTINGEN

Royal Society of Sciences, December 27, 1882.—On an onyx cameo not hitherto known, with a replica of the representations on the upper and middle layers of the large Paris cameo of La Sainte Chapelle, by F. Wieseler.

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THURSDAY, MARCH 15, 1883

THE ZOOLOGICAL STATION IN NAPLES

THERE are few of those interested in biological studies who are not more or less familiar with the history and character of the great international laboratory on the shore of the Bay of Naples, which has had so profound an influence on the progress of zoology in the last nine years; scarcely a volume belonging to recent zoological literature, British or foreign, can be taken up, but the acknowledgment of indebtedness to the resources of the Naples station comes under the eye; the publications of the station are on the shelves of most scientific libraries; and many accounts of its organisation have appeared from time to time in scientific periodicals and even in the daily press. But the institution is much too interesting a topic of discussion to be easily exhausted; it is constantly developing and exhibiting new stages of existence. There is soon to be added a new department that of comparative physiology, the work of which will be carried on in a separate laboratory; and on the eve of an expansion so considerable, it is natural to reflect on the work the station has already accomplished, its present state of activity, and the probabilities of its future.

In no branch of zoological science has such rapid and important progress been made in recent years as in embryology, and the investigations into the development of marine forms of all classes by which this progress has been chiefly effected, have been in great part the result of the special facilities which the resources of the Naples station offer for this kind of research. The brilliant career of the lamented Francis Balfour was begun while he occupied, on the opening of the station in 1874, the table rented by Cambridge University. His stay on this occasion lasted from February to June, and resulted in the publication of his first paper, "On the Development of Elasmobranchs," in the *Quarterly Journal of Microscopical Science*. The material for the researches which he continued to carry on at Cambridge on his return was sent from the station. In 1875 he again spent some months at Naples, and again published the results of his work in the same *Quarterly Journal*, this time under the title "A Comparison of the Early Stages in the Development of Vertebrates." The following year he did not visit the station, but in 1877 he investigated there the spinal nerves of Amphioxus, and added to his work on Elasmobranchian embryology. These studies appeared in the *Journal of Anatomy and Physiology*, vols. x. and xi. In the preface to his "Monograph on the Development of Elasmobranchs," which, published in 1878, was at once recognised by all biologists as a classical work, Balfour gratefully acknowledges how much his researches owed to the resources of the zoological station and the support of its *personnel*. It is unnecessary to dilate here on the importance of Balfour's work; the significance of the discoveries which he made, such as the openings of the renal organs into the body cavity in Selachians as in Annelids, the epiblastic origin of the sympathetic system, the history of the blastopore in vertebrates, and its relation to the medullary canal, the head cavities, &c., and the masterly way in which he applied the results of his observations to the solution of the great problems of vertebrate morphology, have given him a place among those whose names mark epochs in the progress of science.

Another English name connected with work in the field of vertebrate embryology which does honour to the Naples station is that of Mr. Milnes Marshall, who has more than once occupied the British Association table. Much of our knowledge of the development of Salpa, the excentric relation of the vertebrates, is due to the work in the station of Professors Salensky and Todaro.

Molluscan embryology has benefited by the existence of the station through the work of Prof. Lankester and the Russian embryologist, Dr. Bobretzky. The former carried on researches in the laboratory in the spring of 1874, and obtained many of the important results which are embodied in his memoir "On the Development of Cephalopoda" (*Quart. Journ. Mic. Sci.* vol. xv.), and his paper "On the Development of Mollusca" (*Phil. Trans.* 1875). Dr. Bobretzky of Kiev occupied the Russian table in 1874, and applied the methods of technical histology to the study of the ova of various Gasteropods, Nassa, Fusus, &c., and of Loligo and other Cephalopods. His Russian memoir on the latter (Moscow, 1877) contains the most complete and reliable series of figures we have of the anatomy of Cephalopod embryos.

In the embryology of sponges, Prof. Oscar Schmidt of Strassburg has published the results of important researches carried on in the station in the years 1875 and 1877. Prof. Selenka of Erlangen worked out the development of various Holothuria at the Bavarian table in 1875, and of Echinidæ in 1879. The work of Dr. Carpenter on the development of Antedon (*Proc. Roy. Soc.* 1876) was done at the British Association table, and the contributions of Dr. Goette to the same subject are based on studies made in the station in 1875. One of the best known of recent studies in development which have proceeded from the station is that of Dr. Spengel, on Bonellia, published in 1879.

Leaving works of a strictly embryological character, we will mention some of the principal contributions to general morphology, which have taken their origin in the station. Prof. Grenacher's great work on the eyes of Arthropods, which forms one of the chief recent additions to our knowledge of the class, is based on researches begun at the Mecklenburg table in 1876. Dr. Hubrecht's researches on Nemertines were carried out at the Dutch table. The contributions to and confusions of Molluscan morphology, which we owe to Von Jhering, proceeded from work done in the station, and both are not without value in the progress towards truth. Dr. Spengel's important paper on the "Geruchsorgan der Mollusken" (*Zeitschr. f. wiss. Zool. Bot.* xxxv.), was produced while he was a member of the staff of the institution. The remarkable volume of the brothers Hertwig, "Die Actinien," describing a nervous system still existing in the primitive condition, was the result of an occupation of two of the German tables.

The honour of the discovery of Symbiosis in animals is shared by two zoologists, who both carried out their researches in the station, Mr. Geddes and Dr. Brandt; and the studies which the latter is still carrying on there have resulted in many other contributions to our knowledge of the Radiolarians.

X

The investigations of Von Koch into the relations of the skeleton in corals, Flemming's researches on the ova of Echinoderms, Metschnikoff's on Orthonectidæ, those of Dr. Vigelius on the anatomy of Cephalopoda, of Prof. Greef on Alciopidæ, are a few more examples of good work, of which some of the credit belongs to the station. Since the laboratory was opened more than 200 scientific workers have studied at its tables.

Besides this success which the institution has obtained as an international laboratory, it has also produced great results by its own individual activity. A vast amount of complete and careful work is devoted to the preparation of the series of monographs which commenced with the Ctenophoræ of Dr. Chun in 1880. Of these six have appeared—four zoological and two botanical—and a large number, embracing many important classes of animals, are far advanced towards completion. The Planarians, by Dr. Lang, will be received with interest on account of the discoveries and original views which his work has already produced. The Actiniæ are being worked out thoroughly, for the first time, by Dr. Andres. The Sponges, the Radiolarians, the Copepoda, and the Capitellidæ are also at present undergoing a complete study in the station, and two of the volumes already announced will treat of families of Algæ. An enterprise of such magnitude has never before been undertaken in the field of zoological investigation; only an organisation of the power and resources of the station at Naples could attempt it; an organisation which is able to offer to zoologists, of energy and zeal, unlimited material in the living condition, unlimited leisure for work, and immunity from all distractions save some slight duties connected with the routine of the laboratory.

The other two publications of the station are of a less colossal character. The *Mittheilungen* was begun in 1879, for the sake of publishing the numerous discoveries and new views which result from the work of the staff occupied with the "Fauna and Flora," or from the researches of those occupying the rented tables. The circulation has already reached 400 copies, and the few volumes which have appeared constitute a valuable addition to the literature of biology. In its pages are described the new processes in the *technique* of microscopical work which have been invented in the station, one of which, the method of preparing series of sections, devised by Dr. Giestrecht, and now used in every laboratory in Europe, is an improvement whose importance it is impossible to estimate too highly.

The object of the *Zoologischer Jahresbericht* was to supply a bibliographical report, which should not only give a list of published works but a *résumé* of the matter contained in each, and which should give perfect facilities for reference. The latter object is attained by means of two indices—one of the names of authors, the other of subjects. The English *Zoological Record* and the report of the *Archiv für Naturgeschichte* are devoted chiefly to systematic zoology; in the *Jahresbericht* every publication on anatomy, embryology, morphology, or physiology, is catalogued and summarised.

In contrast with the activity exhibited by the station in the directions which we have hitherto considered, activity whose results are as conspicuous as they are important, is the unobtrusive work of the department presided over by the energetic conservator, Salvatore Lo Bianco,—the department for the preservation and distribution of marine animals. All the material procured by the expeditions of the two steam launches, and the smaller boats belonging to the station, or by purchase from Neapolitan fishermen, passes first into the control of this department. Whatever is needed by the various occupants of the work-tables and by the scientific staff is selected and allotted according to applications made from day to day. The rest is either put into the tanks of the public aquarium, or preserved. Marvellous progress has been made in the art of preserving delicate and sensitive creatures in their naturally extended condition, and inland laboratories can be provided with specimens of Alcyonaria, Zoantharia, Medusæ, Ctenophora, Annelids, &c., which show the form if not the colour of the living animal, and in which all the organs are in a perfect condition for anatomical and generally even histological study.

There is scarcely a biological laboratory in Europe which has not had recourse to the preparing department of the Naples station in order to procure material for investigation or for teaching purposes. An example of the work of the department is to be exhibited in the approaching International Fisheries Exhibition—a most beautiful collection of preparations is now in the station, ready to be sent to London.

In connection with this department arrangements have been made with the naval authorities of Germany and Italy, by means of which an officer is sent from time to time to the station to learn the methods of obtaining and treating marine creatures for the purpose of scientific study; so that the cruises of war-ships in remote seas may contribute to valuable scientific results when each has an officer on board who understands what is of zoological interest and how it should be preserved.

In conclusion it will be of interest to give a few details concerning the finances and arrangements of the station. The annual income is between 5000*l.* and 6000*l.*, of which 1200*l.* is derived from the public aquarium, 1600*l.* from the rented tables, about 800*l.* from the sale of the publications, including 260 annual subscriptions of 50*s.* each for the monographs, 600*l.* from the preparation department, and 1500*l.* is the amount of the German Government subsidy.

The total number of those in the permanent service of the station is thirty-seven, of which eight comprise the scientific staff, and the rest are made up by the engineers under the direction of Mr. Petersen, the fishermen, and the conservator and his assistants. The number of tables at present rented is twenty-one, but the station has space for thirty. At the beginning only seven tables were taken, two each by Prussia, England, and Italy, and one by Holland. The School of Biology at Cambridge has derived much support and benefit from its connection with the station, and the taking of a table by Oxford would probably give to zoological studies there an impetus which is much needed. Of the few zoologists which Oxford produces, some have already had recourse to the British Association table. It is probable that some one of the many rich institutions in America will soon take a table for the use of American zoologists, many of whom, imperfectly acquainted with the organisation of the station, and therefore unaware that no table can be occupied unless taken either by a corporation or a private indi-

vidual for a whole year, have applied for permission to work there. Last year Mr. Whitman, whose observations on the development of Clepsine are well known, received this permission under special circumstances by the courtesy of the staff, and carried out some excellent researches on Dicæmidæ, which are published in the last number of the *Mittheilungen*. Recently an increased number of similar applications have been received from American zoologists.

In speaking of the arrangements of the station, the perfection of the organisation for the supply of material, by means of the dredging and fishing of the gulf, cannot be too warmly praised or admired. Except in continuously bad weather, the beautiful and wonderful creatures comprising the rich Mediterranean fauna are brought in to the station in an abundance that is perfectly bewildering to a zoologist on his first visit. The possession of two steam launches, the larger of which, the *Johannes Müller*, was given by the Berlin Academy in 1877, while the smaller was purchased subsequently, gives to the fishing department the facilities for rapid locomotion and transport, without which such abundance and perfect condition of the living material could not be obtained; especially as some of the most fruitful localities are widely separated, and a great many of the creatures, including all pelagic forms, are of extreme sensitiveness and delicacy.

The zoological station, although only nine years have passed since its first opening, has become a necessity for the progress of zoology; its international character enables every country to contribute to its support and share in the benefits derived from it; it is a great organisation by which forces of various kinds are brought together to aid in the attainment of one great object, the investigation of the facts and phenomena of marine life in all its diversities, and their explanation in accordance with the principles of evolution. The progress which is brought about by the work actually done in the station is not more important than the indirect influence it exerts in various ways; its example has produced similar enterprises in various parts of the world; the benefit of the experience it gains extends to other centres of scientific research, and other branches of biology than marine zoology, and by its own vitality and its influence on the zoologists who study at its tables it has done much to sustain and develop the great impulse which the genius of Darwin gave to zoology twenty-three years ago. J. T. CUNNINGHAM

EPPING FOREST

THE House of Commons divided last Monday afternoon upon the Chingford and High Beech Railway Bill. An amendment was proposed by Mr. Bryce, Chairman of the Commons Preservation Society, and was supported by Mr. Thorold Rogers, Sir H. J. Selwin-Ibbetson, who framed the Epping Forest Act of 1878, Mr. Fowler, Mr. Firth, Mr. T. C. Baring, Lord Eustace Cecil, Mr. Ritchie, Mr. James, Mr. Caine, and Mr. Waddy. As a fitting sequel to Mr. Meldola's paper, which we published last week, the result of the division, which was announced amidst cheers, was: For the second reading, 82; against it, 230; majority against the Bill, 148. It is to be hoped that this will be the last attempt to tamper with what Mr. Bryce justly described as "a priceless heritage of the people of London."

It is inevitable from the growth of our great towns that the student of Nature dwelling in their midst must go farther and farther afield for the objects of his study. It seems, moreover, that our science is at present inadequate to prevent the lethal influence of smoke and acrid fumes from dealing destruction to vegetation over a wide region outside the actual boundaries of these towns. The sanitary necessity of open spaces has been amply demonstrated; but it was not as a mere open space or people's park that Parliament allowed the Corporation of London to acquire Epping Forest in 1878.

The so-called rights of those who had inclosed the Forest, were overridden in order that an expanse of natural and, in some senses, primeval forest might be secured for the benefit of all classes of the public free from encroachment for ever. Parliament directed that it was to be preserved "in its natural condition as a forest," and conferred upon a Committee—composed of some members of that Corporation which holds the manorial rights, together with four resident gentlemen as Verderers, elected nominally by the commoners—the position of Conservators.

Unfortunately Common Councilmen seem to share the popular ignorance as to what constitutes the natural aspect of a forest. Many people believe a forest to be a large wood or plantation, and the Conservators seem to have been mainly actuated by fears lest visitors should get their feet wet or find the Forest less amusing than other suburban resorts. Draining and roadmaking have been their main tasks with a view to maintain the natural aspect the Forest wore for centuries, while during the five years they have been in office no attempt has been made at reafforesting the now unsightly fallows that the intruders had reduced into an arable condition. Pieces of artificial water have been constructed, mostly with outlines reminding one of the so-called Round Pond in Kensington Gardens; pleasure-boats have been licensed upon them at a rental estimated at over 200*l.* per annum; free displays of fireworks in connection with a huge tavern, shooting-galleries, and steam-roundabouts have been authorised as contributing to a truly ideal forest.

These steps have of course been taken with the idea that the Conservators had the power to act in the way they think best calculated to elevate and refine the working-classes; but they are diametrically opposed to the spirit of the Act of 1878, which did not aim at establishing a tea-garden or at pandering to the lowest tastes of any class of the community.

As is seen from Mr. Meldola's article, the Essex Field Club and other scientific societies have more than once protested against such mismanagement; but the Conservators had not yet filled up the full measure of their iniquities. They must promote a railway, if not a tramway as well.

English public opinion is beginning to awaken to the idea that we have now almost as many railways as are required for any purposes but providing fees for directors and engineers and feeding the jealousies of rival companies. In the present session of Parliament the railway companies have evinced in the Bills they are promoting a partiality for common land that would be remarkable were not the reason for it sufficiently obvious. Common land can be had cheap; for it is everybody's business to

oppose its spoliation, and everybody's business is proverbially nobody's. It is to be hoped, however, that the knell of these schemes was sounded on Monday last, when the House of Commons, on the motion of leading men of both parties, rejected the Chingford and High Beech Extension Bill, promoted by the Corporation and the Great Eastern Railway, by an overwhelming majority.

The House was fully aware that the line then proposed by Sir Thomas Chambers and Lord Claud Hamilton was only the first section of a longer one which would ultimately surround the Forest, and that it was intended to serve at first mainly as a feeder to another large tavern. All lovers of nature will rejoice that the collecting ground of Edward Forster, the Doubledays, and thousands of London naturalists less known to fame, has been rescued from destruction.

Authorities inform us that lopping and smoke have reduced the number of lichens and insects even during the last twenty years, and Conservatorial draining may have a similar effect upon other groups of organisms, so that the help of a railway in the work of devastation is certainly not required.

It is to be hoped that the verdict of Parliament will show the Conservators that forest management has a scientific basis and that their powers are not unlimited. It is equally desirable that the public interested in the Forest will form some organisation for its protection from encroachment and mismanagement in the future, so as to relieve a scientific body such as the Essex Field Club, which has borne the chief labour of opposition, from a task which, from its political and litigious character, must necessarily be uncongenial.

G. S. BOULGER

PERRY'S "PRACTICAL MECHANICS"

Practical Mechanics. By John Perry, M.E. (London: Cassell, Petter, and Galpin, 1883.)

THIS book is one of a series of manuals now being published by Messrs. Cassell and Co., intended for the use of technical students, and claims, to quote the preface, "to put before non-mathematical readers a *method* of studying mechanics," which, if carefully followed, will supply "a mental training of a kind not inferior to that the belief in which retains in our schools the study of ancient classics and Euclid." A principal feature of the method consists in "proving" the various formulæ of mechanics by quantitative experiments. Of these many are described in the book, several of which, such as those relating to torsion and other stresses, &c., are carried on in many physical laboratories, and belong rather to physics than to mechanics. Another feature of the method more novel than the last is the gathering together of a few of the definitions and elementary theorems of mechanics, such as the parallelogram of forces, in a chapter at the end of the book called a glossary. Even then no formal proofs are given, probably because they are unnecessary, since on p. 2 we are told that the reader "cannot know the parallelogram of forces till he has proved the truth of the law half a dozen times experimentally with his own hands."

This kind of proof is very different from the evidence usually tendered for the fundamental laws of mechanics, but we must not forget the class of readers, entirely different as they seem to be from any we have ever encountered, for whom the book is intended. We are reminded of this on p. vii., when we are told that "the standpoint of an experienced workman in the nineteenth century is very different from that of an Alexandrian philosopher or of an English schoolboy, and many men who energetically begin the study of Euclid give it up after a year or two in disgust, because at the end they have only arrived at results which they knew experimentally long ago."

Thus the empire of the Greeks in geometry must give place to the supremacy of the intelligence of the working man, and even Euclid himself must fall from his high estate to be compared and contrasted with the modern schoolboy. But this latest born of time apparently possesses even higher powers. If made "to work in wood and metal," "to gain experience in the use of machines and use drawing instruments and scales," he will arrive at a condition in which "he may regard the 47th proposition of the First Book of Euclid as axiomatic," and "he may think the important propositions in the Sixth Book as easy to believe in as those in the First." Truly here at last has been found in geometry a royal road. But when Prof. Perry has raised our opinion of the modern schoolboy and working man to this high eminence we feel a rude shock on reading the second page of the book, when we discover that these rarely gifted, ideal beings, so favoured of the gods in geometry, may perhaps not be able to apply to a practical example a simple algebraical rule.

In reading the book, especially in its earlier chapters, we are struck by the want of logical arrangement and of strictness in the definitions, by the frequent use of terms which have not been previously defined, or not adequately defined, and of writing so careless in its style as frequently to become unintelligible. The theory of friction, ... the limited extent to which alone it is given, is inserted piecemeal into parts of the two first chapters and into the glossary, and the ordinary laws are not explicitly given until nearly the end of the book, but in their place we have the loose statements, "friction is proportional to load," and "friction is a passive force, which always helps the weaker to produce a balance." The English of the last sentence is as curious in character as that of one on p. 13, "This rubbing is a very slow motion."

The doctrine of the conservation of energy or of the conversion of energy into heat is nowhere explicitly given, although the theory is assumed in numerous applications. Can it be that the modern schoolboy, duly equipped, is able not only to surpass Pythagoras by regarding the 47th proposition of Euclid as axiomatic, but that he has come to view the great physical theory as equally self-evident? It must be so; otherwise, having only been told of energy as the equivalent of mechanical work (p. 5), he would not understand the meaning of the obscure sentence—"Every experiment we can make shows that energy is indestructible, and consequently, if I give energy to a machine, and find that none remains in it, it must all have been given out by the machine."

We find the leading laws of hydrostatics inserted in a paragraph on water, which is included in the chapter on materials, fifty pages after the uniform transmission of fluid pressure has been assumed in the article on the

hydraulic press, and we are told (*note*, p. 75) that a cubic foot of water possesses, "in virtue of the steadiness of the motion, pressure or potential energy," &c. On p. 74 "total pressure" is used for resultant pressure. Nowhere throughout the book is the theory of the centre of gravity given, or the name even defined, yet the author – to the chagrin of any student who believes it—does not hesitate on p. 142 to preface with the words "it is evident" an application o he usual formulæ defining the position of the centre of gravity to the case in hand. The term "radius of g ration" is used on p. 144, but not defined until p. 196. The statement that "velocity is the speed with which a body moves" reminds one of Lord Palmerston's definition of an archdeacon, and we wonder what kind of notion will be gained of the motion of a body in a curve by any one who is told in a definition of centrifugal force that, "if a body is compelled to move in a curved path, it exerts a force directed outwards from the centre." We have also the following as a definition of the pitch circle :—"Two spur wheels enter some distance into one another, and the circle on one which touches a circle on the other, the diameters of these circles being proportional to the numbers of teeth on the wheels, is called the pitch circle." Could even the common sense of high quality, postulated of the readers of the book, enable them to select, from the infinite number of pairs of circles satisfying the above conditions, those which represent the pitch circles required?

In the rule for the differential pulley block we are surprised to find that the movable pulley rises through the whole, instead of half, the difference of the amounts of rope uncoiled from the two pulleys in the upper block. On p. 30 it is said :—"In the study of the motion of a slide-valve it is much too usual to assume that the piston's motion is what is shown in Fig. 18 as pure harmonic motion." How shall we reconcile this with the information we have already received on the previous page that Fig. 18 (a skeleton drawing of a crank and connecting rod) does not represent pure (why not "simple?") harmonic motion except when the connecting rod is infinitely long?

In the rule which is inserted on p. 46 to find M, the constant should be twice that given, or about 59,500. On p. 64 our powers of comprehension are baffled in endeavouring to attach a meaning to the assurance that "50 foot-pounds is the total energy stored up in the wire *in the shape of a strain*." (The italics are ours.) In the rule given in Art. 192—we presume for *perfectly* elastic bodies—the momentum communicated from the one body to the other is just twice that stated.

We are told (p. 193) that the motion of a point in the balance of a watch is very nearly pure harmonic, if we suppose the point to move in a straight line instead of a circle, but we confess that the advantage of so describing the motion is not apparent, nor should we be disposed to call the friction in a twisted wire fluid friction (p. 199) because the friction in this case, as in that of fluids moving slowly, is proportional to the velocity.

The long array of mistakes given above, which by no means exhausts our list, forms a very serious accusation against the author.

His book has much disappointed us, for although some of the chapters, such as those on shear and twist, beams, graphical statics, and spiral springs, treat in a simple manner subjects which in parts present some difficulty, yet the defects to which we have alluded are far too grave to be compensated by any excellence in particular parts of the work. In the earlier chapters especially, the author has failed in the fundamental excellences of book-writing, in logical arrangement and clearness and exactness of expression, in just those qualities in fact in which he would have been most successful if he had aimed at writing more from "the standpoint of an Alexandrian philosopher." J. F. MAIN

OUR BOOK SHELF

Der Norske nord-hass-expedition, 1876-1878. VIII. Zoologi, Mollusca. I. *Buccinidæ*, ved Herman Friele. Med 6 plancher og 1 kart. 4to. (Christiania: Grön-dahl and Sons, 1882.)

I HAVE already, in the *Annals and Magazine of Natural History* for this month, given some account of the scientific expeditions which were made by the Norwegian Government during the years 1876, 1877, and 1878, to explore the sea-bed lying between the coasts of Western and Upper Norway and Iceland, Jan Mayen, and Spitzbergen; and I also noticed the series of publications which embody the result of these expeditions, including the present volume. I now propose to say a few more words on the subject of Herr Friele's work.

The great family of *Buccinum*, which is treated in it, is most perplexing in a taxonomical point of view; and its generic type, *Buccinum undatum*, is so unusually prolific and abundant, and consequently so variable, that no two conchologists agree as to the number of species belonging to it. In a short paper of mine on the northern species of *Buccinum*, which appeared in the *Annals* for December 1880, I ventured to consider as varieties of that species and of *B. grænlandicum* (which is probably also a variety of the polymorphous *B. undatum*) no fewer than 25 other so-called species. Such amalgamation will doubtless not be admitted by many conchologists; but the examination and careful comparison of an immense number of specimens from all parts of the North Atlantic which have fallen under my examination, warrant me in forming the above opinion. If we were to substitute the German word "geste'lt" or form for species, subspecies, and varieties, it might perhaps be a more safe and convenient mode of definition; but naturalists are not yet prepared to change the time-honoured system of Linnean and Lamarckian classification.

Herr Friele's work and the other publications to which I have referred are written in excellent English, as well as in his native language. The descriptions of new species are in Latin, which is scarcely so well adapted as English or French for the terminology of natural history at the present time; although his descriptions are far superior to the barbarous if not illiterate productions of Reeve and some other modern conchologists. The distinctive characters of new species are for the most part given in the same order, so that the description of one species can be more easily compared with that of a congener. This is an important and nearly indispensable desideratum. One new genus (*Jumala*) is proposed, having *Fusus Turtoni* for its type; and it appears to be based on Prof. G. O. Sars's description of the odontophore or dentition. Ten species are also for the first time described and figured, viz. one of *Jumala*, seven of *Neptunea*, and two of *Buccinum*. I regret that I must disagree with my friend the author as to the number of genera (six) into which he has divided the northern species of *Buccinidæ*. I should be disposed to attach more value to the operculum than to the odontophore as a generic character. Nor can I accept all his

new species. The species which he considers my *Fusus curtus* is very different from the *F. Sabini* of Gray, or the *F. togatus* and *F. Pfaffi* of Mörch (all enumerated by Friele as synonyms); and I regard the last-named three species as the *F. ebur* of Mörch and not as my *F. Sarsi*. However, notwithstanding any trifling errors, if they be errors, the work of Herr Friele is not only admirable and valuable, but is imbued with that scientific merit and modesty which are peculiar to our fellow-workers in Scandinavia; and we shall look forward with great interest to the continuation of his papers on the Mollusca of the Norwegian North-Sea Expedition.

J. GWYN JEFFREYS

Tables for the Use of Students and Beginners in Vegetable Histology. By D. P. Penhallow, B.S., late Professor of Chemistry and Botany in the Imperial College of Agriculture, Japan. (Boston, 1882.)

THIS little work by no means meets the expectations which its title arouses. The author states, indeed, in his preface that the scope of the work is purposely limited, but the limits are so narrow that the work will not be of much use to the student who has a competent teacher, and it will not be of any use to the beginner who is attempting the study of vegetable histology by himself. The book deals simply with the micro-chemistry of plants; the reagents are enumerated, as are also the various substances to be met with in the cells, but no attempt is made to give an account of the mode of application of the reagents for the detection of the substances, and in certain important cases (the chloriodide of zinc, for example) the mode of preparation of the reagent is not given. Not a word is said about imbedding, nor is any mention made of staining. The general mode of treatment of the subject is thoroughly unpractical. For example, silica is said to appear in plants "as a transparent deposit"; but every histologist knows that the silica in a cell-wall can only be made evident by incinerating with nitric acid.

The priority which the author claims can hardly be granted in view of the fact that Poulsen's valuable "Microchemie" has been in the hands of European histologists for several years. The selection of literature given at the end also betrays the author's want of acquaintance with his subject, inasmuch as no mention is made of such important works as Dippel's "Mikroskop" and De Bary's "Vergleichende Anatomie."

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

The Matter of Space

IN his paper on "The Matter of Space," in NATURE, vol. xxvii. p. 349, Mr. Charles Morris has given us an excellent exposition, and, as I believe, in general a perfectly correct one, of the fundamental laws and properties of matter and motion. But as I have for some time been investigating the views which he describes with exactly the results and consequences at which he has arrived (excepting only in one material difference to which I will presently return), a little outline of the mathematical form which I found that the discussion of the subject could receive, and to which it was accordingly submitted in my examinations of its scope and contours, will aid readers of Mr. Morris's paper, perhaps, in attaching clear ideas to some of the expressions which he uses, and in thereby discussing and estimating very easily and fairly the positive truth, in general, or in a few points, of the paper's considerations, the just degree of reliability at all events, which the marvellous maze of intermetted motions possesses, which he has most tersely and graphically, and at least in the main, as it appears to me, correctly and truthfully described.

Angular momentum, or (for a particle of unit mass) the rate of description of sectorial areas, is, like actual energy, a quantity of two dimensions in space; it is in fact the vector-product of (or the quadrilateral area between) the two radii of the particle's orbit and hodograph. Tractive momentum, or the product of the unit-particle's radius-vector and the resolved part of the particle's velocity *along* (instead of across) the radius-vector, is equally a quadratic product (but differently estimated) of the two foregoing orbit and hodograph radii. It is not the rate of description of *an area*, like angular momentum, but the time-rate of the square on the orbit-radius. The time-rates of each of these momenta are similar to them in space-relation, and are respectively angular moment or twirl (of a force-couple) and tractive moment or wrest (of a motor-couple). But if a small step of angle is the ratio of a circular-arc step (or of a small step along its tangent) to the circle's radius, this being numerical, a twirl's work through this small step of angle is similar in space-relation to the twirl itself and to its time-effect, or angular momentum.

The same similitude in space-relation will exist between a wrest, or motor-couple, and its time-effect (or tractive momentum), and its small step of work, if, in imitation of the practice for a twirl's or force-couple's action, a wrest's space-step is defined to be the ratio of the particle's *step along* the radius to the orbit-radius. This counterpart of angle-step may be called a traction-step; and it is the small percentage of elongation which the radius undergoes. If this construction is assumed, there ensues from it a close, and evidently significant, analogy between the time-rate of *orbit radius* square (which denotes at once, in space-relation, a *motor*-couple and its time- and space-effects) and the *hodograph-radius* square (which expresses simultaneously in space-relation a *force*-couple and its time- and space-effects). Although the square of the hodograph-radius signifies the square of the material point's velocity, or its *directed* actual energy, I conceive that the square of the orbit-radius represents a square of undirected velocity, or an undirected energy of "higgledy piggledy" motion of the material point; and its time-rate is *a horse-power of* the point's quaquaversal, or undirected actual energy. Viewed in this light, twirls or force-couples and their time- and space-effects are all graphically synonymous with actual undirected energy; but wrests or motor-couples and *their* time- and space-effects are all graphically synonymous with *horse-power* of *undirected* actual energy. For these latter quantities Mr. Morris uses indifferently the various words, "momentum," "heat momentum," "heat velocity," "heat," "motor energy," "heat energy," "heat vibration," "centrifugal energy," and "centrifugal or motor vigour," of a moving point; but while they are all, as he rightly opines, convertible quantities in their relation to graphic space, yet the theory of force-couples with which (*mutato nomine*) they are equally convertible in the same space, teaches us that a twirl-group falls mechanically, according to its association with time and angle, into three distinct divisions, of an action (the couple) and its time- and space-effects (angular momentum and accumulated work). It is so also with the motor-couple's graphic-space measure, "vigour." In proper combinations with time and traction-ratio[1] it becomes either an action or a kind of momentum or a form of work. But in discussing these new quantities' properties two maxims of construction and interpretation must be kept constantly in view.

In the first place, we must not expect a motor-couple (although it tends to alter ϕ) which endows a point with undirected horse-power, to tend to lengthen or shorten the point's radius-vector *in the same way* that a force would do. If by their actions motor-couples can *in any way* oppose the action of a force or force-couple, it must be, not by exerting force themselves, but by giving rise to force where they act. Now motor-couples can no more act intelligibly upon a single point (to range a radius's extremities towards or from each other) than a force-couple can (to turn a radius's two ends round each other). Hence motor-couples must produce force in a material point in virtue of the point's being an aggregation of material points, or in other words the appearance of force is a sign of the compositeness of the material point upon which it acts. *Per contra*, forces can produce force-couples, or

[1] The integral of traction-ratio, $\int d\phi = \int \frac{dr}{r} = \log \frac{r}{c} = \phi$, I identify with Rankine's "thermodynamic function" (for which he uses the same symbol, ϕ) usually termed "entropy" in works on thermodynamics.

if properly combined can balance them, on a collection of material points, if certain internal conditions (always including conservation of force-effects and conservation of twirl-effects) of the component points' mutual force and couple-actions on each other, which we call certain static relations of the system, are fulfilled; and then we have forces on such an aggregation either giving rise to or holding in check force-couples acting on it. But no combination of force-couples, on the other hand, can either produce in the system, or resist in it, the action of a single force.

Now as a motor-couple and its parts exert time-flow of one form of energy, they differ from a force-couple and its parts in the same way that these differ from uniform rotation and translation; and as it happens that while rotations can combine on a system to produce translation, and not the opposite arrangement, and just the reverse of this relation prevails in force-couples and their forces, so we may infer that in a system of connected points motor-couples would have the opposite property to force-couples, and in combination together on the system, instead of being produced by, they would either wholly or partially produce, a form of resultant of the nature of a motor-couple part. This kind of resultant, too, will exert a tendency on the system as a whole, with equal and similar intensity at all its points. Such a combination of motor-couples on a body, therefore, will in general communicate to it by their conjunction, not horse-power of undirected actual energy in the same manner as a single couple would, but some or no resultant couple-part, and some or no resultant couple, just as a set of forces, (or rotations) applied to a body may yield a mixed resultant of a force and couple (or of a rotation and translation), the couple in one case and the translation in the other both taking effect upon the body as a whole, since each is quite devoid of any particular point of application in the body. This property, which we may reasonably assign to motor-couples, of furnishing in combinations on a group of material points a dual resultant in general, and the condition that they exert singly a time-flow of undirected energy, are together the first maxim to be kept in view in discussing their effects; for the double-resultant's nature, of a congregation of motor-couples, in general resembles that of a screw's motion, which is partly translative along and partly rotative round a polar axis. Along a given line through the system therefore this resultant acts jointly, partly as a wrest, or residual motor-couple, and partly as a couple-half, of whose nature and effects no attempt, in what precedes, has yet been made to give an explanation.

Although such views as these of matter and motion largely invite investigation, it is rather their conformity to observation and to such slender mathematical evidence as is derivable from the laws of graphic space than any rigid demonstration of their validity which has led me to put faith in them. Where time and entropy (which linear dilatation is above surmised to be) clasp and bind undirected energy in new ties of space, so singularly like but yet distinct from the well-known ones which regulate the transformations of directed energy, intrusion into the mathematical avenues of the problem is almost warded off by the obduracy of the new inquiry, and only scattered fields of cultivation, occurring ever and again along his road, assure the venturesome wanderer in the new tract that the course before him still always lies in habitable regions.

It would be presumptuous therefore to insist, until the mathematical field has been thoroughly explored, upon a preference of one view or hypothesis of motor-actions, as decidedly superior to another; but adopting, as Mr. Morris does, the opinion that the effects of motor-actions are conserved, and adding to this an assumption that in groups of points subjected to them the mutual conservation may not be (as it always is among the mechanical connecting forces of every piece of ponderable matter) perfect and complete in the system by itself, *without* reckoning on to it one other external point, then a material simplification of the views unfolded in his paper would, I believe, be introduced, by adopting a different hypothesis from that which Mr. Morris advances of the nature of the ether as an exceedingly attenuated form or "fourth state" of gross "gravitating" (or ponderable) matter.

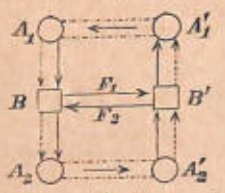
If Nature's course could be retraced to the beginning of time, we may suppose in that *gouffre* of antiquity ether to have been differentiated from gross matter in this way, that whereas internal conservation of motor-effects suffices to weld a group of material points into a resultant yielding system, then, no limits of smallness being imposed upon the group, it is allowable to define a point (in the language of graphic space) of gross or ponderable "gravitating" matter as an originally differentiated mass of aërilian points, upon which the dual resultant of the conjoined motor-couples on the aërilian points will take effect. A part of this resultant is a couple-half, about which we know nothing; and we may reasonably suppose it to be attractive and repulsive force acting on the baric point. The other part of the resultant is an unbalanced motor-couple, only susceptible of conservation as to its effects by an equal and opposite one in some other similar mass or aërilian assemblage. A certain integrity can, I conceive, be imparted to this first hoard of scaffolding of the new theory's construction, by locating the conserving couples, of which the motor-couples supposed to be aboriginally welded together are the counter-equivalents, not in a single, but partly in one, and partly in another, set of free-moving aërilian points in such a way that, while the resultant motor-*couple* is balanced by the first set, the *force*-resultant of the massed couples' combination, will be balanced *through* a counter-equivalent force-resultant in another mass-point *by* the free motor-couples of the other aërilian set. The residue of this set's couples will be occupied in opposing the unbalanced couple-residue of the couples massed together in the second baric point, while these couples' transmittent force-resultant will be opposed *by* the still uncompensated portion of the motor-couples acting on the first free-moving set. Perfect compensation of the two dual resultants cannot then take place under these conditions, without exact counter-equivalence of the half-couple (or force-) resultants, and therefore also exact counter-equivalence in their native state between the two groups of motor-couples acting on the two free aërilian sets; at least, if we assume massed and moored aërilian points to have been all originally endowed in pairs with equal counter-couples, and if their modes of collection into mass-points and of producing force-resultants were aboriginally all alike.

In our present undeveloped knowledge of the mathematical properties of tractive or motor-couples, and of their random-energy horse-powers' geometrical relations to the common mechanical modes of exertion of directed energy in forces and couples, it would be premature and vain to speculate as Mr. Morris does, I believe too boldly and fearlessly, in his paper, upon Nature's established order of progressive collection of baric points into "spheres," or into the atoms and molecules which further build up atmospheres, suns, planets, and all ponderable bodies. My views diverge here from his in, at least, one salient point, that the ether (as we must still in sober science term his "interspheral matter") is held, in his opinion, to be ponderable or "gravitating," and to be endowed with a vigour of motion which exempts it from yielding to its vigour of gravitation. By thus identifying "interspheral matter's" or the ether's particles with those of matter "employing its motion secondarily about new centres of gravity" (of *really* gravitating or ponderable "spheres"), the way is barred at once of explaining the ultimate sources of attractive and repulsive force by exercises of motor "vigour." But further than this we must evidently abandon definitely all reasonable hope of constructing out of particles' "incessant leaps in nodes of an interminable network of motions, affecting in long motor lines myriads of interspheral particles," any intelligible framework of the important laws of radiation, magnetism, and electricity which we know that a clear comprehension of the "interspheral" ether's real constitution would immediately unfold to us, if its real nature and that of its relations to ponderable matter were rightly understood. In the form therefore in which Mr. Morris's theory presents itself to us, it fails completely (by only the slightest possible illusion, as I venture to submit, in the choice of its principal hypothesis) in attaining the admirably well pursued and well nigh compassed object of its otherwise exhaustively clear and excellently propounded arguments and demonstrations.

In the view which I have here advanced, massed assemblages of aërilian points form irrevocably the points of gross or ponderable matter, while an equal number of moored points, inseparably connected two and two with the former ones, form bound aërilian assemblages equally untransformable and forming active individual parts of the unchanging ether. That the latter points, unlike those of the massed group, may rove at large in graphic space, does not preclude them from all occupying a common point in another space domain, just as a number of balloons may be all at one height, whatever the courses of their tracks upon a map may be. Nor, again, does an encounter of two balloons' courses on a map necessarily entail collision between the two balloons, since at the time they may be at different

heights. It is thus quite conceivable that, in a scale of space foreign to our graphic measures, the free roving aërilian set may all occupy a common place in this foreign scale of space, and that a massed and a moored aërilian point may have the same position in graphic space without impinging on each other, as such points are not at the time at one and the same place in the foreign space. The moored or bound ether may thus traverse the space occupied by the massed ether of gross matter without mutual interference; but, whether superposed or not in ordinary space, the pair of ether sets which compensate resultant actions of two gross-clustered sets of a pair of points of baric matter, will form, however they may mingle graphically, two orbs of ether exerting each (corporately) exactly counter-equal free-orb couples.

If for example the baric points B B′ are urged towards each other with a ponderomotive force or flow of ordinary baric momentum F_1, by the motor actions on them of two ether-orbs A_1 A_1' in counter equal intensities, force-momentum only will be



transferred from A_1 to A_1'; while the tractive momentum (or as we may presume, the heat-energy) accompanying it will only be transferred from A_1 to B, and a similar transfer of thermal energy or tractive-momentum will at the same time take place from B′ to A_1'.

Should it, in the next place, be required to oppose the action F_1 by an equal counter-force F_2, a pair of ether-orbs A_2 A_2' must be superadded to those already urging B and B_1', so as to urge them in the opposite direction. It will be seen from the figure that the total effect of this and of the previous orb-pair's actions will simply be that the pair of ether-orbs connected with B will transmit motor energy from one to the other (from A_1 to A_2), and the other ether-pair will also transfer an equal amount of energy contrariwise from one orb to the other, without any leakage of ordinary momentum occurring at B and B_1', by the neutralised action, into the channel B B′.

Newcastle-on-Tyne, February 10 A. S. HERSCHEL

(*To be continued.*)

Terrestrial Radiation and Prof. Tyndall's Observations

IN NATURE, vol. xxvii. p. 377, I see a notice on Prof. Tyndall's observations on terrestrial radiation, with the author's concluding remarks, that meteorologists should not be offended by his saying that from out-siders equipped with the necessary physical knowledge they may expect valuable aid towards introducing order and causality among their observations. May I be permitted to state that Prof. Tyndall will give no offence, at least to the meteorologists whose works are advancing this science at the present time.

Prof. Tyndall tries to prove by his observations the extreme importance of vapour of water as a check to terre-trial radiation, and he mentions the much greater difference between a thermometer in the air four feet from the ground and another on cotton wool on a morning when snow was lying on the ground than on other nights, equally clear, but with higher temperatures of the air and no snow. Now it is well known that, *pari passu*, a surface of snow will be colder than a surface without, because (1) snow is an excellent radiator; (2) because, as a very bad conductor, it shelters the surface from the influence of the higher temperature of the soil. In the observation on December 10, the thermometer on cotton wool was so cold because it was immersed, so to say, in the coldest stratum of air near the ground. To my mind, the manner in which the observation was conducted does not prove what Prof. Tyndall advances. To isolate, so to say, the influence on radiation of the atmosphere itself, he should have placed, between two poles, at some feet above the ground, a plank, and on it his cotton wool and thermometer. No doubt that this thermometer, isolated from the snow, should have shown a higher temperature than his thermometer placed on the surface on cotton wool.

Prof. Tyndall lays great stress on the fact that the difference between the temperature in the air and on the ground was less in clear nights with a higher temperature and greater quantity of vapour of water in the air, and sees in this a confirmation of his opinion on the great influence of vapour of water in checking radiation. I do also see in this the influence of vapour of water, *but not of its absolute quantity*, but of *relative humidity*. Once the dew-point is attained, the cooling of the thermometer on the ground is arrested. The whole question between Prof. Tyndall and many physicists and meteorologists is this: nobody negates the influence of vapour of water on terrestrial radiation, but Prof. Tyndall ascribes this influence to vapour in the gaseous state, while his opponents hold the opinion that in this state vapour of water has a diathermacy scarcely different from dry air, while, condensed in small ice crystals or water droplets, it really interposes a very efficient screen to terrestrial radiation, even if, which sometimes is the case, it is perfectly transparent to light, *i.e.* invisible to our eye. Another influence of water on terrestrial radiation is admitted by all: that is, that of the latent heat in the deposition of dew and hoar frost.

If we wish to make meteorological observations bearing on the question, the following *modus operandi* should be adopted: (1) observations should be made in climates where, with a tension of vapour greater than that which obtains in England in winter, the relative humidity is yet so small that there is no dew on clear nights, or at least it appears rather late; (2) three thermometers placed on cotton wool, but at different heights above the ground should be observed, say one on the ground, and the others at heights, say from 10–100 feet above.

If Prof. Tyndall's views are right, the highest of the thermometers should show by far the lowest temperature, as it is not screened from radiation by the vapour of water diffused in the lowest stratum of air. I think every meteorologist will express the opinion that there will be scarcely a difference in this case. As to the observations in different climates, those made where the relative humidity is low should give no greater difference between the thermometer in the air and on the wool than the observations in England on clear nights, with the same vapour tension, if Prof. Tyndall's hypothesis be admitted. I think we have already many observations which prove that, with vapour-tensions much above 0″·181 (or 4·6 mm.), *i.e.* above that of saturation at 32° F., terrestrial radiation is very great, if only the sky is clear and the relative humidity small. No doubt the decrease of the temperature of the air from the midday maximum to the night minimum is caused by terrestrial radiation. I give some figures from the observations at Biskra, in the Algerian Sahara.[1]

	Difference of daily max. and min.	Mean temperature.	Tension of vapour.	Relative humidity.	Amount of cloud.
January ...	25·4	56·8	0·264	61	1·6
August ...	39·2	89·6	0·557	40	0·8
October ...	35·6	68·4	0·432	58	0·9

In an arid climate in low latitude the non-periodic variations are but small, and the difference between the maxima and minima is very near to the daily range of temperature. As the amount of cloud is very small in all three months taken here, the conditions for terrestrial radiation are very favourable. If vapour of water in the gaseous state impeded terrestrial radiation so much as stated by Prof. Tyndall, we should expect to find the daily range smaller in August than in January, on account of the double amount of vapour in the air. The reverse is the case, the daily range being by 14·8 greater in August than in January. Has anybody observed a daily range of 39·2 in England, be the amount of cloud and the vapour-tension ever so small?

I must add that in all observations bearing on terrestrial radiation we must not forget that other substances besides water in its three states may interpose a screen to radiation. I mean especially dust and smoke of all kinds. Now far from large cities, there are many reasons why in winter, especially when the ground is covered with snow, the air will hold less of these impurities than in summer, as in winter there are no fires of forests and peat-bogs, there is little inorganic dust, because the humidity of the soil, and still more so the snow, prevent it; organic dust, germines, &c., are also absent, or present in very small quantities, on account of the small amount of plant and lower animal life. The absence of dust and smoke explains the great purity of the air in winter, so favourable to solar and terrestrial radia-

[1] "Annales du Bureau Central Météorologique de France," 1879, vol. ii.

tion, as well as the purity of the air at great heights, especially above the snow-line.

Prof. Tyndall has certainly lost sight of this when he attributes the diathermacy of the air in winter only to the small amount of vapour of water. The same is the case when he points to the relatively small nocturnal radiation on clear nights in many tropical countries. In the case of many of them, besides dust and smoke, the *high relative humidity* has much to do with the small amount of cooling during the night. What quantities of latent heat are liberated by the formation of dew in humid climates of low latitudes, and how much the nocturnal cooling must be impeded by it, everybody can imagine who has been in these countries, or only read scientific travels to them.

A. WOEIKOF

Diurnal Variation in the Velocity of the Wind

THE observations discussed in Mr. Buchan's interesting article on this subject leave little to be desired, and with most of the conclusions meteorologists in general will agree. I am surprised, however, to find such an eminent authority accounting for the large diurnal oscillation on land, solely on the ground of its being due "to the superheating of the surface of the ground, and to the ascensional movement of the air consequent thereon, which tend to reduce the effects of friction and viscosity of the air."

There may perhaps be more hidden within this sentence than appears from the wording of it; but, taking it as it stands, it certainly omits what I believe to be the most important factor in the whole result, viz., *the interchange of motion between the upper and lower layers of the atmosphere, occasioned* by the ascensional movements during the day over superheated land. This has been most clearly shown by Dr. Köppen in an article in the Austrian *Zeitschrift für Meteorologie*,[1] by successive rejection of inefficient causes, to be the only means by which such increase of velocity could be occasioned near the earth's surface.

It is not clear, moreover, how the ascension currents could otherwise diminish the friction of the air enough to account for such a large diurnal increase of velocity. The effect of the increased temperature alone, would certainly be to increase the friction, but as Köppen shows from Meyer's formula for the coefficient of gaseous friction, the daily range of temperature would only cause the friction of the air to vary from ½ to 1 per cent. of its whole amount,[2] so that this factor is evidently without any appreciable influence on the diurnal period.

In the paper already referred to, Dr. Köppen has gone into the whole question most minutely, and a perusal of it will, I think, convince most persons, that the chief factor in causing the diurnal increase of wind-velocity over land is the intermixture of air (*luft-austausch*) resulting from the uprise of heated air from the surface, and the consequent downfall of cooled air to it, "bringing down with it," as Espy told the British Association in 1840, "the motion which it has above, and which is known to be greater than that which the air has in contact with the asperities of the earth's surface."

Among the facts cited by Köppen in favour of his theory may be noted the following:—

1. The fact that in Europe the ratio of the velocity of the wind to the gradient, is greater for N.E. winds and in summer than for S.W. winds and in winter; together with the circumstance that the temperature decrement, and therefore also the facility with which local ascension and descension currents may be formed, is greater under the former conditions than under the latter.

2. That simultaneously with the diurnal increase in the velocity of the lower layers of the atmosphere, those above appear to be retarded.

3. That on stations near the earth's surface the curve of absolute humidity reaches its minimum about the time of maximum wind-velocity, while at elevated stations, such as the Faulhorn, the humidity reaches its maximum at the same time.

In fact it may be concluded, as Köppen graphically puts it, "that the greater the difference of the temperature of the air in a vertical direction, the smaller are the differences in the humidity, barometric pressure, and motion of the air, and that in the early hours of the afternoon the inhabitants of plains are placed to a certain extent on a higher, and the dwellers of Alpine heights on a lower, level, relatively to these elements."

E. DOUGLAS ARCHIBALD

[1] "Die tägliche periode der Geschwindigkeit und Richtung des Windes," September heft, 1879.

[2] Meyer's formula in English measure is $\nu = \nu_0 (1 + 0.0014t)$, where ν_0, ν, are the friction coefficients at 1° and 32° Fahr. respectively.

The Large Meteor of March 2, 1883

THE meteor described by Mr. R. W. S. Griffith in the last number of NATURE was also observed at Bristol and Bath. At the latter place it was seen by Mr. J. L. Stothert at 9h. 33m. 40s., passing in the direction from α Hydræ to η Canis Majoris. The brilliancy of the meteor was equal to twice that of Venus; colour yellow; motion slow; no train. Comparing this observation with that obtained by Mr. Griffith, it would seem that the meteor probably belonged to a radiant point near Lyra, rising in the north-north-east at the time of its appearance. A meteor shower was observed by the writer on March 14, 1877, between 14h. 12m. and 15h. 43m. from the point α 277°, δ 25°+, the members of which were somewhat slow and devoid of streaks or trains, and the fireball of March 2 last appears to have belonged to the same stream.

It would be important to hear of additional observations of this meteor. Its considerable brightness, and the fact that it appeared at a time when it must have been widely observed, lead me to hope that many other records of its path have been preserved. In all such cases it is very desirable to give the R.A. and Dec. + or − of the beginning- and end-points of the observed path. Descriptions by the stars or compass-bearings are likely to be less accurate, and are often difficult to reduce.

In the *Observatory* for September, 1879, p. 129, I mentioned that "during the first four days of March fireballs have been very numerous, especially on the 1st, 2nd, and 4th." This meteoric epoch is therefore well confirmed by the fireball of the 2nd inst. which it is hoped will aid us in determining one of the chief radiant points of the date.

W. F. DENNING

Bristol, March 12

A VERY brilliant meteor was seen here on March 2 at 9.35 p.m. It burst forth in the immediate neighbourhood of Sirius, and passed downwards to the west at about an angle of 40° from the perpendicular, disappearing after a course of about 25°. Its light was so strong as to make the distant trees, fields, and hedges perfectly visible, brighter than the brightest moonlight. Its colours also were very decided, changing quickly, much as does Sirius to the naked eye, but showing more of the violet at first, and afterwards more of the red.

J. L. J.

Capel, Surrey

On the Movements of Air in Fissures and the Barometer

I SHOULD be glad to add to my article "On the Movements of Air in Fissures and the Barometer" (NATURE, vol. xxvii. p. 375) a reference to an instrument devised by Mr. Whitehouse, and described in 1871 before the Royal Society (*Proc. Roy. Soc.* vol. xix. p. 491). The apparatus, which was intended to record minute variations of atmospheric pressure, consisted of two hydraulic chambers, connected by a tube or siphon, and buried in the ground. One of the chambers was left open at the top and exposed to atmospheric pressure, the other was closed and removed from such pressure; the difference in the level of the water in the two was a measure of the variation in the atmospheric pressure. This instrument reproduces those conditions to which the oscillation of the water-level in certain chalk-wells, coincident with the barometic changes, has been attributed. It was believed by the inventor that by its aid he had been able to detect atmospheric waves or pulsations at a distance from a storm-centre; it has not however come into scientific use.

I may further add to my brief allusion to colliery explosions a reference to the paper by R. H. Scott, M.A., F.R.S., and W. Galloway, Mining Engineer, entitled "On the Connection between Explosions in Collieries and Weather" (*Proc. Roy. Soc.* vol. xx. p. 292, 1872).

A. STRAHAN

28, Jermyn Street, March 10

THE PITT-RIVERS COLLECTION

IT will be remembered that some time past Major-General Pitt-Rivers, F.R.S., most munificently offered his far-famed Anthropological Collection to the University of Oxford on the condition that the University should erect a building adequate to contain it and display it properly. On Wednesday, the 7th ult., a vote was passed by Convocation authorising the Curators of the Univerity

Chest to expend a sum of 7500*l.* on the erection of an annex to the east side of the present University Museum to contain the collection and to provide the requisite cases and fittings; a vote of thanks to General Pitt-Rivers was also passed.

This most important collection, therefore, which commenced its public existence at Bethnal Green, and has so long been exhibited at South Kensington, will rest finally at Oxford, where it cannot fail to be studied with ever increasing interest and benefit to learning generally. The title of the collection as the "Pitt-Rivers Collection" is to be maintained, and the developmental and gradational system of arrangement devised by the donor, and carried out by him in the greater part of the collection, with such valuable and interesting results is to be retained. The new building, which will be provided with two galleries, will be entered by two doorways at different levels from the present University Museum.

The delegates of the Museum have elected Dr. E. B. Tylor to be Keeper of the Museum in place of the late Prof. Henry Smith, so that the new collection, as well as the anthropological collection of the late Prof. Rolleston, will fall into the hands of the man most suited to arrange and explain them.

JOHN RICHARD GREEN

THE death of Mr. Green, at the early age of forty-five years, we regard as a serious loss not only to historical literature but to science. We have frequently maintained that science has no peculiar sphere, that every field of human research is capable of scientific treatment. As we pointed out in reviewing Mr. Green's famous "Short History" and his "Making of England," he has the credit of having been the first historian who appreciated the function of science in a State, or the moulding power of the environment of a people. Not only so, but he distinctly aimed at showing that the history of a people is simply an evolution dependent for its course and outcome on the action and reaction between the entity and its surroundings. This conception of the function of the historian was probably even more distinctly brought out in the "Geography of the British Isles," by Mr. Green and his accomplished and congenial wife. As we pointed out in our notice of the "Short History" moreover, Mr. Green not only wrote his "History" on a scientific method, but gave large space in that history to a record of the progress of science and of scientific societies, as distinct and influential elements in the life of our nation. Indeed he may be regarded as the first historian who, breaking away from the old conventional methods of writing history from the outside, and thus mistaking the shell for the kernel, adopted the method of the physical geographer as distinct from the mere topographer, and, penetrating deep beneath the surface, traced the forces which have actuated the nation and brought it to its present standpoint. Although the impulse given by Mr. Green to historical study will certainly bear fruit, his loss cannot be overestimated. His "Making of England" was evidently only a prelude to a series of volumes in which he intended to show in minute detail the interaction between the various elements that go to make up the life of these islands,—the ethnical and moral elements on the one hand, and the encompassing physical elements on the other. Happily he has left behind him in a nearly complete state a second volume on "The Coming of the Northmen," which brings his scheme down to the point when it may be said that all the forces were in the field, the continued action of which has gone to make up the England of to-day. Since Mr. Green's death ample testimony has been borne to his rigidly scientific method of work, and the patience with which he wrote and rewrote ere his own severely critical standard was reached. It will be difficult to find a successor to Mr. Green so far as stirring eloquence of style is concerned, but we trust that his scientific method may find favour, and that historians in future will endeavour to trace the life of a nation as he did, after the manner of the biologist and physical geographer.

THE BOTANY OF THE "CHALLENGER" EXPEDITION

FROM time to time various contributions to the Botany of the *Challenger* Expedition have been published in the *Journal* of the Linnean Society, chiefly in the fourteenth and fifteenth volumes; but hitherto no part of the botanical results has appeared in the series of sumptuous volumes in which are recorded the discoveries and observations of the expedition. The Government have at length decided to devote one volume of about 350 pages and fifty plates to the elucidation of the flora of the more interesting countries visited, which the writer of the present article has undertaken with the assistance and under the superintendence of Sir Joseph D. Hooker. There can be no doubt that the Government are right in their estimate of the relatively small importance of the results obtained in botany as compared with those obtained in other branches of science; yet we think we shall be able to show that the botanical collections are sufficient to form the basis of a most interesting volume. It is almost superfluous to state that the botanist of such an expedition has little chance of exhausting the flora of any of the numerous countries or regions visited; and the task of elaborating the materials seemed at first an unpromising one. At many of the places visited, and especially some of the more interesting ones, the stay was too short and the means inadequate for making and drying large collections of plants. Nevertheless the naturalist, Mr. H. N. Moseley, seems to have lost no opportunity, having collected in almost every place touched at. Unfortunately the plants of the least-known countries, such as the Aru and Admiralty Islands, reached England in a very much damaged condition. But imperfect as they are, they include a large proportion of novelties, and indicate a flora rich in endemic species. The best collections, so far as number and quality of the specimens are concerned, are those from Chili, Juan Fernandez, Japan, the Sandwich Islands, &c.; yet they contain little or nothing new to science, and by no means fully represent the vegetation of the several countries. There remain the collections made in the remote islets of the Atlantic and Southern Oceans, which, with what was previously known, afford material for a practically complete flora of these isolated spots, so interesting to the student of the distribution of plants and animals. And it has been decided that this shall be the scope of the work.

The Bermudas, the oldest English colony, come first in the arrangement adopted. These islands, having an area of about one-seventh of that of the Isle of Wight, are situated about six hundred miles from the American continent, and although settled as long ago as 1612, nothing approaching a complete and critical account of their vegetation has hitherto been published. The flora is a poor one, especially in regard to number of species, and is evidently of comparatively recent origin, being in this respect in striking contrast to that of various other Atlantic islands—that of St. Helena, for example. The indigenous element has been, almost without exception, derived from the West Indies and the extreme south-east of the mainland of North America. By the indigenous element we mean those species which have reached the islands independently of human agency, direct or indirect. With unimportant, though rather numerous, exceptions, the indigenous and introduced elements are easily dis-

tinguished. A remarkable feature in the vegetation is the almost total absence of endemic forms. The possible important exceptions are the native palms. There are two or possibly three species, of which one belongs to the genus Sabal. Without due investigation, it has been generally accepted as a fact that there was only one indigenous palm, and that this was identical with the Sabal Palmetto of south-eastern North America; but in elaborating the palms for the "*Genera Plantarum*," Sir Joseph Hooker became aware that the imperfect herbarium specimens in this country represent two species, one of them at least evidently different from *Sabal Palmetto*. Several historical passages in Sir J. H. Lefroy's work on the Bermudas confirm this view. Thus, in one place it is recorded that the only food certain fishermen took out to sea with them on a given occasion was "Palmitoe berries"; and in another place that the workmen did not hesitate to share this fruit with pigs and other animals, and even preferred it to bread to eat with their meat. Every effort is being made to obtain material this season to set this question at rest. The earliest references we find to the vegetable productions of these islands are in the "Historye of the Bermudaes," edited by Sir J. H. Lefroy, and some of these are valuable, because they enable us to say with certainty that one species of *Opuntia*, for example, existed in abundance previous to the settlement of the islands.

François André Michaux was the first botanist who visited the Bermudas. In his case it was unintentional, the fortunes of war having been the cause of his spending a week there in 1806. He published an interesting sketch of the vegetation, though the following extract reveals a want of exactitude: "Parmi ces plantes [*i.e.* les plantes naturelles au pays] on en trouve plusieurs de l'ancien continent, qui ne paroissent pas de nature à y avoir été transportées: telles sont *Verbascum thapsus*, *Anagallis arvensis*, *Mercurialis annua*, *Leontodon taraxacum*, *Plantago major*, *Gentiana nana*, *Oxalis acetosella*, &c." The two last names must have been a slip of the pen. Since Michaux's time two imperfect lists of Bermudan plants have been published, both in 1873. One, by J. M. Jones, F.L.S., is marred by some rather gross errors in classification and nomenclature, yet it contains some interesting information. The other, by Dr. J. Rein, was prepared with greater care, and contains 128 species of introduced and indigenous flowering plants and ferns, besides upwards of 100 algæ. Altogether Mr. Moseley collected 162 species of plants. In addition to these is a considerable number sent to Kew by Sir J. H. Lefroy during his governorship of the islands, making a total of about 320 species that occur in a wild state. These may be classified as follows: indigenous, 130; probably indigenous, 57; certainly introduced, 133. The last number would be higher if we included solitary waifs of other species.

Next in order of the *Challenger* collections come those of St. Paul's Rocks and the island of Fernando Noronha, in which Mr. Moseley collected about sixty species, including a new species of *Oxalis*, one new Asclepiad, and one fig, &c. Had permission to collect objects of natural history not been withdrawn after the first evening, there is no doubt this collection would have been an important one.

Proceeding southward and taking the other islets on our way, we have Ascension, St. Helena, Trinidad (off the coast of Brazil, in about 20° 30′ S. lat.), Tristan d'Acunha, and the neighbouring islets Inaccessible and Nightingale; and thence southward and eastward, Gough Island, Lindsay and Bouvet Islands, Prince Edward and Marion Islands, the Crozets, Kerguelen Island, the Heard group, St. Paul and New Amsterdam. With the exception perhaps of Kerguelen Island, the published accounts of the botany of these oceanic islets are all most imperfect and scattered. We are unaware of any complete enumeration of the exceedingly meagre indigenous flora of Ascension. St. Helena has fared better; but the fifty or so indigenous species are lost amongst the 1000 species of introduced plants enumerated in Mr. Melliss's book "St. Helena," the botanical value of which consists chiefly in the figures of the endemic plants. Moreover Mr. Melliss did not elaborate the synonymy of the flora, and some of the Cyperaceæ were undetermined, whilst a few, we believe, were omitted.

The island of Trinidad is rather farther from the coast of Brazil than the Bermudas are from North Carolina, and very little is known of its vegetation. On the outward voyage of Sir J. Ross's Antarctic Expedition, Sir Joseph Hooker and some of the other officers landed on a small rocky cove, where they were unable to scale the barrier cliffs, so they could not penetrate to the interior of the island, and they brought away only one fern (*Polypodium lepidopteris*) and one sedge (*Fimbristylis*, sp.), though there were tree-ferns and other trees, in sight from the ship, on another part of the island. In 1874 Dr. Ralph Copeland, of the Dunecht Observatory, who accompanied one of the transit expeditions, landed on the east side of the island, and succeeded in reaching the elevated centre, where he found several ferns in great luxuriance, and collected a few scraps of plants, including a new tree-fern. The most interesting plant, however, was *Asplenium compressum*, a fern previously known only from St. Helena, though Melliss, by some unfortunate slip, records it from South Africa, Madagascar, &c. Dr. Copeland further states that, although most of the valleys of the north side of the island contained enormous numbers of dead trees, not a single living one was to be seen, except near the highest points. They appeared to have been dead many years and were mostly overturned. He was unable to investigate the phenomenon, but suggests that they may have been destroyed by goats, though he adds not a mammal of any kind was seen.

Tristan d'Acunha itself was explored by Dupetit Thouars in 1793, and he described the plants in a paper which he read before the *Institut* of France in 1803. The next botanist who visited the island was Carmichael, who published an enumeration of the plants he collected in the *Transactions* of the Linnean Society. Mr. Moseley botanised the same island and the neighbouring Nightingale and Inaccessible Islands, and collected not only those previously known, but also some new species of Cyperaceæ. Previously, too, *Gnaphalium pyramidale*, Thouars, was unknown at Kew, or rather a young plant of it collected by Carmichael could not be identified as such with certainty.

We have little space left, so we can merely mention the groups of islets in the Southern Ocean. Mr. Moseley added considerably to our knowledge of the flora of Marion Island and the Heard Group, and Kerguelen Island, whilst the Americans, Germans, and French, of their respective expeditions, investigated the Crozets and New Amsterdam and St. Paul's Islands. Kerguelen Island, the largest by far of all these oceanic islets, being about eighty miles in diameter, has been explored by the naturalists of the English, German, and American transit expeditions, and the results published. One of the most interesting discoveries of late years connected with the vegetation of these islets was made by the late Capt. Goodenough, about ten years ago, when he collected *Phylica arborea* in Amsterdam Island, till then only known in the island of Tristan d'Acunha, separated therefrom by ninety degrees of longitude, which in this latitude are equal to a distance of about 4700 miles. Mr. Moseley also found it abundantly in Inaccessible and Nightingale Islands. *Phylica arborea* is likewise remarkable in being the only plant of these southern islets that is arboreous in habit, though at the outside it is only about twenty feet high in the most sheltered localities.

W. BOTTING HEMSLEY

THE SHAPES OF LEAVES[1]

II.—*Extreme and Intermediate Types*

WHERE access to carbonic acid and sunlight is habitually unimpeded by the competition of other plants in any direction, the leaf of each species tends to assume a completely rounded form; the conditions are evenly distributed on every side of it. Such absolute freedom to assume the fullest foliar perfection is best found on the surface of the water. Hence most water-plants which have leaves lolling on the surface assume a



FIG. 10.—*Lemna minor.*

more or less distinctly rounded shape, the venation and other details remaining in accordance with the ancestral habit. Foliage of this character is found in the water-lilies and many other aquatic plants. The little entire lenticular fronds of the common duckweed, *Lemna minor* (Fig. 10), which coats all our small ponds and ditches, form an excellent example of the type in question. Here the shape is almost orbicular; the edge is entire; and the smallness of each separate frond is due to the minuteness of the plant and the obvious necessities of its situation. In the waterlilies we get a similar example on a much



FIG. 11.—*Nelumbium speciosum.*

larger scale, for these plants recline on broader and more permanent sheets of water, and draw nourishment from their large rhizome, sunk securely in the mud beneath, and annually accumulating a rich store of food-stuffs for the growing foliage.

Mr. Herbert Spencer (by whose kind permission two accompanying diagrams are copied from "The Principles of Biology") points out a distinction between the shapes adopted by such plants, according to their relations to a central axis. In the sacred lotus, *Nelumbium speciosum* (Fig. 11), the leaves grow up on long and independent footstalks, without definite subordination to any such axis; and they therefore assume an almost perfectly symmetrical peltate form. In the *Victoria regia* (Fig. 12) the footstalks, though radiating almost horizontally from a centre, are long enough to keep the leaves quite remote from one another; and here they assume an almost symmetrically peltate shape, but with a bilateralness indicated by a long seam over the line of the footstalk. The leaves of our own white waterlily, *Nymphæa alba* (Fig. 13), are more closely clustered, and have less room to expand transversely than longitudinally; hence they are somewhat longer than broad, and have a cleft where the *Victoria regia* has only a seam. *Limnanthemum* shows the same type on a smaller scale.

Among land plants, the conditions under which leaves



FIG. 12.—*Victoria regia.*



FIG. 13.—*Nymphæa alba.*

can fill out to the full rounded shape occur less frequently than among floating aquatic species; still, even here a very interesting set of gradations may be observed. The best example of all is that given by the common American May-apple, *Podophyllum peltatum*, where the separate radical leaves grow straight up from a stout rootstock on very thick and tall stalks, so as to overshadow all the other vegetation; and they assume a regular, circular, peltate form, exactly like a Japanese parasol. The radical leaves of our own English *Cotyledon umbilicus* (Fig. 14), springing from a perennial rootstock, for the most part on bare walls or unoccupied hedgerows, are able similarly to expand without interference, and catch carbonic acid and sunlight to their hearts' content. Hence they are orbicular and peltate, though they retain the characteristic crenate edge of most flat-leaved Crassulaceæ.



FIG. 14.—*Cotyledon umbilicus.*

But the upper leaves, springing from the flower-stalk, are more bilateral, as shown in the figure, though even these round out to a more or less orbicular form, owing to their exceptional access to air and light. The so-called garden nasturtium, *Tropæolum majus*, with leaves growing out at right angles into open space, has also peltate leaves, as has likewise the usually aquatic *Hydrocotyle*.

When the plant sends up leaves from a rich buried rootstock, so tall as to overshadow the surrounding vegetation, but subordinated to a common centre, they usually assume the reniform shape. This type is particularly well seen in the various coltsfoots—for example, in *Tussilago farfara*, *T. petasites*, and *T. fragrans* (Fig. 15). Similar types occur in *Asarabacca*, and in the marsh marigold, *Caltha palustris*. Extremely similar to the leaf of *Caltha*, though on a smaller scale, is that of one true buttercup,

[1] Continued from p. 442.

Ranunculus ficaria, the lesser celandine, which produces its foliage in early spring from buried tubers, and so anticipates other plants, having the air all to itself for a couple of months, after which it gets overshadowed by later comers. The same type recurs pretty closely in the radical leaves of its allies, *R. auricomus* and *R. parviflorus*, as also somewhat more remotely in the ivy-leaved crowfoot, *R. hederaceus*, which creeps, unimpeded, over soft mud. Many early spring plants have lower or radical leaves at least of this reniform type, because they grow in comparatively unoccupied ground. As an example, take ground-ivy, *Nepeta glechoma* (Fig. 16). The violets represent a closely similar case. Many of these plants,



FIG. 15.—Typical leaf of *Tussilago* genus.



FIG. 16.—*Nepeta glecoma*.

however, produce later on, when foliage grows thicker, much more lanceolate leaves. In the burdocks, docks, &c., this type is persistent.

On the other hand, where the distribution of carbonic acid is most scanty, or where the competition is fiercest, or where the competing plants are supplied with no reserve to enable them to send up shoots which overtop their competitors, immense subdivision into leaflets takes place, and these leaflets are often almost or quite filiform. The extent to which leaflets are subdivided depends upon the relative paucity of carbon in their environment; the general resulting form depends mainly upon the inherited type of venation. Among submerged aquatic plants, the

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FIG. 17.—*Chærophyllum silvestre*.

filiform condition is habitual, because carbonic acid is so comparatively scarce in water. Among British species, the water violet, *Hottonia palustris*, is a good example. All terrestrial primroses have undivided foliage; but in *Hottonia* the leaves, still preserving the pinnate character of the venation, as in the common primrose, are cut into very deep segments, forming a close mass of narrow, linear, waving threads, more like a *Chara* than a flowering plant at a first glance. *Utricularia* shows the same result with a different ground-plan. In *Myriophyllum*, water milfoil, we have whorls of leaves each minutely subdivided into hair-like pinnate segments, and moving freely through their still ponds in search of stray carbon particles diffused in the water. *Hippuris* has the separate leaves undivided, but attains the same result by crowding its long, thin, linear blades in whorls of ten or twelve, so as closely to resemble an *Equisetum*. Our common *Ceratophyllum* looks at first sight much like water-milfoil, but here the whorled leaves, instead of being pinnately divided, are repeatedly forked into subulate or capillary segments, the result of a branching rather than of a pinnate venation. Other instances will occur at once to every botanist.

On land we get very much the same condition of things



FIG. 18.—Floating leaf of *Trapa natans*.

in the fierce competition that goes on for the carbon of the air between the small matted undergrowth of every thicket and hedgerow. The common weedy plants, and especially the annuals or non-bulbous perennials, which grow under such conditions, cannot afford material to push broad leaves above their neighbours' heads, and they are therefore compelled to fight among themselves for every passing particle of carbon. Hence they are usually very minutely subdivided, though in a less waving and capillary manner than the submerged species; their



FIG. 19.—Submerged leaf of *Trapa natans*.

leaflets are oftener flat, and definitely exposed on their upper surface to the sunlight. That essentially weedy family, the Umbellates, contains a great number of such highly segmented hedgerow leaves. Common wild chervil, *Chærophyllum silvestre* (Fig. 17), forms a familiar example: other cases are *C. temulum*, *Sison Amomum*, many *Carums*, *Œnanthes*, *Pimpinellas*, *Daucus*, *Caucalis*, &c., all of which belong by habit to greatly overgrown localities. Compare these with the free-growing, almost orbicular, radical leaves of *Astrantia* and *Sanicula*, in

the same family ; or with the still freer peltate leaves of *Hydrocotyle ;* or again with the divided but more broadly segmented leaves of those tall open-field species, cow-parsnip, *Heracleum sphondylium*, and Alexanders, *Smyrnium olusatrum*, which have only to compete against the grasses and clovers ; or, finally, with the large waterside forms, *Apium graveoleus*, *Sium latifolium*, and *Angelica silvestris*. So, too, take the much segmented herb—Robert, *Geranium Robertianum*, of all our hedgerows, growing side by side with the like-minded chervils and carrots, and compare it with that persistent rounded geraniaceous type which recurs, not only in our English *G. molle*, &c., but even in many exotic *Pelargoniums*. Among composites the crowded type is best exemplified by that thicket weed, milfoil, *Achillea millefolium*, with its infinite number of finely-cut, pinnatifid segments ; while in the taller but closely-allied sneezewort, *Achillea ptarmica*, growing on high open pastures, we get the same general type in outline and venation, only entire save for the slight serrations along its edge. In tansy, *Tanacetum vulgare*, also a hedgerow plant, the same type as milfoil recurs on a far larger and handsomer scale. Compare these with coltsfoot and burdock, or even with the tall eupatory and the tufted, close-packed daisy. Other good miscellaneous instances of the weedy type are fumitory, *Corydalis*, moschatel, the camomile group, &c. ; while among larger cryptogams the majority of thicket ferns display an equally marked subdivision of the fronds and pinnæ. It may be added that highly civilised countries like England are particularly rich in these subdivided types of foliage, owing to the predominance of hedgerows and of tall grasses.

As in the submerged plants, so in the matted terrestrial undergrowth, whorling of linear leaves may practically answer the same purpose as minute segmentation. Some plants solve the difficulty of catching stray carbon in the one way, and some solve it in the other. Each adopts the easiest modification of its own ancestral type. For example, take the stellate tribe. Their tropical allies, the larger Rubiaceæ, have simple, usually entire, opposite leaves, with interpetiolar stipules. In the small, weedy, northern forms however, the interpetiolar stipules have grown out into linear leaf-like foliar organs, forming with the true leaves an apparent whorl of six members. Sometimes, too, the whorl is enlarged to as many as eight leaves, and sometimes reduced to four. These thick whorls of small leaves, always well turned outward to the sunlight, have become practically analogous in their action to minutely segmented leaflets, in our English *Galiums*, *Asperulas*, and *Sherardia*. Two of them at least, *G. mollugo* and *G. aparine*, are extremely common hedgerow plants. Compare them with the broad-leaved free-climbing *Rubia peregrina*, which has only four large members to each whorl.

Among monocotyledons, where (as will be afterwards explained) the type is given by the peculiarity of the cotyledon and governs the venation, minute subdivision is replaced in the matted undergrowth by single, linear, lanceolate blades, which answer the selfsame purpose in the long run. The grasses, sedges, and woodrushes are sufficient examples. Here the numerous leaves, all long and narrow, and all with long thin flower stems, strive to overtop one another, and run up side by side to a considerable height. They may be compared with the large rich leaves of the bulbous lilies, tulips, amaryllids, and orchids. In both cases the type is the same, but the development is different. Plants that consort much with the grasses, as for example ribwort plantain, though wholly unlike in type, are apt to be drawn up and assimilated to them, not merely in general character, but even in venation and mode of fertilisation. Other grass-like dicotyledons are found among the *Polygonums*, *Armerias*, *Bupleurums*, pinks, &c., all under similar circumstances to those of the grasses themselves.

Intermediate types between these two extremes of entire obicularity and minute subdivision occur everywhere. Compare, from this point of view, the common meadow buttercups, which grow in fully occupied meadows, with *Caltha* and the lesser celandine. Compare, again, the mallows on the one hand with the peas on the other, or the docks with the crucifers. Throughout these intermediates, various stages can be easily observed. For example, the South European water-chestnut, *Trapa natans*, beautifully illustrates the gradations which have finally given us our own *Hippuris* and *Myriophyllum* from an Onagraceous or Saxifrage ancestor. It has a number of floating leaves (Fig. 18) supported by bladder-like petioles filled with air, and arranged radially round the stem. Hence, though large and spreading, they are distinctly bilateral, and they do not interfere with one another's food supply. But the submerged leaves (Fig. 19, very diagrammatic) are mere pinnate skeletons of the venation, waving about in the water below. Among monocotyledons, the *Potamogetons* show us some very instructive similar cases, altered in character by the peculiarities of the very persistent monocotyledonous foliar type. In the floating leaves of *P. natans* they come as near the waterlilies as a monocotyledon can reasonably expect to do ; in *P. pectinatus*, the wholly submerged leaves look like long blades of grass, proceeding from the thread-like stems.

Less minutely subdivided than the hedgerow plants are a large class of somewhat weedy forms, well typified by our smaller English crucifers. These are often pinnately divided to a considerable extent, as in *Cardamine hirsuta* and *Senebiera didyma*. Compare them with the taller kinds, such as cabbage and charlock. Much the same type reappears in the lowly forms of Papilionaceæ, as for example in *Anthyllis*, *Astragalus*, *Ornithopus*, *Hippocrepis*, &c. On the other hand, in the tall climbing *Vicias*, and still more in *Lathyrus*, the leaflets, having more carbon, more sun, and less competition, fill out rounder, and generally decrease in number, the upper ones being transformed into tendrils. But in the grass-encumbered clover-like types, *Ononis*, *Medicago*, *Melilotus*, *Trigonella*, and, above all, *Trifolium* itself, the leaflets are dwarfed and reduced to three, the lower members being suppressed, and only the three terminal ones left, so as to raise them on a long footstalk up to the air and sunshine. Compare the very similar leaflets of wood-sorrel. Again, look at the various conditions under which the following Rosaceous plants grow : pear, blackthorn, strawberry, cinquefoil, silver-weed, great burnet, salad burnet, and compare some of them with clover, lady's-fingers, and *Hippocrepis*. The comparison tells its own tale at once.

Finally, we must briefly allude to a large class of tufted plants, usually with entire, ovate, obovate, or ovate-lanceolate leaves, which grow in a rosette from a centre, and insure themselves a good supply of carbon and of light by keeping under all competitors with their close tufts. Of these, our common daisy forms an excellent example : notice the tight way it fits itself against the ground so as to prevent grass from growing beneath it. Another good case in point is *Plantago media :* compare form and habit with those of *P. major* and *P. lanceolata*. To the same class, more or less, may be referred *Arabis thaliana* and many crucifers, London Pride, the common primrose, *Hieracium pilosella*, &c. ; and, with more pinnate, lyrate, or prickly leaves, the young thistles, and the radical foliage of many ligulate composites.

The shapes of leaves thus depend upon the average surrounding conditions, modifying a given ancestral type. How these ancestral types themselves were first developed we shall have to inquire in our next paper.

GRANT ALLEN

(To be continued.)

ON THE NATURE OF INHIBITION, AND THE ACTION OF DRUGS UPON IT[1]

III.

THE first important contribution to our knowledge of inhibitory centres in the brain and spinal cord was that of Setchenow. He found that when the cerebral lobes in a frog were removed, voluntary motion was abolished, but reflex action became somewhat more marked. On removal of the optic lobes, the reflex action became very greatly increased, and if, instead of removing them they were stimulated either chemically by a grain of salt laid upon them, or electrically, reflex action in the limbs was greatly retarded or completely abolished.

These experiments were repeated by Herzen, who, like Setchenow, considered that there was no inhibitory mechanism in the spinal cord itself, but disbelieved also in inhibitory centres in the brain. He explained the depression of reflex which occurred on irritation of the optic lobes by supposing that any intense nervous irritation, no matter whether it was central or peripheral, caused great depression of reflex action both when the brain was intact and when it was divided, as in Setchenow's experiments. Setchenow again repeated his experiments, and came to the conclusion that it was uncertain whether the inhibitory mechanism could be excited reflexly from the periphery. He made, also, a sharp distinction between tactile and painful impressions upon the skin. For tactile impressions he considered that there was no inhibitory mechanism in the brain. Further investigations still, showed that both chemical and electrical irritation would excite the inhibitory apparatus, and he, therefore, considered that both excito-motor and depressor fibres were present in the same nerve-trunk.[2] Goltz found, in opposition to Setchenow, that there was an inhibitory apparatus for tactile reflexes also in the frog's brain, but this he found in the cerebral lobes,[3] while Setchenow denied any inhibitory function to that part of the brain altogether.

He found also, however, like Herzen, that complete abolition of reflex action could be produced by powerful irritation of any peripheral sensory nerve, and considers that the irritation is conveyed to the reflex centre, and diminishes or destroys its excitability for the original stimulus, without supposing that there is any special inhibitory centre.

Lewisson found that by powerfully compressing the neck, or by squeezing the feet, or some other part of the body of a frog, or by irritation of the cutaneous or muscular nerves, or by electricity, the reflex excitability could be much depressed. He found, however, that unless the irritation was strong it produced stimulation both of the reflex and motor centres of the brain instead of depression.[4]

The general conclusion to which all these experiments, as well as those of Fick,[5] Freusberg, and others lead is, either that the nerves contain both excito-motor and reflex depressing fibres, or that excitement and depression can be produced by the same nerves under different conditions.

Freusberg,[6] who discusses the question of inhibition in an able and thorough manner, comes to the conclusion that all instances of inhibition including the different effects of weak and powerful stimuli applied to the same nerve, and also the inhibitory effects of stimulation of different nerves on each other, are not due to specific inhibitory centres, but to a remarkable property of the central nervous system, which does not allow of its different parts being simultaneously set in action by different causes. This conclusion, although it may be nearer the truth than the hypothesis of separate inhibitory centres, is not satisfactory, for it still leaves us in the dark regarding the way in which the central nervous system comes to possess the remarkable properties which he attributes to it.

Setchenow explains the increased rapidity of reflex action after section of the cord below the medulla oblongata, by supposing that there are two paths along which the stimulus usually passes, from the sensory to the motor tracts. The one goes directly across, and this is the path taken after section. The other goes up to the medulla, and then down the cord. This is the path taken under ordinary conditions; but besides the apparent unlikelihood that the stimulus should take this longer path under normal conditions, an objection has been raised to it by Cyon which seems fatal.

Cyon finds that when the so-called inhibitory centres are stimulated, although reflex contraction of the leg is apparently delayed for a long time, this delay is to a great extent only apparent and not real.[1]

It is true that the vigorous contraction of the muscles which suffices to raise the limb is much delayed, but a contraction of these muscles commences at very nearly the same time that it would do if the inhibitory apparatus were not stimulated. This shortening of the muscle goes on very gradually for a considerable time, and then culminates in a sudden vigorous contraction, the total height of which is greater than that of the contraction which would have occurred without irritation of the inhibitory centres. It is very difficult to explain this result on the ordinary hypothesis, but easy enough on that of interference. According to it we suppose that a stimulus applied to the foot has been transmitted as usual from the sensory to the motor cells of the cord, and thence to the muscles, so as to initiate contraction in them. This stimulus would correspond to the first half wave in the diagram (Fig. 2). The subsequent waves of stimulation which would have proceeded from the motor ganglia have been interfered with by the stimuli passing down from the so-called inhibitory centre, but their times being not arranged so that each wave from the brain should fall half a wave-length behind that in the cord, the stimuli at length cease to interfere, and the contraction, which has gone on gradually increasing as the interference diminishes, at last finishes abruptly.

The part of the brain which ought to correspond in higher animals to the optic lobes in frogs is the corpora quadrigemina, but irritation of these parts has not been found to have any marked inhibitory action upon reflexes in the limbs.[2]

Irritation of the frontal lobes in puppies has, however, been found by Simonoff[3] to exercise an inhibitory action; but, according to Ferrier, abolition of the frontal lobes in monkeys does not produce any very obvious effect upon the animal.[4] We know that by an effort of the will, we are able either to increase or diminish reflex action, and it might appear probable that irritation of the motor tracts in the cerebrum might have an inhibitory action on reflexes. Irritation of the cerebral motor areas has not been found to exercise any definite inhibitory action upon reflexes, but on the other hand Exner[5] has found, if a stimulus be applied simultaneously to a motor area in the brain and to an extremity, the two stimuli aid one another, and produce a greater effect than they would separately. As irritation

[1] Continued from p. 439.

[2] Über die elektr. und chem. Reizung der sensiblen Ruckenmarksnerven des Frosches, 1868. Quoted by v. Boettticher, *op. cit.* p. 6.

[3] Goltz, *op. cit.* p. 42.

[4] Lewisson, "Ueber Hemmung der Thätigkeit der motor. Nervencentren durch Reizung sensibler Nerven," Archiv. f. Anatomie u. Physiol. 1869.

[5] Fick, Verhandlungen der physikalisch medicinischen Gesellschaft zu Wurzburg, April 23, 1870.

[6] Freusberg, "Ueber die Erregung u. Hemmung d. Thätigkeit d. nervösen Centralorgane," Pflüger's Archiv. x. 174.

[1] Cyon, Ludwig's Festgabe, p. clxviii.

[2] Setschenow Physiologische Studien über die Hemmungs-mechanismen für die Reflexthätigkeit des Rückenmarkes im Gehirn des Frosches, p. 3 (Berlin: Hirschwald, 1863).

[3] Simonoff, Arch. f. Anat. u. Phys. p. 545, 1866.

[4] Ferrier, Functions of the Brain, p. 230 (London, 1876).

[5] Exner, Pflüger's Archiv. xxviii. 487.

of the cerebral motor areas, therefore, does not exercise a definite inhibitory action upon reflexes, but does under certain conditions markedly increase them, one might expect that their removal would diminish reflex action. Such a diminution actually occurs when they are destroyed in disease, but when the brain is removed layer by layer in operations upon animals, it is usually found that the reflex increases in proportion to the quantity removed. When the whole brain is removed, the reflex action is greater than when it is present, and as the cord is cut away layer by layer, the excitability of the segment below appears to be increased; each layer, as has already been mentioned, appearing to have an inhibitory influence on the one below it. But this is not always the case, because we sometimes find on removal of the various parts of the brain or of the spinal cord that the section completely abolishes reflex action for the time.

We are accustomed frequently to cloak our ignorance of the true cause of this abolition by saying it is due to the shock of operation or something of that sort; but looking the facts fairly in the face, we find that sometimes removal of the upper part of the brain or spinal cord causes increase and sometimes diminution of reflex-action in the parts below. At present we have no satisfactory explanation of this phenomenon, but if we suppose in the one case the nervous matter to have been removed in such a way as to cause an interference of the stimuli passing along from cell to cell, and in the other to cause a coincidence, we can readily understand the occurrence of the two different conditions. Moreover, we have said several times, that inhibition or stimulation are only relative conditions depending on the length of path along which the stimulus has to travel, and the rapidity with which it travels. The length of path remaining the same, the occurrence of stimulation or inhibition depends upon the rapidity of passage of the stimulus. The same length of path which is just sufficient to throw successive impulses of a slowly travelling stimulation half a wave-length behind the other, and produce inhibition, may be just sufficient to throw the vibrations of another more rapidly transmitted stimulus a whole wave-length behind, and produce increased instead of diminished action.

If the hypothesis that inhibition is produced by interference be true, we shall be able to test it by seeing whether stimulation of certain nerves which, under the ordinary conditions produce inhibition, do so when the rate of transmission of nervous impulses is altered. The length of path being the same, if we alter the rapidity of transmission it is probable that as the rapidity diminishes, the inhibition will be converted into stimulation, again possibly passing into inhibition, according as the stimuli, which we normally suppose to be half a wave-length behind each other, are thrown a whole wave-length, or a wave-length and a half behind each other. At a certain period, also, the waves of stimulation will be neither a whole nor a half wave-length behind each other, but the fraction of a wave-length. In such cases we shall neither have constant coincidence, nor constant interference, but we shall have rhythmical coincidence and rhythmical interference, the result of which will be that we shall neither get constant motion, nor constant arrest of motion, but alternate motion and rest. In other words we shall neither have complete rest nor tonic contractions, but intermittent or clonic contractions. Now this condition is exactly what we do find when one sciatic of a frog is irritated twenty-four hours after it has been exposed. We have already mentioned that when irritated immediately after exposure it had the effect simply of abolishing reflex action in the other leg; but the same irritation applied in the same manner after many hours, instead of causing arrest in the other leg, causes clonic convulsions.[1]

This occurrence is very hard to explain on the ordinary hypothesis of separate and distinct inhibitory centres, but it agrees perfectly with the hypothesis that inhibition and stimulation are merely relative conditions.

I have repeated Nothnagel's experiments, but I have not got the same results. Irritation of the sciatic nerve indeed caused a certain diminution in reflex at first, but irritation after twenty-four hours caused no clonic convulsions, it merely appeared somewhat to stimulate reflex action in the other leg. The reason of this discrepancy in our results is probably that the temperature was different in the two cases. Nothnagel's results were published in March, and his experiments were probably performed during cold weather, while mine were done during very mild weather. If the effects which he noticed were due to definite inhibitory centres in the spinal cord similar experiments should have had similar results in his hands and mine If on the other hand the effects simply depend on the rate of the transmission of nervous impulses it is easy to understand why the results were different in the two cases.

There are also certain phenomena connected with the action of drugs on the spinal cord which are almost inexplicable on the ordinary hypothesis, but which are readily explained on that of interference. Thus belladonna when given to frogs causes gradually increasing weakness of respiration and movement, until at length voluntary and respiratory movements are entirely abolished, and the afferent and efferent nerves are greatly weakened. Later still, both afferent and efferent nerves are completely paralysed, and the only sign of vitality is an occasional and hardly perceptible beat of the heart and retention of irritability in the striated muscles. The animal appears to be dead, and was believed to be dead, until Fraser made the observation that if allowed to remain in this condition for four or five days, the apparent death passed away and was succeeded by a state of spinal excitement. The forearms passed from a state of complete flaccidity to one of rigid tonic contraction. The respiratory movements reappeared; the cardiac action became stronger, and the posterior extremities extended. In this condition a touch upon the skin caused violent tetanus usually opisthotonic, lasting from two to ten seconds, and succeeded by a series of clonic spasms. A little later still the convulsions change their character and become emprosthotonic. These symptoms are due to the action of the poison upon the spinal cord itself, for they continue independently in the parts connected with each segment of the cord when it has been divided.

This action may be imitated by a combination of a paralysing and exciting agent such as strychnia and methyl-strychnia. Fraser concludes that the effects of large doses of atropia just described are due to a combined stimulant and paralysing action of the substance on the cord, and that the difference in the relations of these effects to each other, which are seen in different species of animals, may be explained by this combination acting on special varieties of organisation.

T. LAUDER BRUNTON

(*To be continued.*)

[1] Nothnagel, Centralblatt f. d. med. Wiss. March 28, 1869, p. 211.

NOTES

THE Queen has signified her intention of opening the International Fisheries Exhibition, at South Kensington, on Saturday, May 12.

BARON NORDENSKJÖLD writes to us that he has definitely settled to start for the interior from Auleitsivik Fjord on the west coast, and then, in September, to go round Cape Farewell along the east coast to the north.

A MOST interesting letter has been received at Kew Observatory from Mr. Cooksley, of Capt. Dawson's expedition to Fort Rae. They arrived on August 30, started the meteorological

observations on September 1, and the magnetical observations on September 3. Apparently all was well at the date of the letter, December 19, 1882.

MR. WILLIAM HENRY M. CHRISTIE, F.R.S., Astronomer Royal, has been elected by the Committee to be a Member of the Athenæum Club, under Rule 2, which provides for the admission of persons distinguished in literature, science, or the arts, or for public services.

M. DUMAS was not able to be present at Monday's sitting of the Academy of Sciences. His recovery is not quite so rapid as it was hoped and expected to be.

IN the Civil Service Estimates for 1883-4 the total vote for education, science, and art amounts to £4,748,556, a net increase of £165,531 over the previous year.

THE sixth International Congress of Orientalists will be opened at Leyden on September 10 next.

MR. MILNE, who has recently returned to his post in Japan, has suggested to the Japanese Government the great utility of establishing a series of observations for the study of earthquakes; earth-tremors; earth-pulsations; earth-oscillations, or permanent changes of level; terrestrial magnetism; fluctuations of underground water; earth temperatures; eruptive phenomena, &c. We trust that the Japanese Government will see it to be their interest, in a land of earthquakes, as well as the interest of science, to take the advice of Mr. Milne, who has already done so much for seismology. Mr. Milne writes that he is more and more convinced that there are "earthquakes" of so slow period that neither observers nor ordinary instruments record them. The Japanese papers report that a volcano in the Asuma Yuma range has burst out.

MR. A. H. KEANE has been elected Corresponding Member of the Italian Anthropological Society.

MR. ROBERT LINDSAY has been appointed Curator of the Edinburgh Botanic Garden.

A SPECIAL general meeting of members only of the Association for the Improvement of Geometrical Teaching will be held at 8 p.m., on March 20, at University College, (1) to authorise the publication of Books i. and ii. of the Elementary Geometry as revised by the committee; (2) to appoint three trustees of the property of the Association.

THE Institution of Naval Architects began its annual meeting yesterday, and continues to-day and to-morrow. Among the papers in the programme are the following:—On certain points of importance in the construction of ships of war, by Capt. G. H. Noel, R.N.; The influence of the Board of Trade rules for boilers upon the commercial marine, by J. T. Milton; Sea-going torpedo-boats, by M. J. A. Normand; Some experiments to test the resistance of a first-class torpedo boat, by A. F. Yarrow; On the modes of estimating the strains to which steamers are subject, by Wigham Richardson; On the extinctive effect of free water on the rolling of ships, by P. Watts; A description of a method of investigation of screw propeller efficiency, by H. B. Froude; The speed and form of steamships considered in relation to length of voyage, by James Hamilton; On fog-signalling, by J. MacFarlane Gray; Method of obtaining the desired displacement in designing ships, by R. Zimmermann.

THE Royal Commissioners for Technical Education—Messrs. Samuelson, M.P., Woodall, M.P., P. Magnus, and Swire Smith—accompanied by Mr. G. R. Redgrave (secretary), visited Birmingham on March 8, and devoted several days to a careful inspection of the Mason College, Midland Institute, &c. The Commissioners were much interested in the system of practical science instruction which is being carried on in the Board Schools under the direction of Mr. Jerome Harrison, F.G.S., and both heard lessons given in the new Icknield Street Schools, and examined the newly built laboratory, &c. We hope shortly to present to our readers an account of the system by which about 2500 of the elder boys and girls in the Birmingham Board Schools are now receiving lessons in elementary science, at, practically, little or no extra cost to the town of Birmingham.

IT is proposed to establish the new Professorship of Physiology at Cambridge in the ensuing Easter term. The appointment of a Professor of Pathology is also declared by the General Board of Studies to be urgent. The Medical Board has recently unanimously reported that the appointment of a Professor of Surgery is urgently necessary; and Prof. Humphrey has offered to resign the Professorship of Anatomy and accept the Professorship of Surgery for the present, without stipend.

THE death is announced of William Desborough Cooley, the author of a History of Geographical Discovery, a Physical Geography, and other geographical works, and who at one time wrote largely on theoretical African geography.

THE half-yearly General Meeting of the Scottish Meteorological Society will be held to-day. The business before the meeting is: (1) Report from the Council of the Society; (2) Address by Prof. Piazzi Smyth, at request of the Council, on Rainband Spectroscopy; (3) the Meteorology of Ben Nevis in 1882, by Clement L. Wragge.

THE *Réforme*, the new Paris paper, which has established telegraphic communication with London, publishes daily a translation of the previsions issued by the Meteorological Board of London, which is read by the French public at the same time as in England.

M. LALANNE, Member of the Academy of Sciences, has been elected a Life Senator in the Liberal interest. It seems to be becoming almost a constant practice of the French Senate to select its "Irremovables" from among the several classes of the Institute.

AN Electrotechnical Society has been formed at Vienna, similar to the one existing and flourishing at Berlin.

THE German astronomers who had proceeded to Punta Arenas in Magellan's Straits in order to observe the last transit of Venus have at last returned to Germany.

A METEORIC stone weighing a hundredweight fell near Alfianello, near Brescia, on February 16 last. It entered the ground to a depth of two metres, and caused a shock like that of a slight earthquake.

A MEMOIR, for which a gold medal (600 francs) has been awarded by the Belgian Academy, is by Prof. Fredericq, of Liege; it is on the influence of the nervous system on the regulation of temperature in warm-blooded animals. After many experiments, the author affirms that cold acts on the sensitive nerves of the skin, and through them on centres of thermogenesis in the *medulla oblongata*. These centres react, and through centrifugal nerves cause an increase of the phenomena of interstitial combustion, especially in the muscles; but we also fight with cold by a diminution of the losses of heat, the vessels of the skin being constricted, owing to an excitation of the vaso-constrictor centres, through impression of the sensitive nerves of the skin by cold. M. Fredericq considers that the system does not (as most physiologists say) contend against heat by diminishing the production of heat. The regulation of temperature is simply based on increase of the losses of heat, by dilatation of the cutaneous vessels, by acceleration of the outer circulation, increased secretion and evaporation of sweat, and greater ad-

mission of air to the lungs. The vaso-dilator nerve centres (sudorific and respiratory) are excited directly by superheated blood.

AN interesting trial of an electrically-moved tramcar took place on Monday at Kew, and, notwithstanding some inevitable hitches, may be regarded as fairly successful. The peculiarity of the application of electricity in the present case lies in the use made of accumulators. The car was constructed at the Electrical Power Storage Company's works at Millwall, and is of the usual dimensions for carrying forty-six inside and outside passengers. It weighs, with its accumulators and machinery, but without any passengers, four and a half tons. Under the inside seats of this tramcar is placed the accumulator, consisting of fifty Faure-Sellon-Volckmar cells, each measuring 13 inches by 11 inches by 7 inches, and each weighing about 80 lbs. This accumulator, when fully charged, is capable of working the tramcar with its maximum load for seven hours, which means half a day of tramway service. From the accumulators the current is communicated by insulated wire to a Siemens' dynamo placed underneath the car, which acts as a motor, the motion being transmitted to the axle of the wheels through a driving-belt. To start the car the current is switched on from the accumulator to the dynamo, the armature of which being set in motion, the power is communicated to the driving wheels. The car can be driven from either end, and the power required can be exactly apportioned to the work to be done by using a greater or lesser number of cells. On a level road, for instance, with a light load, only a comparatively small number of cells will be necessary, but with a heavy load or on a rising gradient greater power will be required, and additional cells must be switched in. The action of the motor, and consequently the direction of the car, can be readily reversed by reversing the current, and the car can also be as readily stopped by shutting off the current entirely and applying the handbrake with which the car is fitted. At night the car is lighted by means of four Swan incandescent lamps, two of which are placed under the roof and one at each end of the car. All the lamps derive their current from the accumulator. The car is also fitted with electric bells, worked from the same source, and is to be run regularly on the Acton tramway line. The Storage Company also had a successful trial on Monday at Kew of a launch fitted with a battery of forty cells and a Siemens' dynamo.

WE learn from the last number of the *Journal* of the Russian Chemical and Physical Society (1883, fascicule 1) that, at a recent meeting of the Society, Prof. Mendeléeff made a communication on the applicability of the third law of Newton to the mechanical explanation of chemical substitutions, and especially to the expression of the structure of hydrocarbons. If we admit not only the substitution of hydrogen by methyl, but also the substitution of CH_2 by H_2, and of CH by H_3—as must be according to the law of substitutions as deduced from the third law of Newton—we can not only explain, but also predict, all cases of isomerism, without recurring to the usual conceptions as to the connections and atomicities of elements. Thus, benzene can be understood as a normal butane, $CH_3CH_2CH_2CH_3$, or $(CH_3CH_2)_2$, where a double symmetrical substitution of H_3 by CH has taken place, the H_2 having been taken from CH_2 and the third H from CH_2, so that only the CH groups are left; benzene being thus $=\begin{pmatrix}CHCH\\CH\end{pmatrix}^2$. It would explain the isomer of benzene, diproparzyl the formation from acetylene, and the substitution and addition products from benzene.

THE additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus erythraus* δ) from India, presented by Mr. C. F. Henshaw; a Grey Ichneumon (*Herpestes griseus*) from India, presented by Mr. F. C. H. Dadswell; a Herring Gull (*Larus argentatus*), British, presented by Miss Ella Vicars; three Common Swans (*Cygnus olor*), British, presented by Mr. J. Hargreaves; four Prairie Grouse (*Tetrao cupido*) from Iowa, North America, presented by Mr. Henry Nash; a Daubenton's Curassow (*Crax daubentoni* ♀) from Venezuela, presented by Mr. Rowland Ward, F.Z.S.; a North American Turkey (*Meleagris gallo-pavo* δ) from North America, presented by His Grace the Duke of Argyll, K.T., F.R.S.; a Malbrouck Monkey (*Cercopithecus cynosurus*) from West Africa, deposited; a Gaimard's Rat Kangaroo (*Hypsiprymnus gaimardi* ♀), three Coypu Rats (*Myopotamus coypus*), born in the Gardens.

GEOGRAPHY OF THE CAUCASUS

OF the several branches of the Russian Geographical Society, the Caucasian and the East Siberian are well known for the amount of valuable geographical work they have done during the thirty years or so of their existence. The high scientific interest connected with the exploration of the Caucasus is obvious. The scientific exploration of the Alps has revealed to us a new world; but the highlands of the Caucasus, with the high plateaux of Transcaucasia, afford a still greater variety of geological and physico-geographical features than the Alps; besides, situated as they are on the boundary between the moist climate of the west and the dry one of the east, between the deeply-indented coasts of Europe and the deserts and plateaux of Asia, between the young civilisations of the west and the old civilisations of the east, the Caucasian highlands afford such a variety of climatic, botanical, zoological, and ethnological features as hardly can be met with in any other country of the world. Very much remains to be done to bring these highlands within the domain of scientific knowledge. In what has been done up to the present, the Caucasian branch of the Russian Geographical Society has always had a good share, either by direct exploration, or by bringing to the knowledge of the scientific world such explorations as otherwise would have remained unknown in the archives of different Government offices, or by giving a scientific character to such explorations as were made for military or diplomatic purposes. Besides, the activity of the Caucasian Geographical Society is not limited to the Caucasus. Closely connected with the General Staff of Tiflis, it extends its explorations to the Trans-Caspian region, to Asia Minor, and to Persia; and closely follows the Russian military expeditions, surveyors, and diplomatists who eagerly visit these countries.

Unfortunately the publications of the Caucasian branch—the *Zapiski* or *Memoirs*, and the *Izvestia* or *Bulletin*—are but very insufficiently known abroad, *Petermann's Mittheilungen* being nearly the sole channel through which they are brought to the notice of the scientific world. The following summary, therefore, of the last publications of the Society will be of some use to scientific geographers. Without attempting to review all the volumes of the *Memoirs* and *Izvestia* which have appeared, we shall limit this paper to a review of the two last of each, the chief results of the papers contained in former volumes being already embodied in Elisée Reclus's "Géographie Universelle."

Several papers of the sixth volume of the *Izvestia* are devoted to the geodesy of the Caucasus and adjacent countries. During the war of 1878 a considerable amount of geodetical work was done in the province of Kars and in Asia Minor, and M. Kulberg gives the latitudes and longitudes determined. The longitudes of Kars, Erzerum, and Mysun were determined by means of telegraphic signals (the accuracy of this method being such as to reduce the probable error between Pulkova and Vladivostok, on the Pacific, to 0″·14, that is, to 50 yards on a distance of 7000 miles). Other longitudes were determined by chronometer. A trigonometrical network was extended to Erzerum, and numerous surveys were made. The longitudes of several points at Constantinople were determined with great accuracy by General Stebnitzky, as well as that of Batum by M. Kulberg.

The same volume contains also a list of latitudes and longitudes determined on the banks of the Emba and on the Mangishlak peninsula.—M. Kulberg contributes also an interesting paper on the results of determinations of lengths of the pendulum on the Caucasus, in order to determine the increase of gravity caused by the Caucasian chain. The observations were made at

Tiflis, Elizabethpol, Dushet, Gudaur, and Vladikavkaz with the same pendulums that were used for a similar purpose in Russia and afterwards in India. It results from the observations that in all the above-named localities, the lengths of the seconds pendulum are less than the calculated ones, namely, by 0·0037 Paris lines at Batum, 0·0455 at Elizabethpol, 0·0445 at Tiflis, 0·0476 at Vladikavkaz, 0·1171 at Dushet, and 0·1226 at Gudaur. Thus, the geoid (or the true figure of the earth's surface, as determined by the directions of the pendulum) nearly corresponds with the spheroid on the shores of the Black Sea; it rises above it by 1587 feet at Tiflis, and by 1622 feet at Elizabethpol. It rises further north, reaching 4175 feet at Dushet, and 4371 at Gudaur, but soon falls, and has at Vladikavkaz, on the northern slope of the main chain, nearly the same height as at Tiflis, that is, 1697 feet above the spheroid.

The purely geographical papers are numerous:—M. Bakradze contributes a paper on the Batum province,—the Saatabago of antiquity,—and the basin of the Chorokh River, inclosed by mountains 10,000 feet high, and often of volcanic origin. The vegetation of the province is perhaps still more luxuriant than in other parts on the coasts of the Black Sea, where it altogether develops with a prodigious strength, owing to the great amount of rain; vines cover the trees in the coast district. But the country is thinly peopled. The old Georgian population is forgetting its language, and is disappearing from the upper parts of the basin of the Chorokh; the Lazes occupy only nineteen hamlets; the Armenians number no more than 570 houses; the Abhazes and Circassians, who have immigrated from the Caucasus, and Kurds are also scarce. —Another paper, by M. Levashoff, gives a detailed description of the mountains on the left bank of the Chorokh, between Batum and Artvin; these mountains are spurs of the Anti-Taurus chain which terminates close by the Chorokh in the peak Kvahid, 10,390 feet high. The left affluents of the Chorokh flow in narrow gorges, the bottom of which, and sometimes the slopes, are occupied by hamlets of Mussulman Gurians. Each of these gorges has its own individuality, and communication between them is very difficult. The small villages of each gorge are quite isolated from those of a neighbouring gorge. The fields of Indian corn and rice are often scratched on the small terraces on the slopes of mountains, often at a height of 3000 feet above the sea-level, and close by ruins of old small fortresses, each of which has its own legend. The tributaries of the Chorokh become wild streams after each rain, and the avalanches are dangerous enemies. The forests, which cover the mountains from top to bottom, are peopled with bears, wolves, and foxes. Further down, towards the sea-coast, the gorges become wider, and their bottom is covered with gardens. The Chorokh itself has a breadth of twenty-five to fifty yards, and runs with such rapidity that the *kayouks*, or local boats, managed with great skill through the rapids, pass the distance from Artvin to Batum (more than fifty miles) in four or five hours.—We notice also in the same volume a paper on the villayet of Trebizond, translated from the German; the letter of Mr. Gifford Palgrave on vestiges of glacial action in North-eastern Anatolia, translated from a former volume of NATURE; the account of a party who undertook to climb the Elbrouz, but stopped 3500 feet short of its summit; and a notice on Western Daghestan.—M. Chernyavsky gives a detailed description of periodical phenomena in the life of plants at Sukhum-kaleh, during the autumn, winter, and spring of the years 1871 to 1875.

M. Seidlitz contributes a note on goitre and cretinism on the Caucasus. It is spread in several valleys of the main chain, especially in the Upper Svanetia; in the valley of the Tzhenis-tzhali many cases of cretinism were noticed. Altogether the small people of Svanets, which numbers only 12,000 souls, seem to be in a state of degeneracy, and ought to have an infusion of fresh blood from without. The goitre was noticed also in adjacent parts of the upper basin of the Rion river, among the Osets. On the northern slope of the Caucasus, west of the Kazbek peak, as well as in the basin of the Kuban, the goitre was not noticed; but it is known in Western Daghestan and in the valleys of the Andian Koysou ridge. It is cured by the waters of springs containing carbonic acid. Women are more subject to this disease than men. Another disease, of hysterical character, endemic to the same locality, is worthy of notice. The men and women affected bark like dogs, and the aborigines consider it as the result of bewitching, in which the "barking grass," as the Avars say a kind of *Orchis*), is used by the bewitchers. In the Anti-Caucasus goitre was noticed in the Nakhichevan district and in the Batum province. It is always endemic, and never takes an epidemic character, as was the case in 1877 at Kokan, in Turkestan, where 9 per cent. of the soldiers and officers were seized with this disease after a year's stay at Kokan.

The ethnography of the Caucasus occupies a large place in this volume of the *Izvestia*. M. Zagursky contributes a note on the supposed kinship of the Osets with the Etruscans, and shows that it would be rather difficult to establish this kinship on account of a want of likeness between the Osetian language and the little we know about the language of the Etruscans.—Prof. Patkanoff contributes a valuable paper on the place occupied by the Armenian language among other Indo-European languages. He concludes that, and shows why, the question still remains open. Several linguists consider the Armenian language as decidedly belonging to the Iranian group, whilst others classify it with the European group. Lagarde distinguishes in it three elements: the Haikan, the Arkasid, and the Sassanid elements; the two latter are Iranian, but the Haikan element belongs to a family of languages the oldest of which is the Zend. Hübschmann concludes that it occupies an intermediate place between the Iranian languages and the Slavo-Lithuanians; and Fr. Müller, a partisan of its Iranian origin, admits that it has some kinship with the Slavo-Lithuanian languages. Prof. Patkanoff concludes that it occupies an intermediate place between these two, and is a representative of an extinct group of Indo-European languages, which formerly was spread perhaps in Asia Minor.—We notice also several notes: on the dolmens of the Maykop district; on the descriptions of the first physical training given to children by different Caucasian peoples (these interesting descriptions, comprising nearly all Caucasian peoples, were sent to Moscow to Dr. Pokrovsky); on archæological discoveries in the province of Kuban, &c.

The *Izvestia* contain also many interesting short notices on the scientific work done on the Caucasus by other Societies and private persons; and bibliographical notices on different works dealing with the Caucasus. Elisée Reclus's description of the Caucasus in the "Géographie Universelle" is considered as the best that has yet appeared, and it is proposed to translate it into Russian, with notes and additions.

The Appendix contains several valuable papers, namely: a note on the Bosphorus and Constantinople, by M. Stebnitzky (with a map), containing some new information on currents in the Bosphorus and on the mean temperature at Pera, according to new observations of M. Kumbari (14°·3 Cels.); a note on the Aysors of the province of Erivan; a note on the population of Turkish Armenia, by M. Eritsoff (1,162,957, out of which 214,350 are Turks, 357,577 Kurds, 498,007 Armenians, 41,682 Kizilbashes, 25,516 Greeks, and 17,400 Aysors); and several translations.

The geodetical part is represented in the seventh volume of the *Izvestia* by a paper by M. Kulberg, on the influence of the oscillations of the supporting disc of the pendulum of the Russian Academy on the measured length of the seconds pendulum. The correction due to this cause was found to be equal to +0·0650 Paris lines, which correction closely corresponds to the difference between the Russian pendulum and that of Cater, which was found at Kew to be equal to 0·0056 inches, or 0·0631 Paris lines. The corrected lengths of the seconds pendulum at the above-named localities (at 13° Réaumur, and reduced to the sea-level) would be thus: 440·2734 Paris lines at the Tiflis Observatory, 440·3279 at Vladikavkaz, 440·2126 at Gudaur, 440·2018 at Dushet, 440·3172 at Batum, and 440·2364 at Elizabethpol.—A biographical notice of the late Gen. Khodzko gives an account of the immense work he performed for the triangulation of the Caucasus. He began this work in 1847 with the Anti-Caucasus, always taking for himself the most difficult parts of the work, such as the measurements on the summit of Alaghöz (13,436 feet high), or of Ararat (16,916 feet), 6000 feet above the snow-line, and of other high summits. On June 28, 1851, he observed an eclipse of the sun on the summit of Galavdur, at a height of 10,380 feet, and noticed the protuberances which were doubted at that time as belonging to the atmosphere of the sun. The geodetical determination of 1386 points in Trans-Caucasia was terminated in 1854, but that of Northern Caucasus was begun only in 1860, and was connected with those of Russia in 1864. The accuracy of this immense work and its importance for geodesy and physical geography are well known.

The same volume contains several valuable geographical papers and maps. Among the latter the first place belongs to

those of the frontier between Russia and Persia, from the Caspian to Babadurmaz, and of the frontier between Russia and Turkey, from the Black Sea to Ararat; both are accompanied with maps.—General Stebnitzky contributes a most valuable sketch of all that is known about the Pontian range, which follows the southern coast of the Black Sea from the Yeshil-irmak to the Chorokh.—M. Stepanoff contributes an interesting paper on the province of Kars, recently annexed to Russia; and M. Bakradze one on the ethnography of the same province. The province consists of three different parts: the lowlands of the basin of the Olti River, covered with clay hills intersected with irrigation canals, and offering great advantages for gardening; the 5000 to 6000 feet high plateau of Kars, 50 miles long and 35 miles wide, bordered with mountains the highest of which reaches 9700 feet. It is covered with lavas and basalts, deeply cut by rivers; the mountains are devoid of wood; agriculture is carried on on this plateau, notwithstanding its great height. The third part of the province is again a plateau, 6000 to 7000 feet high, where agriculture becomes impossible, but covered with good pasture-land, and dotted with lakes. The population of the province has suffered much from wars. In the basin of the Olti and in the north-east it was formerly Georgian, who have become Mussulmans; the Kurds make one-sixth of the population. The basins of the Araxes and Kars rivers were formerly occupied by Armenians. The capital of Armenia, Ani, now in ruins, was situated here. After 1830, no less than 90,000 Armenians emigrated into Russian dominions, whilst Turks, Turcomans, Karapakhs, and Caucasian emigrants (Kabards and Osets) occupied their place, forming thus a most mixed population. Presently the Mussulmans emigrated back from the province (no less than 65,447 souls during two years), and 7100 Russian Nonconformists have occupied their place, as well as 10,000 Greeks and about 4100 Armenians. The migration of whole populations is thus still going on in our times, as it was going on formerly after the great wars of the past. It is easy to foresee that the country contains most remarkable Armenian antiquities, such as churches built in the ninth and tenth centuries.

Since the year 1880 the director of the Tiflis Observatory, M. Milberg, has undertaken a series of measurements of the temperature of the ground, together with measurements of temperature by a black-bulb thermometer suspended 1·5 metres above the ground, and M. Smirnoff analyses the results of these measurements. The blackened thermometer has given a somewhat higher average temperature for the year than the usual thermometer suspended in shade (12°·7 Celsius, instead of 11°·6); the same was observed, as is known, in England. At the same time its maxima are obviously higher and its minima are lower than those of the usual thermometer in shade, its range being from −14°·5 to +42°·9, instead of −12°·0 to +37°·6; whilst the range of average temperatures of different months was 28°·6 instead of 27°·5 in the shade. The underground thermometers were placed at depths of 1, 2, 5, 12, 20, 41, and 79 centimetres, and were observed, the six former every hour, and the last each three hours. Two other thermometers, placed at depths of 1·6 and 3·5 metres, were observed once a day. The whole series of observations is published in the *Memoirs* of the Caucasian Agricultural Society, and the *Izvestia* give the monthly averages, as well as a *résumé* of the results. We shall add to this *résumé* that the observations at Tiflis show well the retardation of seasons at a depth of 79 centimetres, the coldest and warmest months being February and August, instead of January and July. The frosts at the spot where the observations were made do not penetrate deeper than 40 centimetres.—M. Maslovsky gives some observations of temperature at Askhabad, in the Akhaltekke oasis, during the summer months; the moisture in May was but 31 to 33 per cent., falling as low as 17 per cent., and reaching sometimes 59 per cent.—M. Chernyavsky gives the Abkhaze, Mingrelian, and Georgian names of different plants.

Several papers deal with the population of the Caucasus: M. Zagursky has contributed a paper on the ethnographical maps of the Caucasus, and, after having sharply criticised the works of M. Rittich, recommends as the best ethnographical map of the Caucasus, that which was published by M. Seidlitz in *Petermann's Mittheilungen*, and in which M. Zagursky has embodied the results of the little-known but remarkable linguistic works of the late General Uslar. Still this map leaves much to desire and ought to be accompanied by an explanatory memoir.—The much-debated question as to the number of Armenians in the Russian dominions is discussed by M. Eritsoff, who comes to the conclusion that it must be (taking into account the increase of population until 1881) 860,456 on the Caucasus, and 56,536 in European Russia.—M. von Eckert gives the results of anthropological measurements he has made, according to the instructions of Virchow, on 30 Adighes, 7 Ingushes, 11 Georgians, 14 Osets, 14 Armenians, 9 Aderbijan Tartars, and 80 Little-Russians from the Government of Kharkoff. They proved to be all brachycephalic, the average indexes being 80·7 for the Osets, 80·9 for the Tartars, 81·9 for the Ingushes, 82·0 for the Adighes, 82·2 for the Little-Russians, 83·3 for the Georgians, and 86·5 for the Armenians. The percentage of broad faces (*chamäprosop* faces, that is, those where the breadth between the cheek-bones is less than 89·9 per cent. of the length of the face, measured from the upper part of the nose to the lowest part of the chin) is 44 for Tartars, 64 for Armenians, 71 to 77 for Osets, Georgians, and Adighes, 86 for Ingushes, and 90 for Little-Russians.

The same volume contains several notes: on the Charjui; a list of heights in the Aderbijan; on the Scotch colony at Kuras and many others; and a bibliographical notice, by M. Stebnitzky, of Elisée Reclus's description of the Caucasus, which is spoken of in high terms.—The Appendix contains the translation, with notes, of the memoir, by Major Trotter, on the Kurds in Asia Minor, and of the Consular Report of W. Gifford Palgrave on the provinces of Trebizond, Sivas, and Kastamuni.

The eleventh volume of the *Memoirs* of the Caucasian Geographical Society contains three papers by M. Petrusevitch: on the Turcomans between the Uzboy and the northern borders of Persia; on the north-eastern provinces of Khorassan; and on the south-eastern coast of the Caspian and the routes to Merv. Some of these papers are already known to English geographers; and the others probably will be translated in full. They are accompanied by a map of the Russian Trans-Caspian dominions and of Northern Persia.

The twelfth volume of the *Memoirs* contains the first part of a large work, by the late General Uslar, on the ancient history of the Caucasus. It deals with the oldest traditions about the Caucasus, and is a most remarkable attempt at a *scientific* inquiry into the remotest history of this country. It is accompanied by a biographical notice of General Uslar, by M. Zagursky, his collaborator and follower. It is certain that M. Uslar, who pursued for many years the truly scientific exploration of Caucasian languages (undertaken first by Sjögren), has done in this branch far more than anybody else. But his works—which were only lithographed in a few copies, and each of which is not only a serious study of separate languages, but also a thorough description of the nation it deals with—are very little known, and this only from the short reports that were made on them by the late Member of the Russian Academy of Sciences, M. Schiefner. The few pages in which M. Zagursky gives an account of the work of Uslar, of the methods he followed, and of the results he arrived at, ought to be translated in full, as surely they would be most welcome to all those in England who are interested in the study of ethnology. They deserve much more than a short notice. P. K.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—Prof. Moseley and Prof. Burdon Sanderson have been appointed *ex officio* Members of the Board of the Faculty of Natural Science.

Prof. Clifton has been elected a Member of the Hebdomadal Council in place of the late Prof. Smith.

The Professorship of Archæology and Art, founded by the late Commissioners out of the revenues of Lincoln College, has been in abeyance owing to the proposed statute not having received the Queen's assent. The College now proposes to endow the professorship, and a statute will be promulgated at the beginning of next term, providing for a Professor of Classical Archæology and Art, "who shall lecture on the arts and manufactures, monuments, coins, and inscriptions of classical antiquity, and on Asiatic and Egyptian antiquities, or on some of those subjects."

Mr. G. A. Buckmaster, B.A., and late Natural Science Demy of Magdalen College, has, after examination, been elected to the Radcliffe Travelling Fellowship. Mr. Buckmaster also obtained the Burdett Coutts Scholarship for proficiency in geology in 1882. The Fellowship is of the annual value of 200*l*., tenable for three years. The candidate must declare that he intends to graduate in medicine in the University of Oxford, and to

travel abroad with a view to his improvement in that study. A Fellow forfeits his Fellowship by spending more than eighteen months within the United Kingdom.

SCIENTIFIC SERIALS

Journal of the Franklin Institute, February.—An account of certain tests of the transverse strength and stiffness of large spruce beams, by G. Lanza.—The abstraction of heat by mechanical energy, by J. Rowbotham.—On the application of the principle of virtual velocities to the determination of the deflection and stresses of frames, by G. F. Swain.—Cone pulleys, by H. W. Spangler.—Dust explosions in breweries, by C. J. Hexamer.—A summary of progress in science and industry, 1882.

THE January number of the *Revue d'Anthropologie* (Premier Fasc., 1883), contains the first part of a valuable memoir—unfortunately left incomplete by Paul Broca at the time of his death—on the cerebral convolutions of the human brain, as shown by casts. Broca, having found from long experience that it is almost impossible to obtain specimens of a normal cerebrum in which both hemispheres are symmetrical, devoted his attention to the preparation, for the special use of students, of exact models of the convolutions divested of the secondary folds, whose extreme variability makes it difficult to determine their true character. The memoir now first printed supplies an exhaustive description of the brain at every stage from fœtal to senile life, with explanations of the significance of the different colours used in the preparation of the models, which have been completed under the superintendence of M. S. Pozzi.—"Buffon Anthropologiste" is the title of a paper by M. P. Topinard, in which he has reprinted the main part of a lecture previously addressed to his class in the École d'Anthropologie. The object of the address is to show that Buffon was the precursor of Darwin and Lamark, both as to the theory of development from one, or at most a few original types, and in his belief in the survival of the fittest. His undoubted contradictions M. Topinard ascribes to the necessity of the times, which compelled him to respect the opinions of the clergy so far as to address to the Faculty of Theology a written retractation of fourteen propositions contained in his "Histoire Naturelle," which that body had condemned. This curious document is here given *in extenso*.—M. C. Sabatier, a former *juge de paix* in Kabylia, in an article on "La femme kabyle," explains the nature of the enactments by which the French Government is endeavouring to ameliorate the condition of women among the Kabyles, who till the present time have virtually been slaves, being treated alike by their fathers and husbands as the least valued of chattels. As the result of long discussions with the heads of the tribes, two new "kanouns," or laws, have been agreed to and put into force, which M. Sabatier believes to be decisive steps towards the social regeneration of the men as much as of the women, one of these enactments restricting the rights of the father to give his daughter in marriage before she has reached a fixed age, and the other freeing a wife from the control of her husband under certain conditions of desertion and neglect.—MM. Corre and Roussel's report of their observations of 200 crania of criminals preserved in the Anatomical Museum of Brest is supplied with various tables exemplifying their precise cranial characteristics, the nature of the crimes committed, the birth-place of the criminals, &c. The general conclusions are in complete accord with those of Bordier, Broca, &c.

Archives des Sciences Physiques et Naturelles, January 15.—On a refractometer for measuring the indices of refraction and the dispersion of solid bodies, by M. Soret.—Theoretical and experimental study of a rapid vessel, by M. Pictet.—On the apparent forces arising from the terrestrial motion, by M. Cellérier.

Bulletin de l'Académie Royale des Sciences de Belgique, No. 12, 1882.—Considerations on the stratigraphic relations of the psammites of Condroz and the schists of the Famenne properly so-called; also on the classification of these Devonian deposits, by M. Mourlon.—Second note on the dynamo-electric machine with solenoid inductor, by M. Plücker.—Determination of the general law ruling the dilatability of any liquid chemically defined, by M. de Heen.—On the aurora borealis of November 17, 1882, by M. Terby.—Reports on prize competitions, &c.—The great discoveries made in physics since the end of last century (lecture at public *séance*), by M. Montigny.—Dwarfs and giants (lecture), by M. Delbœuf.

The Proceedings of the Linnean Society of New South Wales, vol. vii. Part 2 (April–June, 1882); Part 3 (July–September, 1882). The chief contents are, *Botanical*: Botanical notes on Queensland. No. 2, the tropics; No. 3, the Mulgrave River; No. 4, Myrtaceæ.—On a coal-plant from Queensland, by Rev. J. E. Tenison-Woods.—Half-century of plants new to South Queensland, by the Rev. B. Scortechini.—Forage-plants indigenous to New South Wales, by Dr. Woolls.—On *Myoporum platycarpum*, a resin-producing tree of the interior of New South Wales, by K. H. Bennett.—Botanical notes in the neighbourhood of Sydney, by E. Haviland.—*Zoological*: On a new Gobiesox from Tasmania; on two new birds from the Solomons; on a new Coris from Lord Howe's Island, by E. P. Ramsay.—Australian Micro-lepidoptera, No. 7, by E. Meyrick.—On a reported poisonous fly from New Caledonia; new species of fish from New Guinea and Port Jackson; on an insect injurious to the vine, by Wm. Macleay.—On a new species of Allopora, by Rev. J. E. Tenison-Woods.—On Australian fresh-water sponges; on the brain of *Galeocerdo rayneri*; monograph of Australian Aphroditea (Plates 6 to 11); notes on anatomy of pigeons, by W. A. Haswell.—Some new Queensland fishes; on a new species of squill from Moreton Bay, by W. de Vis, B.A.—Habitat of *Cypræa citrina*, of Gray, by J. Brazier.—New variety of *Ovulum depressum*, found at Lifou, by R. C. Rossiter.—On a breeding place of *Platalea flavipes* and *Ardea pacifica*, by K. H. Bennett.—*Geological*: Physical structure and geology of Australia, by Rev. J. E. Tenison-Woods.

Journal of the Asiatic Society of Bengal, vol. li. Part 2, Nos. 2 and 3, 1882 (December 30, 1882) contains:—Some new or rare species of Rhopalocerous Lepidoptera from the Indian region, by Major G. F. L. Marshall, R.E. (Pl. 4).—On an abnormality in the horns of the Hog-deer (*Axis porcinus*), with an amplification of the theory of the evolution of the antlers in ruminants, by John Cockburn.—On the habits of a little-known lizard (*Brachysaura ornata*), by John Cockburn.—Second list of butterflies taken in Sikkim in October, 1882, by L. de Nicéville.

Morphologisches Jahrbuch, eine Zeitschrift für Anatomie und Entwickelungsgeschichte, Bd. 8, Heft 3, contains:—The nasal cavities and lachrymo-nasal canals in amniotic vertebrata, by Dr. E. Legal.—The structure of the hydroid polyps, by Dr. Carl F. Jickeli (Plates 16-18).—The tarsus in the birds and Dinosaurs, by G. Baur (Plates 19 and 20).—Contribution to a knowledge of the development of the vertebral column in Teleostians, by Dr. B. Grassi.—On an hypothesis concerning the phylogenitic derivation of the blood system of a portion of the Metazoa, by Dr. O. Bütschli.

Reale Istituto Lombardo di Scienze e Lettere Rendiconti, vol. xv. fasc. xx.—Reports on prize-awards; announcements of prize-subjects, &c.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, February 15.—"Description of an Apparatus employed at the Kew Observatory, Richmond, for the Examination of the Dark Glasses and Mirrors of Sextants." By G. M. Whipple, B.Sc., Superintendent.

In the *Proc. Roy. Soc.* for 1867, Prof. Balfour Stewart described an apparatus designed and constructed by Mr. T. Cooke for the determination of the errors of graduation of sextants. This instrument has from that date been constantly in use at the Kew Observatory, and since the introduction of certain unimportant improvements has been found to work very well.

No provision was made, however, for its employment in the determination of the errors of the dark shades used to screen the observer's eyes when the sextant is directed to the sun or moon, and it has been found that errors may exist in the shape of want of parallelism in these glasses, sufficiently large to seriously affect an observation accurate in other respects.

It has also been found that sextant makers are desirous of having the shades examined before proceeding to fit them into their metal mountings, and also to have the surfaces of the mirrors tested for distortion before making the instruments up. With a view to the accomplishment of these ends, for some time past the Kew Committee have undertaken to examine both dark glasses and mirrors, and to mark them with a hall-mark when

they are found to answer the requirements necessary for exactitude.

For these purposes the following apparatus has been devised by the author, and brought into use at the Observatory.

A telescope of 3½ inches aperture and 48 inches focal length, a pair of collimators of 1¼ inch aperture and 10 inches focal length, and a heliostat, are firmly fixed to a stout plank, so that their axes may be in the same horizontal plane. The eyepiece of the telescope carries a parallel wire micrometer.

In order to adjust the instrument, the telescope is directed to the sun, a shade being fitted to the eyepiece and then placed in its Y's focused for parallel rays. The collimators are then fixed on their table with their object-glasses opposed to that of the telescope, the eyepieces and wires having first been removed, and a metal plate with a sharply-cut hole in its centre fitted to their diaphragms.

Light is next reflected down the collimator by the heliostat, and the aperture in the diaphragm being viewed through the telescope, is carefully focused by moving the object-glass of the collimator to and fro by means of its rack and pinion.

The diaphragm aperture is next collimated by rotating the collimator in its bearings.

Both collimators being thus adjusted, they are placed side by side, so that their illuminated sights can be viewed simultaneously in the telescope, appearing as superimposed bright disks 12′ in diameter. They are next separated so that the disks remain merely in contact at the extremity of their horizontal diameters.

The instrument is now ready for use, and the examination of the shades is performed in the following manner:—

The glass to be tested is fixed in a rotating frame in front of the object-glass of one collimator, a corresponding shade being placed between the heliostat and diaphragm of the other collimator. The sun is now directed on to the diaphragms. The coloured disks are viewed through the telescope, when, if the sides of the shade, placed between the collimator and the object-glass of the telescope, are perfectly parallel, the relative position of the disks is unchanged; if, however, the shade is not ground true, the disks will appear either separated or to overlap. In the first case the amount of separation is measured by the micrometer, and serves to indicate the quality of the glass. In the case of overlapping images the shade is rotated through 180°, and separation produced which can be measured. A second examination is then made, the shade having been turned through 90°.

If in no position a separation of images is found to exist to the extent of 20″, the glass is etched K.O. 1; if more than 20″ but less than 40″, the mark is K.O. 2; with greater distortion than this, the shade is rejected and not marked.

To examine the quality of the mirrors, a small table, on levelling screws, is put in front of the object-glass of the telescope. The mirror to be tested is placed on its edge on this table, and turned until a distant well-defined object is reflected down the tube of the telescope. The object-glass of the telescope having previously been stopped down to an aperture corresponding to the size of the mirror, the reflected image is contrasted with that seen directly, and if the definition is unchanged the mirror is marked K.O. with a writing diamond, and returned to the maker; if the object appears distorted, its unfitness for use is similarly notified. A small fee is charged for the examination.

Geological Society, February 7.—J. W. Hulke, F.R.S., president, in the chair.—G. D'Arcy Adams, Prof. Ferdinand Moritz Krausé, and the Rev. Alfred William Rowe were elected Fellows, and Dr. Karl A. Zittel, of Munich, a Foreign Correspondent of the Society.—The following communications were read:—On the metamorphic and overlying rocks in parts of Ross and Inverness shires, by Henry Hicks, F.G.S., with petrological notes by Prof. T. G. Bonney, F.R.S. In this paper the author described numerous sections which have been examined by him in three separate visits made to the north-west Highlands. In some previous papers, sections in the neighbourhood of Loch Maree had been chiefly referred to. Those now described are to the south and south-east of that area, and occur in the neighbourhoods of Achmashellach, Strathcarron, Loch Carron, Loch Kishorn, Attadale, Strome Ferry, Loch Alsh, and in the more central areas about Loch Shiel and Loch Eil to the Caledonian Canal. In these examinations the author paid special attention to the stratigraphical evidence, to see whether there were any indications which could in any way be relied upon to prove the theory propounded by Sir R. Murchison that in these areas fossiliferous Lower Silurian rocks dip under thousands of feet of the highly crystalline schists which form the mountains in the more central areas. On careful examination he found that in consequence of frequent dislocations in the strata the newer rocks were frequently made to appear to dip under the highly crystalline series to the east, though in reality the appearance in each case was easily seen to be due to accidental causes. Evidences of dislocation along this line were most marked; and the same rocks in consequence were seldom found brought together. He recognised in these eastern areas at least two great groups of crystalline schists metamorphosed throughout in all the districts examined, even when regularly bedded and not disturbed or contorted; and they have representatives in the western areas, among the Hebridean series, which cannot in any way be differentiated from them. These he called locally by the names, in descending order, of Ben-Fyn, and Loch-Shiel series. The former consist, in their upper part, of silvery mica-schists and gneisses, with white felspar and quartz; in their lower part, of hornblendic rocks, with bands of pink felspar and quartz, and of chloritic and epidotic rocks and schists. The Loch-Shiel series consists chiefly of massive granitoid gneisses and hornblendic and black mica-schists. Thirty-three microscopical sections of the crystalline schists and the overlying rocks are described by Prof. Bonney, and he recognises amongst them three well-marked types. In No. 1 he includes the Torridon Sandstone, the quartzites and the supposed overlying flaggy beds on the east side of Glen Laggan. These are partially metamorphosed, only distinct fragments are always easily recognisable in them in abundance. In No. 2, the Ben-Fyn type, the rocks are crystalline throughout, being typical gneisses and mica-schists. In No. 3, the Loch-Shiel series, he recognises highly typical granitic gneisses of the Lower Hebridean type. Dr. Hicks failed to find in these areas at any point the actual passage from group 1 to group 2; neither did the same rocks belonging to group 1 meet usually the same rocks belonging to group 2. The evidence everywhere showed clearly that the contacts between these two groups were either produced by faults or by overlapping. Group 3, placed by Murchison as the highest beds in a synclinal trough, supported by the fossiliferous rocks, the author regarded as composed of the oldest rocks in a broken anticlinal. They are the most highly crystalline rocks in these areas; and the beds of group 2 are thrown off on either side in broken folds. These, again, support the rocks belonging to group 1. The author therefore feels perfectly satisfied that the crystalline schists belonging to groups 2 and 3, which compose the mountains in the central areas, do not repose conformably upon the Lower Silurian rocks of the north-west areas with fossils, and that these highly-crystalline rocks cannot therefore be the metamorphosed equivalents of the comparatively unaltered, yet highly disturbed and crumpled, richly fossiliferous Silurian strata of the southern Highlands, but are, like other truly crystalline schists examined by him in the British Isles, evidently of pre-Cambrian age. In an Appendix by Prof. T. G. Bonney, F.R.S., on the lithological characters of a series of Scotch rocks collected by Dr. Hicks, the author stated that he observed in the above series, as he had done in other Scotch rocks lately examined by him, three rather well-marked types—one, where, though there is a certain amount of metamorphism among the finer constituents forming the matrix, all the larger grains, quartz, felspar, and perhaps mica, are of clastic origin; a second, while preserving a bedded structure and never likely to be mistaken for an igneous rock, being indubitably of clastic origin, retains no certain trace of original fragments; while the third, the typical "old gneiss" of the Hebridean region, seldom exhibits well-marked foliation. It is sometimes difficult to distinguish between the first and second of these; but this the author believed to be generally due to the extraordinary amount of pressure which some of these Scotch rocks have undergone, which makes it very hard to determine precisely what structures are original. Even the coarse gneiss is sometimes locally crushed into a schistose rock of comparatively modern aspect. The least altered of the above series the author considered to be the true "newer-gneiss" series of the Highlands, but both of the others to be much older than the Torridon Sandstone.—On the Lower Carboniferous rocks in the Forest of Dean, as represented in typical sections at Drybrook, by E. Wethered, F.G.S., with an appendix by Dr. Thomas Wright.

Chemical Society, March 1.—Dr. Gilbert, president, in the chair.—The following gentlemen were elected Fellows :—A. C. Abraham, G. Board, C. N. Betts, E. Bevan, F. J. Cox, A. Collenette, S. Dyson, W. T. Elliott, H. B. Fulton, C. G. Grenfell, B. F. Halford, W. D. Hogg, D. Hooper, J. J. Knight, H. F. Lowe, T. H. Leeming, J. E. Marsh, W. Newton, C. Rumble, F. Scudder, J. O'Sullivan, S. A. Vasey, T. D. Watson, R. M. Walmsley, C. S. S. Webster, F. Watts. —The following papers were read :—On some derivatives of the isomeric $C_{10}H_{14}O$ phenols, by H. E. Armstrong and E. H. Rennie. Lallemand stated that a trinitro-thymol was produced by the action of a mixture of nitric and sulphuric acids on dinitrothymol. The authors find that a trinitro body is formed, but that it has the constitution and properties of trinitrometacresol. The authors could not obtain a trinitro body from carvacrol. When thymolsulphonic acid is treated with nitric acid, paranitrothymol is formed, the sulpho group being displaced. When bromothymolsulphonic acid is treated with chromic acid; an amorphous quinone is formed, but when permanganate is used, no quinone is produced. The authors have also studied the action of nitric acid on bromisobutylsulphonic acid.—Chemico-microscopical researches on the cell-contents of certain plants, by A. B. Griffiths. The author has grown cabbage plants on soils containing ferrous salts: the plants are larger, and their ash contains a considerable quantity of oxide of iron. In sections under the microscope crystals are visible which belong to the monoclinic system and give a blue colour with potassium ferricyanide and an opacity with barium chloride. The author concludes that they consist of ferrous sulphate.—The phenates of amido bases, by R. S. Dale and C. Schorlemmer. The authors have satisfied themselves that, when aurin is heated with ammonia, pararosanilin is at once formed. When aurin is heated with common rosanilin and alcohol, a solution is produced which on concentration yields a crystalline powder of rosanilin aurate; similarly by heating anilin and phenol in molecular proportions, anilin phenate is obtained in glistening plates melting at 29°·5, boiling 184°·5.

Anthropological Institute, February 27.—Prof. W. H. Flower, F.R.S., president, in the chair.—The election of Mr. C. Fountaine Walker was announced.—Dr. Garson exhibited and described a series of photographs of cases of hypertrichosis. —Mr. A. Tylor read a paper on the homological nature of the human skeleton. He finds that in the skull of all vertebrate animals, including man, a general resemblance to the trunk and limbs is carried out—for instance, variations in the limbs are accompanied by variations in the jaws, and the occiput varies with the pelvis, the sternum with the palate, and so on throughout the skull and body. This is due to mechanical causes. Bones, like the parts of plants, consist of stalks and leaves; the stalk-element is shown in the vertebræ and the long bones, and the leaf-element in the apophyses, the plate-bones of the skull, such as the parietals, &c. The elemental shaft-bones always bulge at the extremities where pressure is exerted, hence the peculiar form of all such bones. This form is a mechanical necessity, and, in accordance with the known laws of correlation and repetition of parts, helps us to understand the singular relations subsisting between the skull and the rest of the skeleton.

Institution of Civil Engineers, March 6.—Mr. Brunlees, president, in the chair.—The first paper read was on the productive power and efficiency of machine tools, and of other labour-saving appliances, worked by hydraulic pressure, by Mr. Ralph Hart Tweddell, M.Inst.C.E.—The second paper read was on stamping and welding under the steam-hammer, by Mr. Alexander McDonnell, M.Inst.C.E.

SYDNEY

Linnean Society of New South Wales, December 27, 1882.—Dr. James C. Cox, F.L.S., president, in the chair.—The following papers were read :—Occasional notes on plants indigenous in the neighbourhood of Sydney, No. 2, by Edwin Haviland. This paper treats chiefly of the construction and habits of *Utricularia dichotoma*.—Description of a new *Belideus* from Northern Queensland, by Charles W. De Vis, B.A.—A paper by the same author describing two new Queensland fishes (*Callionymus achates* and *Mugil nasutus*).—By the Rev. Dr. Woolls, on the species of Eucalyptus first known in Europe. Of the twelve species described by Willdenow, eleven are from the immediate neighbourhood of Sydney, and one only from Tasmania. This tree, the Tasmanian Stringy Bark (*E. obliqua*), was the first Eucalypt known in Europe, the specimen having been collected during Furneaux's voyage. On it L'Héritier founded the genus, 1788. The early descriptions are, as it may be supposed, very vague and imperfect, and their identification has been a matter of much difficulty and hesitation, now happily removed.—On some new species of tubicolous annelides, by William A. Haswell, M.A., B.Sc.—On new species of *Agaricus* discovered in Western Australia, by the Rev. C. Kalchbrenner. —On some points in the anatomy of the urogenital organs in females of certain species of kangaroos, Part 1, by J. J. Fletcher, M.A., B.Sc.—The Rev. J. E. Tenison-Woods read a paper on a species of *Brachyphyllum*, which was found in the Tivoli coal mine. In many respects this species resembled the well-known *B. mamillare* of the British and Continental Oolite, but lest any confusion should arise from a doubtful identification, and as the stems and leaves of this specimen were much thicker, and the leaves more fleshy than in *B. mamillare*, the author distinguished it as *B. crassum*. He considered that the discovery of this specimen served to place the Jurassic age of the Ipswich (Queensland) coal beds beyond much doubt.—A note was read by Dr. H. B. Guppy, of H.M.S. *Lark*, on the cocoa-nut eating habit of the *Birgus* of the Solomon Islands. Dr. Guppy had no doubt from what he had observed that the Robber-Crab is in the habit of breaking open the shells of the cocoa-nuts with its powerful chelæ.—Mr. Haswell stated that he had much pleasure in announcing to the Society that, thanks to the intelligent inquiries made by Mr. Morton of the Museum, while recently in Queensland, he had hopes that they were on the way towards learning something of the embryology of the *Ceratodus*. Mr. Morton had ascertained that the *Ceratodus* spawns in the Burnett River during the months of June, July, or August, the spawn being deposited in a slight excavation formed in the bed of the river at a depth of eight or ten feet, the male and female remaining in close attendance on it until hatched. Arrangements had been made by which it was hoped that a supply of the spawn might be obtained for observation next season.

PARIS

Academy of Sciences, February 26.—M. Blanchard in the chair.—The death of Baron Cloquet, Member in Medicine and Surgery, was announced.—The following papers were read :—Note on various points of celestial physics, by M. Janssen. At Meudon Observatory they are studying movements of photospheric matter with the aid of series of images obtained with the "photographic revolver"; they are also working at photographic photometry, the principle being that the intensities of two light-sources are in the inverse ratio of the time they take for the same photographic work (*e.g.* producing the same tint on two quite similar plates). The method will be applied to data of the comet of 1881, the full moon, &c. M. Janssen further hopes to present soon a complete study of the spectrum of aqueous vapour.—Results of a new series of experiments on the apparatus for transport of mechanical work installed on the Chemin de fer du Nord, by M. Deprez: note by M. Tresca (see p. 422).—On the heat of formation of chromic acid, by M. Berthelot.—Rain in the Isthmus of Panama, by M. de Lesseps. A table of observations of rainfall by Mr. John Stiven, for 1879–1882, shows that 1879 was an extraordinarily rainy year (2·152 m.), a large excess occurring in November. The rain-season lasts nearly six months, from May to November, excepting an interruption of a few weeks in June and July. This is explained by the behaviour of the ascending body of air which accompanies the curve of maxima in its annual oscillation on either side of the thermal equator, which movement is connected with the annual movement of the sun. The trade-winds north and south also affect the phenomena.—On the bronze tools used by miners in Peru, by M. Boussingault. A bronze chisel found in an old quarry of trachyte near Quito, evidently served in working the trachyte (softened by water); it contains copper 95, tin 4·5, with minute quantities of lead, iron, and silver.—Nebulæ discovered and observed at Marseilles Observatory, by M. Stephan.—Exhalation of nitrogen in a gaseous state during respiration of animals, by M. Reiset. MM. Petenkoffer and Voit negated such exhalation (affirmed by the author). Recent experiments by MM. Seegen and Nowak confirm M. Reiset's view.—Direct and rapid attenuation of virulent growths by the action of heat, by M. Chauveau. The method may be applied to liquids of artificial cultivation with much better success than to the natural humours of the system, and it may be graduated at will according to the degree of attenuation desired.—Contribution to the

study of refrigeration of the human body in hyperthermic diseases, especially typhoid fever, by M. du Montpellier. He indicates the useful effects of his cooling apparatus.—Researches on the division of acids and bases in solution by the method of congelation of the solvents, by M. Raoult.—On the relations between covariants, &c. (continued), by M. Perrin.—On the theory of uniform functions, by M. Goursat.—Note on a point of the theory of continuous periodic fractions, by M. de Jonquières.—Remarks on a communication of M. de Chardonnet on the vision of ultra-violet radiations, by M. Mascart. He thinks the conclusions too absolute; he showed some years ago that ordinary sight habitually perceives the whole ultra-violet solar spectrum as lavender grey, and some eyes see even further.—On the increase of intensity of scintillation of stars during auroras, by M. Montigny. (Already noted elsewhere.)—On the production of apatites and of bromised Wagnerites with lime base, by M. Ditte.—Researches on the action of zinc-ethyl on amines and phosphines; new method of characterising the nature of these bodies, by M. Gal.—On the products of decomposition by water of fluoborised acetone α, by M. Landolf.—On neutralisation of glycolic acid by bases, by M. de Forcrand.—On a new base of the quinoleic series, phenol-quinoleine, by M. Grimaux.—Derivatives of strychnine, by M. Hanriot. He describes a new dinitro-strychnine, also diamido-strychnine.—On sulphocyanacetone, by MM. Tcherniac and Hellon.—Chloronitrated camphor, by M. Cazeneuve.—On the ice plant, by M. Heckel. His observations some years ago agree with those of M. Mangon.—Researches on the chromatophores of the *Sepiola Rondeletii*, by M. Girod. He regards the protoplasm of the pigmentary cell as the agent of extension; the basilar cell producing contraction.—On the disease of saffrons known as *Tacon*, by M. Prillieux.—On an inversion of temperature observed at a point of the Alps on December 27, 1882, by M. Henry. M. Broch noted a similar case near Christiania, where a rich banker has a châlet at a height of 408 m. In winter the temperature there is often about zero, while at Christiania it is 10 or 15 degrees below zero.—M. Daubrée indicated the contents of a new publication from Lima, *Anales de Construcciones civiles y de Minas del Perú*.

March 5.—M. Blanchard in the chair.—The following papers were read:—Observations of the satellites of Neptune, of Uranus, and of Saturn, with the equatorial of the eastern tower of Paris Observatory, by MM. Henry; Note by M. Mouchez. A new objective having been put in the instrument (acquired in 1849, under Arago) renders it the best instrument the Observatory has ever had.—Nebulæ discovered, &c. (continued), by M. Stephan.—The prolific power of virulent agents that are attenuated by heat, and the transmission by generation of the attenuating influence of a first heating, by M. Chauveau. The attenuation does not involve any alteration of the vitality or prolific power of the agents deprived, by heat, of their infectious properties. It is also shown that the influence is not merely individual, but may appear in the properties of new agents arising through proliferation of the protoplasm which has been directly subject to it.—M. de Lesseps stated that he was about to go to the region of the North African Chotts for a month, to consider the investigations of M. Roudaire.—A letter from M. Nordenskjöld referred to his intended departure for Greenland in August. He believes that vast regions covered with perpetual ice are a physical impossibility on our globe south of the 80th degree of N. lat., and goes to the interior of Greenland to test this view.—On the importance of the *rôle* of inhibition in therapeutics, by M. Brown-Séquard. A morbid activity will disappear suddenly, or nearly so, on irritation at some point (to be sought) more or less distant from that at which the activity prevails.—Practical use of sulpho-carbonate of potassium against Phylloxera in the south of France, by M. Culeron.—On the perturbations of Saturn due to the action of Jupiter, by M. Gaillot.—Observations of the great comet of September, 1882 (II. 1882), made at the Observatory of the Transit of Venus Mission at Martinique, by M. Bigourdan.—Observations of the new comet (Brooks and Swift) made at Paris Observatory (equatorial of the western tower), by the same.—Observations of the same comet at Lyons Observatory with the 6-inch Brunner equatorial, by M. Gonessiat. The comet appeared as a bright, nearly round nebulosity, with nucleus well condensed. In a clear sky, a straight tail of about 13′ long was observed. (M. Bigourdan estimates the brightness as about that of a star of 6th or 7th magnitude.)—On the approximation of sums of numerical functions, by M. Halphen.—On the series of polynomes, by M. Poincaré.—On the trajectories of different points of a connecting-rod in motion, by M. Léauté.—On the theory of electromagnetic machines, by M. Joubert. He calls attention to the loss of work in continuous-current machines through change of direction of the current in the coils of the ring.—On a new collimator, by M. Thollon. The slit is made to take any direction, while its image remains fixed. This is effected by means of a total-reflection prism placed behind the slit, with its hypothenuse face parallel both to the axis of the collimator and to the slit.—Dissociation of the bromhydrate of phosphuretted hydrogen, by M. Isambert.—On sulphuric chlorhydrate, by M. Ogier.—On chloride of pyrosulphuryl, by the same.—Heat of formation of solid glycolates, by M. de Forcrand.—On the hydrocarbons of peats, by M. Durin. From an examination of fresh mosses, he thinks it probable (with M. Dumas) that the hydrocarbons of peat are not formed during vegetal decomposition, but that they existed already in the mosses which formed the peat.—Experiments proving that sanguineous concretions, formed at the surface of an injured part of vessels, begin with a deposit of hematoblasts, by M. Hayem.—On the chromatophores of Cephalopoda, by M. Blanchard. He holds that they do not differ at all in general structure from those of fishes, batrachians, and especially saurians (chameleon). The chromatophore is a sort of amœba charged with pigment, living for itself and independent of the skin which imprisons it; it is, however, under the influence of the nervous system. The radiating fibres are mere fibres of connective tissue, and M. Blanchard has never (like M. Girod) found them to vary in form with the chromatophore.—On a flagellate Infusoria, ectoparasite of fishes, by M. Henneguy. This was observed on trout. The name *Bodo necator* is given provisionally.—On the *Gnetaceæ* of the coal formation of Rive-de-Gier, by M. Renault.—Selenetropism of plants, by M. Musset. Plants of phototropic sensibility were grown from seeds in pots in a very dark place; then, on three nights, exposed at a window to direct moonlight; the stems bent over towards the moon, and followed it in its course.

BERLIN

Physical Society, February 16.—Prof. Kirchhoff in the chair.—Prof. Krech described at length a spectrophotometer which he had made in 1872, and with which, in the years 1873, 1874, 1875, and 1876, he had made a large number of observations for verification of the theory of the apparatus and determination of its errors. The theory of the instrument and the improvements proposed were fully gone into; the experiments had been made before Herr Glan had described his spectrophotometer.

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THURSDAY, MARCH 22, 1883

PATHOLOGICAL ANATOMY

A Text-Book of Pathological Anatomy and Pathogenesis. By Ernst Ziegler. Translated and Edited for English Students by Donald MacAlister, M.A., M.B., St. John's College, Cambridge. 8vo. (London: Macmillan and Co., 1883.)

FOR some years the student of medicine has felt the want of an English manual of modern Pathological Anatomy. He has been compelled either to trust entirely to his teacher, or to consult works and memoirs little adapted for beginners. This felt want the English edition of Ziegler's Pathology, when completed, will in great part meet. The author believing "that the learner gains a readier grasp of his subject when it is first presented to him as a uniform and coherent system of doctrine," has, by avoiding "much matter of controversy," succeeded in making a clear and concise statement of each subject treated. In this the author has been well seconded by the editor, who, by carefully revising and amending the original, by adding numerous references to English and French memoirs, and by otherwise with characteristic ability adapting the work for English readers, has greatly enhanced its value.

Although the authors have kept the student chiefly in view in preparing this manual, a glance at the small print and the numerous references given, will at once prove that those desirous of gaining an exhaustive knowledge of the subject, and those engaged in special investigations, have not been neglected. It seems to us that this is by far the best plan for a text-book. It is to be regretted that students at the present day read so little. In many instances they content themselves with "learning" in order afterwards to retail what they purchase from their teachers; or what is worse, when they are unfortunate enough not to have their teacher as one of their examiners, they "get up" an endless number of often useless facts, derived from all possible sources, before presenting themselves for examination. This waste of time and energy in great part results from the want of good text-books. The books available are generally too large, they are often quite beyond the grasp of the beginner, and at the same time not a little out of date. In order to be able to utilise fully the opportunities now offered for gaining a practical knowledge of pathology, and other allied subjects, lectures are not enough; there must be something to fall back upon, by means of which the impressions received from the teacher may be tested, something that will form a foundation on which an intelligent knowledge of the subject may be built. We believe that the work before us will serve this purpose, and that it will be equally useful to the teacher by enabling him to take for granted that the fundamental facts of his science can be again and again referred to as the student requires, and by providing short, concise statements which he can modify at will, and to which he can add much that is of historical interest, or that is too recent for any manual, however complete, to contain.

The volume now published deals with General Pathological Anatomy. It is divided into seven sections. Those on Malformations, Inflammation, Tumours, and Bacteria deserve especial mention. In treating these subjects the authors have been careful to avail themselves of all the recent investigations, not only in Pathology, but also in Embryology and other branches of Biology, and by making free use of small print and giving abundant references, they have succeeded in drawing up a more complete account than exists in any other English manual.

In a very suggestive introductory chapter some of the special terms used by pathologists are defined, and the functions of pathological anatomy indicated. In the section on the Formative Disturbances of Nutrition the researches of Strasburger and Flemming on the changes in cells and nuclei during subdivision are considered, and a diagram showing indirect cell-division is introduced. In speaking of cell-multiplication it is pointed out that the proposition, "The stronger the external stimulus the greater the proliferation," cannot be accepted; that "one can at most admit that very slight stimuli, sufficient merely to excite the cell without injuring it, may perhaps call into play its power of multiplication; but nothing has been experimentally established concerning the nature, the action, or the mode of application of such stimuli"; further, that "when the nutritive and formative activities of a cell are morbidly increased, the effect is due to augmentation of the physiological stimuli or diminution of the physiological resistances to growth, or the direct influence of external stimuli"; the factors probably favouring proliferation being (1) an increased capacity in the cell to assimilate nutriment, (2) an increased supply of nourishment, (3) the removal of the normal checks to growth. In the same chapter there is an account of the origin of epithelium, fibrous and adipose tissue, and of new blood-vessels; and, in the chapter immediately following, an account of the origin of pus-corpuscles and of the mode in which tissues are regenerated.

Tubercle and other allied diseases, such as lupus, leprosy, and glanders, are spoken of as "Infective Granulomata." A tubercle is defined from a histological point of view as "a non-vascular cellular nodule which does not grow beyond a certain size, and at a certain stage of its development becomes caseous"; but it is afterwards pointed out that when Koch's recent investigations are taken into consideration it must be spoken of as "a cellular nodule containing within it the specific tuberculous virus, the bacillus tuberculosis."

Among these infective granulomata we have the new disease known as "actinomycosis," which is associated with the presence of the peculiar fungus *Actinomyces*. In this disease the infection probably starts from the mouth, and results in the formation of granulations and fibrous tissue and in suppuration.

The classification of tumours has long been a puzzle to pathologists. Later writers have more and more recognised their relation to the embryonic layers, and now we have, we believe for the first time in an English text-book, a purely embryological arrangement, tumours being divided by the authors into: (1) those derived from the mesoblast—the connective-tissue tumours; (2) those containing elements derived from epithelial cells—the epithelial tumours. This classification, which commends itself by its simplicity, is likely to be generally adopted.

The consideration of the different kinds of tumours is followed by a chapter on their ætiology, in which Cohnheim's embryonic hypothesis is discussed at some length, and the objections to its general acceptance pointed out. In answer to the question, How does the tumour assume properties distinct from those of its surroundings? there is as a reply, "We believe that the phenomenon is ultimately due to some change affecting individual elements of a tissue whereby they are rendered dissimilar to their neighbours." The change is manifested especially in this —that the normal checks to the indefinite growth of the proliferous cells are inoperative or inadequate, either because the formative and productive energy is increased, or because the restraining influence of the surrounding structures is diminished, or from both causes together.

The last section of the present volume is devoted to Parasites. On comparing the German account of animal parasites with the English, we note very considerable additions and improvements. The chapter contains a sufficiently complete account of the structure and life-history of the ordinary parasites for all practical purposes. The chapter on Bacteria is extremely valuable. The editor has been careful to incorporate in the text all the important recent discoveries, and references are given to all the memoirs that the student or investigator is likely to require to consult. We thus have in a connected form the results of numerous inquiries into the nature of the organisms which for some time have been claiming not a little of the attention of biologists and physicians. In describing the bacteria, reference is made to the influence of temperature and of the surrounding medium on their growth and development, also to the influence they exercise on the nutrient liquid, and to their presence without and within the living body.

In reference to the existence of bacteria within the body we read :—

"Bacteria are perpetually entering the body with the food we eat and the air we breathe. They must, therefore, be at times found in the tissues, especially in places where access is direct. The fact that they are not easy to demonstrate is readily explained. It must be only a small number that are able to multiply in the tissues they have penetrated; the majority must quickly perish." Bacteria are described as pathogenous and non-pathogenous, the latter being harmless unless the normal secretions undergo some alteration, or the bacteria develop to an unusual extent. Under such conditions, inflammation may be set up, or the whole system may be influenced by the absorption of the soluble products of decomposition, some of which are extremely poisonous, and capable according to Hiller, of altering or even destroying the tissues exposed to them. "The pathogenous bacteria have the power of settling, not merely in the ingesta and secretions, or in dead tissue, but also in living tissue. This happens chiefly in the mucous membranes and in the lungs. The uninjured skin is protected against invasion by the horny epidermis."

"Many of the bacteria can settle in perfectly healthy mucous membranes. In the case of others we must imagine that they do not find a proper soil for their development, unless the mucous membrane is injured or altered. Of course injury or alteration of this kind may seem to make the outer skin or any other accessible tissue the starting-point of a bacterial invasion (wound-infection). All that is necessary is that a bacterium should reach a spot that affords the conditions of its development. If this occurs, it multiplies and forms colonies or swarms. These may, according to the species of the fungus and the nature of its soil, remain in aggregation, forming heaps or masses, or may spread through the tissues. Such a settlement is never without effect on the affected tissues. The bacteria may force their way into the substance of the constituent elements, and especially into the tissue-cells, which are sometimes found to be crammed with bacteria."

All that is necessary is that a bacterium should reach a spot that affords the conditions for its development, *i.e.* "the temperature of the body must be such as favours its development; it must be able to abstract fit nutriment from the tissues in which it settles; it must nowhere encounter substances which check or injure it." When in the tissues, the increase of the bacteria may be arrested by the aggregation of living cells resulting from the inflammation they set up, assisted by the regenerative action of the fixed tissue-cells. If this does not happen, they spread into the surrounding tissues, usually reaching the lymphatics and blood-vessels, some to perish, others rapidly to multiply.

The bacteria are supposed to lead to disease by withdrawing nourishment, setting up chemical changes—partly by their direct action on the nutrient material, and partly by the action of the unorganised ferments they form; and finally, as a result of these changes, by producing poisonous matters. In doing this they enter into conflict with the tissue-cells, influencing their nutritive activity, changing them or even leading to their destruction. Whether it is a change in the fermentive action of the cells, or a disturbance of the functions of the central nervous system which leads to fever, has not been determined. Neither is it known whether the unsusceptible condition of the tissues which usually follows when the bacteria have been eliminated, results from "a modification in the chemical constitution of the tissues, or to a change in the vital activity of the cells."

In referring to the relation of bacteria to infective diseases it is stated "that among the infective diseases there are certainly some which are due to the invasion of a microphyte, and that it is highly probable the others have a like origin." This chapter further gives a short account of the various diseases which have been described as resulting from the influence of bacteria, and concludes by discussing the burning question of the present moment —the mutability of bacterial species. It is well known that Naegeli, Buchner, and others believe "that both the morphological and the physiological characters of the bacteria are mutable"; that "a given bacillus does not invariably produce bacilli of the same structure, and does not always pass through the same developmental stages." "A bacterium which, under given conditions, gives rise to a definite kind of fermentation, may lose this property when cultivated under different conditions." Koch and others believe that bacteria do not alter in their properties, and that "even when the nutrient medium is altered from time to time no recognisable differences are produced."

The authors point out that "at present we are unable

to draw any certain conclusion regarding the relation of non-pathogenous to pathogenous bacteria. Clinical experience would indicate that the activity of the infective virus may vary within certain limits. And we must apparently admit that the infective bacteria have not always possessed their noxious qualities, but have acquired them somehow in the course of ages. But this is not enough to convince us that harmless bacteria can acquire infective properties rapidly. . . . We may therefore provisionally conclude that the transformation of innocuous into noxious bacteria can occur but rarely, and under special conditions."

Recent work both in this country and on the Continent seems to go against the mutability theory, and in all probability it will soon be made clear that Buchner's experiments are capable of another interpretation from that hitherto adopted.

Enough has been said to indicate that the English edition of Ziegler's Pathology will not only prove of immense help to the student, but that it will also be invaluable to the practitioner. It is to be hoped that the second part, on Special Pathological Anatomy, will soon appear, and that it will equally commend itself to English readers.

The numerous woodcuts with which the work is illustrated are beautifully distinct, the type and paper are everything that could be desired, and so successful has the editor been that there is no evidence of the greater part of the work being a translation.

ENSILAGE

Ensilage in America. By James E. Thorold Rogers, M.P. (London: W. Swan Sonnenschein and Co., 1883.)

PROFESSOR ROGERS has contributed a most interesting little book on Ensilage in America. He has no doubt been serviceable to his country in drawing public attention to a subject of importance; but like most persons who focus their eyes upon a single point, he has lost the due proportion in which it stands to its background, foreground, and surroundings. Perhaps this may be forgiven as a common fault, or it may be the secret of strength, in all propagandists. Be this as it may, it is a marked feature in the volume before us. Ensilage is to be the temporal salvation of the farmer. The Professor appears to have been carried away on the full tide of American enthusiasm, buoyed up by a certain youthful airiness scarcely consistent with the gravity of an Oxford Don. He has forgotten the salt, and those who read his book (and we trust they may be numbered by thousands) must add it for themselves.

Ensilage is the preservation of green fodder in its natural succulent condition in pits or *Silos*. These pits must be airtight and watertight, and the fodder must be so well trampled into them and weighted on the top as to arrest fermentation. The theory of the process is that, in the case of fodder so treated, heat is generated and fermentation commences. The small amount of oxygen held in the interstitial air is speedily absorbed, and its place taken by carbonic acid gas. Just as a lighted candle extinguishes itself in a bath of choke-damp of its own making when burnt in a closed vessel: so the fermentation and its accompanying heat are arrested in the mass of closely packed fodder which is in fact immersed in a bath of carbonic acid, and thus securely protected from ordinary atmospheric action. Well preserved ensilage comes out of the pit almost as green and fresh as when it was first put into it, and has acquired a pleasant vinous smell and slightly acid flavour, which has given it its name of sourhay in Germany, Austria, and Hungary. The process is at once simple and effective, but is no doubt expensive when carried out upon the scale which a successful experiment demands. Thus the larger the pit the more assured the success, as all the conditions are more perfectly attained. At p. 22 we read: "M. Havemeyer's silos were four—two fifty-nine feet long and fourteen feet wide; and two thirty-five feet long and twelve feet wide, each pair being twenty-five feet deep. They are under the same roof as the feeding barn, where there is standing-room for ninety-eight cows." The pits are bricked and cemented, or built with concrete walls, and they may be carried up higher than the level of the ground, or may be built entirely from the surface. When the ground is naturally dry and of a clayey or close texture the silo need not be lined. It is recommended that a drain should if possible be carried from the bottom of the silo to take off superfluous water. Simple as these directions undoubtedly are, they point to a heavy initial expenditure, only to be recommended after very mature consideration. On the other hand silos of smaller size, as, for example, $22' \times 9' \times 15'$ deep and other dimensions, are also mentioned. Still the fact remains that in small silos there is more waste and greater uncertainty. Also that for practical purposes a small silo would be of little value. The process of storing the fodder is very easy to understand. It is, in the case of green maize, cut up with a powerful chaff-cutter, trampled into the pit by men or horses, and when the space is filled it is covered with boards and weighted down with boxes of stone or earth to a pressure of about 100 lbs. per square foot. The fodder settles down under pressure, and is found after several months to be perfectly palatable and fresh.

Such is the process which Prof. Rogers now lays before the British public with the strongest possible recommendations. Not only so, but with threatenings or at least warnings also, for we are told that "if the New Englanders and New Yorkers succeed in extending their ensilage system, they will strive to find a foreign market for their increased produce." This process, it is urged, is entirely to revolutionise agriculture. It is to be a new point of departure, a "new dispensation." "Is there not a bonanza (a mining term for peculiarly rich ore) in the farms with this new enterprise? Will it not give the farmer such profits, with less labour, as will enable him to be more independent? Is it not going to create new interests with our sons, when they can find a more profitable employment, with less hard labour than can be found in any business in our cities?" It is to double the population of "our New England cities," and indeed appears to be a veritable *El Dorado* for farmers.

In thus introducing ensilage to the attention of his countrymen, Prof. Rogers is scarcely cautious in the manner in which he discounts the value of scientific and especially of chemical opinion upon this subject. "Ensilage is to be the food of the future for pigs and poultry

as well as for horses and bullocks." But it is only grass after all, and we can hardly believe that it can be superior to the herbage from which it was made. Pigs and poultry will graze in pastures it is true, but the digestive systems of these animals demand more concentrated foods. There is an evident tendency to "forget the rock from which it was hewn," if we may apply such words to the process of ensilage. It is green fodder *preserved* until winter. Well! if preserved until winter it cannot be eaten in summer. If eaten in summer it surely would also have been realised.

It may be better than hay, but we cannot expect from ensilage such superlative results above what might reasonably be expected from hay. Such high-flown anticipations as are embodied in Prof. Rogers's book are usually doomed to disappointment, and the process of ensilage will probably take its place in American and English agriculture as it has already taken its place in the agriculture of the Continent of Europe, among other improvements of the nineteenth century, but without overtopping any of them.

Prof. Rogers does not appear to have informed himself as to the state of our knowledge in England upon this topic. The process was fully described by the present writer for the Royal Agricultural Society in 1874. He also drew attention to it in two letters to the *Times* in 1875, when it evoked considerable discussion, under the title of "Potted Hay." The process was also both described and illustrated by drawings in the *Agricultural Gazette* at the same period. Since then it has been repeatedly tried, but in all cases without marked success. We are ready to allow that this want of success has been due to the experiments having been conducted upon a small scale and probably with too much regard to economy of outlay. The process is too generally successful in many countries to be capable of being challenged. So late, however, as 1875 Prof. Tormay of Pesth wrote to us that practical men were greatly divided as to its value. No doubt the making of sourhay deserves further trial, and there is as little doubt that it will be largely experimented upon during the coming summer. It must, however, be remembered that it may be as profitable to eat the herbage when growing, as to preserve it *in any form* for the winter. Also that our turnips, swedes, and mangels give us a means of producing meat in these countries which is not possessed by American agriculturists. Turnips and hay are probably a better combination of succulent and dry food for winter feeding than turnips and ensilage would be. In this matter we prefer to suspend judgment for a while upon the uses of ensilage to the British farmer. At the same time we cordially agree that it is likely to be particularly useful in the Eastern States of America, where the soil and climate are unfavourable to the growth of roots and favourable to the growth of maize. On this point we have abundant testimony in Prof. Rogers's book. A special attraction towards ensilage is that it can be carried out without delay in any weather, and that it saves the anxiety of haymaking.

Those who have tried it in this country complain that it is very difficult to keep the pit good when it has been once opened. Still the process is worthy of more extended trial, and if carried out without too much fear of the initial expense and risk of failure, may be shown to be of service to English agriculturists.

JOHN WRIGHTSON

OUR BOOK SHELF

Another Book of Scraps, principally relating to Natural History. With thirty-six Lithographic Illustrations from Pen and Ink Sketches of Wild Birds. By Charles Murray Adamson. (Newcastle-on-Tyne: Reid, 1882.)

MR. ADAMSON has been so much amused by the preparation of his first "Book of Scraps" that he has prepared another, and invites our opinion upon it. We cannot say that we have derived much information from looking through his letterpress, although we perfectly agree with him that the study of natural history, which he advocates, "opens out a wide field for the profitable employment of the mind." But the thirty-six illustrations which form the main portion of the book certainly show that the author has studied the forms and habits of wild birds to some purpose, although in an artistic point of view, perhaps, it would not be difficult to criticise the surroundings amongst which he places them. The drawings are a little rough, as Mr. Adamson himself confesses, but no naturalist can turn them over without recognising at once the species which are intended to be portrayed. We have seen pictures in the Royal Academy of which the same remark could not truthfully be made. Mr. Adamson is evidently most at home on the sea-shore. His sea-birds are best. With his woodcocks and partridges we are not so well satisfied. But "Another Book of Scraps" will make a nice addition to a drawing-room table.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Incubation of the Ostrich

I HAVE received the following letter from Mr. J. E. Harting, and with his permission send it to you for publication. I do so partly in justice to Mr. Harting himself—the letter having been originally written to the *Spectator* in vindication of his own accuracy, and having been rejected by the editor—and partly because I think it desirable that the point in natural history which it discusses should be definitely cleared from the erroneous views which, as I shall presently show, are still prevalent with regard to it.

ANIMAL INTELLIGENCE

To the Editor of the Spectator

SIR,—I have just read in your issue of February 3 a letter from Mr. G. J. Romanes to which a long editorial note is appended, and which raises an interesting question relating to the incubation of the ostrich. As my name is mentioned as having written something on the subject, perhaps you will allow me to offer a few remarks.

Briefly stated, the point under discussion is this: Mr. Romanes, in his recently-published work on "Animal Intelligence," has observed that in the case of the ostrich *the task of incubation is shared by both the sexes.*

In reviewing this work your critic alleges that "female ostriches take *no* part in the duty of incubation"—that is, they do not assist the male.

Whereupon Mr. Romanes cites his authorities for the statement made by him, and refers amongst other sources to my book on ostriches, published in 1876, wherein (at p. 41) I remark that "the males are polygamous, each associating with three or four hens, all of which lay their eggs in one large nest scooped

out in the sand, and relieve each other by turns at incubation. Le Vaillant purposely watched an ostrich's nest, and during the day saw four hens sit successively on the same eggs, a male bird coming late in the evening to take his turn at incubation." A little further on, I added: "Incubation lasts six weeks, the cock-bird taking his turn at sitting like the hens."

Your reviewer, still sceptical, replies: "The passage in Mr. Harting's book is based on the statement of Le Vaillant, whose observations, except when confirmed by later experience, are justly discredited by the best-informed naturalists of the present day, as he was notoriously so often unworthy of belief."

Permit me to point out that in making the statement above quoted, I by no means relied *solely* on Le Vaillant. I had before me the evidence of several modern observers on the subject, whose publications are referred to in my "List of Works quoted," at the commencement of my volume. At p. 189 I have alluded to the experiments made at San Donato, near Florence, in 1859 and 1860, by Prince Demidoff, who says that "the female ostrich began to sit as soon as the first egg was laid, and sat for three hours daily, leaving the male for the rest of the time."

At p. 196, quoting a report forwarded in 1873 by a resident of experience in South Africa to the Council of the Zoological and Acclimatisation Society of Victoria, who were then contemplating the introduction of the ostrich into that colony, I find this distinct statement: "The process of hatching is performed by the male and female sitting alternately, one keeping a vigilant look-out as sentry, as well as procuring food."

Again, in a Report by Dr. W. G. Atherstone of Grahamstown, based on observations made by himself and friends on different ostrich farms in the neighbourhood of Grahamstown, and quoted by me *in extenso*, the following passage occurs on p. 202 of my book:—"They sit alternately, the male at night grazing and guarding the females. During the daytime, the time of the male bird going on the nest varies during the period of incubation, as also does the time between the female leaving the nest and the male taking her place, the exposure and cooling being probably regulated by the temperature of the incubation fever at different stages."

In addition to the evidence of these observers I had before me the testimony of Mr. F. Denny of Grahamstown, which is too long to be quoted here, but which will be found embodied in an interesting note published in the *Zoologist* for 1874 (p. 3916); so that I felt perfectly justified in asserting in effect, as Mr. Romanes has done, that *the task of incubation with the ostrich is shared by both the sexes*. It would be easy to adduce further evidence on the subject if necessary, but I will not occupy space further than to observe that if your reviewer will turn to p. 107 of Douglass's "Ostrich Taming in South Africa," published by Messrs. Cassell and Co. in 1881, he will see a full-page illustration thus lettered, "*Hen bird sitting.* From a photograph taken at Heatherton Towers."

Admirers of Le Vaillant will be glad to learn that in this case at least his assertions (to quote your reviewer) "have been confirmed by later experience," and are therefore not to be discredited.—I am, Sir, your obedient servant,

22, Regent's Park Road, N.W. J. E. HARTING

After such a battery of evidence it seems almost needless to adduce more; but as the point is an interesting one to ornithologists, I shall briefly add some corroborative proof from other sources.

In the *Spectator*, besides referring to the above, I gave a reference to two articles published by Mr. E. B. Biggar on the ostrich-farms of the Cape Colony, and also to the recently published work by Mr. Nicols; from each of these sources I shall now quote brief passages. Mr. Biggar writes as follows:—

"Some will sit throughout with the most solicitous maternal instinct; . . . others manifest such anxiety, that when the hen has been a little late in taking her morning turn upon the nest, he has gone our, and, hunting her up, has kicked her to the nest in the most unmanly manner. Some are very affectionate over their young, others the reverse; thus do individuals differ even among ostriches. As a rule the cock bird forms the nest, sits the longest, and takes the burden of the work of hatching and rearing. Contrary to what has been currently understood, and what is still stated even in recent colonial accounts, the cock bird sits at night, not the hen. In this peculiarity the hand of Providence may be seen, for the worst enemies of the nest appear at night, and the cock, being stronger and braver, is better able to resist them; moreover, the feathers of the cock being black, night sitting would not expose him to that exhaustion from the sun's rays which would ensue if he sat during the day; while at the same time time the grey feathers of the female are less conspicuous while she sits during the day."—*Field*, August 21, 1880.

And again, "After turning the eggs over one by one with her beak, she will sit perhaps for hours with her head stretched flat and snake-like on the ground, and her body as motionless as a mound of earth. Occasionally, on hot days, she may be seen with her body lifted slightly out of the nest to admit a current of air over the eggs; and sometimes she will even leave the nest for two or three hours, till instinct tells her that the lowering temperature requires her return" (*Century*, January, 1883).

Mr. Nicols's work, entitled "Zoological Notes," repeatedly states that the hen bird assists the cock in the process of incubation, and on my writing to him to ask whether he had witnessed the fact, he answers that although he has not done so himself, a well-educated friend "who had passed some time in visiting ostrich-farms in South Africa" had done so; and, in answer to his express inquiry on the subject, wrote, "that the female took part in the task, though not nearly to so great an extent as the male," adding that he was surprised to hear there should be any question concerning a fact so well known to the ostrich farmers.

Lastly, having recently been to Florence, I took the opportunity of calling upon the superintendent and proprietor of the Zoological Gardens there, and obtained all the particulars of the case alluded to by Mr. Harting in the above letter as having occurred at San Donato. I found that two broods of young had been raised in successive years by the same pair of ostriches, and that on both occasions the female assisted the male to incubate the eggs: "que le male et la femelle couvent alternativement," in the words of the published report ("Guide du R. Jardin Zoologique de Florence," p. 81, 1868). Here, however, as in all the previously-mentioned cases, the fact which I stated in "Animal Intelligence" was apparent, viz. that the cock bird undertook the whole duty of sitting during the night.

Now when all this evidence is taken together it appears to me impossible to doubt that the female ostrich assists the male in the process of incubation. Yet from the fact of this evidence not having been clearly focused, an old error on the subject still appears to be prevalent. This error arose some twenty years ago from the observations of M. Noel Suchet (? or Suquet) on a pair of ostriches kept in confinement. Thus, in 1863, Dr. Sclater wrote:—"We now know with certainty from the observations of M. Noel Suchet, Director of the Zoological Gardens at Marseilles, that the normal habits of the ostrich (as regards incubation) do not differ materially from those of its allies of the same family" (*Proc. Zool. Soc.*, 1863, p. 233); and Mr. Darwin, following the judgment formed by Dr. Sclater, wrote in the "Descent of Man" (p. 479) that the male bird "undertakes the whole duty of incubation." Again, my reviewer in the *Spectator*—who, although curiously weak in his logic, appears to be strong in his ornithology—pins his faith entirely to this single observation of M. Suchet. Lastly, Prof. Newton in his article on "Birds" in the "Encyclopædia Britannica" (p. 771), relying, I presume, on the same observation, writes:—"A band of female ostriches scrape holes in the desert sand, and therein promiscuously dropping their eggs, cover them with earth, and leave the task of incubation to the male, who discharges the duty thus imposed upon him by night only, and trusts by day to the sun's rays for keeping up the needful fostering warmth."

Thus it appears that the influence of M. Suchet's observations has been very disproportionate to its merits, and has misled some of our principal ornithologists concerning the normal habits of ostriches.[1] Possibly Prof. Newton, with his extensive knowledge of the literature of such matters, and writing since the appearance of most of the counter-evidence which I have given, is cognisant of some other observations on which he rests his statement. But, if so, it becomes desirable that he should supply his references, as otherwise his statement appears to rest, as my reviewer in the *Spectator* would say, "simply on the survival of the old belief." GEORGE J. ROMANES

March 12

Difficult Cases of Mimicry

I HAVE received from Mr. Thos. Blakiston, of Tokio, Japan, a communication to the *Japan Mail* by himself and Prof. Alexander,

[1] I may observe that Mr. R. B. Sharpe, writing in "Cassell's Natural History" (vol. iv. p. 228), has not been thus misled, for he says distinctly that the cock and hen "relieve each other by turns."

commenting on my article in NATURE, vol. xxvi. p. 86, and pointing out some errors as to the estimated advantage derived by the mimicking butterflies. On referring to my article, I find that I have, by an oversight, misstated the mathematical solution of the problem as given by Dr. Fritz Müller and confirmed by Mr. Meldola, and have thus given rise to some confusion to persons who have not the original article in the *Proceedings of the Entomological Society* to refer to. Your readers will remember that the question at issue was the advantage gained by a distasteful, and therefore protected, species of butterfly, which resembled another distasteful species, owing to a certain number being annually destroyed by young insectivorous birds in gaining experience of their distastefulness. Dr. Müller says: "If both species are equally common, then both will derive the same benefit from their resemblance—each will save half the number of victims which it has to furnish to the inexperience of its foes. But if one species is commoner than the other, then the benefit is unequally divided, and the *proportional advantage* for each of the two species which arises from their resemblance is *as the square* of their relative numbers." This is undoubtedly correct, but in my article I stated it in other words, and incorrectly, thus: "If two species, both equally distasteful, resemble each other, then the number of individuals sacrificed is divided between them in the proportion of the square of their respective numbers; so that if one species (*a*) is twice as numerous as another (*b*), then (*b*) will lose only one-fourth as many individuals as it would do if it were quite unlike (*a*); and if it is only one-tenth as numerous, then it will benefit in the proportion of 100 to 1."

This statement is shown by Messrs. Blakiston and Alexander to be untrue; but as some of your readers may not quite see how, if so, Dr. Müller's statement can be correct, it will be well to give some illustrative cases. Using small and easy figures, let us first suppose one species to be twice as numerous as the other, *a* having 2000 and *b* 1000 individuals, while the number required to be sacrified to the birds is 30. Then, if *b* were unlike *a* it would lose 30 out of 1000, but when they become so like each other as to be mistaken, they would lose only 30 between them, *a* losing 20, and *b* 10. Thus *b* would be 20 better off than before, and *a* only 10 better off; but the 20 gained by *b* is a gain on 1000, equal to a gain of 40 on 2000, or four times as much *in proportion* as the gain of *a*. In another case let us suppose *c* to consist of 10,000 individuals, *d* of 1000 only, and the number required to be sacrificed in order to teach the young birds to be 110 for each species. Then, when both became alike, they would lose 110 between them, *c* losing 100, *d* only 10. Thus *c* will gain only 10 on its total of 10,000, while *d* will gain 100 on its total of 1000, equal to 1000 on 10,000, or 100 times as much *proportional gain* as *c*. Thus, while the gain in actual numbers is inversely proportional to the numbers of the two species, the *proportional* gain of each is inversely as the *square* of the two numbers.

I am, however, not quite sure that this way of estimating the *proportionate* gain has any bearing on the problem. When the numbers are very unequal, the species having the smaller number of individuals will presumably be less flourishing, and perhaps on the road to extinction. By coming to be mistaken for a flourishing species it will gain an amount of advantage which may long preserve it as a species; but the advantage will be measured solely by the fraction of *its own numbers* saved from destruction, not by the proportion this saving bears to that of the other species. I am inclined to think, therefore, that the benefit derived by a species resembling another more numerous in individuals is really in inverse proportion to their respective numbers, and that the proportion of the squares adduced by Dr. Müller, although it undoubtedly exists, has no bearing on the difficulty to be explained.

ALFRED R. WALLACE

MR. A. R. WALLACE has been so good as to forward me the extract from the *Japan Mail* above referred to, together with his reply. The article in question bears the title, "Protection by Mimicry—a Problem in Mathematical Zoology." The authors, while admitting the broad principles involved in Dr. Fritz Müller's theory, fail to see why the advantage derived by the mimicking species, in cases where the latter is less numerous than the model, should be as the square of the relative numbers. They admit that "the ingenious explanation seems perfectly satisfactory," but the proportional benefit appeared to them exaggerated. Mr. Wallace has now, I think, cleared up the misunderstanding with reference to this part of the question, but it may be of use in assisting towards the further discussion of the problem if I here give the simple algebraical treatment adopted in the original paper.

Let a_1 and a_2 be the numbers of two distasteful species of butterflies in some definite district during one summer, and let n be the number of individuals of a distinct species which are destroyed in the course of a summer before its distastefulness is generally known. If both species are totally dissimilar, then each loses n individuals. If, however, they are undistinguishably similar, then the first loses $\frac{a_1 n}{a_1 + a_2}$ and the second loses $\frac{a_2 n}{a_1 + a_2}$. The absolute gain by the resemblance is therefore for the first species, $n - \frac{a_1 n}{a_1 + a_2} = \frac{a_2 n}{a_1 + a_2}$; and in a similar manner for the second species, $\frac{a_1 n}{a_1 + a_2}$. This absolute gain, compared with the total numbers of the species, gives for the first (A_1), $\frac{a_2 n}{a_1(a_1 + a_2)}$, and for the second (A_2), $\frac{a_1 n}{a_2(a_1 + a_2)}$. We thus have the proportion, $A_1 : A_2 = a_2^2 : a_1^2$.

With reference to Mr. Wallace's concluding paragraph, I may point out that the advantage of the mimic is "measured solely by the fraction of *its own members* saved from destruction." Thus, taking his last example, the species *c* saves only 1/1000 of its whole number, and *d* saves 1/10 of its whole number by the resemblance to *c*. The fact that these numbers stand to one another in the ratio of $1 : 10^2$, whilst $c : d = 10 : 1$, is a mathematical necessity from which I do not see how we can escape. As the numerical disproportion between the species increases, the advantage derived by the more abundant insect is practically a vanishing quantity; whilst, on the other hand, if the two species are equal in numbers, it is obvious that they both derive the same advantage, each losing only half the number that it would if there was no resemblance between them.

It must not be forgotten in considering the question of mimicry between two nauseous species that the foregoing calculations apply only to the case where the resemblance is perfect, *i.e.* so exact that the insects are absolutely undistinguishable by their foes. The initial steps may be hastened in these cases by the near blood-relationship of the species, and it is a remarkable circumstance that large numbers of species belonging to different distasteful genera have a close similarity of wing-pattern, although the distinctness of the genera has never been called in question. But the genera concerned, although distinct, are very closely related, and this is quite in accordance with the views here advocated.

The general question as to the persecution of distasteful butterflies by young inexperienced birds, &c., is certainly one on which much work remains to be done, and very great service could be rendered if naturalists residing in the tropics would undertake some systematic experiments in this direction. My friend, Mr. W. L. Distant, the author of the "Rhopalocera Malayana," has already given reasons in these columns (vol. xxvi. p. 105) for disbelieving in any such want of experience, and I have discussed this phase of the question with him elsewhere (*Ann. and Mag. Nat. Hist.*, December, 1882).

R. MELDOLA

On the Value of the "Neoarctic" as One of the Primary Zoological Regions

IN the *Proceedings of the Academy of Natural Sciences of Philadelphia* (December, 1882) Prof. Angelo Heilprin has an article under the above title, in which he seeks to show that the Neoarctic and Palæarctic should form one region, for which he proposes the somewhat awkward name "Triarctic Region," or the region of the three northern continents. The reasons for this proposal are, that in the chief vertebrate classes the proportion of peculiar forms is less in both the Neoarctic and Palæarctic than in any of the other regions; while, if these two regions are combined, they will, together, have an amount of peculiarity greater than some of the tropical regions.

This may be quite true without leading to the conclusion argued for. The best division of the earth into zoological regions is a question not to be settled by looking at it from one point of view alone; and Prof. Heilprin entirely omits two considerations—peculiarity due to the absence of widespread groups, and geographical individuality. The absence of the

families of hedgehogs, swine, and dormice, and of the genera *Meles, Equus, Bos, Gazella, Mus, Cricetus, Meriones, Dipus*, and *Hystrix*, among mammals; and of the important families of flycatchers and starlings, the extreme rarity of larks, the scarcity of warblers, and the absence of such widespread genera as *Acrocephalus, Hypolais, Ruticilla, Saxicola, Accentor, Garrulus, Fringilla, Emberiza, Motacilla, Yunx, Cuculus, Caprimulgus, Perdix, Coturnix*, and all the true pheasants, among birds, many of which are groups which may almost be said to characterise the Old World as compared with the New, must surely be allowed to have great weight in determining this question.

The geographical individuality of the two regions is of no less importance, and if we once quit these well-marked and most natural primary divisions we shall, I believe, open up questions as regards the remaining regions which it will not be easy to set at rest. There runs through Prof. Heilprin's paper a tacit assumption that there should be an equivalence, if not an absolute equality, in the zoological characteristics and peculiarities of all the regions. But even after these two are united, there will remain discrepancies of almost equal amount among the rest, since in some groups the Neotropical, in others the Australian, far exceed all other regions in their speciality. The temperate and cold parts of the globe are necessarily less marked by highly peculiar groups than the tropical areas, because they have been recently subjected to great extremes of climate, and have thus not been able to preserve so many ancient and specialised forms as the more uniformly warm areas. But, taking this fact into account, it seems to me that the individuality of the Neoarctic and Palæarctic regions is very well marked, and much greater than could have been anticipated; and I do not think that naturalists in general will be induced to give them up by any such arguments as are here brought forward.

ALFRED R. WALLACE

A Remarkable Phenomenon.—Natural Snowballs

I TAKE the liberty of inclosing a copy of an account of natural snowballs which I furnished to the *Courant* newspaper in this place. It may be well to state that the distance from Long Island Sound to Massachusetts is some seventy miles, and that the Connecticut Valley Railroad is about fifty miles long, and runs close to the bank of the Connecticut River for some forty miles; the rolls of snow on the frozen river are said to have been very large and handsome. SAMUEL HART

Trinity College, Hartford, Conn., U.S.A., February 22

On Tuesday evening a light but damp snow fell upon the crust that had formed over the snow of Sunday's storm; and the south wind, which arose at a later hour, produced an unusual phenomenon. Wednesday morning the college campus, the park, and vacant lots everywhere hereabouts were seen to be strewn with natural snowballs, some of them resembling spheres with diameters of from one to nine inches or more, and others looking very much like rolls of light cotton batting, having a cylindrical shape, but in nearly every case with a conical depression at each end reaching nearly or quite to the middle. It was easy to see how the balls had been formed, as it is easy to see how boys roll up the snow for their forts. The wind had in each case started a small pellet of the moist snow, and it had rolled along until it grew so large that the wind could move it no further. The ball not only increased in diameter as it rolled, but also grew gradually in length as a little more of the snow stuck to it on each side, and thus the snow was formed into the peculiar shape described—that of a cylinder with a hollow at each end, as if a long isosceles triangle were rolled up, beginning at its vertex. The largest of the cylinders measured on the college campus had a diameter of twelve inches and a length of eighteen inches, while others in the fields in the neighbourhood seemed much larger. The path of the balls could in many cases be readily traced for a distance of twenty-five or thirty feet. The snow, it should be added, was not at all closely packed, but lay together very lightly and yielded to a slight touch, so that it was impossible to move a ball without breaking it.

Observers in other parts of the city report that some balls were seen of the size of a barrel which left tracks behind them for more than sixty feet. From East Hartford it is reported that they studded the fields thickly, especially in places where the wind had a long range, and were of every size to that of a half bushel or larger. Similar balls were seen yesterday morning in many places from the Sound north to Massachusetts. All along the line of the Valley Railroad they appeared on every rod of ground, and at some places they had left tracks showing that the wind had blown them in every direction, even in some cases up hill.

This interesting phenomenon, though quite unusual, has been noticed before in different places in this country and elsewhere, the most striking instance on record being one which was observed in New Jersey in 1808; this was in the daytime, when the whole process could be watched. On this occasion some of the masses of snow which were rolled up by the wind attained a diameter of three feet. They appear to have been seen, however, over an area of only some four hundred acres, whereas the snowballs yesterday were spread thickly over many square miles.

[We have received a communication on the same subject from Prof. Brocklesby of Hartford.—ED.]

The Late Transit of Venus

I AM told that, in referring to the observations on the late transit of Venus which were made from a station on our college grounds by the astronomers of the German Imperial Commission, you speak of them as using the photographic process. This is not correct; besides contact observations they restricted themselves to the use of the heliometer. The first and the second contacts were not seen by reason of clouds; but four half sets and six full sets of heliometric measurements were made—128 in all. The third and the fourth contacts were observed by the German astronomers and by myself.

SAMUEL HART

Trinity College, Hartford, Conn., U.S.A., February 22

Rankine's "Rules and Tables"

I DO not know upon what authority your reviewer of Rankine's "Rules and Tables" bases his dictum that the r in the rule for the extension or compression of a spiral spring should be to the second power instead of to the third power. Prof. Rankine's view was that it should be r^3. I would refer your reviewer to vol. xviii. of the *Transactions of the Institution of Engineers and Shipbuilders in Scotland*, where he will find, amongst other results of an experimental committee's investigations upon the important question of the loading of safety-valves by such springs, that the *third* power of the radius or diameter of the spring is also used. W. J. MILLAR

Glasgow, March 10

[The formula given by Mr. Millar is, the writer of the notice informs us, perfectly correct, and the error is his.—ED.]

Meteors

ABOUT five minutes past seven this evening I saw the most beautiful "*shooting star*" I have ever witnessed. It was moving from east to west directly over this town, and disappeared at an apparent distance of ten or twelve miles, after traversing an arc of about 75° as I saw it. It was visible whilst one might count ten or twelve at the usual rate of speaking. In its course it not only left a most unusually long train of light behind, but whole pieces kept *dropping*. What appeared is thus best described. These pieces followed the original for a space, leaving perceptible lines of light. Probably ten or a dozen such pieces were broken off during the time I was looking. Some idea of it may be gathered from the fact that for a time I thought it was a rocket. The light was remarkably white, the brilliance much above that of Venus at any time, and its rate of motion slow. The most remarkable feature, however, was the continuous breaking away of pieces, which left in turn visible trains of light. THOMAS MASHEDER

The Grammar School, Ashby-de-la-Zouch, March 17

IN NATURE, vol. xxvii. p. 434, reading somewhat hastily, I took the brilliant meteor there mentioned to be one I myself saw. Reading more carefully, however, in last week's issue, I see that both day and hour and direction differ. On March 4, about 8.45 p.m., a very large and bright meteor passed at a low altitude from south to north. It was of a greater apparent size than Venus, quite as bright, but with a greener light. The motion was slow, no train; it only became incandescent during

a short part of its transit, and passing behind the roofs of some houses was immediately lost to sight. HENRY CECIL

Bregner, Bournemouth, March 20

P.S.—If a line be drawn north and south, the meteor became visible at a point due east, which direction I was facing.

THE BRITISH CIRCUMPOLAR EXPEDITION[1]

THE journey to Fort Rae, though long, was full of interest and variety. Our party, consisting of myself, two sergeants, and an artificer, of the Royal Artillery, left Winnipeg on June 9 by steamer for Fort Carlton, on the Saskatchewan, *viâ* Lake Winnipeg. We were detained a day in that lake by ice, but reached the mouth of the Saskatchewan on the 13th, where we were delayed four days trans-shipping cargo to the river steamer, which lay three miles off at the upper end of the rapids; a tedious voyage of eight days took us to Carlton, a stockaded port on the south bank of the river. For the first three days the country seemed one immense swamp, with numerous shallow lakes; then the banks gradually grow higher, till at "the Forks" (the confluence of the north and south branches of the Saskatchewan) they are about 150 feet above the river. Here the soil seems very rich and fertile, and about the new settlement of Prince Albert, a day higher up, the country is quite English in appearance—undulating, covered with rich grass, with woods here and there—a far more attractive-looking country than the flat, treeless prairie near Winnipeg.

From Carlton, after a day or two spent in hiring transport carts, we started on the 30th with a train of ten carts, containing our provisions and baggage. The country was very pretty, well wooded and watered, with duck, snipe, and prairie chicken in abundance; it was at times difficult to believe one was not in an English park. But the most vivid imagination cannot picture the swarms of mosquitoes that at times attacked us: they came against our faces like flakes in a heavy snowstorm, and though we found our veils and gloves a good protection whilst travelling, yet, when mealtimes came, veils had to be laid aside, and the wretched insects seized the opportunity of taking their meal too.

On the third day of our journey, on reaching the crest of some rising ground, an extended view opened before us, ridge behind ridge, a sombre sea of pinewood stretching away in the distance. It was the great sub-Arctic forest which extends northwards to the barren grounds at the Arctic circle and east and west to the Atlantic and the Pacific. On entering the woods the mosquitoes were not quite so bad, but our unfortunate animals became the prey of an enormous horsefly, which settled on them in thousands, biting them till they were streaming with blood. Fortunately they only came out during the heat of the day, and we were sometimes obliged to make a halt and light fires so that the animals might stand in the smoke, which they were very willing to do; indeed they often put a newly-lighted fire out by rolling in it.

The road through the woods was very bad, and breakdowns were numerous, but at last on July 9 we reached Green Lake, which we left by boat on the 11th for Ile à la Crosse. Our conveyance was now one of the Hudson Bay Company's inland boats, with a crew of eight Indians. As we had the stream with us, we were able to drift all night, only landing when we required to cook; so we reached Ile à la Crosse early on the 14th.

We left it the same evening with a crew of eight Chipewyans, the best crew we ever had. I think they must have pulled sixty miles on one day, the day after we left the fort. On the evening of that day we had an aurora shortly after sunset, which is unusually early in the evening for one. This one appeared to be remarkably close, from its rapid motion and from its being between us and a cirro-cumulus cloud. It was accompanied by a distinct swishing noise like the sound of a sharp squall in a ship's rigging, or the noise a whip makes in passing through the air. I have not heard it since, though there have been plenty of auroras, but from what I have been told by those who have passed their lives in the country, I am of opinion that this sound is occasionally, though rarely, heard, and that it would be heard oftener were it not that the aurora is generally at too great a height.

Two more days brought us to Portage la Loche, a track of some fourteen miles across the watershed dividing the basin of the Arctic Ocean from that of Hudson's Bay. It is fairly level till the last mile, when the edge of the valley of the Clearwater River is reached, some 600 feet above the stream. From this point the view of the valley is very fine, and it strikes one the more from the monotonous nature of the scenery hitherto. The river flows between two ranges of hills, from 800 to 1000 feet in height, and here and there in rapids between limestone cliffs. The first "portage" (where the boats have to be hauled some distance overland) is particularly picturesque, but the whole valley abounds in bits that would delight an artist's eye.

On July 28 we reached the Athabasca River, a fine stream, half a mile or more in width, and the strong current, aided by a fair wind, took us to the lake in a couple of days. There are several springs of naphtha and one of sulphur on the banks.

On crossing Lake Athabasca to Fort Chipewyan, there is a complete change in the character of the country. On the south side the banks are nearly level with the water, all reeds and mud; on the north side is a savage wilderness of Laurentian rock. From a hill at the back of the fort is an extensive view of this strange and desolate country. To the west the lake stretches away to the horizon; on the other side is a mixture of lake, island, and river, and to the north the land, a wilderness of rock in low rounded hills, with a few stunted pines in the valleys, all pretty enough, but so lonely looking.

We were detained a fortnight at Fort Chipewyan till the arrival of the Mackenzie River boats. The heat was at times extreme—as much as 90° in the house.

The Slave River, or Mackenzie, as it really is, is a magnificent river, especially after its junction with the Peace River, which is at least as big as the Athabasca. The united stream is often a mile in width. About half way to Slave Lake are the rapids, where the scenery is very fine. There are four portages, over three of which the boats had to be hauled, so it was two days' work getting through them. We had a sharp frost on the morning of the 19th, the buckets, &c., that were left with water in them had a quarter of an inch of ice on them in the morning.

On the next evening, while running down the rapid to the last portage, the "Portage des Noyés," after sunset, a bright parhelion made its appearance, some 10° or 12° above the horizon. It was of a bright red colour, and threw a brilliant reflection in the water, remaining visible for about twenty minutes, when it changed into a crimson column, that gradually died away.

On August 22 we reached Fort Resolution—a wretched-looking place on a flat muddy coast—and the same evening we left for Fort Rae. At sunset the pilot of the boat insisted on stopping for the night at a small rocky island at the mouth of the Slave River. I thought it a pity to stop as we had a fair wind, but the natives of the country have a great dread of lakes, and certainly Great Slave Lake is a stormy place. At midnight a heavy swell suddenly arose, and our boat was stove in and sunk in a very few minutes. It was a pretty wet job to land all the baggage and stores, which of course were all saturated

[1] Letter from Capt. Dawson, R.A., in command of the Expedition

See p. 243.

with water; but fortunately the instruments all escaped unhurt, and nothing was lost but a pair of boots and a couple of hats, and all our salt and most of our sugar, which the water dissolved.

For the next two days we were employed repairing the boat, it blowing a gale and raining hard the whole time, so that we could dry nothing; and when at last we started, almost constant head-winds and frequent gales made our journey a slow one. Fortunately our course lay among islands, so that we enjoyed a certain degree of shelter from the wind, and harbours of refuge were always at hand in case of necessity. These islands are all of rock and well wooded, but destitute at this season of the year of game, which was unfortunate for us, as our provisions were getting short, and our crew were reduced to a pound of flour per diem, with a little tea and sugar. There were not even fish to be caught, though they are usually abundant, but I suppose the rough weather had driven them into the deep water. At last we shot some seagulls, and we were all glad enough to eat them.

At length, on the 30th, we reached Fort Rae, which lies in lat. 62° 38′ N. and long. 115° 25′ W., half way up a long gulf that runs for about 100 miles in a north-west direction from the mouth of the Yellow Knife River. The fort is situated at the foot of a rocky hill that rises some 200 feet above the lake, which is about four miles wide at this point. The Indians who resort here for trade hunt for the most part in the "barren lands" near the Coppermine River, whence they bring quantities of skins and beef from the musk-ox, which seems to be very abundant. Deer too are very plentiful, and in the winter they migrate in great herds from the barren lands to the country between the arm of the lake on which Fort Rae lies and the Mackenzie. Sometimes these herds pass quite close to the fort, and take two or three days in passing. Their numbers must be very great; a single band has been known to kill over 15,000 in an ordinary season.

This year the deer have passed at some distance, but the Indians are now bringing in fresh meat daily.

These Indians are of the "Dog-rib" tribe—T'akfwel-ottiné, they call themselves—a quiet, inoffensive race, like all the wood-Indians. They are almost all Roman Catholics, the missionaries of that religion being very numerous in the country, and they are certainly very devoted and hard-working. There are also Protestant missionaries, but they do not appear to have made any converts.

The Dog-ribs are a branch of the Chipewyan family which occupies all that portion of the continent between the Rocky Mountains and Hudson's Bay to the north of the parallel of 55°. They are unprepossessing in appearance, and their language is almost unpronounceable by a European. Their alphabet, if they had one, would contain no less than seventy-one letters, that being the number of distinct sounds. I believe the language is allied to the ancient Mexican—at any rate the Navajo is the nearest to it of existing languages—and the combinations of letters that one sees in Mexican names (*tl*, for instance) are common in this language. The Dog-ribs have the remarkable peculiarity of a national habit of stammering, which is most marked in those who seldom come in to the fort. They treat their women with more kindness than is usual among the American Indians.

Fort Rae is one of the windiest and cloudiest places I have ever seen, but I am told this is an exceptional year. It is certainly a very late autumn; the lake was not frozen till November 1, and it is only within the last day or two that the cold weather has really set in. Last night the thermometer was at −34°.

My space is at an end, but by the next mail I hope to give you an account of our winter here.

Fort Rae, December 1

ON THE NATURE OF INHIBITION, AND THE ACTION OF DRUGS UPON IT[1]

IV.

A CONDITION very nearly similar to that caused by atropia is produced by morphia. When this substance is given to a frog, its effects are exactly similar to those produced by the successive removal of the different parts of the nervous system from above downwards. Goltz has shown that when the cerebral lobes are removed from the frog it loses the power of voluntary motion and sits still; when the optic lobes are removed it will spring when stimulated, but loses the power of directing its movements. When the cerebellum is removed it loses the power of springing at all; and when the spinal cord is destroyed reflex action is abolished.

Now these are exactly the effects produced by morphia, the frog poisoned by it first losing voluntary motion, next the power of directing its movements, next the power of springing at all, and lastly reflex action. But after reflex action is destroyed by morphia and the frog is apparently dead, a very remarkable condition appears, the general flaccidity passes away and is succeeded by a stage of excitement, a slight touch causing violent convulsions just as if the animal had been poisoned by strychnia.[2]

The action of morphia here appears to be clearly that of destroying the function of the nerve centres from above downwards, causing paralysis first of the cerebral lobes, next of the optic lobes, next of the cerebellum, and next of the cord. But it seems probable that the paralysis of the cord first observed is only apparent and not real, and in order to explain it on the ordinary hypothesis we must assume that during it the inhibitory centres in the cord are intensely excited so as to prevent any motor action, that afterwards they become completely paralysed, and thus we get convulsions occurring from slight stimuli.

On the hypothesis of interference, the phenomena produced both by atropia and by morphia can be more simply explained. These drugs, acting on the nervous structures, gradually lessen the functional activity both of cells and of fibres; the impulses are retarded, and thus the length of nervous connection between the cells of the spinal cord, which is calculated to keep them in proper relation in the normal animal, just suffices at a certain stage to throw the impulses half a wave-length behind the other, and thus to cause complete inhibition and apparent paralysis.

As the action of the drug goes on, the retardation becomes still greater, and then the impulses are thrown very nearly, but not quite, a whole wave-length behind the other, and thus they coincide for a short time, but gradually again interfere, and therefore we get on the application of a stimulus, a tonic convulsion followed by several clonic ones, and then by a period of rest. This explanation is further borne out by the fact observed by Fraser, that the convulsions caused by atropia occurred more readily during winter, when the temperature of the laboratory is low and the cold would tend to aid the action of the drug in retarding the transmission of impulses.[3]

The effect of strychnia in causing tetanus is very remarkable; a very small dose of it administered to a frog first renders the animal most sensitive to reflex impulses, so that slight impressions which would normally have no effect, produce reflex action. As the poisoning proceeds, a slight stimulus no longer produces a reflex action limited to a few muscles, but causes a general convulsion throughout all the body, all muscles being apparently put equally on the stretch. In man the form assumed by the body is that of a bow, the head and the heels being bent backwards, the hands clenched, and the arms tightly drawn to the body.

[1] Continued from p. 468.

[2] Marshall Hall, Memoirs on the Nervous System, p. vii. (London, 1837). Witkowski, Archiv für exper. Path. und Pharm., Band vii. p. 247.

[3] Transactions of the Royal Society of Edinburgh, vol. xxv. p. 467.

My friend Dr. Ferrier has shown that this position is due to the different strengths of the various muscles in the body. All being contracted to their utmost, the stronger overpower the weaker, and thus the powerful extensors of the back, and muscles of the thighs keep the body arched backwards and the legs rigid, while the adductors and flexors of the arms and fingers clench the fist and bend the arms, and draw them close to the body.[1] The convulsions are not continuous, but are clonic; a violent convulsion coming on and lasting for a while, and then being succeeded by an interval of rest, to which after a little while another convulsion succeeds. The animal generally dies either of asphyxia during a convulsion, or of stoppage of the heart during the interval.

When the animal is left to itself the convulsions—at least in frogs—appear to me to follow a certain rhythm, the intervals remaining for some little time of nearly the same extent.

A slight external stimulus, however, applied during the interval—or at least during a certain part of it—will bring on the convulsion. But this is not the case during the whole interval. Immediately after each convulsion has ceased I have observed a period in which stimulation applied to the surface appears to have no effect whatever.

It is rather extraordinary also, that although touching the surface produces convulsions, irritation of the skin by acid does not do so.[2]

The cause of those convulsions was located in the spinal cord by Magendie in an elaborate series of experiments.

Other observers have tried to discover whether any change in the peripheral nerves also took part in causing convulsion; but from further experiments it appears that the irritability of the sensory nerves is not increased.[3]

According to Rosenthal, strychnia does not affect the rate at which impulses are transmitted in peripheral nerves; according to him, however, it lessens the time required for reflex actions. Wundt came to the conclusion that the reflex time was on the contrary increased.

In trying to explain the phenomenon of strychnia tetanus on the hypothesis of interference, one would have been inclined by Rosenthal's experiments to say that strychnia quickened the transmission of impulses along those fibres in the spinal cord which connect the different cells together.

The impulses which normally, by travelling further round fell behind the simple motor ones by half a wave-length, and thus inhibited them, would now fall only a small fraction of a wave-length behind, and we should have stimulation instead of inhibition.

Wundt's results, on the other hand, would lead to the same result by supposing that the inhibitory wave was retarded so as to fall a whole wave-length behind the motor one. On the assumption, however, that the fibres which pass transversely across from sensory to motor cells, and those that pass upwards and downwards in the cord connecting the cells of successive strata in it, are equally affected, we do not get a satisfactory explanation of the rhythmical nature of the convulsions. By supposing, however, that these are not equally affected, but that the resistance in one—let us say, that in the longitudinal fibres—is more increased than in the transverse fibres we shall get the impulses at one time thrown completely upon each other causing intense convulsion, at another half a wave-length behind, causing complete relaxation, which is exactly what we find.

This view is to some extent borne out by the different effect produced by a constant current upon these convulsions, according as it is passed transversely or longitudinally through the spinal cord. Ranke found that when passed transversely, it has no effect, but when passed longitudinally in either direction, it completely arrests the strychnia convulsions, and also the normal reflexes which are produced by tactile stimuli.

Ranke's observations have been repeated by others with varying result, and this variation may, I think, be explained by the effect of temperature.

Near the beginning of this paper I mentioned that the touchstone of the truth or falsehood of the hypothesis of inhibition by interference was to be found in the results of quickening or slowing the rate of transmission of stimuli.

Heat and cold are the two agents regarding whose action in this respect we have the most trustworthy experimental data. In peripheral nerves, heat up to a certain point quickens the transmission of stimuli, and cold retards it. In the spinal cord warmth increases the excitability, and at a temperature of 29 to 30 may of itself cause tetanus.[1] Cold also beyond a certain temperature increases the reflex excitability.

The effect of warmth and cold upon strychnia tetanus is what we would expect on the hypothesis of interference. With small doses of strychnia warmth abolishes the convulsions, while cold increases them. When large doses are given, on the contrary, warmth increases the convulsions, and cold abolishes them.[2]

We may explain this result on the hypothesis of interference in the following manner:—

If a small dose of strychnia retard the transmission of nervous impulses so that the inhibitory wave is allowed to fall rather more than half a wave-length, but not a whole wave-length, behind the stimulant wave, we should have a certain amount of stimulation instead of inhibition. Slight warmth, by quickening the transmission of impulses, should counteract this effect, and should remove the effect of the strychnia. Cold, on the other hand, by causing still further retardation, should increase the effect. With a large dose of strychnia, the transmission of the inhibitory wave being still further retarded, the warmth would be sufficient to make the two waves coincide, while the cold would throw back the inhibitory wave a whole wave-length, and thus again abolish the convulsions.

The effect of temperature on the poisonous action of guanidine is also very extraordinary, and is very hard to explain by the ordinary hypotheses, although the phenomena seem quite natural when we look at them as cases of interference due to alterations in the rapidity with which the stimuli are transmitted along nervous structures. Guanidine produces, in frogs poisoned by it, fibrillary twitchings of the muscles, which are well marked at medium temperatures, but are abolished by extremes of heat and cold. Thus Luchsinger has found that, when four frogs are poisoned by this substance, and one is placed in ice-water, another in water at 18° C., a third in water at 25° C., and a fourth in water at 32° C., the fibrillary twitchings soon disappear from the frog at 0° C., and only return when its temperature is raised to about 18° C. In the frog at 18° C. convulsions occur, which are still greater in the one at 25° C. In the frog at 32° C., on the other hand, no trace of convulsions is to be seen; the animal appears perfectly well, and five times the dose of the poison, which at ordinary temperatures would convulse it, may be given to it without doing it any harm, so long as it remains in the warmth,[3] although when it is cooled down the effect of the poison at once appears.

Another cause of tetanus that is difficult to understand on the ordinary hypothesis of inhibitory centres is the similar effect of absence of oxygen and excess of oxygen. When an animal is confined in a closed chamber, without oxygen it dies of convulsions; when oxygen is gradually

[1] Brain, vol. iv. p. 313.
[2] Eckhard, Hermann's Handb. d. Physiol. Band ii. Th. 2. p. 43.
[3] Bernstein quoted by Eckhard, *op. cit.* p. 40. Walton, Ludwig's Arbeiten, 1882.

[1] Cayrade, Recherches critiques et exper. sur les Mouvements Reflexes, p. 48.
[2] Kunde and Virchow quoted by Eckhard, *op. cit.* p. 44; Foster, Journal of Anatomy and Physiology, November 1873. p. 45.
[3] Luchsinger, Physiologische Studien, Leipzig, 1882, p. 44.

introduced before the convulsions become too marked, it recovers. But when the pressure of oxygen is gradually raised above the normal, the animal again dies of convulsions. This is evidently not the effect of mere increase in atmospheric pressure, but the effect of the oxygen on the animal, inasmuch as 25 atmospheres of common air are required to produce the oxygen convulsions, while 3 atmospheres of pure oxygen are sufficient. This effect is readily explained on the hypothesis of interference by supposing that the absence of oxygen retards the transmission of impulses in the nerve-centres; so that we get those which ought ordinarily to inhibit one another, coinciding and causing convulsions. Increased supply of oxygen gradually quickens the transmission of impulses until the waves first reach the normal relation, and then the normal rate being exceeded, the impulses once more nearly coincide, and convulsions are produced a second time.

In discussing the action of the nervous system we have hitherto taken into account only that of the nerve fibrils, and left out of the question the nerve cells. We have assumed that the waves arrived in the reservoir (in our diagram) from a distance, and were simply transmitted along channels, but in the nervous system we have to take into account the origination of the waves in the nerve cells themselves, as well as their propagation along the nerve channels.

There is a great difference between the function of the nerve cell and of the nerve fibre analogous to that which exists between the cell and the wire in a galvanic battery. The particular form of energy which we met with in both cases originates in the cell and is transmitted along the fibre or the wire. In both cases also the energy appears to originate from chemical changes going on in the cell. Material waste of some sort goes on in both, and in both the products of this waste if allowed to accumulate will by and by arrest the action.

We find an indication of the difference between the amount of chemical change which goes on in the nerve cell and in the nerve fibre in the amount of blood supplied to each respectively. The nerve cells are abundantly supplied with blood, and the nerve fibres very sparingly so. The free supply of blood secures to the nerve cells both the supply of fresh material and ready removal of waste products.

Perhaps the best illustration that we can find in physics of the processes which take place in the nervous and muscular systems is however afforded by singing flames in which the sounds and movements are produced by very numerous small explosions: for both in the nervous and muscular systems the tissue change appears to go on as a series of small explosions. The material which yields nervous and muscular energy undergoes oxidation, but the oxygen concerned in the process is not derived directly from the external air. Substances which yield oxygen are contained within the tissues themselves just as nitre is contained along with oxidisable substances in a charge of gunpowder.

In this paper also we have spoken of waves of nervous interference as if they were simple, but it is much more probable that they are very complex, resembling much more the beats of sound produced by two singing flames which are not in unison, than simple waves of water.

The number of nervous discharges which issue from the motor cells of the spinal cord during tetanus and set the muscles in action is, according to Dr. Burdon Sanderson, about 16 per second, but in all probability each of these impulses consists of a large number of small vibrations. In rhythmical actions, such as that of the respiration, we have probably at the very least three rhythms, 1st, exceedingly rapid vibrations in the nervous cells; 2nd, slower vibrations or beats from 16 or 18 per second, which issue from them and excite the muscles to action; and 3rd, a still slower rhythm, of 16 per minute, probably due to interference between groups of cells, which leads to inspiratory movements alternating with rest or with active exspiration. The consideration of these complicated phenomena would, however, at present lead us too far, and they as well as the subject of nervous interference in the heart and rhythmic contraction of muscles, must be reserved for another time.

In this paper I must be content with the attempt to show that inhibition and stimulation in the nervous system are not dependent on special inhibitory or stimulating centres, but are merely relative conditions depending on the length of path along which the stimulus has to travel and the rate of its transmission. The test of the truth or falsehood of this hypothesis is to be found in the effect of alteration in the rapidity of nervous transmission upon inhibitory phenomena. The application of this test appears, so far as our present data go, to support this hypothesis.

T. LAUDER BRUNTON

BEN NEVIS OBSERVATORY

IN NATURE, vol. xxvii. p. 39, I gave a brief notice that on November 1—owing to stress of weather forbidding the regular daily ascents of Ben Nevis—I was obliged to discontinue the daily work of the meteorological observing system on the summit and slopes of the mountain. This was in simultaneous connection with my system of observations near the sea-level at Achintore, Fort William. As in the previous summer, I had the honour to organise and carry on the work under the auspices of the Scottish Meteorological Society. The experience gained in 1881, when I first commenced observing on the Ben, enabled me to draw up and submit to the Society a more elaborate plan of mountain observation for the summer and autumn of 1882; and as I have been fortunate enough to carry it through for five months without any hitch, and as I am not aware that anything of the kind had, previous to my first undertaking, been attempted, I am naturally anxious that NATURE should have a more complete account of my last year's operations. My plan was to have *fixed* stations at different altitudes between the main observatories at the base and on the summit of the mountain, so placed in fact that I could observe *regularly* at half-hourly intervals during the daily ascent and descent of the Ben; to extend the number of summit observations to five sets; and to have in every case simultaneous observations taken at the sea-level station—my grand base of operations. All this was with a view to localising disturbances existing in the stratum of atmosphere between the sea-level and the top of Ben Nevis, to furthering meteorological research generally, and so ultimately to gain forecasting material. I arrived at Fort William from Edinburgh on May 25, and at once proceeded to give effect to my plans. During the next few days I was engaged mainly in erecting Stevenson's thermometer screens, and laying out the sea-level station; in establishing a new "midway" observatory at the lake, erecting screen, and building there a granite cairn for a barometer; and in reopening the temporary observatory on the summit of the mountain. It was only by dint of great exertion and a gang of men that I got all in order on the top of the Ben on May 31. I had no occasion, however, to alter the arrangements of the previous summer; and the heavy work of reopening chiefly consisted in digging out from the vast accumulations of snow the barometer cairn, hut, and thermometer cage which here, as a safeguard, incloses Stevenson's screen. The snow, in fact, was nearly four feet deep, and it was necessary to cut out wide areas around the instruments. I also erected another screen to contain Negretti and Zambra's self-registering clock-hygrometer, most kindly placed at my disposal by that eminent firm for the purpose of obtaining 9 p.m. values. I had also to fix a new roof of ship's canvas to the rude shanty that affords some little

shelter from the piercing cold and storms. The barometer, a fine Fortin, had been left in its cairn built up during the past winter; and great labour was expended before the north side of the cairn was reopened, the stones being so hard frozen that a crowbar had to be employed. The instrument was found in good condition.

Passing over all other details of arranging the stations and fixing instruments, I may say that I had all in order and commenced work on June 1. I now give a list of the stations, with positions, hours, and elements of observation.[1] The distances in the text are given in right lines from the sea-level station. Fig. 1 at once shows the bearings, and distances by the actual track followed. Fig. 2 is a longitudinal section giving total actual distances.

ACHINTORE, FORT WILLIAM, BASE OR SEA-LEVEL OBSERVATORY.—*Position:* About 28 feet above sea, on a level sward, perfectly open on all sides, running parallel and immediately adjacent to Loch Linnhe; soil, gravelly loam.

Hours.—5, 5.30, 6.15, 7, 7.30, 7.55, 8.30, 9, 9.30, 10, 10.30, 11, and 11.30 a.m.; and noon, 0.30, 1, 1.45, 2.30, 3, 6, and 9 p.m.

Elements.—Atmospheric pressure by mercurial barometer, temperature of air and evaporation (dry and wet bulbs), direction and force of wind, kind and amount of

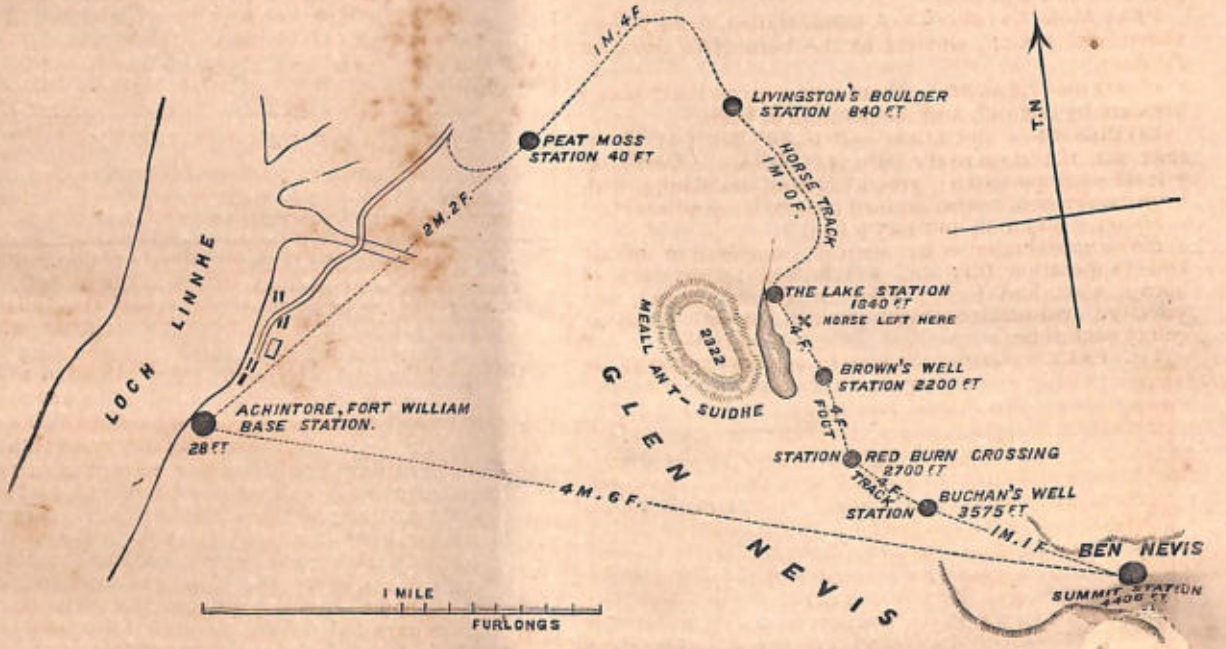


FIG. 1.

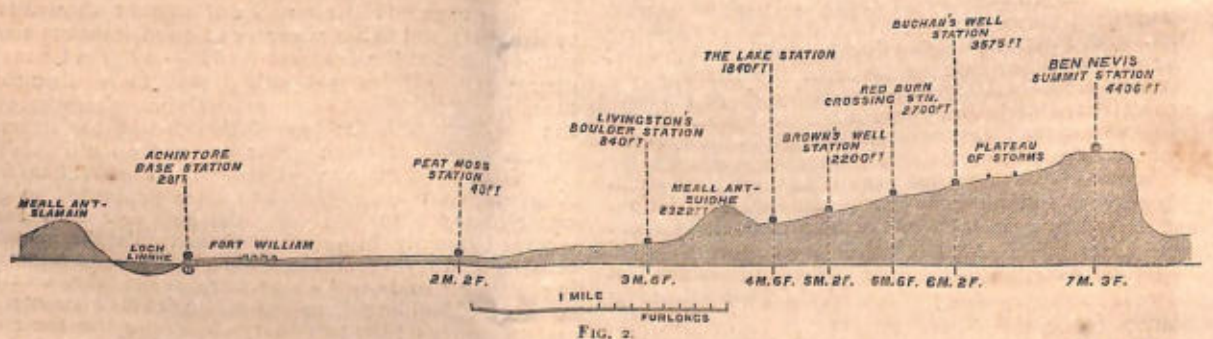


FIG. 2.

cloud, and movements and velocities of the various strata of cloud, hydrometeors and remarks in full detail at all the above times. Maximum and minimum shade temperature, solar maximum and terrestrial minimum temperature, earth temperature (1 and 2 feet), and rainfall at 9 a.m. and 9 p.m. Temperature of Achintore well, and subsequently of Loch Linnhe between 9 and 11 a.m.

Ozone for periods of ½ hour, 1 hour, 1½ hour, and 2 hours between 9 and 11 a.m.; also for periods of 24 and 12 hours, ending 9 a.m. and 9 p.m. Ozone also for the following periods of exposure:—6 hours ending 1 p.m., and 18 hours ending 7 a.m., and subsequently in addition for 15 hours ending 5.30 a.m., and 9 hours ending 2.30 p.m. [It will be seen later that all these ozone observations (except those for 12 hours ending 9 o'clock) were simultaneous with others on the summit of Ben Nevis, at the Lake, and Peat Moss stations.]

Actinism of the sun's rays and of daylight by Dr. Angus Smith's apparatus for 24 hours ending 10.17 a.m.; comparison-pressure by aneroid at 5 a.m. and 3 p.m. on leaving for and returning from the summit and slopes' stations.

PEAT MOSS.—*Position:* About 40 feet above sea; 2m. 2f.; perfectly open; near the middle of the extensive moss at the foot of *Meall an t-Suidhe;* peaty, swampy soil, with hummocks around.

Hours.—5.30 to 6 a.m. (this was the only hour in the entire system that varied, and extra simultaneous read-

[1] Cloud movements and velocities were not, however, noted absolutely *every* time.

ings were taken at Achintore whenever this was the case), and 2.30 p.m. From August 1 also at 9, 9.30, 10, 10.30, and 11 a.m.

Elements.—Temperature of air and evaporation (dry and wet bulbs), wind and force; kind of cloud, amount and velocity; hydrometeors and remarks in full detail as before at all the above times. Pressure by aneroid, 5.30 to 6 a.m., and at 2.30 p.m. Rainfall at 9 a.m. Ozone for 15 hours, ending about 5.30 a.m., and for about 9 hours, ending 2.30 p.m.; also for periods of ½ hour, 1 hour, 1½ hour, and 2 hours between 9 and 11 a.m. (simultaneously with the summit and base stations). Temperature of adjacent water-hole subsequently about 5.30 a.m. and 2.30 p.m.

PEAT MOSS CROSSING.—A minor station about 70 feet above sea, 3m. of., situated at the burn *Allt Coire an Lochain.*

Hours and Elements.—About 5.50 a.m. and 2.17 p.m.: pressure by aneroid, and temperature of burn.

LIVINGSTON'S BOULDER.[1]—*Position:* 840 feet above sea; 3m. 1f.; close to the burn *Allt Coire an Lochain,* on a level swampy patch; ground around undulating, with large boulders of coarse-grained granite lying adjacent.

Hours.—6.15 a.m. and 1.45 p.m.

Elements.—Pressure by aneroid, temperature of air and evaporation (dry and wet bulbs), temperature of burn; wind and force; kind of cloud, amount and velocity; hydrometeors and remarks in full detail as before each time.

THE LAKE.—*Position:* A plateau-valley 1840 feet above the sea, 3 miles, on swampy ground, fairly level, by the

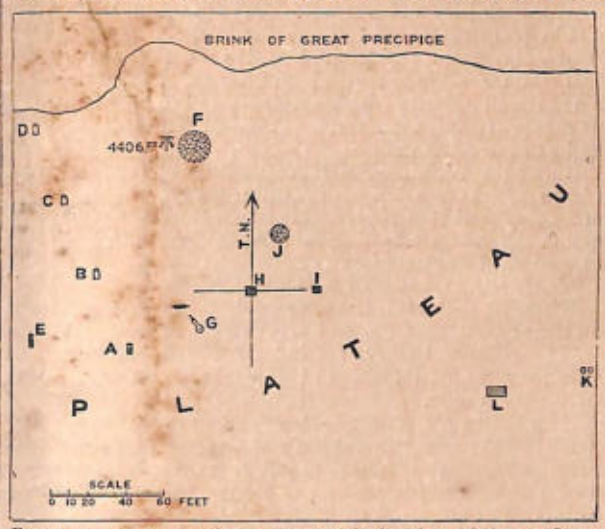


FIG. 3.—A, B, C, D, raingauges; E, notice board; F, Ordnance Survey cairn; G, solar and terrestrial radiation thermometers; H, Stevenson's thermometer cage and ozone tests; I, self-registering hygrometer; J, barometer cairn; K, earth thermometers; L, hut. The apparatus for measuring the actinism of light is near the edge of the precipice N.E. from the hut.

north-east shore of the tarn, *Lochan Meall an t-Suidhe:*



Barometer Cairn. Solar Radiation Thermometer. Thermometer Cage. Hut. Standard Rain-gauge.

FIG. 4.

granite blocks and hummocks of bog, moss, and dwarf-heather to eastward. Main slopes of Ben Nevis on east-south-east side, and *Meall an t-Suidhe* on west side.

Hours.—7 a.m. and 1 p.m.

Elements.—Pressure by mercurial barometer, com-

[1] The altitude of this station and those of the following intermediate stations, except the Lake, were determined by mean results of aneroid readings, and must be accepted accordingly.

parison-pressure by aneroid, temperature of air and evaporation (dry and wet bulbs), maximum and minimum shade temperature, and ozone for periods of 6 and 18 hours, temperature of ground at depths of 1 and 2 feet; temperature of lake; wind and force; kind of cloud, amount and velocity; hydrometeors and remarks in full detail as before each time. Rainfall on 1st, 8th, 15th, and 22nd of each month.

BROWN'S WELL.—*Position:* 2200 feet above sea, 3m. 1f., on a grassy patch with springs and swamps, on the main slopes of Ben Nevis. Boulders and stones of fine-grained granite graduating into felsite lie around. Slope to westward estimated at 35°.

Hours.—7.30 a.m. and 0.30 p.m.

Elements.—Pressure by aneroid, temperature of air and evaporation (dry and wet bulbs), temperature of well; wind and force; kind of cloud, amount and velocity; hydrometeors and remarks in full detail as before each time.

RED BURN CROSSING.—*Position:* 2700 feet above sea, 3m. 2f., above the general limit of vegetation in the deep ravine of and close beside the Red Burn; boulders and debris of porphyritic rock on all sides; slope to westward estimated at 40°.

Hours.—7.55 a.m. and noon.

Elements.—Pressure by aneroid, temperature of air and evaporation (dry and wet bulbs), temperature of burn; wind and force; kind of cloud, amount and velocity; hydrometeors and remarks in full detail as before each time.

BUCHAN'S WELL.—*Position:* 3575 feet above sea, 3m. 5f., source of the Red Burn: entirely in a region of rocks, fragmentary stones, and debris; completely open, and ground more undulating, with comparatively gentle slope to westward.

Hours.—8.30 and 11.30 a.m.

Elements.—Pressure by aneroid, temperature of air and evaporation (dry and wet bulbs), temperature of well; wind and force; kind of cloud, amount and velocity; hydrometeors and remarks in full detail as before each time.

BEN NEVIS, SUMMIT OBSERVATORY.—*Position:* 4406 feet above sea, 4m. 6f., in the centre of a rough rocky plateau, covered with felstone lavas and volcanic agglomerates (see Figs. 3 and 4).

Hours.—9, 9.30, 10, 10.30, and 11 a.m.

Elements.—Pressure by mercurial barometer, comparison-pressure by aneroid, temperature of air and evaporation (dry and wet bulbs), wind and force; kind of cloud, amount, and velocities of strata; hydrometeors and remarks in fullest detail as at the sea-level and intermediate stations at all the above times.

Maximum and minimum shade temperature, solar maximum and terrestrial minimum temperature, and rainfall by four gauges at 9 a.m.

Temperature of Wragge's Well and of ground at depths of 1 and 2 feet between 9 and 11 a.m.

Ozone for periods of ½ hour, 1 hour, 1½ hour, and 2 hours between 9 and 11 a.m., also by two differently exposed tests for 24 hours ending 9 a.m.

Actinism of the sun's rays and of daylight by Dr. Angus Smith's apparatus for 24 hours ending 10.17 a.m.

Hygrometric conditions prevailing about 9 o'clock the previous night by self-registering dry and wet bulbs, were noted at 10.50 a.m.

A moment's consideration, then, will show that the observations at the sea-level station were in every case simultaneous with those at the summit and intermediate stations, and that the hours at the latter were so arranged as to "mean" to the 10 a.m. readings at the base and summit of the mountain, and also at the Peat Moss.

Rainband by Browning's spectroscope was observed at various altitudes, and its indications proved of considerable value. Full notes were taken of the cloud limits, and of any important changes observed between the stations.

Of course my first business was to get the main observations—pressure, temperature, hygrometric conditions, wind, cloud, &c.—into full swing by June 1; and as I felt my way and got my hours and distances well under command I added to my work. Thus the ozone observing-system and the three extra rain-gauges on the summit were added on June 15, and the delicate operations for measuring the actinism of light on July 9. The additional gauges were established to discover if and to what extent the rainfall varies in connection with the wind at different points of the plateau from the centre to the edge of the great precipice.

During June, Stevenson's screens were in use only at the sea-level, lake, and summit; and hence at the other places the dry and wet bulbs had to be swung and the latter moistened afresh from adjacent water at each swinging. But aching wrists and sore fingers soon made me determine to have louvred screens at all the stations, and by July 1 they were in their places and dry and wet bulbs supplied by Hicks and Negretti and Zambra fixed permanently in each. So above all was accuracy the better insured, and the whole system went like clockwork. I left Achintore before 5.30 a.m., and returned about 3 p.m., and the rate of ascending and descending was so regulated as to insure punctuality usually within a few seconds—often to the second—at the various stations.

The new screens were a trifle smaller than the others. I need hardly add that the instruments at all stations were the best observing-standards procurable, and that the arrangements in every respect were those approved by the Meteorological Office and the English and Scottish Meteorological Societies. The condition of the wet bulbs, fixing of ozone tests, clamping self-registering instruments to prevent vibration in gales, levelling rain-gauges, and numerous other matters of important detail required the closest attention. The Beaufort wind and cloud scales were in use, and the ozone tests were Moffat's. Two assistants, educated by Mr. Colin Livingston of Fort William—a sufficient guarantee for their ability—and trained by myself, helped in the work; and relieved me in the ascent of the mountain three times a week, and on these occasions I took the sea-level station. One of the greatest difficulties I had to contend with in the Ben Nevis routine was as to the pony on which I rode to and from the Lake, where it was left to graze and await my descent. Occasionally the stable-boy overslept, and I had to make up for lost time,—no easy matter, as the wretched track leads over deep ruts and treacherous swamps, and the poor brute had a trying time of it. Still more frequently the person to whom it belonged gave me rotten saddlery in spite of all remonstrance; and on commencing the ascent the girth would break, and I had to turn the animal adrift and plod on to the Lake my fastest. This was decidedly hard, inasmuch as I was obliged to climb afoot some 2500 feet from the tarn in less than two hours by a circuitous route and over rough rock stopping to observe at the other intermediate stations. Again, the pony often wandered in his hobbles or having broken the tethering rope had made off to the moss; so also on the homeward journey I had sometimes to leave him and run my hardest over ruts and through swamps, by a short cut, to get my readings at the next station. Other trying parts of the work consisted in the journeys between Buchan's Well and the top in the allotted time, in having the two hours' exposure on the summit in bad weather, and in becoming chilled after profuse perspiration. The rude hut, with its walls full of holes of all shapes and angles through which the wind whistles and the snow-drifts drive, afforded but a poor shelter from the drenching rain and cold, and it was impossible to keep anything dry. My hands often became so numbed and swollen, and my paper so saturated that I had

the utmost difficulty in handling keys, setting instruments and entering my observations. Usually so laden was the air with moisture and so very dense and lasting was the cloud-fog that, even when no rain had actually fallen, all the fixings and instruments were dripping; and although, of course, I made a point of wiping the dry bulb, it almost immediately became wet again. Occasionally I timed the interval between wiping and fresh condensation on the bulb, and have found it wet again within *thirty seconds*.

After November 1, then, I had to discontinue the work. The hut had become choked with snow, and the carrying on of the undertaking satisfactorily impossible. I was, however, satisfied; and very pleased that I had secured five months' observations without the break of a single day.

It is not my part to refer in this paper to any results. Such I must in duty leave to be discussed and made known by the Scottish Meteorological Society. But, from what I myself know of the meteorology of Ben Nevis from the experience of two summers and autumns, I do most strongly urge the establishment of a permanent observatory on the summit, firmly believing that most important and unexpected results will accrue to meteorology from continuous observations there. Such, in connection with others at the sea-level, would in my opinion enable the energetic staff at the Meteorological Office, under Mr. Robert H. Scott's able direction, to forecast storms with far greater certainty.

I cannot conclude this account without expressing my best acknowledgments to Dr. Angus Smith for placing at my disposal his apparatus for measuring the actinism of light, which I consider an immense acquisition to a meteorological observatory; to Mr. John Browning for his rainband spectroscope; to Messrs. Negretti and Zambra for their clock-hygrometer; and finally to the Scottish Meteorological Society for the kind encouragement and liberal assistance they have given me.

CLEMENT LINDLEY WRAGGE

HYDROGEN WHISTLES

IT may be recollected by some of the readers of NATURE that a few years ago[1] I contrived a whistle for testing the upper limits of the power of hearing very shrill notes by different men and animals. When properly made, it easily suffices to do this, in the case of men and most animals, but it cannot, neither can any other instrument hitherto devised, emit such notes as it is conceivable that insects may hear. The problem whether any insects can hear notes whose numbers of vibrations per second is manyfold greater than those of the notes audible to men has not yet been fairly put to the test of experiment. I wish to show that this can now be done.

The whistles of which I speak have their lower ends closed with a piston that admits of being inserted more or less deeply, and thus of varying the depth of the whistle and consequently its note; but as a whistle will not give its proper note unless its depth be greater than its width, say, 1½ times as much, and as the depth of a whistle that gives, say, 24,000 vibrations per second is is only 0·14 inch, it follows that their bores must be very small, and that a limit of minuteness is soon reached.

Having had occasion lately to reconsider the subject, it occurred to me that I could greatly increase the shrillness of any whistle by blowing a gas through it that was lighter than common air.

The number of vibrations per second caused by whistles is inversely proportional to the specific gravity of the gas that is blown through them; therefore by the use of hydrogen, which is thirteen times lighter than air, the number of vibrations per second produced by a given whistle would be increased thirteenfold.

I have made experiments with most satisfactory results with common coal gas, whose specific gravity, though much greater than that of hydrogen, is not much more than half that of common air, and I have little doubt in consequence that a number of vibrations may be excited by one of my small-bore whistles through the use of hydrogen gas, that very largely exceeds the number attainable hitherto in any other way. They would of course fail to excite the sense of sound in any of ourselves, or perhaps to produce any physical effect that we can appreciate, whether on sensitive flames or otherwise, and the note to those creatures, if any, who could hear it, would be feebler on account of the lightness of the medium in which the vibrations originated, but it would be (so far as I can anticipate) a true note, and ought to be powerful enough to be audible at the short distances at which small creatures may be tested. The whistle I used was made for me by Hawksley, 357, Oxford Street; its bore is 0·04 inch diameter, and it gives a loud note for its size. After some prefatory trials, I proceeded as follows:—I attached the whistle to a gas jet by a short indiarubber tube. Then, without turning on the gas, I retested my range of hearing by setting the piston at various lengths and giving sharp squeezes to the tube as it lay in the hollow of my hand. The effect of each squeeze was to force a little air through the whistle, and to cause it to emit a sharp "cheep." When I relaxed the grasp, air was sucked in through the whistle, and the tube became again filled with air, ready for another squeeze.

My range of hearing proved to be such that when the depth of the whistle was 0·13 inch, I could hear no musical note at all—only a puff; at 0·14 inch I could just perceive a very faint musical note enveloped, as it were, in much puff; even at 0·20 some little puff remained, but before 0·25 the note had become purely musical. This having been established, I kept the whistle set at 0·25 and turned the gas on, giving it abundance of time to expel all air from the tube. Then, turning the stopcock to shut the indiarubber tube from behind, I gave a sharp squeeze as previously, but the whistle, instead of emitting a pure note, gave to me just the same barely perceptible sound that it did when it was set at 0·14. I relaxed my grasp and instantly retightened it, and then the whistle emitted a pure note. A little common air had regurgitated into the whistle when my grasp was relaxed, and it was the reissue of this that gave the note. I repeated the experiment several times with the same result. With a depth of 0·24 I could hear no note at all when using the gas. Then I pulled out the piston to 0·35, and the gas gave a clear musical note; on the second squeeze the note was considerably deepened. The specific gravity of the gas from the jet, as calculated from these data, would be to that of the air at the time, as 14 to 25, or as 0·56 to 1. This happens to be the specific gravity of carburetted hydrogen, but that of common street gas is heavier. Perhaps my measurements were not quite accurate; probably the note given by the gas being really fainter (though not perceptibly so) than that given by air somewhat falsified the judgment. A very slight difference in the data would raise the 0·56 to 0·60 or more.

By the use of hydrogen the little whistle when set at 0·14 inches would produce 312,000 vibrations per second. I know by experiment on others that it will give a true musical note when made much shorter than this, and I see no cause to doubt that it will sound truly at half the above length, and therefore be capable of emitting twice the above enormous number of vibrations per second.

Mr. Hawksley is making for me an apparatus with small gas bag for hydrogen pure or diluted, valves, and an indiarubber ball to squeeze, to enable hydrogen to be used with the whistle when desired. The whistle is

[1] "South Kensington Conferences, in connection with Loan Exhibition of Scientific Apparatus, 1876," p. 61.

fixed to the end of a small indiarubber tube in order to be laid near the insect whose notice it may be desired to attract.

FRANCIS GALTON

PRELIMINARY NOTE ON THE BACILLUS OF TUBERCULOSIS (KOCH)

I. THE absorption and consequent retention of certain stains by this bacillus does not appear to be effected by the hydrates of potassium, sodium, and ammonium and by aniline alone. Sodic phosphate, potassic acetate, vegetable alkaloids, &c., appear to exert a similar action. Further experiments are in progress. I have some very good preparations which were rapidly stained with a *very faintly* coloured stain containing sodic phosphate (sod. phos. cryst. B.P.).

II. The sections of tissue shown (by the kind arrangement of Mr. Blaker) at the Brighton meeting of the British Medical Association, in which the bacilli were very distinct, were stained, &c., then floated on to the glass slides, *dried over concentrated sulphuric acid* (or fused $CaCl_2$), and mounted in balsam. Hitherto my attempts to fix the colour of the bacilli, by means of a mordant, in such a way that it might remain unaffected by alcohol, and by oil of cloves, have not proved successful.

III. Treatment with a solution of potassic acetate will probably prove well adapted to free preparations from those last traces of nitric acid which so often cause their ultimate destruction.

From (II.) I should omit a very beautiful and remarkable preparation showing the spores of this bacillus in the lymphatics of the lung. This slide was prepared by Dr. Barron, of University College, Liverpool, and for his kindness in lending it to me and for much invaluable advice I am very grateful.

To Mr. Blaker, M.R.C.S., of Brighton, and to Mr. Black, M.R.C.S., of the Sussex County Hospital, I am under many obligations for their kindly interest and assistance.

J. W. CLARK

THE SHAPES OF LEAVES[1]

III.—*Origin of Types*

THE two most general and distinctive types of foliage among angiosperms are those characteristic of monocotyledons and dicotyledons respectively. They owe their principal traits of shape and venation to the manner in which these two great fundamental classes have been separately evolved from lower ancestors.

Mr. Herbert Spencer has shown that there are two chief ways in which a central axis or caulome may conceivably be developed from an integrated series of primitive stalkless creeping fronds. The *first* way is by the in-rolling or folding of the fronds so as to form a complete tube, often with adnate edges, as represented in the accompanying diagram (Fig. 20), modified by Mr. Spencer's kind permission from the "Principles of Biology." For details of the explanation, the reader must be referred to that work (vol. ii. part iv. chap. iii.); it must suffice here to note that as in such case each frond must envelop the younger fronds within it, the process is there shown to eventuate in an endogenous stem and a monocotyledonous seed—two characteristics found as a matter of fact constantly to accompany one another in actual nature. The *second* way is by the thickening and hardening of a fixed series of midribs, as shown in the next diagram (Fig. 21), also modified after Mr. Spencer; and this method must necessarily result in an exogenous stem and a dicotyledonous seed. The diagrams in Figs. 22 and 23, which represent according to Mr. Spencer (slightly altered) the development of the monocotyledonous and dicotyledonous seedling respectively, will help further to illustrate the primitive characteristics of the two types.

[1] Continued from p. 466.

The monocotyledonous type of foliage is for the most part extremely uniform and consistent, in temperate climates at least, for in the tropics the presence of large arborescent forms, such as palms and screw-pines, as well as of gigantic lilies, amaryllids, and grasses, such as the bananas, yuccas, agaves, and bamboos, gives a very distinctive aspect to the *ensemble* of the class. Being in principle a more or less in-rolled and folded frond, every part of which equally aids in forming the caulome or stem, the monocotyledonous leaf tends as a rule to show little distinction between blade and leaf-stalk, lamina and petiole. For the same reason, the free end also tends to assume a lanceolate or linear shape, while the lower part usually becomes more or less tubular or sheathing in arrangement. Again, for two reasons, it generally has a parallel venation. In the first place, since the leaves or terminal expansions are mere prolongations or tips to the stem-forming portion, it will follow that the vascular tissues will tend to run on continuously over every part, instead of radiating from a centre which must in such a case be purely artificial. In the second place it is clear that parallel venation is the most convenient type for long narrow leaves, as is plainly shown even among dicotyledons by such foliage as that of the plantains, descended from netted-veined ancestors, but with chief ribs now parallel. Still better are both these principles illustrated in those cases among dicotyledons where the lamina is suppressed altogether, and the flattened petiole assumes foliar functions, as in *Oxalis bupleurifolia* and *Acacia melanoxylon* (Fig. 24). These phyllodes thus resembling in their mode of development the monocotyledonous type, and continuous throughout with the caulome-portion of the primitive leaf, exhibit both in shape and venation the chief monocotyledonous characteristics. A typical monocotyledon in shape and venation is represented in Fig. 25.

The dicotyledonous type, though far more varied, is equally due in its shape and venation to the original characteristics implied by its origin. Only the midrib instead of the whole leaf being here concerned in the production of the stem, there is a far greater tendency to distinctness between petiole and lamina, and a marked preference for the netted venation. The foliar expansion is not here a mere tip; it becomes a more separate and decided element in the entire leaf. And as the petiole joins the lamina at a distinct and noticeable point, there is a natural tendency for the vascular bundles to diverge there, making the venation palmate or radiating, so as to distribute it equally to all parts of the expanded surface. Fig. 26 shows the resulting characteristic form of dicotyledonous leaf. Its variations of pinnate or other venation will be considered a little later on.

Among monocotyledons, the central type is perhaps best found in the mainly tuberous or bulbous orders, such as the orchids, lilies, and amaryllids. These orders, having rich reservoirs of food laid by underground, send up relatively thick and sturdy leaves; but their shape is decided by the ancestral type, and by their strict subordination to the central axis. Hence they are usually long, narrow, and rather fleshy. Familiar examples are the tulips, hyacinths, snowdrops, daffodils, crocuses, &c. Those which have small bulbs, or none, or grow much among grass, like Sisyrinchium, are nearly or quite linear; those which raise their heads higher into the open, like Listera, are often quite ovate. Exotic forms (bromelias, yuccas, agaves) frequently have the points sharp and piercing, as a protection against herbivores. In the grasses there is generally no large reservoir of food, and their leaves accordingly show the central type in a stringy drawn-up condition. So also in sedges, woodrushes, and many others. But where the general monocotyledonous habit has been more lost, and something

like the dicotyledonous habit acquired, the leaves become more like those of the opposite class. Thus the Arums, with their very unlilylike mode of growth, and their long petioles rising high into the open air, have usually a very distinct broad lamina, and have the veins accordingly branched or netted, almost as in dicotyledons. Very much the same type recurs under similar circumstances in *Sagittaria sagittifolia* (Fig. 27). Still more markedly dicotyledonous-looking are the leaves of certain very aberrant Amaryllids, such as *Tamus* and the other Dioscorideæ, which have taken to climbing, and have therefore acquired broader leaves with netted veins between the

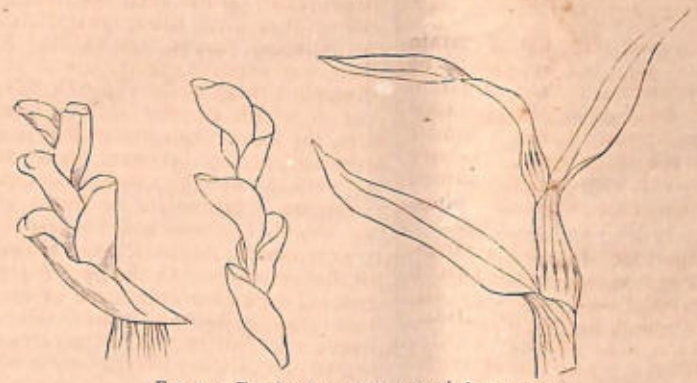


FIG. 20.—Developmen of Monocoty'edonous stem.

ribs. Compare with these the like result in *Smilax;* and then look at both side by side with such dicotyledons as *Convolvulus*. The influence of the ancestral type is here seen in the arrangement of the main ribs; the influence of environment is shown both in the approximation of general shape, and in the netting of the minor veins. Once more, the ovate type of Listera leads on readily enough to the whorled leaves of *Paris* and *Trillium*, where the venation has become similarly netted. A bushy type, like *Ruscus*, develops broad leaf-like peduncles, which closely simulate the true leaves of dicotyledonous bushes with like habit, such as box or privet.



FIG. 21.—Development of Dicotyledonous stem.

But the widest departure of all from the central monocotyledonous type is found in leaves like those of the tropical arborescent forms—the palms, screw-pines, &c. Most of these have long pinnate foliage, whose origin may best be considered when we come to examine the

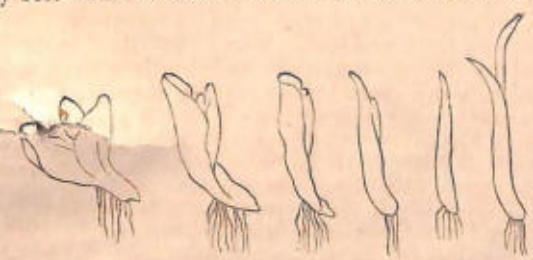


FIG. 22.—Development of Monocotyledonous seedling.

chief dicotyledonous types; meanwhile such forms as the cocoanut or the date-palm may be advantageously compared, as to conditions and general shape, with the tree-ferns in one direction, and the cycads in another. The bananas cast much analogous light upon the origin of these tropical pinnate forms. Where the plant is less arborescent, as in *Chamærops*, the leaf assumes rather a fan-shaped than a pinnate development.

Among dicotyledons it may be fairly assumed that the earliest form of leaf was simple, ovate, and nearly ribless, or with faint digitate venation. This is shown both by the nature of the earliest leaves in most seedlings, and the constant recurrence to such a type wherever circumstances are favourable for its reproduction. Hence, as a whole, digitate venation seems the commonest in most humble dicotyledons; and the only problem is how pinnate venation came to be substituted for it in certain cases. The answer seems to be that wherever circumstances have caused leaves to lengthen faster than they broadened, and so to assume a lanceolate rather than an ovate shape, the tendency has been for the main ribs to be given off, not from the same point, but a little in front of one another. If the technical botanists will pardon such a phrase, the internodes of the midrib, usually suppressed, seem here to have been fully developed. Figs. 28, 29, and 30 show the stages by which such a change

may be brought about. Figs. 31, 32, and 33 exhibit a slightly different form of the same tendency.

That this is the real origin of pinnate venation seems pretty clear on a comparison of a good many otherwise closely related forms. Look for example first at the rounded, almost orbicular leaf of *Geranium molle* and its allies,



FIG. 23.—Development of Dicotyledonous seedling.

with palmate ribs; and then look at the long, narrower, doubly pinnate, and pinnately-ribbed leaves of *Erodium cicutarium*. Or again, look at the common cinquefoil, erect and palmate; and then at silver-weed, long, creeping, closely pressed to the ground, and with numerous pinnate leaflets. Once more, compare *Alchemilla* with



FIG. 24.—*Acacia melanoxylon*.

Poterium and *Sanguisorba*. As a still simpler instance, where we get the difference in its first beginning, contrast *Ranunculus acris* with *R. repens*, or the least compound leaves of the blackberry bramble with its own most compound foliage. As a rule the most pinnate groups, such



FIG. 25.—Typical Monocotyledonous leaves and venation.

as the lesser crucifers, the peaflowers, &c., have very long leaves.

This suggested origin of pinnate venation in dicotyledons becomes even more probable when we look at the pinnate members of other classes. Among monocotyledons the long-leaved arums, though their venation is fundamentally parallel in type, have yet acquired a branching and practically pinnate set of ribs. The plantains and bananas, with very long and broad foliage, carry the same tendency yet further; for their leaves are pinnately ribbed from a stout midrib. The lower shrubby or bushy palms, like Chamærops, have fan-shaped leaves, with veins diverging in rough parallelism from a common centre; that is to say, they are in fact palmate; but in the taller arborescent palms, with their long leaves, the internodes of the midrib (to use the same convenient

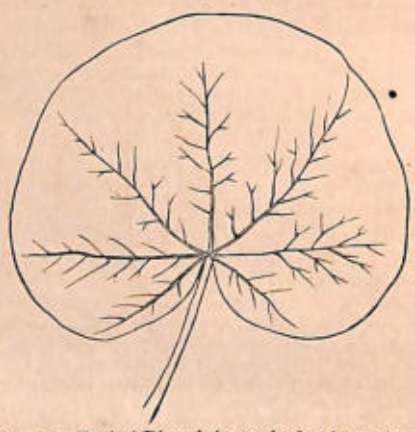


FIG. 26.—Typical Dicotyledonous leaf and venation.

phrase once more) are fully developed, so that the leaf becomes pinnatifid. In this case the subdivision into leaflets is probably protective against tropical storms. The broad-leaved plantains and the Chamærops, though so much shorter than the pinnate palms, are often torn by the wind, and a plantain leaf so torn into ribbons closely resembles a cocoanut leaf: in the taller palms this disruption between the ribs becomes normal. Compare Zamia and the other cycads among gymnosperms.

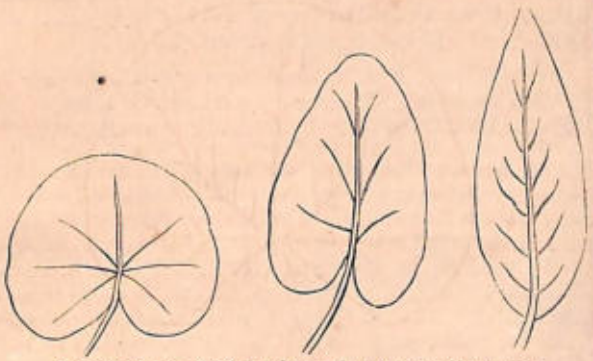


FIG. 27.—*Sagittaria sagittifolia*.

Once more, the ferns are a class with long lanceolate fronds as a rule, and their venation is almost always pinnate; the only ferns that vary much from the central type being some like the Maidenhairs, which are tufty and rather ovate in general form, and have so modified their venation as closely to approach the herb Roberts and other hedgerow plants in the outer effect. We may

fairly conclude, therefore, that pinnate venation is best adapted to very long leaves, both because of the support it gives to the cellular mass and because of the easy manner in which it distributes sap to every part alike.

It seems also probable that pinnate ribs are especially adapted to forest trees. Most of these indeed have their leaves rather long in outline—like the ash, the oak, the chestnut, the walnut, the mountain ash, the laurels, the hornbeam, and the willow—while others in which the primary ribs are palmate—like the horse-chestnut and



FIGS. 28, 29, 30.—Gradation from palmate to pinnate venation.

the plane—have their secondary ribs pinnate and their lobes or leaflets very long, so that the total effect is in the end pretty much the same. But even when the leaf is rather shortened in general outline, as in the elm, the beech, the alder, and the poplar, the venation is still pinnate. Doubtless this form of ground-plan protects the leaves of these exposed trees best against the wind; and where the leaflets are much subdivided, as in the acacias, the subdivision may be regarded as a protection against severe storms.



FIGS. 31, 32, 33.—Gradation from palmate lobes to pinnate leaflets.

The shapes of leaves in each particular species of plant thus depend in ultimate analysis upon two factors: first, the ancestrally-inherited peculiarities of type and venation; and second, the actual conditions to which the species is now habitually exposed. Accordingly, under the same conditions, a monocotyledon and a dicotyledon will tend to assume approximately similar general external forms; but their underlying ancestral peculiarities may generally be perceived through the mere analogical resemblance produced by an identical environment. By the interaction of the two factors we must endeavour to explain every particular form of leaf. To do this throughout the whole vegetable kingdom would be of course an endless task, but to do it in a few selected groups is both a practicable and a useful botanical study. The ground-plan will always depend upon the ancestral type; the outline, degree of segmentation, and minuteness of cutting, will always depend upon the average supply of carbonic acid and sunlight. GRANT ALLEN

(*To be continued.*)

NOTES

SIR JOHN LUBBOCK did right to ask the Prime Minister on Monday, whether, in remodelling the department of the Lord President of the Council, he would consider the desirability of separating the actual Minister of Education in the House of Commons from that office, and of transferring to him the power of appointing the inspectors and other officers on whom the satisfactory working of the education of the country so greatly depends. As might have been expected, Mr. Gladstone held out no hope of any change being made for a long time; that, however, is no reason why the efforts of the friends of science and education in this direction should cease.

THE Grocers' Company have issued a scheme for the encouragement of original research in sanitary science. It consists of two forms of endowment: the one, meant as maintenance for work in progress, in fields of research to be chosen by the worker himself; the other, meant as reward for actual discovery in fields of research to be specified from time to time by the Company. With the former intention the Company establishes three Research Scholarships, each of 250*l.* a year; with the latter intention they appoint a Discovery Prize of 1000*l.*, to be given once in every four years. The Research Scholarships are intended as stipends for persons engaged in making exact researches into the causes of important diseases, and into the means by which the respective causes may be prevented or obviated. The Court of the Company propose to appoint to two of the scholarships in May, and to a third in May, 1884. The Discovery Prize is intended to reward original investigations, which shall have resulted in important additions to exact knowledge, in particular sections of sanitary subject-matter. The Court will, once in four years, propose some subject for investigation; and the first subject will be announced in May.

THE Annual Report of the City and Guilds of London Institute, taken in conjunction with the Annual Meeting held last week, shows that technical education has taken firm root and is making rapid progress in this country. Though hardly yet so universal as on the Continent, there is every reason to believe that it soon will be, and Lord Selborne, who presided at the Annual Meeting, was justified in congratulating the Institute on its success. As the *Times*, in a sensible article on the Annual Meeting, says: "Lord Selborne did not dwell at length upon the general aspects of technical education. He assumed, and he had good reason to assume, that the need for a systematic development of it is proved beyond question, and is almost universally accepted. No observer now doubts that if the English artisan is to hold his ground in the struggle for existence, he must be kept up to the mark by proper teaching; and no one who has at heart the moral well-being of the working classes doubts the enormous importance of giving them an insight into principles and processes which will raise their work as much as possible out of the mere mechanical groove."

THE following are the arrangements for the lectures after Easter at the Royal Institution:—Prof. J. G. McKendrick, ten lectures on physiological discovery; Dr. Waldstein, four lectures on the

art of Pheidias; Prof. Tyndall, three lectures on Count Rumford, originator of the Royal Institution; Mr. R. J. Poole, three lectures on recent discoveries in (1) Egypt, (2) Chaldæa and Assyria, (3) Cyprus and Asia Minor; Mr. A. Geikie, six lectures on geographical evolution; and Prof. C. E. Turner, four lectures on historical sketches of Russian social life. The discourses on the Friday evenings will be as follows:—April 6, Dr. Archibald Geikie, F.R.S., The Cañons of the Far West; April 13, Dr. Waldstein, The influence of athletic games on Greek art; April 20, Prof. Bayley Balfour, The island of Socotra and its recent revelations; April 27, Dr. C. William Siemens, F.R.S., Some of the questions involved in solar physics; May 4, Robert H. Scott, F.R.S., Weather knowledge in 1883; May 11, Prof. Huxley, V.P.R.S., Oysters and the oyster question; May 18, Prof. C. E. Turner, of the University of St. Petersburg, Kustarnoe proiezvodstro: or, the peculiar system of domestic industry in the villages of Russia; May 25, Prof. Flower, F.R.S., Whales, present and past, and their probable origin; June 1, Frederick Pollock, LL.D., The sword: its forms and its history; June 8, Prof. Dewar, F.R.S.

In reference to the course of ten lectures on physiological discovery, to be given at the Royal Institution on Tuesdays, beginning April 3, by Prof. J. G. McKendrick, we may say that the object of the course will be to trace the progress of physiological research from about the beginning of the sixteenth century to recent times, and more especially along those lines which have led to great results. Admitting that the deepest foundation of physiological science is a knowledge of structure, both of organ and of tissue, it will be the aim to show how physiology has gradually attempted to solve some of its problems by the methods of physics and of chemistry, and has thus become a branch of experimental science. The method followed will be to describe briefly the lives of the great discoverers, to indicate the influence of contemporary science on their ideas and opinions, and to show how their labours have brought us to our present position. As far as possible, the fundamental experiments of discoverers will be shown or illustrated, and these will be compared with present methods.

Baron Nordenskjöld, having inspected the Royal Mail Steamer *Sophia*, which the Government have asked the Swedish Parliament to lend for his expedition to Greenland, finds that the vessel is not large enough to carry the quantities of coals and provisions required, although very suitable in other respects. He has therefore decided that a vessel shall be despatched from Denmark with these requisites, and depots established in convenient places on the coast. The *Sophia* will be overhauled and refitted for her voyage in Gothenburg, and as her commander Capt. Nilsson, of that city, has been selected.

The position of the Lena Meteorological Station is 73° 22′ 30″ N. lat. and 126° 34′ 55″ E. long. The house erected there for the observers is reported to be quite comfortable, and the health of the expedition is satisfactory.

The group of fishing Chukches, which Baron Nordenskjöld has prepared from materials collected in the *Vega* expedition for the coming Fisheries Exhibition, is now on view in Stockholm.

With the completion of the buildings in which the varied collection of the great International Fisheries Exhibition is to be housed, the preparatory work of the executive committee is drawing to an end. Not much remains to be done to the buildings which now almost cover the southern half of the Horticultural Society's gardens, and the nature and distribution of the exhibits may now be approximately given. Before handing over to the care of representatives of the colonies and of foreign Powers the places allotted to their countries, the committee on Friday invited members of both Houses of Parliament and their friends to see the buildings. To add to the interest of the aquaria, Lord Walsingham has offered to let off a lake, of about seventy-two acres in extent, on his estate at Merton, in Norfolk, and to send all the fish worth forwarding alive, and besides pike, perch, tench, and other coarse fish, he promises 1000 specimens of the celebrated golden tench. Additional value will be given to the natural history department by the exhibition in a building near the new Natural History Museum of the fine collection of fish preserved in spirit now to be brought from Bloomsbury. In order to make the exhibition as truly popular as could be desired, it will be kept open in the evening, and brilliantly lighted by electricity.

At the installation of Mr. Bright to-day as Lord Rector of Glasgow University, the degree of LL.D. will be conferred on Dr. Joseph H. Gilbert, F.R.S., and Prof. Fleeming Jenkin.

On February 26 there was discovered in the snow in several places in Trondhjem Amt, in North Norway, a fine dust, which, it was believed, originated from the Iceland volcanoes, such an occurrence having taken place in 1876. Dr. H. Reusch, of the Mineralogical Faculty of Christiania University, having examined the samples forwarded to him, now states, however, that the dust is not of an eruptive nature, but consists of common sand, fine stones, quartz, hornblende, and talc, mixed with very fine particles of vegetable matter. The phenomenon is nevertheless very remarkable, as the dust must have been carried a very long distance, the whole of the country having for months been covered with deep snow. The dust fell over a district of several degrees. The wind blew strongly from north-north-west.

On the night of the 4th inst. there was observed in Gestrikeland, in Sweden, a display of aurora borealis, the extent, vividness, and magnitude of which, it is reported, has not been observed in that country for years. An interesting feature of the phenomenon was that the big clouds, which from time to time passed below the aurora, did not in the slightest degree affect the phenomenon.

A telegram from Messina states that on the afternoon of March 20 a shower of small stones began to fall, proceeding from an eruption of Mount Etna. The atmosphere was thick and dark.

Prof. Virchow has started on a journey to Sicily, whither he goes for archæological purposes. He contemplates returning in two months.

A communication from Dr. Joule, F.R.S., was read at a recent meeting of the Manchester Literary and Philosophical Society, on the use of lime as a purifier of the products of combustion of coal gas. The slaked lime is placed in a vessel the bottom of which, about one foot diameter, is slightly domed and perforated with fine holes. The vessel is suspended about six inches above the burner. It is found that a stratum of four or five inches of lime is sufficient to remove the acid vapours so far as to prevent them from reddening litmus paper. The lime seems in many respects to present important advantages over the zinc previously recommended.

Mr. Ellis Lever has offered a prize of 500*l.* for a new miners' safety lamp, and has requested the Council of the Society of Arts to appoint one of the judges to award the prize. In accordance with this request, the Council have appointed Prof. F. A. Abel, C.B., F.R.S.

An enormous aërolite fell on February 16, a little before 3 p.m., in a ploughed field near Alfianello, between Cremona and Brescia, sinking more than one metre in the ground, and producing a rumbling noise, heard twenty kilometres off, and a

reeling of the nearest houses as by an earthquake. Unhappily the ignorant country people, when the first fright passed, with mattocks and sticks smashed it and took away the pieces, so that Prof. Calderoni, who directly ran up from Cremona, could obtain only some little fragments for chemical analysis and for scientific cabinets.

A SCHEME is proposed for introducing electric lighting into the Canton of Vaud. The motive force would be derived from turbines of 5000 horse-power at Vallorbes, and the water supply being constant and abundant, it is believed that gas, which is very costly in Switzerland, may be entirely dispensed with throughout the district.

A VERY severe shock of earthquake was experienced in Cyprus on the morning of March 5, at 7.30, lasting about fifty or sixty seconds. At Limassol the houses swayed and rocked in the most appalling manner, and uncemented walls fell to the ground. It was impossible for foot passengers in the streets to keep their balance without assistance. The mules and horses staggered about as though in fits. It was altogether the severest shock which has been recorded for many years.

WE have received copies of the circulars just issued by the Local Scientific Societies Committee of the British Association to 324 societies, for the purpose of obtaining such information as will be useful in suggesting further action. Appended is a list of about 120 local societies which publish Proceedings.

THE Easter excursion of the Geologists' Association will be to Hythe, Romney Marsh, Sandgate, and Folkestone (March 26 and 27). On April 7 the Association will visit Westcombe Park, Greenwich; on April 14 the College of Surgeons; and on April 21 Berkhampstead and Boxmoor.

WE understand that a new weekly journal, devoted to the popular exposition of sanitary matters and to the education of the people in the laws of health, will be shortly issued by Messrs. Wyman and Sons, London. The new journal will be entitled *Health*.

THE former limits of the ice-sheet of the Glacial period appear to be still more and more extended by Russian geologists, in proportion as the post-Pliocene formations of Russia are better explored. We notice in a recent monograph on the Geology of the Volga, by M. Krotoff, that the author, who is well acquainted with this region, considers the glacial formations described by Prof. Miller in the southern parts of the province of Nijni-Novgorod, as due to the action of glaciers, and not of floating ice.

THE young Society for Caucasian History and Archæology, founded in 1881, has already published a first fascicule of its *Bulletin*; the second will soon follow. Prof. V. Miller has published his linguistic "Osetian Studies," containing in the appendix a paper on the religious beliefs of the Osets; and Prof. Patkanoff has published the first part of his "Materials for an Armenian Dictionary," as well as a pamphlet "On the Cuneiform Inscriptions of the Van system discovered in Russia."

THE Administration of Public Instruction of the Caucasus has conceived an excellent idea which cannot be too much recommended for other countries; it is to invite schoolmasters to write descriptions of their localities, and to collect local traditions, folk-lore, &c., and to publish the papers received in the shape of a special collection. It is easy to conceive, indeed, the amount of knowledge which might be gathered in this way, and the attraction which is thrown by a scientific pursuit into the wearisome life of a schoolmaster, who is lost in a small town or village, far from intellectual centres. When he knows that his work will not be lost, and when he is supplied from an intellectual centre with the scientific works he needs, he surely will find interest in his pursuit. This of course applies more to Russia than England. The two first parts of the collection thus started on the Caucasus wholly confirm these previsions; as is seen from an analysis of them published in the *Izvestia*, they contain, indeed, much valuable information. The descriptions of Erivan, Gori, Wakhichevan with its district, and of Chernolyesskoye village are spoken of as very useful work. Two papers, on the formation of Lake Paleostome, and a summary of all places where the Caucasus is mentioned by the ancients, are very elaborate; whilst a series of smaller papers and notes contains a variety of ethnographical sketches, folk-lore, and traditions.

LAMPART AND CO. of Augsburg are issuing in parts a third revised edition of Hellwald's "Kulturgeschichte in Ihrer Natürlichen Entwicklung bis zur Gegenwart." Trübner and Co. are the London publishers. The work will be completed in twenty parts.

AT the last meeting of the Meteorological Society of France, M. Moureaux, physicist to the Bureau Central, read a paper showing that the regimen of the rains south of the Central Plateau was independent of the meteorological conditions on the oceanic side. This communication is considered as an argument in favour of granting to the Bureau Météorologique of Algiers the privilege of being in direct communication with the other offices, and issuing warnings for the northern side of the Mediterranean.

THE additions to the Zoological Society's Gardens during the past week include a Common Seal (*Phoca vitulina*), British Seas, presented by Mr. William Whiteley; a Common Squirrel (*Sciurus vulgaris*), British, presented by Mrs. Campbell; two Prairie Grouse (*Tetrao cupido*) from North America, presented by Mr. Henry Nash; six Common Trout (*Salmo fario*), British fresh waters, presented by Mr. S. Wilson; two Common Seals (*Phoca vitulina*), British Seas, eight Prairie Grouse (*Tetrao cupido*) from North America, deposited; three Common Sheldrakes (*Tadorna vulpanser*), three Common Pintails (*Dafila acuta*), four Shovellers (*Spatula clypeata*), European, four Chilian Pintails (*Dafila spinicauda*) from Antarctic America, two Bahama Ducks (*Dafila bahamensis*) from South America, two Chiloe Wigeons (*Mareca chiloensis*) from Chili, nine Summer Ducks (*Aix sponsa*) from North America, six Mandarin Ducks (*Aix galericulata*) from China, purchased; an Axis Deer (*Cervus axis* ♂), two Black Swans (*Cygnus atratus*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

THE OBSERVATORY AT MELBOURNE.—The seventeenth annual Report of the Board of Visitors of this establishment, together with the Report of the Government astronomer, Mr. Ellery, for the year ending June 30, 1882, has just been received. The meridian work with the transit-circle was for the most part limited to observations of standard stars, for the ordinary purposes of an observatory and the determination of places of stars used for positions of comets. The 8-inch equatorial had been arranged for the observation of the small planets *Victoria* and *Sappho*, during the last autumn, according to a programme agreed upon with several European and American, and other southern observatories, with the view to another determination of the solar parallax. The large reflector was employed on celestial photography, for sketching a number of Sir John Herschel's smaller nebulæ, for drawings of comet 1881, IV., &c. The nebula about η Argus was examined on three evenings, and was found to agree very closely with the drawing made in 1875. The majority of the smaller nebulæ were found to accord well with Herschel's descriptions. Nos. 57 and 1423, however, were much fainter than Herschel indicated, and Nos. 1655 and 2181 differed considerably from his description. The positions of these nebulæ for 1883·0 with Herschel's notes are as follows:—

		R.A. h. m. s.	N.P.D. ° ′
No.	57 ...	0 21 43	147 37·7
,,	1423 ...	6 25 6	121 12·3
,,	1655 ...	8 16 27	125 50·9
,,	2181 ...	10 37 27	125 45·0

No. 57.—Pretty bright, small, round, much brighter in the middle.

,, 1423.—Pretty bright; considerably large, round, very little gradually brighter in the middle; 4′.

,, 1655.—A double star = *h.* 4023 in a pretty small nebula, among some seventy stars.

,, 2181.—Pretty faint, small, much extended in 0°±; very suddenly, very much brighter in the middle; the first of three.

The photo-heliograph was used on every fine day possible, and 217 pictures were obtained in the year.

The necessary funds have been voted for a new transit-circle more in accordance with the modern requirements of astronomy, and its construction has been intrusted to Messrs. Troughton and Simms. Mr. Christie, the Astronomer-Royal, was invited to modify the specification sent to England, if he found reason to do so.

THE SUPPOSED VARIABLE μ DORADÛS—A SPURIOUS STAR.—Dr. B. A. Gould has made a very unexpected discovery, from which it appears that μ Doradûs of our catalogues, long supposed to be a variable star, was never observed by Lacaille in the position he assigns it in the Catalogue of the *Cœlum Australe Stelliferum*, and further, that by similar error, five other stars observed by Lacaille on the same day, which are found in the reduced catalogue published by the British Association, have no existence in the positions given. The case is a curious one, and as the *Cœlum Australe* of Lacaille is now a scarce work, we may be excused for transcribing the observations in question as they stand. They were made in Zone XI., 1751, December 16, *in parte inferiore* of Lacaille's rhomboid; the numerals are our own:—

	mag.	h. m. s.		mag.	h. m. s.
No. 1 ...	6 ...	4 17 38	No. 6 ...	6·7 ...	4 46 27
		24 2			53 34
,, 2 ...	7 ...	4 39 51	,, 7 ...	7 ...	4 51 5
		47 9			54 33
,, 3 ...	7 ...	4 25 23	,, 8 ...	5 ...	4 59 22
		31 0			5 8 28
,, 4 ...	6·7 ...	4 30 16	,, 9 ...	7 ...	6 5 53
		32 38			9 8
,, 5 ...	7 ...	4 41 33			
		43 41			

Lacaille appears to have entered correctly the times of beginning and ending of describing the chord of his rhomboid for Nos. 1 and 2, but instead of 4h. 25m. 23s. for the third star, the time was really 5h. 25m. 23s., and this error of 1h. runs on up to No. 8 inclusive; No. 9 is correct. This will be readily seen by inspecting the above times. The star entered in the Catalogue as μ Doradûs is No. 8, called 5m. in the observations but 6m. in the Catalogue, which gives its place for 1750·0, R.A. 76° 11′ 17″, Decl. −62° 7′ 4″. The place given by the B.A. reductions is R.A. 5h. 4m. 44·3s., N.P.D. 152° 6′ 57″, which is correctly deduced from the transits as printed. With the correction of +1h. to the times, the position for 1750 becomes R.A. 6h. 4m. 44·2s., N.P.D. 152° 6′ 49″, and the star "μ Doradûs" is seen to be identical with Brisbane 1172 = B.A.C. 2000 = Stone 2836, in Pictor. The other spurious stars introduced in the Catalogue by the error which Dr. Gould has brought to light are Nos. 1542, 1554, 1633, 1680, and 1706. The following identifications of the stars really observed may be useful:—

Spurious stars of the reduced Catalogue.		Stars really observed by Lacaille.
No. 1542 Reticulum	7m.	= Stone 2497, Dorado 7·6m.
,, 1554 ,,	6·7	= ,, 2532, ,, 6
,, 1633 Dorado	7	= ,, 2630, ,, 6·7
,, 1680 ,,	6·7	= ,, 2707, ,, 6.7
,, 1706 ,,	7	= Brisb. 1109, Taylor V. 516
,, 1766 (μ Doradûs)	5	= Stone 2836, Pictor 5m.

Brisbane observed a star close upon Lacaille's erroneous position of his μ Doradûs, and according to his general custom gave it Lacaille's magnitude. Moesta (*Astron. Nach.*, No. 1545) stated that he had observed this star at Santiago de Chile from February, 1860, to January, 1865, and had found it 8½ or 9 of Argelander's scale; he therefore considered it to be variable, and thought the period of variation would prove to be of long duration.

THE COMET OF 1812.—MM. Schulhof and Bossert's sweeping ephemerides for this comet are continued in No. 2489 of the *Astronomische Nachrichten*.

INSECTS VISITING FLOWERS

THE interest arising out of the writings of Darwin, Lubbock, and Hermann Müller relative to the part played by insects in their oft-recurring visits to flowers has of late years attracted much attention. The subject, in fact, has created a taste for observation, and an incentive has been given to watch the frequency of visits of various species to certain flowers, and especially to the insects' choice of colours of flower. While the mere registering of visits may seem a comparatively simple one, the reason why insects should show a preference to alight upon flowers of a certain colour, or choose certain species of plants, is a much more complicated problem than at first sight it would appear. Sir John Lubbock has shown by experiment that blue is the bees' favourite colour; H. Müller avers that in the Alps bees are attracted to the yellow rather than the white flowers. However this may be, certain it is that a much larger number of observations are yet needed before a positive law can be deduced. Two papers read at the last meeting of the Linnean Society (March 1): one by Mr. Alf. W. Bennett, "On the Constancy of Insects in their Visits to Flowers," and the other by Mr. R. M. Christy, "On the Methodic Habits of Insects when Visiting Flowers"—point out that a strict watch and ward is being kept on the movements of the busy bee and its kindred. Mr. Bennett states that butterflies show but little constancy in their visits, citing only a few instances to the contrary; but according to him, to some extent they seem to have a choice of colour. The Diptera exhibit greater constancy, though by no means absolute. The Apidæ, especially the hive-bee, manifest still greater constancy. From these data he infers that the ratio of increase is in proportion to the part performed by the insects in their carrying pollen from flower to flower. As respects preference for particular colours, in a series of observations Mr. Bennett has noted among the Lepidoptera that 70 visits were made to red or pink flowers, 5 to blue, 15 to yellow, and 5 to white; the Diptera paid 9 visits to red or pink, 8 to yellow, and 20 to white; Hymenoptera alighted 303 times on red and pink flowers, 126 on blue, 11 on yellow, and 17 on white flowers. Mr. Christy records in detail the movements of 76 insects, chiefly bees, when engaged in visiting 2400 flowers. He tabulates the same, and concludes therefrom that insects, notably the bees, decidedly and with intent confine their successive visits to the same species of flower. According to him, also, butterflies generally wander aimlessly in their flight: yet some species, including the Fritillaries, are fairly methodical in their habit. He believes that it is not by colour alone that insects are guided from one flower to another of the same species, and he suggests that the sense of smell may be brought into play. Bees, he avers, have but poor sight for long distances, but see well at short distances. Of 55 humble-bees watched, 26 visited blue flowers: of these 12 were methodic in their visits, 9 only irregularly so, and 5 not at all; 13 visited white flowers, whereof 5 were methodic and 8 the reverse; 11 visited yellow flowers, of which 5 were methodic and 6 not; 28 visited red flowers, 7 appearing methodic, 9 nearly so, while 12 were the contrary.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—In the last local examination 17 per cent. of those Juniors who sent up papers in Trigonometry obtained no marks, although some questions were of the very simplest nature. Among the Seniors Hydrostatics produced unsatisfactory answers. Many candidates had no ideas worth the name about pressure at a point, density, specific gravity, and weight. This is due partly to corresponding imperfections in some current text-books, and partly to the habit of teaching Hydrostatics apart from general physics or practical applications. The answers in Statics were the least satisfactory; yet according to the examiner there are few subjects in which good teaching tells more quickly than in elementary mechanics. Thus many who passed did very good papers.

Junior Chemistry obtained a favourable report; but the Seniors displayed lack of reasoning power, with great readiness to reproduce cut-and-dried statements from books. In Heat the results of the examination were encouraging and satisfactory. In Experimental Physics generally there was great lack of practical acquaintance with the subjects. Only one candidate did really well among the Seniors. In Botany the answers were weak throughout, showing great lack of teaching by real objects handled by the students. Zoology appears to have been too much studied by Juniors from older and worthless text-books. The Seniors did better, but spread themselves over too wide a field of work. The knowledge of Physical Geography was better than that of Geology, but neither was good.

The report recently made by Mr. R. D. Roberts of his visits to the centres where local lectures have been established, and on the present state of the local lectures' scheme, contains many most interesting facts regarding the high appreciation with which the intelligent working classes regard the lectures, and the difficulties which the cost of the lectures occasions. Of the results of a course of electricity at Newcastle, the examiner says that the work done in answer to a long and difficult paper of questions was fully equal to that attained in a scientific University course. The greatest difficulty that occurs is not lack of demand for or interest in education, but the provision of funds to meet the expenses. If a solution of this could be found, the scheme would be taken up largely in towns where it is now out of the question. The people who are eager for knowledge and travel long distances to obtain it, in all kinds of weather over the roughest roads, are just those who, if they must pay for the lectures, must have less bread for their families. This is certainly the case with the Northumberland and Durham miners. Whether the State will in some way assist in providing the knowledge and teaching which are so eagerly desired, must be again made a practical and urgent question.

The following are the lectures in Chemistry, Physics, and Mineralogy for the Easter Term (el. signifies elementary, ad. advanced):—

Elementary Course of Chemistry, by a Demonstrator; General Course, continued, Mr. Main, St. John's College; Non-metals, continued, and Organic Chemistry, el., Mr. Pattison Muir; General Principles, continued, and Organic Chemistry, ad., Mr. Muir, Caius College; Organic Chemistry, el., Mr. Scott (Prof. Dewar's assistant); Demonstrations in Gas Analysis, Mr. Scott; Sound, Lord Rayleigh; Heat, Mr. Trotter, Trinity College; Physics, el., Mr. Glazebrook, Trinity College; Physics, el., Mr. Shaw, Emmanuel College; Physics, ad., papers, Mr. Glazebrook and Mr. Shaw; Chemistry and Physics, el., papers, Mr. Pattison Muir and Mr. Shaw.

Practical Chemistry, University, St. John's, Caius, and Sidney College Laboratories.

Practical Physics, Cavendish Laboratory; Demonstrations in Light and Acoustics; and in Optics and Electricity, el.

Mineralogy, Course by Prof. Lewis, and Demonstrations for both parts of the Natural Sciences Tripos.

The following arrangements have been made by Prof. Hughes for lectures during the Easter term:—Local Stratigraphy, Prof. Hughes; Geology (General Course, continued), by Dr. R. D. Roberts, Clare College; Petrology, Mr. Harker, St. John's College; Palæontology, Mr. T. Roberts, St. John's College. Dr. R. D. Roberts will continue to set papers and superintend the course of reading of students in the Museum.

The Strickland Curatorship being about to become vacant, Mr. Salvin having completed his valuable catalogue, a new code of regulations for the Curatorship has been drawn up. The Strickland Curator is to be appointed by Mrs. Strickland, the foundress, during her lifetime; then by Mrs. Catherine Strickland in case she shall survive the foundress; and, after her decease, by the Superintendent of the Cambridge Museums of Zoology and Comparative Anatomy. In addition to caring for the Strickland Collection, the Curator is to take charge of any University ornithological collections, to catalogue them, to assist scientific visitors in studying the ornithological collections, and to aid and promote the progress of ornithological science.

University College, London.—Twenty lectures on Quantitative Analysis will be delivered by Richard T. Plimpton, Ph.D., on Mondays and Fridays at 3 o'clock, during the third term. The first lecture will be given on April 13.

Prof. Stokes, Lucasian Professor of Mathematics in the University of Cambridge, has been appointed to deliver the first course of lectures on Natural Science under the auspices of the Burnett Literary Fund, Aberdeen.

The Earl of Zetland has given 500*l.* to the Edinburgh Association for the University education of woman to found a bursary for the benefit of its students. This bursary will be known as the Earl of Zetland's Bursary.

SOCIETIES AND ACADEMIES

London

Royal Society, March 8.—"Note on the Order of Reversibility of the Lithium Lines," by Professors Liveing and Dewar.

In their communications on the reversal of the lines of metallic vapours, the authors have several times noticed (*Proc. Roy. Soc.* vol. xxviii. pp. 357, 369, 473) the reversal of the lithium lines, and concluded that the blue line is more easily reversed than the orange line. This, however, does not appear to be really the case. When much lithium is introduced into the arc, a second blue line is developed close to but slightly more refrangible than the well-known blue line. This second blue line produces with the other the appearance of a reversal, which deceived the authors until they became aware of the existence of the second line. The blue line (wave-length 4604) is really reversed without difficulty when sufficient lithium is present, but under these circumstances the orange line is also reversed. The latter line is also the one which first (of the two) shows reversal, and also the one which is more persistently reversed. Hence they place the lines in order of reversibility as follows: red, orange, blue, green, violet.

Mathematical Society, March 8.—Prof. Henrici, F.R.S., president (and subsequently Sir J. Cockle, F.R.S., vice-president), in the chair.—Mr. Alfred Lodge, Ferday Fellow of St. John's College, Oxford, was elected a Member, and Major Allan Cunningham and Mr. H. T. Gerrans were admitted into the Society.—Prof. Henrici feelingly announced, in a few well-chosen sentences, the loss the Society had sustained since its last meeting by the death of Prof. Henry Smith, one of its most distinguished ornaments, and who had been a Member almost from its commencement in 1865. The loss to mathematics in this country was almost irreparable, and it would be hard to find anywhere a fitting successor to him as an exponent of the higher geometry. It had been said that there were not half a dozen mathematicians in Europe who could breathe on the mathematical heights to which he was accustomed; it was further true that few were so fitted as he for introducing others to those heights. His charm of manner and power of fixing the attention of his hearers were wondrous, and were as strikingly exhibited at the December meeting of the Society (the last meeting at which he was present) as on any previous occasion. What Clifford once said when reading a paper by Hesse might be said with equal truth of Henry Smith's papers: "This is reading poetry." [Perhaps this Society will miss him more than any other; he was always willing, if possible, to respond to the Secretary's request for a paper, and he was a true imitator of the Jewish king, for he never gave us of that which cost him nothing. "Everything that he did was as perfect as he could make it." In a letter now before us the writer says truly: "Of all who 'knew' him, none knew or saw *him himself* as we did at the Mathematical Society." "Very pleasant" was he to us, and his death has left a void in our ranks which time will hardly fill.] —Mr. J. W. L. Glaisher made a communication on the calculation of the hyperbolic logarithm of π.—Mr. Tucker read (in its entirety) a paper by Prof. Cayley entitled "On Monge's 'Mémoire sur la Théorie des Déblais et de Remblais.'"—Mr. J. Hammond made a few critical remarks on a recent paper by Prof. Sylvester in the *American Journal of Mathematics*.

Zoological Society, March 6.—Osbert Salvin, F.R.S., vice-president, in the chair.—The Secretary exhibited, on behalf of the Rev. F. O. Morris, the drawing of a bird shot in Hampshire in November, 1882, which it was suggested represented a Tinamou of some species that had escaped from captivity.—Mr. J. E. Ady exhibited some microscopical preparations of bone, in one case showing the growth of blood-vessels into cartilage previous to ossification, and in another case presenting a hard section in which the lacunæ and canaliculi were extremely well shown.—Dr. Hans Gadow read a paper on the laryngeal muscles of birds, and pointed out first that the muscles of the syrinx are developed from the sterno-hyoid muscles; and,

secondly, that the cutaneous muscles are derived from superficial layers of the common muscular stratum. Thirdly, the author considered the connection between muscle and nerve-supply, illustrating his remarks by diagrams.—A communication was read from the Rev. H. S. Gorham, F.Z.S., containing the descriptions of some new species of Coleoptera belonging to the family Erotylidæ. Twenty-nine new species of this family were described, of which ten were from the Philippine Islands, three were from the Andaman Islands, two from Assam, two from the Malay district, six from Africa, and six from Peru. The species treated of belonged chiefly to the subfamilies *Encaustini* and *Dacnini*, the author reserving the remaining subfamilies for a future communication.—Dr. Gwyn Jeffreys read the sixth part of his communications on the Mollusca procured during the *Lightning* and *Porcupine* Expeditions. This included an account of the specimens of the groups of *Scissurella*, *Trochus*, *Turbo*, and part of *Littorina*, referable altogether to seventy species. Four genera and twenty species were for the first time described as new.—A communication was read from Mr. H. O. Forbes, F.Z.S., describing a species of scarlet *Myzomela* obtained in the Island of Boeroe, one of the Ceram group.—Mr. G. A. Boulenger read a paper on the Geckos of New Caledonia. The object of the author in preparing this paper was that it might serve as a guide to the identification of the Geckotidæ of New Caledonia, and at the same time bring the synonymy into order. To this end the author had compared the typical specimens in the Museums of Brest, Lisbon, Paris, and Brussels with those in the British Museum, and had given short descriptions of every species taken from typical or well-authenticated specimens. The number of species of Geckotidæ actually known from New Caledonia was fourteen: of these two were recorded for the first time, one being new to science.

Geological Society, February 21.—J. W. Hulke, F.R.S., president, in the chair.—Rev. John Birks, Capt. James Scott Black, John Bradford, Thomas Alexis Dash, Henry Lewis, and Thomas Morris were elected Fellows of the Society.—The following communications were read:—On the relation of the so-called "Northampton Sand" of North Oxfordshire to the Clypeus-Grit, by Edwin A. Walford, F.G.S.—Results of observations in 1882 on the positions of boulders relatively to the underlying and surrounding ground in North Wales and North-West Yorkshire; with remarks on the evidence they furnish of the recency of the close of the Glacial period, by D. Mackintosh, F.G.S. The author entered into a consideration of the time which has elapsed since the close of the Glacial period, and stated the main results of his observations as follows:—1. That the average *vertical extent* of the denudation of limestone rocks around boulders has not been more than six inches. 2. That the average *rate* of the denudation has not been less than one inch in a thousand years. 3. That a period of not more than six thousand years has elapsed since the boulders were left in their present positions by land-ice, floating-ice, or both.—Notes on the Corals and Bryozoans of the Wenlock Shales (Mr. Maw's washings), by G. R. Vine. Communicated by Prof. P. Martin Duncan, F.R.S.

Entomological Society, March 7.—Mr. J. W. Dunning, M.A., F.L.S., president, in the chair.—Three new members were elected.—Exhibitions: A specimen of *Polistes hebræus*, Fabr., an East Indian wasp, captured alive in one of the London docks, by Mr. R. McLachlan; Two British Ichneumons, and an orthopterous insect (*Copiophora cornuta*, De Geer) from Central America, by Mr. T. R. Billups; A preparation showing the structure of the thorax in a large beetle (*Chalcolepidius porcatus*, Linn.), by Dr. D. Sharp.—Paper read: "Further additions to Mr. Marshall's Catalogue of British *Ichneumonidæ*," by Mr. J. B. Bridgman.

Physical Society, March 10.—Prof. G. C. Foster in the chair.—New Member, Major W. S. Boileau.—Mr. Shellford Bidwell read a paper on a new method of measuring resistances with constant currents. It consists in placing a resistance-box in the arm of the bridge which afterwards has to contain the unknown resistance. A balance is effected by unplugging resistance in this box. The unknown resistance is then inserted in the same arm, and the balance restored by plugging resistance out of the box. The amount plugged out equals the unknown resistance.—Prof. F. Guthrie made a communication on liquid slabs. Films of liquid, spread out like a flattened drop on a solid surface, are found by the author to have a thickness which is a physical constant for the same liquid, provided the area is very great in proportion to the thickness. Sodium amalgam inserted in a mercury slab causes it to spread out further. Prof. Guthrie also finds that an electric current increases the diffusion of sodium amalgam through mercury in the direction of the current.—Mr. Baily suggested that, as the diffusion produces a current, an opposing current might be found to stop the diffusion. Mr. Stanley said the largest water-drop he had measured was one-fifth of an inch in diameter.

EDINBURGH

Mathematical Society, March 12.—Mr. J. S. Mackay, M.A., F.R.S.E., president, in the chair.—Prof. Chrystal, in his address on "Present Fields of Mathematical Research," remarked at the outset that the times seemed peculiarly suitable for the foundation of such a society in Scotland where, as in England and America, the tide of mathematical research had certainly begun to flow. The direct effect of the work of the society would be to keep alive the interest of its members in mathematics, and especially, by division of labour, to benefit the teacher whose daily tasks leave him somewhat unfitted to undertake in moments of leisure the reading necessary to keep him abreast of the time. Further, such benefits would surely extend their influence to the improvement of secondary education in Scotland. The lines along which members might advantageously work were then indicated in a suggestive sketch of the history in modern times of geometry and algebra. Descartes' system of analytical geometry was the first great step, though for long it remained simply a series of solutions of special problems. The discovery and development of the calculus no doubt kept analytical geometry for a time in the background; but there is every reason to believe that great progress in developing geometrical methods was effected by Pascal, Desargues, Newton, and Maclaurin. With them originated the idea of projection, which was systematised into a powerful geometrical method by Monge and his disciples, Poncelet, Chasles, Brianchon, and others. Monge also, however, established the analytical side of geometry, as well as the synthetic, upon an independent basis; his work has been ably supplemented by Dupin and others, and more especially by Plücker. The treatment by the latter geometer of the singularities of higher plane curves, his introduction of the abridged notation, and his invention of the system of line geometry, have been developed each into an extensive branch of mathematics. At the same time algebra has been differentiating itself into well-marked parts. The theories of forms, of equations, of substitutions, and of determinants have been greatly developed by Abel, Jacobi, Galois, Cayley, Sylvester, Jordan, Clifford, and others. The address concluded with a reference to the theory of transcendents and the closely-related properties of the complex variable. A hearty vote of thanks was accorded to Prof. Chrystal on the motion of Prof. Blyth of Glasgow, seconded by Mr. Muir of Glasgow.

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THURSDAY, MARCH 29, 1883

THE AMERICAN ASSOCIATION

Proceedings of the American Association for the Advancement of Science. Thirteenth Meeting, held at Cincinnati, Ohio, August, 1881. (Salem: Published by the Permanent Secretary, 1882.)

IN the same year that the British Association for the Advancement of Science was celebrating its jubilee, this, its eldest daughter, had reached the mature age of thirty years. The volume which embodies the results of the Cincinnati meeting is considerably smaller than the corresponding one published on this side of the Atlantic. The latter, containing the reports and proceedings of the York meeting, is a bulky and closely-printed volume of 824 pages, besides 82 pages of introductory matter and a list of members. The corresponding American volume only contains 416 pages, with about the same amount of introductory matter, in which the list of members is included. The ratio of the one to the other is even less than the above number indicates, for the type used in the American is on the whole larger than in the English volume, the smallness of which, in the Transactions, can only be justified by the necessity for restricting the bulk of the volume.

The American Association appears to be constituted very nearly on the same plan as the British, but there are some minor points of difference. The American consists of Members, Fellows, Patrons, and Honorary Fellows: of which the former two appear to correspond roughly with the Members and the General Committee of the British Association. A donation of one thousand dollars constitutes a Patron—only two persons, however, one of each sex, appear to have availed themselves of that avenue to distinction. The sections, or sub-sections, into which the Association divides itself for purposes of business at the time of meeting, are nine in number, as were those, including departments, at the York meeting of the British Association. But the distribution of the subjects differs. The American Association has a section for Physics separate from Mathematics and Astronomy, rendering permanent the fission which only occasionally takes place with us. Geology and Geography are placed in one section, which certainly would be found impracticable in Britain, as both these departments are in general well supplied with papers. Our Section D (Biology), with its three departments, Zoology and Botany, Anatomy and Physiology, and Anthropology, is divided in America into Biology, Histology and Microscopy, and Anthropology. The remaining sections correspond exactly. The two most noteworthy differences in the American volume are the general absence of the Presidential addresses, so marked and often so valuable a feature of the British, and the small number of Special Committees (and consequently of their Reports). Only in the sub-sections of Entomology and of Anthropology is there any record of addresses by the chairmen, and there does not appear to have been any general address by the President of the whole Association. The number of Special Committees also is smaller than we should have expected. Of these we find but eight, excluding those connected with executive business. They are "On Weights, Measures, and Coinage," "For Obtaining a New Survey of Niagara Falls," "On the Best Method of Science Teaching in the Public Schools," "On Standard Time," "On Stellar Magnitudes," "On State Geological Surveys," and two others, the purpose of which seems a little singular to English readers, one being "On the Registration of Deaths, Births, and Marriages" (the order of sequence seems a little curious), and a "Committee to cooperate with the American Philologica Association in relation to the proper restriction of the degree of Ph.D.," a subject which, we imagine, even if the necessity existed here, the British Association would be a little shy of touching.

It is of course difficult to express an opinion on the requirements of an American institution, but we cannot help thinking that as some of the most useful work of the British Association has been and is being done through its Committees, and that their reports (including those of individuals) are the most valuable part of its volume, the Transatlantic society would do well to develop this feature in its constitution. At the York meeting in 1881 thirty reports were read and forty-eight committees appointed.

There were a considerable number of mathematical and physical papers read before the American Association, but of the majority only the titles are printed. Of the few reported at any length, one, we should have thought, would have been more appropriate placed in the section of Mechanics, as the mathematical reasoning is of the simplest kind. The conclusion, however, is interesting, for it shows, as the result of a number of experiments, that "timber may be injured by a prolonged stress, far within that which leaves the material uninjured when the test is made in the usual way, and occupies a few minutes only." Bars of timber (most of the experiments were made with yellow pine) yielded and broke, generally suddenly, under loads below their average breaking weight under ordinary test. When the load was about three-fifths of the average breaking weight, it was sometimes a full year before they gave way. This suggests pleasant reflections for occupiers of newly-built "jerry" houses in London!

Among the physiological papers there is one on a subject which must, we think, be novel. It is entitled "A Study of Blood during a Long Fast" (by Lester Curtis, of Chicago). In May, 1881, a Mr. John Griscom, of Chicago, commenced a fast of forty-five days. The author was invited by the "managers" to make any investigations that he pleased, and after satisfying himself that the fast was to be conducted honestly, he chose the blood as a subject of study. The first examination made, at the commencement of the fast, shortly after the patient had eaten his last meal, showed the red corpuscles abundant, bright coloured, pure in appearance, regular and smooth in outline. Four days afterwards two kinds were noticed, one pale, almost colourless, large, with a "sticky" aspect, the other deeper in colour than the ordinary corpuscles, smaller and covered with nodules. By the fifth day the colourless corpuscles had disappeared, but they returned in a few days, and continued in greater or less amount to the end. The darker corpuscles assumed various shapes, and many very small ones appeared, apparently by subdivision of the larger. Their aspect was most abnormal on the thirty-ninth day of the fast, when Mr.

Griscom was extremely exhausted; but on the fortieth, after he had been refreshed by a rather long excursion on the lake, the corpuscles returned to a normal condition, except as regards size. This improvement was not lost during the remainder of the fast, though the abnormal appearance to some extent returned.

In the joint section of Geography and Geology are some interesting papers—one, the substance of an evening lecture, describes the Grand Cañon of the Colorado River, and shows that the denudation, of which it is a consequence, commenced in Middle Eocene, and has been continued to the present time, the greater part however having been accomplished by the end of the Miocene. During the whole period there has been a vertical uplift of from 16,000 to 19,000 feet, and a removal of a total thickness of rock equal to about 10,000 feet.

Another interesting paper connected with physiography is by Mr. J. W. Spencer, "Notes on the Origin of the Great Lakes of North America," together with one by Mr. W. Claypole, on "Evidence from the Drift of Ohio, Indiana, and Illinois, in support of the Preglacial Origin of the Basins of Lakes Erie and Ontario." The authors discuss the physiography and geology of the districts in which these lakes are situated, and show the most probable theory of their origin to be that they are fluviatile valleys of preglacial age, which during glacial times were obstructed by the accumulation of drift. This, aided by submergence owing to change of level, has produced the lakes in their present form. These papers are well worth the study of some English geologists, to whom no work seems too small or too great for a glacier, and whose faith at one time seemed quite equal to gulping down Lake Superior itself, sooner than falter in supporting a fascinating theory.

We would venture in conclusion to suggest to the American Association one improvement in detail: this is to imitate the British, and give their volume a cloth binding instead of sending it forth merely stitched in a paper cover, so loosely as to tumble to pieces after a few days' use.

T. G. BONNEY

PRINGSHEIM'S BOTANICAL YEAR-BOOKS

Jahrbücher für wissenschaftliche Botanik. Herausgegeben von Dr. N. Pringsheim. Vol. XII. Part 4, and Vol. XIII. Part 3. (Leipzig: W. Engelmann, 1881 and 1882.)

THE two parts now before us include six papers dealing with anatomical and physiological subjects, illustrated by 13 plates, some of them of great beauty. In the concluding part of vol. xii. there are papers by Westermaier, Ambronn, and Zimmermann; while in the third part of vol. xiii. the editor, Dr. Pringsheim, contributes a long controversial paper, and there are two papers—a long one by Godlewski, and a short one by Tschirch.

Westermaier's paper is on the "Intensity of Growth of the Apical Cell and of the Youngest Segments." From an examination of figures of Dictyota, Hypoglossum, Metzgeria, Salvinia, Equisetum, and Selaginella, as given by Naegeli, Goebel, Pringsheim, Cramer, Rees, and Pfeffer, Westermaier concludes that the maximum of the increase in volume in the apical region occurs in general either in the apical cell itself or in the youngest segments, and that taking the region which includes the apical cell itself and the four youngest segments, in none of the plants examined was the increase in volume of the apical cell found to be the minimum for the region. The results are represented graphically and afford very instructive curves.

A paper on the "Development and Mechanical Properties of Collenchyma, a Contribution to the Knowledge of the Mechanical System of Tissues," is contributed by Dr. Ambronn, and is illustrated by six plates of microscopical sections. The Collenchyma with the prosenchymatous fibres of the wood and bast form the mechanical system of Schwendener and Haberland. When the mechanical elements form separate plates, or bundles, or individual isolated cells, the cells are known as *Stereides*, and the whole tissue as *Stereome*. When on the other hand the mechanical cells are united with others which are non-mechanical, as in wood and bast bundles, then Schwendener has distinguished them as *Mestome*. The investigation of the structure of a number of plants shows that the Collenchyma may be arranged in bundles or in the form of a ring, and that in both the arrangement of Collenchyma and Mestome may follow a uniform plan, or the arrangement of the Collenchyma may be quite independent of the Mestome. Ambronn confirms the statement of Haberland that Collenchyma does not originate from any special morphological series of cells, but has the most variable origin: and further confirms the statement of Schwendener that the grouping and arranging of the cells depends entirely upon mechanical and not upon morphological causes. In *Fœniculum vulgare* the bast and Collenchyma of the external bundles are connected together and lie in the same radii, while in *Clematis vitalba* the bast and Collenchyma lie in the same radii but are not connected. In *Philodendron eximium* the Collenchyma forms a ring and is connected with the separate peripheral fibro-vascular bundles, both developing from a zone of secondary meristem. In *Peperomia latifolia* a ring of Collenchyma is formed, but it is independent of the bast. These plants afford examples of the four great types of structure. The Collenchyma cells are always prosenchymatous, often two millimetres in length, or even longer, and they frequently contain secondary partitions, being chambered by numerous fine transverse walls. They always contain fluid very rarely with any chlorophyll. The walls when viewed in a longitudinal section present elongated slit-like pores. Other collenchymatous cells are more parenchymatous in character, and originate by secondary collenchymatous thickening of parenchymatous cells. The wall always colours blue with Schultz's iodochloride of zinc, but is not coloured by the combined action of phoroglucin and hydrochloric acid, Wiesner's exceedingly delicate test for lignin, which is coloured a fine and intense rose-red by the reagents. Collenchyma swells up but little in water, contrary to the usual opinion, and only contracts about ½ per cent. when deprived of water. Collenchyma may originate from Cambium, from Meristem, or from Parenchyma, but the origin is found to be unimportant. The strength of collenchymatous cells is very little inferior to that of bast fibres, which have been shown by Schwendener to equal that of wrought iron wire.

The last paper is by Albrecht Zimmermann, "On the

Mechanism of the Scattering of Seeds and Fruits, with Special Reference to Torsion." The paper deals with the torsion in the awn of grasses, such as *Avena sterilis* and *Stipa pennata*, torsion of the legume of Orobus and Caragana, the curving and torsion of the awn or beak of the fruits of Geraniaceæ, and the scattering of the seeds of Oxalis, and is illustrated by three plates. The author points out the relation of the different phenomena observed to the mechanical cells in the part, as demonstrated by a microscopical examination of the different structures. Sometimes the cells of the part contract, at other times they swell up, and one or other or a combination of both these causes, gives rise to the effects noticed in the different plants under examination. Thus swelling of the cell-walls causes the remarkable ejection of the seeds of Oxalis. Unequal contraction of the mechanical cells causes the movements in the beaks of Geraniaceæ, and combined contraction and swelling in different layers of cells may be observed in the awns of Stipe and Avena.

In the second part under consideration, namely, vol. xiii. part 3, there is a paper by E. Godlewski, with the title "Contributions to the Knowledge of Vegetable Respiration." The details of a large number of experiments are given which were made, with an ingeniously contrived and simple apparatus, upon the respiration of germinating seeds with both fatty and starchy endosperm, and a smaller series of experiments made on the respiration of the flower buds of *Papaver somniferum* and on the ripening fruits with oily seeds of the same poppy and the castor oil plant. Some of the more important results as set forth by Godlewski himself may here be alluded to. During the early stage of germination in which the seeds swell up by imbibing water, the volume of CO_2 given off equals or is only a little less than the volume of oxygen taken up, both in fatty and in starchy seeds. When the swelling takes place under water or when air is excluded, *intramolecular* respiration takes place. When air is admitted the intramolecular respiration does not immediately cease, but is gradually replaced by normal respiration. As the rootlets of the seedlings are developed the volume of carbonic acid gas evolved gradually diminishes in proportion to the quantity of oxygen taken up, so that at the period of most active respiration only from 55–65 volumes of CO_2 are given off for every 100 volumes of O taken up. The formation of transitory starch during the germination of fatty seeds probably depends upon the action of atmospheric oxygen in each molecule of fat, converting it into CO_2, water, a certain quantity of an undetermined substance, and three molecules of starch. In the later stages of germination of fatty seeds the transitory starch is used as well as the fat, so that the difference between the volume of CO_2 given off and O taken up became gradually smaller, until at last the volumes are equal.

In the germination of starchy seeds the volume of CO_2 given off in all stages nearly equals that of O taken up, in peas sometimes a little more or a little less, but in wheat maintaining a seemingly constant relation of 1 to 1·05, the CO_2 being a little in excess of the O taken up.

In the buds of *Papaver somniferum* the CO_2 given off practically equals the O taken up (100·9 CO_2 for every 100 vols. O). In ripening fruits with oily seeds more CO_2 is given off than O absorbed; in *Papaver somniferum* 150 CO_2 for every 100 vols. of oxygen.

When oxygen is supplied under diminished pressure, respiration is variously influenced in different parts of the plant, but respiration is more affected in fatty than in starchy seeds. When the pressure of the oxygen is very slight *normal* respiration is reduced to a minimum, and *intramolecular* respiration commences. Intramolecular respiration is, under normal conditions, not a primary phenomenon as Pfeffer and Wortmann assert. *Normal* respiration consists in the immediate action of atmospheric oxygen upon the molecules of living protoplasm. Intramolecular respiration only begins when the normal respiration is rendered difficult by the want of atmospheric oxygen. Under ordinary conditions intramolecular respiration only begins when processes of reduction are going on in the plant as when fat is formed from carbohydrates.

The concluding paper in this part is "Contributions to the Anatomy and Mechanism of the Rolling up of the Leaves of certain Grasses," by Dr. A. Tschirch, with three plates. In this paper the author fully describes the mechanism by which such grasses as *Macrochloa tenacissima* (Esparto), *Lygeum spartum*, *Aristida pungens*, and others, which he groups as Steppe grasses, roll up their leaves in dry weather to protect the upper surface which bears the stomata, and prevent too great evaporation.

The first paper in the part and the longest is by the editor, Dr. N. Pringsheim, himself, "On the Function of Chlorophyll and the Action of Light in the Plant." This paper is a controversial one, issued in the form of an open letter to the Philosophical Faculty of the University of Würzburg. The first part of the paper includes a "personal defence," in which the statements contained in a paper by Dr. A. Hansen, with the title "History of Assimilation and the Function of Chlorophyll," published separately as a "Habilitationsschrift," and also reprinted in Sachs' "Arbeiten," vol. ii. p. 557, are minutely criticised. The second part of the paper is an historical discussion of the theory of assimilation, of the function of chlorophyll, and of the action of light on the plant. In this part Pringsheim does not seem to bring forward any new experiments, but gives a careful *résumé* of the whole subject under three heads. These are (1) Problem of the primary action of light on the cell; (2) the function of the colouring matter of chlorophyll in the exchange of gases in the plant; and (3) the function of the chlorophyll bodies and the primary product of assimilation of carbon. Into the merits of the controversy we cannot enter.

W. R. McNAB

OUR BOOK SHELF

Mexico To-day. By Thomas Unett Brocklehurst. (London: Murray, 1883.)

DURING a recent tour round the world Mr. Brocklehurst turned from the beaten track in the United States southwards to Mexico, where he spent seven profitable months in the capital and neighbourhood in the year 1881. Since the suspension of our diplomatic relations with that country in 1860, great difficulties have been felt in procuring accurate information regarding its internal rela-

tions. All the more welcome will be this pleasantly written volume, which gives a far brighter picture of the Republic and its prospects than its most sanguine sympathisers may have anticipated. Since the expulsion of the French in 1867, profound peace has prevailed both at home and abroad, interrupted only by a few feeble and aimless pronunciamientos in the years 1868 and 1869; signs of moral and material progress are everywhere perceptible; security for life and property is being extended from the capitals to the remotest districts of the several states; the whole country is already covered with a network of railways connected in the north with the United States system, and affording several alternative routes between the Atlantic and Pacific Oceans; lastly, the Liberal party, which has guided the destinies of the Republic for over twenty years, has succeeded in establishing free institutions on a firm basis. "I have every confidence," writes our author, "that the favourable terms in which I have spoken of the country will not hereafter be found to be exaggerations; that my ideas as to the future prosperity of Mexico being early realised are true, and that such ideas are held by most of its leading men." And he adds that the time has come for England to bring about "a reconciliation with a country, in whose aid her influence and power could be so beneficially exerted" (p. 259).

The contents of this work, which is sumptuously illustrated by no fewer than fifty-six coloured and other plates from sketches by the author, are extremely varied, special chapters being devoted to the present state of the capital and surrounding districts, to the public institutions, the Roman Church, Protestant missions, trade, manufactures, farm life, the Pachuca silver mines, antiquities, the ruins of Teotihuacan, the remarkable limestone caves of Cacahuampila, Popocatapetl, and many other topics of general interest. During the ascent of Popocatapetl, the traveller ascertained that, according to the latest survey, the edge of the crater was 19,000 feet above sea-level, the usual estimates being 17,850 to 17,880, and that the peak still rose 1000 feet higher. Should these calculations be confirmed, Popocatapetl will again take its place as the culminating point of North America, a position from which it had recently been deposed by Mount St. Elias on the Alaska coast.[1] On the same occasion another curious discovery was made. General Uchoa, present owner of the crater and its rich sulphur deposits, told our author that the eruption of 1521, as described by Diego Ordaz, one of Cortes's captains, must have been due to some misapprehension. All geologists who have lately visited the crater, or who have examined specimens of its minerals, are now convinced that no eruption can have taken place for the last 10,000 years. This is a great confirmation of the opinion now generally entertained that the underground energies diminish steadily in vigour as we proceed from the Southern Cordilleras, through Central America, northwards to the Anahuac tableland. The financial condition of Mexico is described, contrary to the current impressions, as far from hopeless.

Of the numerous illustrations a large number are occupied with curious little clay heads, obsidian knives, stone pestles, arrowheads, and other objects found amongst the debris of the Teotihuacan ruins and elsewhere. There are also excellent reproductions of the famous Aztec Calendar and sacrificial stones, of a beautiful vase from Teotihuacan, of Teoyamiqui, the goddess of death, and of an exquisite vase of Centeotl, or the Mexican Ceres, a perfect gem of Aztec art. Many of the objects brought home by the traveller have been placed in the hands of Mr. Franks of the British Museum, and are no doubt ultimately destined to enrich the Christy collection.

A. H. K.

[1] On the British Admiralty charts this mountain is marked 14,800 feet, but by the late United States Survey it was raised to 19,500 feet.

LETTERS TO THE EDITOR

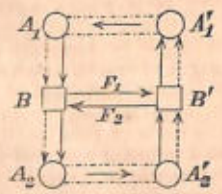
[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

The Matter of Space[1]

II.

In the aggregations of points which form ponderable bodies, other means exist of suppressing the effects of the points' attractions for each other than the simple counteracting forces of the above figure. Clausius's equation of stationary motion in fact informs us that this will take place when there is no exertion of tractive moment, or no total instantaneous sum of motor-couple actions in the system. This simply appears to imply that the pair of orbs A_1 A_2 and the pair A_1' A_2' are in that case no longer independent of each other in their transference and counter transference of motor energy, but that the twofold action of such energy is then a self-neutralising one; or in other words that the energy given off at A_2 passes on to A_2'; and that discharged at A_1' is taken up by A_1; so that in the case where B B' are in stationary motion, or combine to form a "sphere" of two gravitating points, or again where many such points collected together form a permanent ponderable body, orb-couples intervene between the otherwise free extremities, $A_1'A_1$ and A_2A_2' of the two ether systems (in the directions shown by arrows in the figure), and bind them together conservatively by an endless circuit of motor energy through the ether-orbs, while a similar endless flow of ordinary momentum through the ponderable channels of the system in the meantime constitute also the usually recognised internal, gometrical, or



"lost" forces of such a permanent aggregation, "sphere" or "body" of ponderable matter.

This subjection of two or more baric points B B' &c. to the condition of stationary motion, as the bond of neutrality of "lost" geometrical or "internal" forces between them differs therefore from the case before supposed of absolute suppression of all interforce between them in this, that when (in the latter case) the motions of the points B B' &c. are absolutely free and entirely exempted from disturbing force action, the motor-vigours of the couplets A_1A_2, $A_1'A_2'$ &c. of the ether-orbs in permanently bound binary attendance upon the baric points B B', &c., respectively, will then also be equally exempt and free from disturbing actions of any other orb-couples upon them, than those only by which they are dually and counter-equally bound to each other through the channels of their respective baric centres B B', &c.

Throughout the whole of a baric point B or B_1's state of undisturbed rest or motion, the ether-couplet attached to it is constantly transmitting from one of its ether-orbs to the other a ceaseless flow of undirected energy, or it is etherially exercising a ceaseless undirected horse-power, whose supply of energy is drawn from and is returned again without mal-destination to the universal ether's general stock of energy, if the meaning of the principle of conservation of energy in this case may be said to be that, for the entire sum of all its parts, the universal ether's whole stock of energy never undergoes any alteration. We may next consider also the case where the interforces between baric points are not entirely absent, but may present us with a resultant algebraic sum of any number of interforces, instead of with a neutral sum only of two equal and opposite ones. Although in that case there is no counter-equality between the motor-couples which act on the ether cortège A_1 A_2 &c. of each baric point B or B' &c. yet if the motions of these latter points are subjected to no condition of stationariness under the influence of the forces acting on them we may yet recognise the universal ether's whole

[1] Continued from p. 460.

stock of energy as being the source and destination of all the flows of undirected energy exerted by the collective cortége's couples, if we assume for the whole of the ether together the same obedience to the law of conservation of energy as before, because, for each one of the interforces between B and some other baric point B′, and therefore in the sum of all such mutual forces and points paired with B to produce them, the orb-couples at A_1 A_1' which yield the force F_1 are exactly counter-equivalent to each other, and so will abstract from such a general stock of energy just as much at one of their points of action A_1 or A_1' as they restore to the same general stock of their energy requirements at the other one.

The presumption here used that the undirected energy funded and effunded by the motor-couples acting on the ether-retinues of the reacting baric points, belongs to an invariable stock of that description of energy residing in the universal ether as a whole, and that it is not extracted from and rendered up to any other imaginable sources, or in other words the theory of the conservation of motor energy in the universal ether by all the motor orb-couples together which are in action in it, acquires an important meaning, when we recur again to the nature, as above explained, of the condition to which these orb-couples are subjected when they act upon the ether-retinue of a collection of baric points which compose a "sphere" or body, or which are together in stationary motion. The description already given of this case informs us that when the state subsists, the simple sum of all the motor actions or tractive couples on the body's retinue of aërilians [1] is either nil at all times instantaneously, or, when it is a periodic sum, its average value for a time-period or recurring time-cycle of its changes is so. No instantaneous resultant can be formed at all, if the sum's value is perceptibly periodic, and it is not in our power to say whether ether orbs of aërilian points originally differentiated from weighty material points (or whether those points themselves) yield sums which have periodic or instantaneous resultants; it belongs to a strict examination of the subject to pronounce and illustrate the rules by which stationary or periodic resultant sums can, in combinations of orbs or aërilian parts, afford by proper means either periodic or stationary resultant actions on a collective aërilian assemblage. The mode of combination of such actions on subordinate parts into a resultant action of one or both kinds on a united group which they compose is certainly not a hopeless problem, when its character is once regarded as the essential problem of etherial mechanics. What free or unbound ether may exist besides the ether enrolled in the retinues of ponderable matter, and what actions these free and enrolled portions of the ether may have upon each other, and separately or together upon the originally sundered multitudes of matter composing the ponderable parts of gravitating bodies, so as (with time as another element of the reactions) to explain the gradual process of condensation which appears to be a perfectly regulated progressive principle of material economy, are all questions which, by a closer discussion of the surmises here explained and indicated, may without doubt be certainly expected to follow from their careful consideration, in due course. But as the phenomenon of stationary motion is shown by Clausius's equation (which states its condition) to be at least a rigidly true absence of average total tractive moment in a system which presents it, when the average is a time-average taken over a sufficiently long fixed or over a proper repeatedly recurring term or movable interval of time, and since, to senses incapable of discriminating exceedingly minute quantities, this time-average becomes an instantaneous quantity when the time-term for which it is reckoned diminishes without limit, a conclusion may be readily drawn from this which will fairly justify us in accepting the presumption used above, that the instantaneous effects of individual motor-couple actions are conserved in the universal ether as a whole. For we are unable to discriminate *what* periodic variations the sum of these effects may or may not have in their total value for the universal ether; and we have therefore exactly the same grounds for regarding the instantaneous effects of all motor-couples as being instantaneously conserved by the occult fluctuating terms or periods of the universal ether, as we have for viewing them as instantaneously conserved (so as to give a sensibly stationary zero resultant sum) throughout the parts of a ponderable body's mass in which we cannot detect any periodic motion, or any perceptible vibration.

[1] Those entering into the baric body's actual composition may be left out of the enumeration, since this body's baric motions being themselves (all taken together) stationary, they satisfy the equation of condition identically; and in general instantaneously, unless a common periodic motion is given to the baric points.

A bent bow, when its string is released, a soap-bubble or an air-gun's charge, when they give way and burst, or a bubble of hydrogen and chlorine gas mixture when an active light-ray strikes it, ignition of a train of gunpowder by hot iron, or of fire-damp in a safety lamp, of gases and gold leaf by the electric spark, are instances, if we could penetrate the process, of suddenly infringing by a forced vibration the gradually attained subsidence of all perceptible periodicity in a system's inner motions, with instant disintegration for its consequence of the stationariness of the motor-actions of its parts. A little universe in effigy has collapsed, leaving to the universal ether the task of saving and storing up, by means of individual free motor-couples, the vibrations let loose, and of so modelling into something else the scattered fragments.

But on the other hand the resisted jet, as well as the shutting of a water-pipe or steam-boiler valve, the swing of a hammer as well as its stroke on a rock or bell, directed radiations of all kinds as well as their radiometer-like interceptions, the steadily resisted flow as well as the breaking or making of an electric current, conduction between bodies of a steady flow of heat, sound, and all perceptible horse-power exercises of motor-vigour's effective, or unreversible operations, can only be conducted (as the ether does conduct such effective works there conservatively) by the individual periodic actions of unbalanced motor-couples acting on some free-coursing ether-orbs or orb-clouds forming equally free-coursing heavy bodies' retinues. These all rely directly on the universal ether's store of motor energy to maintain in their isolated severance (and in that of the free-coursing bodies also) from other works' and matters' motions a constant conservation of their unreversible motor-activities' effects.

This view of the ether's function as a whole to conserve the individual effects, both of primordial and of resultant motor-couples on ether aërilians and orbs and clouds, whether those couples' intensities are stationary, or fluctuate and vary in any periodical or unperiodical manner, is the second maxim above noted, to be kept in view along with that of description of couples' intensities as a time-rate of a certain kind of energy, in discussing the properties, or the etherial mechanics, of motor-couples' balanced and unbalanced actions. The maxim, as thus laid down, also cautions us against confusing the kind of *instantaneous* energy effects of motor-couples, which the ether conserves as a whole, with any periodically *term-averaged* semi-mean square of a collection of particles' rhythmically fluctuating velocities, or with any temporary or enduring "peræval sum," as it may be called, of the collection's total undirected energy, since the instantaneous undirected sum thus obtained, is not really instantaneous as long as the length of the term or period over which the average is taken is a perceptible and measurable one. The resultant quantity whose effects the ether conserves at every instant, on any individual aërilian assemblage, by a total sum of counter-equivalent quantities acting on other aërilian assemblages or aërilians is the sum (treating every aërilian as of the same inertia $m = 1$)

$$\Sigma \frac{d}{dt}\left(r \frac{dr}{dt} \right) = \Sigma \left(Rr \right) + \Sigma \left(v^2 \right);$$

where, for a single aërilian, r is a vanishingly small distance between at least two parts of which it must consist, and for groups of aërilians the sum also includes, under the general symbol r, the distance between centres of every pair of aërilians possessing, relatively to the assemblage's centre of inertia (just as the aërilian parts do relatively to their aërilian centres), counter equal accelerations, R, $-R$. We are not at liberty in applying the equation to include in its sum any other distances and accelerations, nor any other velocities v, of the aërilians' parts and centres than these barocentric ones, relative to centres of inertia included in the given system, because as an equation of couples having no truth or meaning, except in virtue of its composition of pairs of quantities (so furnished by pairs of inert points contained in the system as to be independent in its sum of the origin of reference used in its formation), all distances, velocities, and accelerations of the given system's centre of inertia cannot form part of the equation conformably to its physical use and applications, but must form part of physical actions in some other system, of which the given assemblage and its centre of inertia forms one individual member.

It is this necessary view of the above equation of stationary motion drawn from such views as those here offered of its physical interpretation, which obliges us to regard the simplest aërilian point of the ether as consisting of at least two parts; and this assumption agrees with the dual view of the ether's nature taken

by Professor O. Lodge in his address on the "Functions of the Ether" (NATURE, vol. xxvii. p. 328), while this system also explains the kind of conservation which has been noticed independently by Dr. G. Lippmann and Professor S. P. Thompson[1] as characterising the phenomena of electricity, and the close resemblance which not only exists between the processes of conduction of heat and of electricity, but also, as noticed by the former writer, between the laws of electrical potential and the thermal principle of Carnot's law.

It seems scarcely probable that so many converging views can be all fallacious, and ingenuity may without doubt be spent with profit and advantage in further attempts to adapt and reconcile some comprehensive theory of the ether's properties, of a mechanical description, to embrace in a common review all the multiplicity of remarkably analogous laws, which physics in its various branches at present offers to our contemplations.

If the description which precedes of a connected outline of such a system of synoptic views has extended to a much greater length than I was originally prepared to offer as comments on Mr. C. Morris' communication, it is because the logical developments which I have repeatedly found them to admit of induced me to try to establish them on a satisfactory foundation. In many trials, moreover, of their applications, I have met with such frequent proofs of the validity of some such general principles as those here indicated, that the results to which they have easily conducted me in numerous directions, are in general so accordant with those which Mr. Morris has skilfully reviewed, that, save in the small divergence between us, upon which I have dilated, in the main principles adopted for explaining them, Mr. Charles Morris's and my views of the properties and laws of motion, of the distribution of the "Matter of Space," and of the mechanical behaviours of "motor-vigour," are for the most part only *variæ lectiones* of each other. A. S. HERSCHEL

Newcastle-on-Tyne, February 10

P.S.—It will perhaps serve to correct some misconceptions which, although they were not intended to be produced, may yet not impossibly have arisen from the form of defective reasoning, which at some few points of this letter's descriptions it has been unavoidably necessary to introduce, to notice in recapitulation that it formed at the outset no part of its main object to define and represent exactly the extremely complicated part which (at least in combinations of its periods) it is evident that time discharges in determining the operative efficacies and strengths of motor-couples, in exerting "vigour" of undirected motion. With a well-grounded geometrical substructure, there need be no occasion to entertain a doubt that the first principles at least of time's use in definitions of the actions and effects of wrests or motor couples will be easily identified and laid down with all the precision and accuracy needed for purposes of their mathematical applications.

The principal object, however, here aimed at and sought to be attained has not been to offer such an exact description of time's relations of form and economy to the different states of action and repose of undirected motion (which do not actually admit of successful discussion without much more abstract elementary conceptions than those ordinarily recognised of geometrical principles), but simply as a preliminary step towards this question's future surer treatment, to point out clearly and plainly the distinctive and peculiar character of undirected motion's space-relations.

This kind of motion, it has been endeavoured to explain, consists in change of magnitude of a certain ratio-index, ϕ, of tractional configuration between collocated points. In the capacity of a ratio-index ϕ very closely simulates, in its algebraical and geometrical properties, all the analogous properties of angles. But it differs from them in this important particular, that it possesses no directional qualification. For a crank-arm's description of angle at once assigns the plane of the crank's revolution, and this plane has direction; but extension of a connecting-rod is a ratio which affects the rod's length equally in all positions, without giving rise in so doing to any new direction.

On the other hand, neither motion in angle nor in traction-index can by their unaided to-and-fro presence in a crank or connecting-rod give backward and forward motion to a piston or piston-rod, but only by virtue of certain constraints involving the properties in one case of trigonometrical, and in the other case of hyperbolic, sines and cosines. It is probably because changes of angle are, like the changes of the angle's sine which result from them, directed quantities, that the relations of angle-variations to changes of coordinate lines by means of trigonometrical ratios is a familiarly applied and well-established theory. But no such theory having the same scope and extent of application connecting changes of coordinate lines with variations of the ratio-index ϕ, by means of hyperbolic sines and cosines, and showing what necessary conditions directed geometrical quantities (including angle) *must satisfy* to make a fixed law of hyperbolic connection with the undirected quantity ϕ have any possible existence, has yet been brought into general notice and acceptance.

But that the directed and undirected geometrical quantities do satisfy and fulfil such a condition, and that the fixed law of connection between them does actually exist, there is sufficient evidence to assure us in the consistency with which such reasonings as those which Mr. Morris has produced, and which I have tried to base on that geometrical assumption, represent correctly a very large array of physical phenomena. Forming therefore, as the undirected quantity ϕ does, a position-scale in space, of whose possession of certain distinctive and special geometrical and physical properties no theoretical employment has yet been made, and no sufficient proof of satisfactory evidence has, in fact, heretofore been produced, no excuse, it is opined, for hyperbole or figurative use of speech need be presented, for describing the new quantity, as it was termed in a former part of this letter, as a new position-scale of interspheral, ethereal, or aërilian motions foreign to and independent of our ordinary graphic field of space.

[1] NATURE, vol. xxiv. p. 140; and pp. 78, 164.

Mr. Stevenson's Observations on the Increase of the Velocity of the Wind with the Altitude

I AM sorry if I took Mr. Stevenson too literally when I understood his remark, "great heights above sea-level," to mean *absolutely* great heights; but I certainly think the phrase is extremely liable to such interpretation, and as no superior limit was assigned, I naturally inferred that the author deemed the formula $\frac{v}{V} = \frac{h}{H}$ applicable to such heights as those considered in my paper.

On his own showing, however, this formula succeeds no better than mine on Arthur's Seat; while mine possesses the unquestionable advantage of approximating to the truth throughout the higher levels, where all Mr. Stevenson's formulæ fail. According to Vettin's data. If the data furnished from Arthur's Seat correctly represent the conditions in a free atmosphere up to the same level, which I very much doubt; we must infer that the velocities increase in a more rapid ratio with the heights than that given by the formula $\frac{v}{V} = \left(\frac{h}{H}\right)^{\frac{1}{2}}$, which is preferred by Mr. Stevenson, but in not so rapid a one as that given by $\frac{v}{V} = \frac{h}{H}$; and in fact, if we make the index $\frac{3}{4}$ instead of $\frac{1}{2}$, we get a formula which gives far better results, as far as the Arthur's Seat observations are concerned, than that preferred by Mr. Stevenson. The agreement is so close for nearly all the velocities, that I give below a comparison of the results afforded by both formulæ:—

Velocity recorded at high elevation, 775 feet above sea-level.	Velocities computed for lower station		Velocity recorded at lower station, 550 feet above sea-level.
	By Mr. Stevenson's formula $\frac{v}{V} = \left(\frac{h}{H}\right)^{\frac{1}{2}}$	By the formula $\frac{v}{V} = \left(\frac{h}{H}\right)^{\frac{3}{4}}$	
885	703 [1]	704	720
1,698	1,430	1,351	1,364
2,620	2,206	2,084	2,133
3,416	2,876	2,718	2,718
4,328	3,646	3,443	3,465
5,575	4,697	4,435	4,592
6,763	5,698	5,381	5,040
8,035	6,765	6,393	6,782
9,368	7,893	7,453	7,862
10,820	9,115	8,609	8,765
12,410	10,455	9,874	9 789
13,700	11,542	10,900	10,639
15,058	12,687	11,980	11,680
Sums	79,713	76,325	76,149
Means	6,132	5,871	5,857

[1] This value is wrong as given by Mr. Stevenson. It should be 745

From the above table it is seen that in ten cases out of thirteen, the formula I have proposed gives results clo er than Mr. Stevenson's, while the means differ by a quite insignificant amount.

If then, as seems probable on all grounds, the higher we ascend, the slower the increase of the velocities with the heights, Mr. Stevenson's formula, $\frac{v}{V}=\left(\frac{h}{H}\right)^{\frac{1}{2}}$, should hold for a level somewhat higher than 775 feet, and not below it. Above that, again, a formula, $\frac{v}{V}=\left(\frac{h}{H}\right)^{\frac{1}{3}}$ should apply; and finally, the formula, $\frac{v}{V}=\left(\frac{h}{H}\right)^{\frac{1}{4}}$, recommended in my paper.

I cannot believe, however, that the formula $\frac{v}{V}=\left(\frac{h}{H}\right)^{\frac{1}{2}}$ holds up to such a comparatively large height as this inference would postulate, since it gives such an excessive value at 1600 feet with Vettin's data (more than twice that observed), and I can only conclude, therefore, until experiments in a free atmosphere corroborate Mr. Stevenson's data from Arthur's Seat, that these latter do not correctly represent the actual rate of increase in the velocity between such levels in the atmosphere, away from the disturbing influences of mountains and valleys.

In any case, however, I must enter a distinct protest against having my name prefixed to the pressure formula $\frac{f}{F}=\sqrt[4]{\frac{h}{H}}$. If Mr. Stevenson carefully examines my paper, he will nowhere find the remotest allusion to such a formula. The formula for the velocity which I there recommended for the higher levels, was in fact shown to be directly deducible from Mr. Stevenson's first formula for the pressure, viz. $\frac{f}{F}=\sqrt{\frac{h}{H}}$, to which it is exactly equivalent on the ordinary assumption that $\frac{f}{F}=\frac{v^2}{V^2}$.

Moreover, the paradoxical result which Mr. Stevenson arrived at in violation of this assumption, viz. that the same formula was practically applicable both to force and velocity, is controverted by the conclusion entertained in his letter, that the formula $\frac{v}{V}=\sqrt{\frac{h}{H}}$ agrees best with the recorded results of velocity, and the formula $\frac{f}{F}=\frac{h}{H}$ with those of pressure.

While these two formulæ can hardly be called the *same*, it is somewhat striking to find that on the assumption force varies as (velocity)^2, which is supposed to be annulled by the diminished density as we ascend, they are *identical*.

Finally, Mr. Stevenson has evidently quite misunderstood my allusion to sea-level. When I spoke of sea-level, I simply meant the approximate equivalent to the level of the sea *on land*, as at Berlin for example, where Vettin's observations were made. When Mr. Stevenson therefore maintains that the velocity of the wind at 100 feet above sea-level over land, is probably not so great as that near the surface over the sea, he entirely misses the point of the argument, which lies in the *relatively* excessive velocity of the wind at 100 feet above, to that near the surface, *over land which lies approximately at sea-level*.

The very fact mentioned by Mr. Stevenson regarding the greater friction encountered by air in passing over land than over water, as well as the results of his experiments, point to a considerable increase in the velocity from the surface to an elevation of 100 feet above it. For the very same reason, I should expect to find a more moderate increase up to the same height over water.

E. Douglas Archibald

On the Formation of Mudballs

The letter from Mr. Hart in Nature, vol. xxvii. p. 483, on the natural formation of snowballs, has recalled to my memory the similar formation of balls of mud.

About eight miles south of Bromley in Kent the soil is clayey, and after rain the country lanes are apt to be very muddy. Some five or six years ago there was a very violent storm of rain, whether or not accompanied with melting of snow I cannot now remember. The steep lanes were in many places regularly scoured with water, and it looked afterwards as though the whole surface had in places been a sheet of water, for the soil was quite washed off and the flints were left bare. After this storm my brother and I noticed in the lanes a considerable number of mudballs, usually almost perfectly spherical, but in some cases with a tendency to a cylindrical shape. They varied in size from small pellets up to four or five inches in diameter. On seeing the first one or two, they looked to us like the handiwork of some boy with an enthusiasm for mud pies, but the number of them, and the fact that they were always found on the slopes of hills proved them to be a natural formation. They were formed throughout of soft clayey mud, and I do not remember finding any nucleus in the middle when we cut them open. We concluded that they were formed by accretion to pellets of mud washed down the hillside and rolling as they went. I have only once since seen a similar ball, and that was in a furrow in a ploughed field in the same country; it is possible that this ball may have been made inside an agricultural roller, although there were no marks on the field of recent rolling and there had been heavy rain. The comparative rarity of the appearance of these balls seems to show that they can only be formed with some precise degree of stickiness of the mud. Closely similar are the marvellously spherical balls of matted vegetable fibre to be found on the seabeach in some places. Sir Anthony Musgrave informed me that on the beach in Australia, I think near Adelaide, he had seen tens of thousands of such balls, all perfectly spherical. It seems rather obscure why the fibres should begin to mat together in such a form as to be rolled by the surf, but the perfection in shape is clearly due to incessant rolling. It is probable that, with a flat bath and some cocoanut fibre or oakum, the process of formation might be watched, but I have never tried the experiment. It is very common to see after rain matted lines of such objects as pine-leaves or the flowers of lime-trees, but I have never seen any apparent tendency to rolling, and such lines are left lying flat after the water has drained off.

G. H. Darwin

Cambridge, March 23

Snow Rollers

The phenomenon described in Nature, vol. xxvii. p. 483, under the title of "Natural Snowballs," is known to British meteorologists under that of snow rollers, and as the latter agrees more closely with the phenomenon, I venture to plead for its adoption.

I believe that the first person who carefully examined their formation was that excellent and venerable observer, the Rev. Dr. Clouston of Sandwick Manse, Orkney, and I am under the impression that he published a description of their formation in an early number of the *Philosophical Magazine*. He has observed them on the lawn at Sandwick more than once, and has always noticed the hollowness at the ends; in fact, he described them to me as resembling ladies' white muffs.

I remember only one instance of their being reported in England, namely in the following letter from the late Admiral Sir F. W. Grey, which appeared in the *Meteorological Magazine* for May, 1876.

G. J. Symons

62, Camden Square, N.W.

Sir,—The snowstorm of Thursday night (April 13, 1876) was marked by one circumstance which I have never witnessed before, though it may not be uncommon. It was this:—

On Friday morning I observed that for a considerable distance, and following a regular line, the lawn, to leeward of the house, was strewed with masses of snow like boulders, varying from the size of a snowball to a cubic foot at least, and as the snow melted, a track either straight or curved led up to the large ones, following, apparently, the direction of the wind. I had observed before dusk that the eddies of the wind and the swirls of the snow were very marked, and I have since heard from a friend who observed the same thing, that he saw the snow rolled along by the wind, and forming masses such as I have described.

As I have said, I know not whether this has been observed in other cases, and perhaps it may interest you to have this account of it.—Yours faithfully,

F. Wm. Grey

Lynwood, Sunningdale, Staines, April 16

Incubation of the Ostrich

It seems strange that there should have existed an uncertainty in the mind of an ornithologist as to the mode of incubation of the ostrich in confinement at the Cape of Good Hope. The habits of the birds are of course as familiar to the ostrich-farmers

as those of barndoor fowls to ourselves. I have stayed at a farm at Cape Point, where a pair of the birds were nesting within fifty yards of the house, in a small paddock, and have seen the hen on the nest.

An interesting subject of inquiry, however, seems to me to be still open in the matter. It is, How far do the habits of nidification of the ostrich vary in the different climates through which it ranges? The nest of the ostrich is commonly described as a heap of sand, and so no doubt it is in warm desert regions; but the nest which I saw at the Cape was carefully built of grass and other warm materials, so as to aid in retaining heat. The birds kept the nest almost constantly covered between them.

In warmer regions, however, the hen appears often to leave the nest in the daytime, and it is just possible that where the temperature is very high the hen may not incubate at all, and the cock alone may do so at night. I merely wish to point out that it should not be assumed that the habits of the ostrich as to incubation are necessarily the same in the various climates of Africa with those to be observed in the Cape region.

I have noticed that at the Zoological Gardens the ostriches at the breeding season are supplied every year with a cartload of silver sand as the traditional nest. It would not be amiss to try them with some more substantial materials as an experiment, and prove whether in our climate they would not build a warm nest as at the Cape.

That birds' eggs can be hatched like those of turtles in mere sand is undoubtedly a fact. The Megapodius inhabiting Cape York, Australia, makes, as is well known, a huge mound of vegetable matter, which by decomposition supplies the necessary warmth to hatch the eggs; but at the Philippine Islands another Megapodius buries its eggs in the perfectly clean calcareous sand near the seashore.

The habits of the emu in nesting have been carefully watched at Blenheim. The head keeper told me not long ago that the cock alone incubates. The hens lay their eggs anywhere about in the grass, the cock builds a nest, and rolls the eggs to it, the hen sometimes endeavouring to prevent him and to break them. I believe an account of observations on the habits of the emu at Blenheim were published by Mr. Frank Buckland.

H. N. MOSELEY

Bonchurch Hotel, Isle of Wight, March 26

Holothurians

MY experience of about three months in Bermuda and Jamaica fully bears out Mr. Guppy and Mr. Kent's view that the Holothurians do not feed on living coral. They were moderately common in both localities close to the shore, where corals are comparatively scarce, and are mainly of the massive kinds, such as the Astræas, against which the tentacles of a Holothurian would be useless. There were a few branching Oculinas here and there, but not enough to support the Holothurians. But further, some of the latter bury their bodies in the mud or sand, leaving only the tentacles exposed; and I have watched these thrusting their tentacles into their stomachs right up to the base in the comical way described by Mr. Kent. It is quite clear that these were not feeding on living coral. I did not, however, see them actually taking up sand and shell and thrusting it down, as Mr. Kent did; in fact I was puzzled as to what they were feeding on. From the way the tentacles were set, standing nearly erect, I fancied they were catching swimming creatures, as other tentacled animals do. This idea is supported, though not proved, by a fine specimen from the Zoological Station at Naples, which has a half-swallowed fish protruding from its mouth. The specimen is in the Bristol Museum. It proves at all events that they do not reject this kind of food. Possibly in default of it they may fall back upon sand and shell, and the minute organisms contained in these. Some of my experiences with these creatures were interesting. At Bermuda two large kinds used to lie quite exposed in shallow water. I might have guessed from this that they would probably be protected in some way. I used to wade along shore carrying a fishing-basket and a landing-net, and one day, as my basket was full, I put a couple into the landing-net to carry home. As their skins were quite hard, I thought they would travel well so. After handling them, I found my hands smarted a little, and the irritation lasted till bedtime. I found that little bits of their skin had got under mine, and this caused the irritation. As I was going home, I found my Holothurians were literally melting away; long streamers of a colourless gelatinous substance were hanging down between the meshes. Of course I threw the nasty things away, and had a dreadful job to get the net clean. I attributed my misfortune to the sun, so another day I packed a couple up comfortably at the bottom of my basket, which is very closely made. After an hour or two I was horrified to find long streamers hanging down from the basket of the same horrible substance. They had literally gone to pieces again, and spoilt everything in the basket. Shortly after, I left for Jamaica, and there I took out a wide-mouthed bottle and brought one home in triumph. Being engaged that evening, I left the Holothurian in the bottle all night. Next morning the creature was all there, but he had cleared out the whole of his inside; his intestinal canal and the beautiful tree-like organ were perfect. The latter was still alive and was waving about in the water in the prettiest way, and looking remarkably like branchiæ. Some accessory organs along the intestinal canal were exhibiting rhythmical pulsations. Altogether it was a most interesting sight. But my poor Holothurian was only a tube. I did not know at the time that he could grow a complete new inside.

Clifton College J. G. GRENFELL

The British Circumpolar Expedition

SUPPLEMENTARY to the very interesting notice in NATURE (p. 484) of the above expedition, permit me to give a brief extract from a letter recently received from Capt. Dawson, as follows:—"I have heard of a large cavern about a day from this (Fort Rae), which I shall try and explore. There are some eyeless fish that live there, that I hope may turn out to be a new species." I do trust Capt. Dawson may be able to carry out his intention, but he must be heavily weighted with work, in which he appears to take a deep interest. I had long ago been told of this cave and its fish, but had no time to visit it, never having been within one or two hundred miles of the place.

March 24 J. RAE

Meteor

MR. MASHEDER'S account in your last number of NATURE (p. 483) of the meteor seen by him at Ashby-de-la-Zouch on March 17, corresponds in some particulars with the inclosed note of one seen by myself on the same evening at Malvern. I am therefore inclined to send it you.

The discrepancies are in the time, which Mr. Masheder states to have been 7.5, while here the meteor passed at 6.56 p.m.; also in his description of "pieces dropping," I noticed no such appearance, but simply the not unusual one of rapidly recurring scintillations in the train.

Great Malvern, March 17, 6.56 p.m.

This evening a bright flame-coloured meteor with a short scintillating train, nucleus the brightness of Jupiter, passed rapidly across the sky. When first seen it was beneath the moon, then shining brightly, and was apparently about the altitude of Betelgeux, at that time nearly 10° past the meridian. It disappeared behind the hills almost due west, but so quickly that it was difficult to determine its course with any exactitude.

Lambert House, Great Malvern, March 25 E. BROWN

Mimicry

SUCH remarkable instances of mimicry as that described by the Duke of Argyll in NATURE, vol. xxvii. p. 125, as occurring in a moth, make heavy demands upon the faith of the non-scientific reasoner, since it seems to him impossible that organic Nature in her "blind groping in the dark" could, under any imaginable combination of circumstances, have succeeded in taking the successive steps requisite to bring her to such a state of perfect adaptation to condition. But the proverbially keen sight of birds, *as at present organised*, is apt to lead to erroneous inferences with regard to the evolution of protective mimicry in their insect prey, seeing that the high development of this faculty now attained by them renders nugatory any disguise that is not almost perfect. The theory of natural selection, however, requires the gradual perfecting of this, no less than of other structural and physiological acquirements; and there is no reason for supposing that the Ornithoscelidan ancestors of the feathered tribes possessed exceptional visual powers, but rather that the reverse was the case; so that it may be concluded that improvement in vision and in rapidity of flight proceeded *pari passu*. This being granted, the initiatory steps of mimicry in

the *Lepidoptera* may have been tentative, and well within the competence of ordinary variability.

The above sufficiently trite train of thought has been suggested to me by the consideration of analogous facts known to every angler. Many fishes greedily snap at anything that glistens or is highly coloured, especially if it be rapidly drawn through the water, and the slight additional disguise imparted to artificial bait of this description by a spinning motion renders it very attractive. The highly specialised salmon is easily deceived, and the most killing artificial flies for this fish make no pretence to resemble anything in nature, and are attractive in proportion to their gaudiness. The same is true of his congener the trout, although this fish appears to be somewhat more æsthetic in his tastes; and the most useful artificial flies employed to entice him are mere generalised imitations of his natural food. Indeed, on these grounds no less than on those of anatomy, it cannot be doubted that the *Teleostei*—albeit highly specialised of their kind—have failed to develop that acuteness of vision which their rapid movements would seem to render desirable, and are yet in the stage in which a very imperfect mimicry misleads them; and it is not an unreasonable presumption that birds were once in a very similar condition, from which they have emerged in consequence of the necessity for frequent and abundant supplies of food entailed upon them by their active mode of life. Under these circumstances it must have gone hard with the helpless caterpillar, so toothsome and nutritious, seeing that he could not, like the mature *Phryganidæ* and *Ephemeridæ*, keep out of harm's way by shunning the element inhabited by his natural foe; and hence arose the necessity for his protective modification. How urgent was the need for this is amply shown by the fact that several distinct modes of protection have been enlisted in his defence, viz. cuticular hypertrophy resulting in hairiness, mimicry of the vegetation on which he feeds and lives, and unpalatable flavour; to which has been superadded mimicry of the unpalatable forms by those of good flavour. But even with all this adventitious aid the struggle would probably have proved exterminating to him by reason of the voracity of birds, had not the teeming imago participated in the protective modifications, and thereby been enabled to maintain the balance of supply and demand necessary for the survival of the order.

Wycombe Court, Bucks PAUL HENRY STOKOE

Threatened Extinction of the Elephant

The threatened extinction of any existing species of plant or animal cannot fail to be matter of real concern to all students of science, who ought to neglect no feasible means for preventing so deplorable an occurrence.

Of the few gigantic mammals still living on the surface of our planet, none possesses more interest and none are more worthy preservation than the elephant. Yet it is an accepted conclusion that the elephant is doomed to extinction, and that within a measurable period of time this majestic quadruped will have suffered the fate of the Dodo. Cannot such a calamity be prevented? Surely the destruction of elephants might be legally controlled (in India, at any rate), and their capture (for domestication) might be limited, as it is well known they never breed in confinement. The continuous rise in the market-price of ivory, and its recent unprecedented scarcity as an article of commerce, are ominous signs, and renders it incumbent on the votaries of science to consider what may be done in the matter. It is no question of mere sentiment—it is of vital importance; and if "ancient monuments, ruins, &c.," are worth protecting, it cannot be denied that so remarkable and interesting a creature as our colossal Pachyderm merits some effort in his behalf.

EDWARD E. PRINCE

United College, University of St. Andrews, March 15

A Curious Case of Ignition

ONE fine morning recently, as two ladies were standing together in the drawing-room of a house in this neighbourhood, smoke was observed to rise from the dress of one of them. This was found to be due to ignition by the solar rays focused on her dress by the lens of a graphoscope which stood on the table. Similar cases of accidental concentration of the sun's rays have, I am aware, been recorded. It would be interesting to know whether any serious fires have thus originated. One can easily imagine circumstances which would favour such results from a simple cause. M.

Finchley, March 26

SINGING, SPEAKING, AND STAMMERING[1]

I.—SINGING

THE voice, essentially a musical instrument, has only of late been scientifically considered. Even now singing is too much dealt with as an art, and its acquirement as an accomplishment. The professional mystery with which it is surrounded serves no good purpose, and favours empiricism. At ladies' schools the old fiction of what are quaintly termed "finishing lessons" still survives; they often succeed in finishing any prospects the pupil may have had of becoming a singer. Most of the current primers and tutors are ludicrously vague and feeble, many methods are absolutely injurious to the voice; for the improvement of which one ingenious inventor has suggested the use of a false palate, and another the fitting of singers' mouths with a sort of bell-shaped snout or proboscis to act as a resonator. A chorus of such proboscidians on the Handel orchestra would be an appalling sight. The real foundation of our knowledge rests on the researches of Helmholtz on the physical, and of Garcia on the physiological, side. The classical discoveries of the former as to the production of vowel-sounds by the superaddition of a varying harmonic in the mouth-cavity, and of the latter by the examination of the larynx in action by means of a mirror, brought before the Royal Society in May, 1855, have formed the substratum of much which has now become the common property of scientific men. Dr. Bristowe, in his Lumleian lectures of 1879, has added some pathological data of considerable value, and Dr. Walshe, in his "Dramatic Singing, Physiologically Estimated," has touched on points connected with the sympathetic and emotional power which this most perfect of instruments can be made to exercise. It owes this in a great measure to the fact that it can combine musical sounds with significant words, and thus interest at once the ear and the intelligence. After a demonstration of the action of the larynx and fauces in phonation, illustrated by some excellent photographs taken from his own larynx by Mr. Emil Behnke, and thrown on the screen, vowel sounds were shown to be thirteen in number in the English language, with six more in French and German, fifteen of these being oral in origin, and four, all French sounds, nasal. Consonants were about sixteen in number, and had been called "noises" by Max Müller, owing to their comparatively unmusical character. They are chiefly caused by some check or obstruction to the laryngeal note. A diagram of Madame Seiler's was, however, shown which indicates that there is an oral resonance-note even for consonants, though it is much more obscure and uncertain than that of the vowels. Melville Bell's division of vocal sounds into vowels, consonants, and glides or semivowels was adverted to, and his ingenious device of visible speech briefly explained, but left for fuller consideration in the second lecture. The contrast was then pointed out between singing, in which the musical notes predominate and are separate or discrete; intoning, which is speech intentionally rendered monotonous for better transmission in large spaces like cathedrals; recitative, which is the converse of the former, being singing partially loosened from the trammels of time, rhythm, and melody, so as to approximate to speaking; speech itself, which uses continuous inflection; declaiming, which is speech with the addition of a histrionic and emotional element; reading, which is a faint and as it were distant reproduction of speaking in a lower key, quieter and less marked in accent than in speaking *viva voce*; and whispering, which is purely oral, without a laryngeal ground note, and which may be termed voiceless speech.

The different qualities, compass, and register of voices

[1] Abstract by the Author of three Lectures at the Royal Institution, by W. H. Stone, M.D.F.R.C.P.

were then described. The larynx of the child, like its head, is large relatively to the rest of the body. At the age of fourteen or fifteen, rather earlier in girls than in boys, the vocal apparatus enlarges and strengthens. In boys the vocal chords about double in length; in girls they increase from five to seven. In the latter case the pitch of the voice is not materially altered; in the male it usually descends an octave or more.

Garcia adopted the division of Registers into three, namely, the chest, falsetto, and head voice, due originally to Müller. This remains the most practical mode of classification, though the word falsetto is misleading, being liable to confusion with the artificial male voice bearing the same name, and may well be replaced by the phrase Medium. The term register has been enveloped in much professional mystery, and has been far too much refined upon. There has also been a confusion of octaves, from which even Madame Seiler is not free, due mainly to the modern and objectionable method of scoring music for the tenor voice in the soprano clef, and an octave too high. Register evidently marks an alteration of mechanism in the voice-reed and resonator to enable it to obtain the very remarkable compass, amounting to nearly five octaves, of which the human voice is possessed. Single voices run to three octaves or more. Catalani had $3\frac{1}{2}$; Bastardella, heard by Mozart in 1770, had the same. Madame Carlotta Patti can reach G♯ in alt. Bennati, a tenor, had three full octaves, and Tamberlik reached the C♯ of 544 double vibrations.

The words Head and Chest obviously only represent subjective sensations which accompany the shifted mechanism. In many parts of the voice similar notes can be reached in two registers, but with different force and quality, on either side of the break.

In using chest-voice the vibration can be seen to involve the whole vocal chord and the arytœnoid cartilages. At about A in the male and C in the female the chords act alone, though the first mechanism can by an effort be continued. The second form of vibration takes the voice up to F, the usual limit of bass voices and of the chest register. Above the F the chords are stated to lengthen, giving by a second elongation the second series of the chest register, which forms the bulk of the tenor compass, the remainder being formed by a variable number of falsetto notes. These seem to be produced by a thinning of the edges of the chords. Czermak lighted the larynx of thin persons strongly from the outside, and found that sufficient light was transmitted to show a decided increase of transparency in the chords at this point. All observers agree in placing this change, both in males and females, between F and F♯. In this region, common to both males and females, an amusing experiment can be made by causing a tenor male and a contralto female singer to execute the same passage behind a screen, or in an adjoining room. It is difficult, and at times impossible, to discover the sex of the singer from the quality of the tone. There still remains among male voices the curious and only partially explained counter-tenor. Sometimes by arrest of development or by accident the boy's compass is retained in after-life. This accident may be quite independent of masculinity, as those who have heard lusty, rubicund Yorkshiremen, with their wives and children round them, trolling out a sweet treble in glees on the terraces of the Crystal Palace after the Handel Festival, can testify. But besides this rare accident, most basses and baritones can cultivate an artificial and peculiar voice which most properly bears the name falsetto. Its production appears to be in great measure a matter of education. It was seemingly commoner in the madrigalian epoch and in the time of Queen Elizabeth than it is now. Dr. Bristowe says truly that the mechanism of its production is still doubtful, though many attempts have been made to determtne it. Such voices are not only artificial, but complex and uneven, being a compound of high chest notes and others of special quality. There is a serious break between the two both in production and in quality, which practised singers disguise by running the one into the other at different places, according as the passage to be sung ascends or descends.

It will have been seen that female voices overlap the compass of the male voice by an octave or more. Many contraltos take the E on the bass stave, which is well in the middle of the bass voice, and a low note for a tenor singer. Hence we sometimes hear of female tenors, though the effect is usually more peculiar than pleasing. Our great English contralto, Madame Patey, however, drops to this note with fine effect in Handel's oratorio of Solomon, which was written for the exceptional and now fortunately obsolete voice of Farinelli.

In females the break is somewhat higher than in males, but the transition to the falsetto takes place at the same note G. The contralto does not use the head-register.

This, otherwise called the Small, begins as just stated. Its upper limit varies, the extremes having been already given. Mozart wrote the fine air *Gli Angui d'Inferno* in the "Magic Flute" for such an exceptional voice reaching to F in alt. A commoner and perhaps pleasanter limit is the C below this.

All authorities agree in describing a curious appearance of the glottis in singing these notes. This is a folding together of its posterior half with vigorous vibration of its anterior part. Such an appearance can only be produced either by some stopping of the chords at the middle by contact with structures lower down, or by overlapping from vigorous approximation of the arytœnoid cartilages. The former supposition lacks anatomical confirmation, and the latter, which is anatomically possible, has the implied, though not the expressed sanction, of Helmholtz. The drawing of this appearance is given by Madame Seiler, who alone of laryngoscopists, possesses a register peculiar to the female.

Dr. Stone was materially assisted in his first lecture not only by Mr. Behnke, but also by his colleague Dr. Felix Semon, who gave admirable demonstrations of the healthy larynx, as seen in Mr. Williams, and some other pupils of St. Thomas's Hospital.

ACCLIMATISATION OF EDIBLE MOLLUSKS

A RECENT and interesting notice by Mr. F. P. Marrat of Liverpool, who is an excellent conchologist, mentions the introduction into the Cheshire coast of what he calls the "wampum clam," or *Venus mercenaria* of Linné; and he concludes that there is "a fair prospect of the naturalisation, on the extensive shallow shores of Lancashire and Cheshire, of an extremely nutritious and highly esteemed food-product, new to Great Britain." The late Prof. Gould says that this mollusk is known in Massachusetts under the name of "Quahog," given to it by the Indians. According to him and other American writers on the subject, the true "clam" *par excellence* is *Mya arenaria* of Linné. I was present as a guest at one of the fashionable "clam-feasts"; but the muddy flavour derived from the habitat of that mollusk does not agreeably commend itself to my palatable recollection. However, *chacun à son goût!* *Mya arenaria* inhabits the western coasts of the North Pacific as well as both sides of the North Atlantic.

The American oyster (*Ostrea virginica* of Gmelin = *O. borealis* and *O. canadensis* of Lamarck) is peculiar to North America, and has now found its way into the London market. It differs from the common European oyster (*O. edulis*, L.), and is equally variable as regards size. *O. virginica* has been within the last few years introduced into the mouth of the Tagus, and is called the Portuguese oyster. Our own or "native" oyster was

highly esteemed by the Romans, as we know from Juvenal; but there are no grounds for imagining that it was in those times imported into Rome from Britain. The facility of transport was not then so great as it is at present; and a gamy flavour was probably not so much relished by the Romans as it is said to have been by our King George the First, who preferred oysters a week old at Hanover to those which he afterwards got in England.

Within the last few years the "periwinkle" (*Littorina litorea*, L.), which is a favourite delicacy of our poorer classes, has spread with unusual rapidity along the eastern shores of the North American continent. Mr. Arthur F. Gray, in *Science News* for April, 1879, attributed its origin to Europe. It certainly does not seem to have been observed in America by Gould or any other conchologist before 1870.

Preeminent among land shells, as a dainty article of food in France, is *Helix pomatia*, L. We are more fastidious or more conservative in our gastronomic notions. It is a mistake to suppose that the Romans, when they possessed and inhabited Great Britain, brought this snail with them to indulge their luxurious tastes. In all probability it was not even known to them, because another species (*H. lucorum*, Müller) takes its place in Central Italy. *H. pomatia* has not been found at Wroxeter or York, or in any other part of England or Wales where the Romans built cities or had important military stations. Among the debris of an extensive Roman villa discovered in Northamptonshire, in which the shells of cockles, oysters, mussels, and whelks abounded, not one of *H. pomatia* occurred, although at Woodford, a few miles distant, that species is plentiful in a living state. J. GWYN JEFFREYS

THE ALFIANELLO METEORITE

SIGNOR DENZA, Director-General of the Italian Meteorological Association, sends us an account of the remarkable aerolite which fell in the province of Brescia on February 16, and to which we referred last week. On that date, at 2.43 p.m. local time, a strong detonation was heard in many places of the province of Brescia and even in the neighbouring provinces of Cremona, Verona, Mantua, Placenza, and Parma. The detonation was quite awful in the commune of Alfianello, in the district of Verolanuova, Brescia. This was found to be caused by a meteorite which exploded a few hundred yards above Alfianello. A peasant saw it fall in the direction of N.E. to S.W., or, more exactly, N.N.E. to S.S.W., at a distance of about 150 yards. When the meteoric mass fell to the earth, it produced on the ground, in consequence of the transmission of the shock, a movement similar to that of an earthquake, which was felt in the surrounding districts; the telegraph wires oscillated and window frames shook. Before the meteorite fell a confused commotion was seen in the sky, and immediately after a prolonged noise was heard similar to that of a tram moving rapidly along the rails. No light was seen; but the meteor must have been accompanied, as usual, by a light vapour, produced by the volatilisation of the substance melted at the surface; for some of those who saw it fall compared it to a chimney falling from the sky, and surmounted by a wreath of smoke. The meteorite fell in a field about 300 yards south-west of Alfianello. It penetrated the ground obliquely, nearly in the same direction as it was seen moving in the air, from east to west, sinking to the depth of about a yard, deducting the height of the meteoric mass. The peasants who saw it fall and who were the first to touch it, found it somewhat hot. The meteorite fell entire, but unfortunately was soon broken to pieces and carried away by the crowd who gathered to see the strange sight. The form was ovoid, but a little flattened at the centre; the under part was broad and convex, presenting the form of a cauldron; the upper part was truncated. The surface was covered with the usual blackish crust, and studded with small concavities, partly separate, partly grouped together.

As to the dimensions and weight of the aerolite, the estimates differ. According to the evidence of some, its height was about 75 centimetres, greatest breadth 60 centimetres, and its volume about 25 cubic decimetres. Its weight has been variously estimated at 50, 100, 200, and 250 kilograms. Its real weight was probably not much under 200 kilograms. It is certain that Prof. Bombicci carried more than 25 kilograms to Bologna, to add to the rich collection of meteorites in the Mineralogical Museum of the University; that a specimen weighing 13½ kilograms was taken possession of by MM. Ferrari, the owners of the field in which the meteorite fell; that about 40 kilograms remained in possession of other persons; that the municipality of Alfianello sent a specimen of 5 kilograms to the Athenæum of Brescia; and that two pieces weighing 12 kilograms each were thrown into a stream and lost; without speaking of a considerable quantity of small fragments, distributed here and there, of which Signor Denza possesses four, of a total weight of 39 grammes.

By its structure the Alfianello meteorite belongs, according to Prof. Bombicci, to the sporasiderite-oligosiderite group, being almost identical with the New Concord (Ohio) meteorite. The substance is finely granulated, of an ashy grey; the bright glossy surface has elements showing varied gradations of colour. Metallic particles abound; they are found scattered like small nuclei, in which are iron and perhaps one of its alloys, in brilliant crystalline aggregations, of a yellowish or bronze white. Circles of rust of a yellowish brown rapidly form around the particles of iron. In the places where there are no metalliferous nuclei, the grains of iron are attached to the stony portion in the proportion of 68 per 1000 of weight. The blackish crust is rough, and to some extent rugged in some parts of the surface, and rather smooth and uniform in others; in general it is somewhat lustrous. The total specific weight of the stone is from 3·47 to 3·50. The chemical analysis of the meteorite is being made in two different laboratories at Bologna. Signor Denza's information has been obtained from Prof. Bombicci of Bologna University, and from Professors Briosi, Ragazzoni, and Casali of Brescia.

THE SHAPES OF LEAVES[1]

IV.—*Special Types in Special Environments*

FROM the previous papers it will be clear that degree of subordination to the stem accounts in large measure for the extent to which leaves vary from the primitive ovate-lanceolate type. Where they are still so most subordinated, there will be a strong tendency towards the long pointed ribbon-like form, and also a marked inclination towards decurrence. This combination of peculiarities is well seen in several thistles, and in comfrey, as also to a less extent in many epilobes and stellarias. Compare *Verbascum thapsus*, and other mulleins. From these extreme cases, in which leaf and stem are not fully differentiated from one another, one can trace several gradations, through square stems with sessile leaves (as in certain St. John's worts) up to merely sessile stem-leaves, or leaves that clasp the stem with pointed or rounded auricles. Wherever lines exist along the stem, they may be observed in pairs up to the point where a leaf is given off, and they are undoubtedly surviving marks of the primitive unity of stem and leaf. The same may be said of rows of hairs, like those of *Stellaria media* and of *Veronica chamædrys*. There can be little doubt

[1] Concluded from p. 495.

that special selective causes (protection against creeping insects, &c.) have often come into play in preserving or modifying such decurrent wings, stem-lines, auricles, clasping stipules, and rows of hairs ; but as a whole they nevertheless point back distinctly to the origin of dicotyledonous stems from superposition of leaves and midribs upon one another. They are rudimentary forms of stem-lamina.

Sessile leaves are particularly apt to be lanceolate. They approach nearest among dicotyledons to the monocotyledonous type. The botanist will readily fill in examples for himself.



Fig. 34.—White Deadnettle (*Lamium album*).

On the other hand, it is clear that the conditions under which leaves assume the orbicular and peltate types can only occur where there is least subordination to a central stem. And these conditions must have occurred for immense numbers of generations in order to overcome the ancestral tendency towards the lanceolate or ovate form. For a leaf must first pass through a cordate or reniform stage, like that of the coltsfoots, before it can reach an orbicular shape, like that of our common waterlily ; and even when it becomes completely circular, like the *Victoria regia*, it may still retain a mark of junction where the

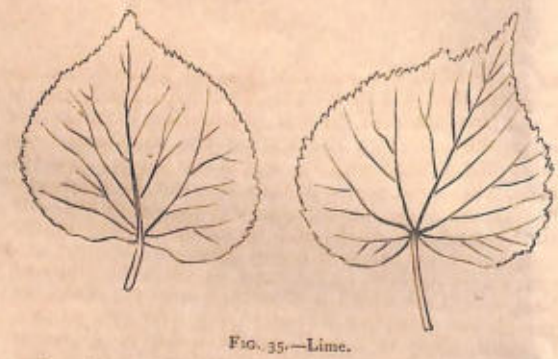


Fig. 35.—Lime.

overlapping edges have met without becoming connate. In the case of *Victoria regia* the transformation has been traced during germination. The first leaves produced by the young plant are linear and submerged ; the next are sagittate and hastate ; the later ones become rounded, cordate, and orbicular ; and even when they assume the peltate form, the line still marks the point of union. This sufficiently accounts for the rarity of perfectly peltate leaves, such as those of *Tropæolum*, *Hydrocotyle*, and *Podophyllum*. Radical leaves growing on long footstalks will be oftenest orbicular cordate ; stem-leaves on the same plant may pass from ovate-cordate to ovate, lanceolate, and linear. Large cordate radical leaves will be most frequently produced from perennials with richly-stored rootstocks. The sagittate and pointed leaves of *Arum* and *Sagittaria* show the furthest step attained in the same direction by monocotyledonous foliage, starting from the liliaceous form.

Where the stem, or, what comes practically to the same thing, solitary ascending branches, rise high into the air, especially with opposite leaves, we get a common type which may be well represented by the white deadnettle



Figs. 36 and 37.—Begonias.

(Fig. 34). Hedgerow plants with perennial stocks frequently assume this type. It reappears almost identically, under the very same conditions, in so distant a group as the true nettles ; and though it is possible that the causes which produce mimicry in the animal world may here have come somewhat into play, so as to modify sundry *Lamiums* into the similitude of the protected *Urtica*, yet the analogy of other Labiates shows that the circumstances alone have much to do with producing the resemblance. For a great many tall-stemmed hedgerow Labiates closely



Fig. 38.—Cow-parsnip.

approximate to the same type : for example, *Lamium galeobdolon*, *Ballota nigra*, *Galeopsis tetrahit*, *Stachys silvatica*, and *S. palustris*. Compare, *mutatis mutandis* for ancestral peculiarities, the other hedgerow plants, *Scrophularia nodosa* and *Alliaria officinalis*. On the other hand, notice the orbicular long-stalked lower leaves of the latter (especially when biennial) side by side with the lower leaves of some Labiates, such as *Nepeta glechoma*. Indeed, the Labiates as a whole present an excellent study of local modification in an ancestral type,

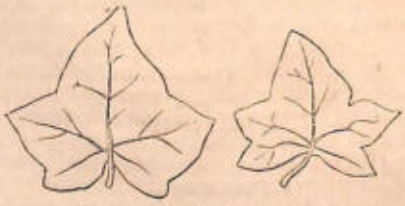


Fig. 39.—Creeping leaves of ivy.

according to habit and habitat. Take as other genera of this family the following : first, *Mentha* and *Lycopus* ; then, *Salvia pratensis*, *Prunella*, *Marrubium*, radical leaves of *Ajuga reptans*, and lower leaves of *Nepeta glechoma* ; finally, the typical form dwarfed in little prostrate retrograde types, such as *Thymus serpyllum* and *Mentha pulegium*. Compare these last with other prostrate or dwarfed types elsewhere, like *Veronica serpyllifolia*, *Peplis portula*, *Hypericum humifusum*, *Montia fontana*, and *Arenaria serpyllifolia*.

As grassy types, the best familiar examples are those

of the flaxes, *Stellaria graminea*, Toadflax, Bastard Toadflax, &c.; all of which have been largely influenced by monocotyledonous competition. Even a pea, *Lathyrus nissolia*, has got rid under such circumstances of its leaflets, and has flattened its petiole into a grass-like blade. Intermediate forms occur in Southern Europe. The peas, indeed, are papilionaceous plants which have largely cast off their ancestral leaf-type, in order to avail themselves of new conditions. *L. aphaca* has lost its leaflets, and flattened and enlarged its stipules so as to resemble simple opposite leaves; and *L. hirsutus* and *pratensis* have reduced the leaflets to one long almost



Fig. 40.—Ascending leaves of ivy.

linear pair. Marshy plants have also often been forced into adopting grass-like forms. The great spearwort is a swampy buttercup, whose ancestral leaf has been lengthened out into a long ribbon, with almost parallel ribs; the lesser spearwort shows the same tendency to a less degree, still retaining ovate lower leaves, with lanceolate upper ones; and *Veronica scutellata* is a similar marshy case among the Scrophularineæ.

When the tree-like form is attained, or free access to air is otherwise gained (as by climbers), the supply of carbon, being practically unlimited, becomes relatively little important, and the supply of sunlight assumes the



Fig. 41.—Sundew.

first place in the economy of the plant. Under such conditions the great object must be to prevent the leaves from overshadowing one another. Now this result may be obtained in a great number of ways, and we must not expect that every tree or shrub will solve the problem for itself in exactly the same fashion. It is enough that the shape into which the ancestral form is finally modified should sufficiently answer the purpose in view. As a matter of fact, the suitability of the actual forms and arrangements of tree-leaves to the functions they have to perform can be readily tested by observing any tree in bright sunshine. On the one hand, almost every leaf is in full illumination, no leaf unnecessarily shading its neighbour; and on the other hand, there is hardly any interspace between the leaves, as may be seen by the fact that the shadow thrown by the tree as a whole is almost perfectly continuous. In short, there is no waste of chlorophyll, and there is no waste of sunshine.

Mr. Herbert Spencer has called attention to the results of varying exposure to light in the various parts of the same leaves, which often causes them to become unequally developed. In the lime (Fig. 35) such obliquity is normal. In the various *Begonias* (Figs. 36 and 37) the resulting asymmetry is very noticeable. In the cow-parsnip (Fig. 38) it is the leaflets of the same leaf which are asymmetrically developed, so as not to overshadow one another. In more symmetrical leaves, there is an equal provision for preventing overshadowing, only here it takes the form of indentation of the edge, as in the oak, or of subdivision into leaflets, as in the horse-chestnut. In the latter case, indeed, the two outermost leaflets are habitually asymmetrical. On the whole, however, the mass of forest trees in temperate climates have almost entire leaves; and full exposure to sunlight is secured rather by their special specific arrangement at the end of the minor branches. Most often they are more or less ovate, as in the elm, beech, alder, birch, and poplar. Where the

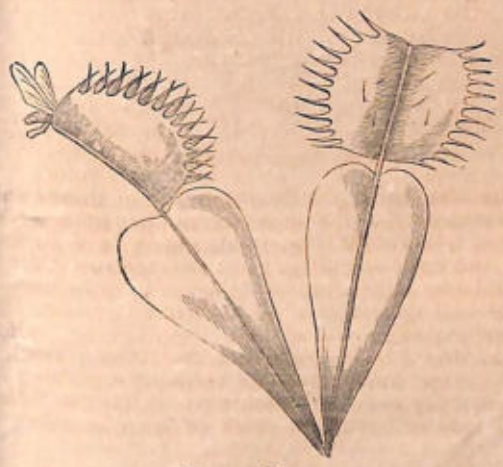


Fig. 42.—*Dionæa*.

leaves are divided, the separate leaflets assume the appearance of almost entire leaves: compare the leaflet of the horse chestnut with the leaf of the true chestnut; the leaflet of the ash with the leaf of the hornbeam; the leaflet of the walnut with the leaf of the beech; and the leaflet of the mountain ash with the leaf of the blackthorn. In all these cases, almost identical results are practically produced in the end by similar circumstances acting upon wholly unlike original types.

Some minor typical forms exist in certain groups of climbers, which are worth a moment's notice. Take as an example the creeping leaves of ivy. As long as this plant grows close to a wall or the trunk of a tree it assumes the well-known shape shown in Fig. 39. But as soon as it branches out its flowering sprays into the open, acquiring a tree-like habit, which it often does on the top of a wall, it takes a simpler and totally different form of leaf, as shown in Fig. 40, growing on the same plant. This last type is quite comparable to that of the pomegranate. That both types admirably suit their particular situation can easily be seen by noting how well they fit in with one another without overshadowing. It would be difficult to point out the geometrical grounds for this relation, but the relation itself becomes obvious on watching an ivy-plant in broad sunshine. Moreover, the first or truly ivy-like form of leaf tends to recur among

many plants which similarly press close to a flat surface. In *Veronica hederæfolia* we get it in a weed that climbs over banks of earth; in *Linaria cymbalaria* we get it in a trailer hanging upon stone walls; in *Campanula hederacea* and *Ranunculus hederaceus* we get it in a creeper along the edge of rills or over soft mud. Compare in each case other forms of the typical generic leaf, as seen in germander speedwell, toadflax, harebell, and meadow buttercup.

Another special climbing type, proper to more open habits of twining round alien stems, is that of the common bindweed. This, the ordinary convolvulus form, reappears exactly in so distant a plant as *Polygonum convolvulus*, whose habits are exactly similar. Even among monocotyledons we get it closely simulated by *Smilax*, with precisely like conditions, and somewhat less closely by *Tamus*. Indeed, this form of leaf may be said to be almost universal among lithe twining creepers.

The hop type belongs rather to mantling than to mere twining climbers. It reappears under identical conditions in the vine, and less closely in true bryony. More subdivided into leaflets, it produces the Virginia creeper, and many forms of clematis.

Among ground plants, it is only possible very briefly to refer to the succulent types which abound in dry situations. A regular gradation may here be traced from rich forms with rather thin, flat, ovate leaves, growing in favourable situations, like *Sedum telephium*, through dwarfish forms, with oblong leaves, like *Sedum album*, to forms with knobby, globular leaves, growing in very dry spots, like *Sedum anglicum*. Where the stem becomes very succulent, the leaves may be dwarfed out of existence altogether, or reduced to prickles, as in those dry desert plants, the cactuses. Compare some tropical Euphorbias. Miscellaneous examples of these dry types are also found among Mesembryanthemums and other Ficoideæ, natives of hot, sandy plains in South Africa. The succulence here acts as a reservoir for water. Special precautions are taken against evaporation. We see the first symptoms of such a habit in some English dry-soil saxifrages.

Proximity to the sea, whether the plant grows in sand or mud, also tends to produce succulence. This effect is seen casually in many seaside weeds, and habitually in such cases as samphire, *Inula crithmoides*, *Spergularia rubra*, *Cakile maritima*, and common scurvy-grass. *Suæda maritima* is in this group the exact analogue of *Sedum anglicum*, while *Salicornia* is similarly the analogue of the leafless cactuses. Compare also *Salsola kali*. There is a somewhat similar tendency to fleshiness in certain freshwater weeds of moist spots, such as *Chrysosplenium*, and many saxifrages.

In such a brief sketch as the present it is impossible to do more than allude in passing to sundry more special developments of leaves, for protective or other purposes. One development of this character is seen in the growth of prickly tips (*Agave*, *Aloe*, *Salsola*, *Juncus acutus*, *Bromelia pinguin*), or of prickly edges (thistles, *Carlina*, holly, *Stratiotes*, *Dipsacus*, *Rubia peregrina*). Such prickles may be purely defensive, or they may assist the plant in clambering (*Stellatæ*, *Smilax*, hop). Again, the leaf as a whole may be reduced to a prickle, as in gorse, where the very young seedling has trefoil leaves like its allies: but these give way gradually to entire lanceolate blades, and finally to mere thornlike spines. Another very different development is that of the insect-eating plants, which grow in very boggy spots, and so require animal matter not yielded them by the roots. Our English sundew (Fig. 41) is an example of the first step in such a process; essentially its leaves belong to the obovate tufted or rosetted type represented by the daisy, only a little exaggerated; but they have been specialised for the insect-eating function by the evolution of the little glandular hairs. Even simpler is the type of the butterwort, which belongs to the same foliar class as the London Pride, *Draba aizoides*, *Samolus Valerandi*, *Sempervivum tectorum*, &c., but with the edges folded over so as to inclose its insect prey. From these simple forms we progress at last to highly specialised types like *Dionæa* (Fig. 42), *Sarracenia*, *Darlingtonia*, *Nepenthes*, and *Cephalotus*. Once more, the connate form in opposite leaves (*Dipsacus*, *Chlora*) or the perfoliate in alternate ones (*Bupleurum*) may be due, as has been suggested, to the facilities these arrangements afford for storing a little reservoir of water, which acts as a moat to protect the flowers from climbing ants. But such minor selective actions are too numerous and too diversified to be noticed in full here; it must suffice to point out the general principles upon which the forms of leaves usually depend, leaving the reader to fill in the details in every case from his own special observations.

GRANT ALLEN

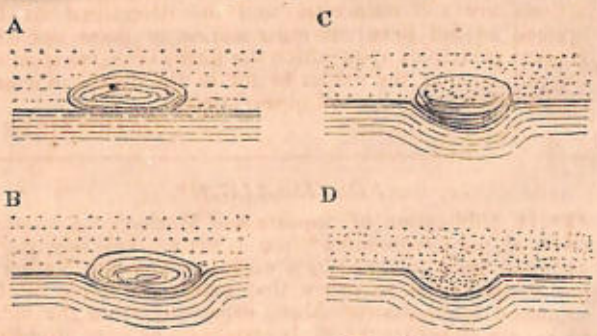
FOSSIL ALGÆ[1]

THE publication of Saporta and Marion's "Evolution of the Cryptogams" (see NATURE, vol. xxiv. p. 75, 558) has been followed by a work in which Dr. Nathorst has endeavoured to prove that nearly the whole of the supposed fossil marine Algæ, especially from the older rocks, are either tracks of Invertebrata or were produced by mechanical agency. "Florideæ, Laminarieæ, Chondriteæ, Alectorurideæ, Arthrophyceæ, Bilobites, and other algæ; comprising among them forms curious and remarkable by the regularity of their branching thallus, their phyllome with raised periphery and striated surface; all had disappeared as if by enchantment, and in their place there remained but tracks of Invertebrata, moving upon the ooze, swimming or creeping, and impressing the extremities of their tentaculary palpæ around them, or of larvæ gliding through the slimy mud." When these are insufficient, the movement of water acting on inert bodies, or waving tufts of sea-weed, are appealed to, for no fossil imprint either sunk or in relief, unless preserving carbonaceous matter, is admitted in Dr. Nathorst's hypothesis to have ever been a plant. This view is energetically combated by Saporta in the present work. The issue however does not very materially affect either the general theory of plant-evolution, as traced by Saporta and Marion, since this relies but little upon the evidence of doubtful fossil algæ, or the succession of marine algæ in time, which seems to have been probably Laminarieæ, Fucaceæ, and Florideæ. The main point in dispute is whether the supposed primordial algæ, Eophyton and Bilobites, are of vegetable or of other origin. There are numerous *a priori* reasons for supposing plant life to have existed in palæozoic seas, and the complexity of life seen in even the older rocks renders their presence almost a necessity. The question is whether certain impressions which are abundant in Silurian rocks reproduce some of these forms, or whether we are still without indications of the primæval algæ.

Dr. Nathorst appears to rely very greatly upon the fact that many of these supposed sea-weeds are marked in relief upon the under-sides of slabs, proving, as he supposes, that they are the filling-in of furrows, and also upon the very general disappearance of all trace of carbon. In denying the plant-origin of certain impressions lately described as algæ by Prof. Walter Keeping in the *Geological Magazine*, he lays particular stress on the former hypothesis. Saporta however devotes two or three pages to clearing up this, as he believes, misconception. The fact that very unmistakable impressions of even terrestrial plants do occur in this condition, is known to most collectors of them, and is explained by the author as follows:—A plant-stem of sufficient sub-

[1] "A propos des Algues Fossiles." Le Marquis de Saporta. (Paris: G. Masson, 1882.)

stance to resist pressure, but destined in the long run to decompose, would, if resting on the sea-bottom, become covered with sand or silt, if such deposit were taking place. (*a*.) As the weight increased above, its under-surface would become pressed into the bed upon which it chanced to be resting. (*b*.) As it decomposed, infiltrated sediment would replace the organic matter (*c*.), until finally the decomposition being complete, the sediment from above entirely fills in the space, leaving on the under-surface a reproduction in semi-relief of the decayed organism, while the upper part is merged in the sand.



(*d*.) Instances of this form of fossilisation are by no means rare, but cases in which all carbonaceous matter has disappeared from vegetable impressions are still more common, especially with sea-weeds, which, as M. Grand 'Eury has remarked, decompose into a semi-fluid gelatinous matter when imbedded in mud. Nor does the destruction of carbon cease when the mass they are buried in becomes consolidated, for percolating water brings oxygen to them, which slowly destroys every remaining vestige of organic matter.

The author is careful in the present work only to select specimens for illustration about which little or no reasonable doubt can exist. Commencing with impressions from the Tertiaries of almost existing species of sea-weed, he compares these with the more doubtful secondary Chondrites. The Chondrites of the Flysch, strongly impregnated as they are with carbonaceous matter, are admitted on all hands to be Algæ, and the author asks how the same origin can be denied to casts of specifically identical Chondrites of the Cretaceous, and so on to the Liassic forms. The algous nature of most of those selected for illustration is indeed so obvious that no shadow of doubt respecting them can exist. The gigantic Liassic Laminarias with reticulated structure are more problematic, but it seems at least highly improbable that any movements of invertebrata could have produced such markings. The Alectoruridæ, an extinct group of algæ which existed from the Silurian into the Tertiaries, and their equally extinct ally Glossophycus, whose vegetable nature is even more apparent, may challenge reasonable criticism on account of their divergence from recent algæ. While the algous nature of these, and many other types, is maintained, the author does not hesitate to acknowledge that many forms which it was previously considered might be algæ, are probably tracks of invertebrates. He simply holds that Dr. Nathorst's generalisations are far too sweeping, and in many cases utterly against the evidence. The true nature of Bilobites, however, is still open to some question. They are always preserved in semi-relief, a process explained above, but the arguments, while abundantly proving that they cannot be due to tracks of invertebrates, fall short of absolute proof that they must be Algæ, and can be nothing else. In like manner the Eophyton of the Lower Cambrian, alleged by Nathorst to be furrows made by moving sea-weed on a muddy bottom, is almost proved by its occasionally cylindrical form and interlacing fragments, and wholly confined as it is to this most ancient formation, to be something more than mere scratches upon ooze, however produced, yet the evidence does not prove conclusively that it is a plant. The discussion has at least produced two most valuable works, the one serving to show how even the most accomplished palæophytologists may be deceived in dealing with so perilous a subject as fossil algæ, and the other proving that in spite of numerous errors, there is a considerable basis of truth in even the most speculative branch of their science. J. S. G.

NOTES

WITH reference to the scheme of the Grocers' Company for the encouragement of sanitary research, it is stated that so far as the administration of the scheme will involve scientific considerations, the Court proposes to act with the advice of a committee of eminent scientific men, and the following gentlemen have consented to form the first committee:—Messrs. John Simon, C.B., F.R.S., John Tyndall, F.R.S., John Burdon Sanderson, M.D., F.R.S., and George Buchanan, M.D., F.R.S.

A PRIVATE test took place on Monday of a telephone between New York and Chicago, a distance of 1000 miles, and the result was a complete success. Previously the longest distance over which a telephonic message had been sent was 700 miles, between New York and Cleveland. The present result is not due solely to the telephone, although that possesses some novelty, but is mainly due to a novelty in the conductor. This consisted, it is stated, of a steel wire core, copper plated, the electrical resistance of which to Chicago was only 1522 ohms. This new achievement is regarded as marking a new era in the development of telephonic communication.

AFTER assuming threatening proportions, the eruption of Mount Etna has almost subsided. Eleven new fissures had opened on the side of the mountain, giving out smoke, scoriæ, and showers of small stones, accompanied by a rumbling sound, and a trembling of the earth. Strong shocks of earthquake were felt at various parts of the surrounding country, and crevices were formed in the earth. A telegram from Prof. Silvestri, dated the 25th, states that the eruption is without importance and seems ceasing. Later news on Monday night states that there is still cause for some uneasiness in respect to Etna. The lava has not flowed, but has formed a new cone. On Monday strong shocks of earthquake were felt at Pedara, and slight ones at Catania. The site of the present eruption is further down the mountain than any previous eruption in modern times, and it is the first eruption which has occurred on the southern side of the mountain for more than a century.

WE regret to record the loss to science of a gifted and energetic young worker through a gun accident. A telegram from Hong Kong informs us that Mr. Frank Hatton, mineralogist and scientific explorer for the British North Borneo Company was killed by the accidental discharge of his gun while hunting in the jungle. The deceased gentleman was the only son of Mr. Joseph Hatton, and gave promise of a brilliant and useful scientific career. He was a student of the Royal School of Mines, South Kensington, where he distinguished himself by the extraordinary rapidity and accuracy with which he worked through the course of studies in that institution. He was especially distinguished in the Chemical Section, in which he made and published some valuable researches on Bacteria, &c., for which he obtained the Frankland prize of the Institute of Chemistry, entitling him to the degree of Associate. Mr. Hatton had great linguistic aptitude, and this, with a considerable amount of natural tact, contributed much to his success in dealing with the natives of Borneo during his exploring expeditions for the Company. During the last eighteen months he has explored the greater part of the Company's dominion, an area about as large as France, without losing a man, and in regions in which

in many cases he was the only individual able to speak the Malay and Dusun dialects. A large number of scientific observations and notes on climate, geology, &c., of Borneo made during these expeditions will probably be published. Mr. Hatton, who had scarcely attained his twenty-second year at the time of his death, was a Fellow of the Chemical Societies of London and Berlin and of the Asiatic Society.

THE Gothenburg Museum will be represented at the coming Fisheries Exhibition by a magnificent selection of exhibits from its Zoological Section, the expenses of which will be borne by Dr. Oscar Dickson. This collection will be selected, arranged, and taken care of to London by Dr. A. H. Malm of that Museum. The collection will consist of the choicest gems of the Museum, among which are five rare species of whales, and the ichthyological fauna of the province of Bohus, as well as the well-known collection of herrings of various kinds and from different countries belonging to the Museum. There will also be sent a collection of skeletons of the fishes and birds comprising the fauna of Southern Sweden. The entire selection made by Dr. Malm is remarkable for its scientific accuracy, as well as finish. He will also show privately a splendid collection of Mollusca from the Cattegat.

THE Commission, consisting of Baron Nordenskjöld, Consul Elfwing, and Prof. Gyldén, which the Royal Swedish Geographical Society had appointed to report on the question of an international meridian and a common time, has come to the conclusion that it would undoubtedly be a matter of great difficulty to decide as to the former on account of national jealousies, but it has offered a solution of the latter question which is worthy of notice. If the Greenwich meridian is fixed on as the common one, it would strike a point 180° from Greenwich, east of New Zealand, and if another circle is drawn 90° from Greenwich, its western half would nearly touch New Orleans, and its eastern a point a few minutes east of Calcutta. This system would furnish four cardinal times, viz. one European, one American, one Asiatic, and one Oceanic. As it would however be necessary to find several mean times for Europe, Prof. Gyldén proposes that twelve meridians be drawn from Greenwich, which he numbers at intervals of 2½°, which will make the time of the places falling under each differ from those under the nearest meridian by 10 minutes of actual time. These meridians as numbered would either touch the places mentioned below, or fall so near them that the actual difference would be of no consequence. The difference of time from 10m. is however shown in the parentheses: No. 1, Paris (40s.); No. 2, Utrecht and Marseilles (1m. 29s.); No. 3, Bern (16s.) and Turin (42s.); No. 4, Hamburg (6s.), Altona (14s.), Gottingen (14s.), and Christiania (2m.); No. 5, Rome (50s.), Leipzig (26s.), and Copenhagen (20s.); No. 6, Sweden (15s. from common mean time); No. 7, Brieg (Prussia); No. 8, Königsberg (2m.); No. 9, Abo (1m.) and Mistra (Greece) (5s.); No. 11, no place of importance; No. 12, St. Petersburg (1m. 14s.), and Kiev. Further east it is not suggested to carry the system. Should the various European countries decide on adopting the mean time of the nearest meridian they might be arranged as follows:—No. 1 for France; No. 2 for Holland and Belgium; No. 3 for Switzerland; No. 4 for Norway and Western Germany; No. 5 for Denmark, Central Germany, and Italy; No. 6 for Sweden and Austria; No. 7 for Eastern Germany; No. 8 for Hungary; No. 9 for Poland and Greece; No. 10 for Finland, Roumania, and Bulgaria; No. 11 for Turkey; No. 12 for Eastern Russia. West of Greenwich No. 1 would serve for Spain, and No. 3 for Portugal. By this system Prof. Gyldén thinks it would be a simple matter for every one to remember that the difference between two meridians, as, for instance, between London and Paris, was exactly 10 minutes. Prof. Gyldén also suggests that, for the convenience of travellers and others, all public clocks should be provided with coloured rings showing the differences of time between the various meridians.

THIS winter, at a large number of private and official *soirées* in Paris, the electric light has been used from storage batteries in a very simple manner. The accumulators are carried in a vehicle which is stationed in front of the house, and electric wires are conducted into the building through the windows. Incandescent lamps are placed in the ordinary candelabras, and the fitting of the most complex lighting is an affair of a very few hours.

THE new Elphinstone-Vincent dynamo machine was shown the other evening to a large party of visitors at Messrs. Unwin's printing offices; 411 Swan incandescent lights of twenty candle-power being well sustained by an engine of not at all large dimensions. The exhibition seems to show that the Elphinstone-Vincent machine in its present form of maturity is one of considerable merit. Its most notable feature is that the armature works between both external and internal magnets, that the saddles of wire of which the armature is formed constitute a very simple construction, that there is close proximity in the working parts to the magnets, and that, all the parts of the machine being duplicated, taking to pieces and repairing can be most readily effected.

THE Commissioners on Technical Education—Mr. Woodall, M.P., Mr. Samuelson, M.P., Mr. Wyer Smith, and Mr. Magnus, with Mr. Redgrave, secretary—paid a flying visit to Edinburgh last week. They visited, we understand, the Watt Institute—where they were received by Prof. Fleeming Jenkin and Lord Shand, to whom they expressed themselves highly satisfied with the tuitional and other arrangements of the Institute—the Museum of Science and Art, and Heriot's Hospital. One half of them afterwards inspected the Merchant Company's Schools, and the other half several of the Board Schools.

THE Surveying Expedition, under the direction of M. de Lesseps, has left Hamma, Tunis, and visited the mouth of the Oued Melah, which is to form the outlet of the projected Inland Sea Canal. It is declared that the result of the investigations shows that the cutting of the earth may be accomplished without difficulty.

M. COCHERY, the Minister of Postal Telegraphy, presided over the first monthly dinner of French Electricians, which is to take place on the 21st of every month, at the Café Durand. English electricians wishing to join should communicate with the director of *L'Électricité*, 16, Rue du Croissant, Paris. The president of the meeting for April 21 will be M. Berger, ex-director of the Electrical Exhibition of 1881.

A TELEGRAM from Copenhagen states that "volcanic ashes" have fallen in the neighbourhood of Trondhjem, Norway, and that a serious eruption of Mount Hecla is therefore supposed to have taken place. If these "ashes" are the dust referred to in our note last week (p. 496), then they are not of a volcanic character, according to the examination of Dr. H. Reusch of Christiania University. On this subject a Glasgow correspondent writes:—"My son, who is a passenger by the P. and O. steamer *Deccan* to the East, writes on February 27, when the steamer was in the Red Sea: 'Nothing of note occurred till evening, when G. and myself determined to sleep on deck, on account of the heat. We accordingly did so, and retired to our bunks about 4 a.m. . . . During our sleep on deck we were much annoyed by a quantity of small particles of dust which covered our faces, pillows, &c., and indeed was spread all around. . . . I am convinced it must have been a shower of lava dust, which, it is well known, is often carried hundreds of miles from the crater where it has origin. The dust was of hard particles. I

am convinced that lava dust it was, but can get no one to coincide with my opinion.' Can this be a relation to the Norway dust? I see your Norway note says the wind blew strongly from north-north-west, which would bear towards the Red Sea."

M. NAPOLI, electrician to the French Great Eastern Railway, has published in the *Aéronaut* an article showing that electricity supplies a less ponderous motive power than steam for propelling balloons. He supposes that 3230 grammes of material is enough to generate, by means of Bunsen elements, an electric current able to give with a Gramme machine of a convenient construction one horse-power working during an hour.

ON the evening of February 28, at 8·40 p.m., two travellers sledging over the Lesjö, a remote lake in Värmland, in Sweden, saw a meteor of remarkable size and lustre fall about a mile off. Their backs were turned at the time of its appearance, but its luminosity was so strong that the whole country round was illuminated, and when they turned its brilliancy blinded them for a few seconds. Its track was marked by a vivid band, to the eye one foot broad and three yards long, of a yellowish colour. The meteor, after about five seconds, burst with a shower of sparks of the same colour before striking the earth. The night was perfectly clear.

THE Swedish Chamber of Agriculture has granted a Mr. A. Carlsson 50*l.* for the practical study of English agriculture during the coming season.

IT is undoubted that Gramme was the first to construct a dynamo-electric machine with continuous induction, using (independently of Pacinotti) a ring-armature similar to Pacinotti's ring. But regarding the question, who it was that first produced continuous dynamo electric currents, and so was the first to combine experimentally the principles of Siemens and Pacinotti, Prof. von Waltenhofen offers proof (*Wied. Ann.* No. 2) that this priority belongs to Prof. Pfaundler of Innsbruck. In 1867 Herr Kravogl of Innsbruck showed his electromagnetic motor at the Paris Exhibition; this consists of coils forming a hollow ring which rotates round a horizontal axis, while it incloses a bent cylindrical rod tending by weight to take the lowest position, but kept suspended in a certain raised position by currents in the coils, whereby also the ring is rotated. In a letter on this machine in 1867 Prof. Pfaundler proposed to apply Siemens's principle to it, and get electric currents from mechanical work of rotation (the battery being included at first with a shunt, then quite excluded). This he tried and effected about three years later, as a letter dated February 11, 1870, records. Thus Pfaundler seems to have produced continuous dynamo-electric currents before Gramme, and to have indicated the possibility of getting such currents from the Kravogl ring machine in the same year (1867) as Siemens's invention of dynamo-electric machines acquired publicity.

THE Committee of the Annonay Montgolfier celebration have already collected 60,000 francs, and subscriptions are pouring in. They have decided upon the publication of a special organ, of which the first number will be issued in a few days. The celebration will consist in the erection of a statue to the two brothers, several ascents, the sending up of a Montgolfier similar to the original one, and a cavalcade representing the provincial officials, who witnessed the proceedings on June 5, 1783.

IT seems to result from recent researches by A. W. Pehl, brought before the Russian Chemical Society, that the poisonous action of the ergot, the bad effects of which are so often witnessed in Russia, is due to putrefaction poisons called ptomaines, which appear during the decomposition of the albuminoids in flour. The ergot, that is the sclerotium of the small mushroom, *Claviceps purpurea*, has energetic peptic qualities and thus would directly contribute to the formation of ptomaines in the flour.

WE have received the last number of the Caucasian *Izvestia*, which appeared at Tiflis on February 24. It contains several interesting papers; M. Stebnitzky contributes a paper on the measurements by Parrot, in 1829, of the seconds pendulum on the Great Ararat, and, introducing all necessary corrections for rendering them comparable with recent measurements, he arrives at the result that the length of the pendulum at the monastery of St. Jacob on the Ararat is 440·1613 Paris lines. The anomaly would be thus equal to 7·7 swings per day, and corresponds to an elevation of geoide on the normal spheroid of 855 metres. Compared with Tiflis (1343 metres), this diminution of gravity would point out the existence of great cavities in the Ararat. We notice also a paper on the changes of height of the level of the Caspian Sea, by M. Filipoff; measurements of heights in the villayet of Trapezunt; complementary notes to the formerly-published anthropological measurements, by M. Erxert; and a summary of the first part of M. V. Miller's researches on the Osetian language. In the bibliographical part we find an interesting sketch of the climate of the Caucasus, on the ground of the meteorological observations published by Dr. H. Wild in his work, "Die Temperatur-Verhältnisse des Russichen Reichs," and a report, by M. Zagursky, on Baron Uslar's posthumous work on the Tabasatan language; it is a serious work, containing a very elaborate grammar of the language, a list of words, and a chestomathy. The same fascicule contains the necrologies of Dr. Land and Count Sollogoub, and a variety of notes. In the appendix we find a translation of Mr. Palgrave's reports on Anatolia and Lazistan, which are considered as the more reliable with regard to population.

A SERIES of shocks, lasting several seconds, believed at present to be due to earthquake, were felt at Amsterdam at 5 a.m. on March 17. The movement was in a vertical direction, and caused mirrors and other pendent articles of furniture to oscillate.

THE additions to the Zoological Society's Gardens during the past week include a Common Wigeon (*Mareca penelope* ♂), British, presented by Lieut.-Col. C. Birch Reynordson; three Sirens (*Siren lacertina*) from South Carolina, presented by Mr. G. E. Manigault; six Common Squirrels (*Sciurus vulgaris*), British, a Lemur (*Lemur* ——) from Madagascar, two Robben Island Snakes (*Coronella phocarum*) from Robben Island, South Africa, purchased; a Gayal (*Bibos frontalis*), born in the Gardens.

OUR ASTRONOMICAL COLUMN

THE COMET 1883 *a*.—From elements calculated by Dr. Hepperger of Vienna upon observations extending from Feb. 24 to March 4, the following ephemeris for midnight at Berlin results:—

		R.A. h. m. s.	Decl. ° ′	Distance from Earth.	Distance from Sun.
March	30	3 29 24	+21 49·2	1·497	1·073
	31	3 33 59	21 19·5		
April	1	3 38 25	20 50·3	1·536	1·098
	2	3 42 44	20 21·4		
	3	3 46 55	19 52·9	1·576	1·124
	4	3 50 58	19 24·8		
	5	3 54 55	18 57·0	1·616	1·150
	6	3 58 46	18 29·6		
	7	4 2 32	18 2·7	1·657	1·177
	8	4 6 13	17 36·2		
	9	4 9 48	+17 10·2	1·698	1·204

The ascending node of this comet falls at a radius-vector of about 2·36 in the region of the minor planets, the descending node at a radius-vector of 1·12, or 0·14 outside the earth's path; but, for the comet to pass at its least distance from our globe, the perihelion passage must occur about November 16.

THE MINOR PLANET No. 228.—The nearest approach to the earth's orbit made by any one of the 232 small planets so far known appears to occur in the case of No. 228, discovered by Herr Palisa at Vienna on August 19, 1882. At the perihelion point this planet may be distant from us only 0·662 of our mean distance from the sun, and on this account would prove a favourable object for a determination of solar parallax. But unfortunately the brightness of the planet at discovery was only 12·5m., though the mean anomaly was then 1½°, or the perihelion passage took place five days subsequently. Hence it is very questionable if such an object could be utilised for the purpose. No. 132, *Æthra*, has the smallest perihelion distance (1·6038), but in consequence of the large angle between the lines of nodes and apsides, and an inclination of nearly 25°, this planet is much further from the earth's track at perihelion than No. 228. *Andromache*, No. 175, recedes furthest from the sun, the distance at aphelion being 4·7234, or within 0·48 of the mean distance of Jupiter.

BINARY STARS.—According to Dr. Doberck's orbit of γ Coronæ Borealis, this very difficult object should now be measurable with our larger instruments. For 1883·5 the calculated position is 123°, and the distance 0″·34. This object was single, with the great refractor at Washington, from 1875 to 1879. In June, 1881, it was pronounced round, or doubtfully elongated, by Mr. Burnham, who remarks, "It has been apparently single with all apertures since about 1871." Doberck's period of revolution is 95½ years: periastron passage, 1843·7.

The following calculated angles and distances of several other binaries may serve for comparison with observations :—

Epoch.	Star.	Position.	Distance.	Authority for orbit.
		°	″	
1882·5	η Cassiopeiæ	163·3	5·52	Doberck.
	"	161·8	5·38	Duner.
1882·5	ξ Böotis	268·9	3·56	Doberck.
1883·5	"	267·6	3·20	"
1882·5	ω Leonis	86·5	0·60	"
1883·5	"	88·2	0·61	"
1882·5	η Coronæ Bor.	140·9	0·51	"
1882·5	ζ Herculis	105·9	1·43	"
1882·5	μ^2 Herculis	297·3	0·88	"
1882·5	70 Ophiuchi	63·5	2·98	Tisserand.

ELECTRICAL TRANSMISSION OF FORCE AND STORAGE OF POWER[1]

DR. SIEMENS, in opening the discourse, reverted to the object the Council had in view in organising these occasional lectures, which were not to be lectures upon general topics, but the outcome of such special study and practical experience as Members of the Institution had exceptional opportunities of acquiring in the course of their professional occupation. The subject to be dealt with during the present session was that of electricity. Already telegraphy had been brought forward by Mr. W. H. Preece, and telephonic communication by Sir Frederick Bramwell.

Thus far electricity had been introduced as the swift and subtle agency by which signals were produced either by mechanical means or by the human voice, and flashed almost instantaneously to distances which were limited, with regard to the former, by restrictions imposed by the globe. To Dr. Siemens had been assigned the task of introducing to their notice electric energy in a different aspect. Although still giving evidence of swiftness and precision, the effects he should dwell upon were no longer such as could be perceived only through the most delicate instruments human ingenuity could contrive, but were capable of rivalling the steam engine, compressed air, and the hydraulic accumulator, in the accomplishment of actual work.

In the early attempts at magneto-electric machines, it was shown that, so long as their effect depended upon the oxidation of zinc in a battery, no commercially useful results could have been anticipitated. The thermo-battery, the discovery of Seebeck in 1822, was alluded to as a means of converting heat into electric energy in the most direct manner; but this conversion could not be an entire one, because the second law of thermo-dynamics, which prevented the realisation as mechanical force of more than one-seventh part of the heat energy produced in combustion under the boiler, applied equally to the thermo-electric battery, in which the heat, conducted from the hot points of juncture to the cold, constituted a formidable loss. The electromotive force of each thermo-electric element did not exceed 0·036 of a volt, and 1800 elements were therefore necessary to work an incandescence-lamp.

A most useful application of the thermoelectric battery for measuring radiant heat, the thermopile, was exhibited. By means of an ingenious modification of the electrical pyrometer, named the Bolometer, valuable researches in measuring solar radiations had been made by Prof. Langley.

Faraday's great discovery of magneto-induction was next noticed, and the original instrument by which he had elicited the first electric spark before the members of the Royal Institution in 1831, was shown in operation. It was proved that although the individual current produced by magneto-induction was exceedingly small and momentary in action, it was capable of unlimited multiplication by mechanical arrangements of a simple kind, and that by such multiplication, the powerful effects of the dynamo-machine of the present day were built up. One of the means for accomplishing such multiplication was the Siemens armature of 1856. Another step of importance was that involved in the Pacinnoti ring, known in its practical application as the machine of Gramme. A third step, that of the self-exciting principle, was first communicated by Dr. Werner Siemens to the Berlin Academy, on January 17, 1867, and by the lecturer to the Royal Society on the 4th of the following month. This was read on February 14, when the late Sir Charles Wheatstone also brought forward a paper embodying the same principle. The lecturer's machine which was then exhibited, and which might be looked upon as the first of its kind, was shown in operation; it had done useful work for many years as a means of exciting steel magnets. A suggestion, contained in Sir Charles Wheatstone's paper, that "a very remarkable increase of all the effects, accompanied by a diminution in the resistance of the machine, is observed when a cross wire is placed so as to divert a great portion of the current from the electro-magnet," had led the lecturer to an investigation read before the Royal Society on March 4, 1880, in which it was shown that by augmenting the resistance upon the electro-magnets a hundredfold, valuable effects could be realised, as illustrated graphically by means of a diagram. The most important of these results consisted in this, that the electromotive force produced in a "shunt-wound machine," as it was called, increased with the external resistance, whereby the great fluctuations formerly inseparable from electric-arc lighting could be obviated, and that, by the double means of exciting the electro-magnets, still greater uniformity of current was attainable.

The conditions upon which the working of a well-conceived dynamo-machine must depend were next alluded to, and it was demonstrated that when losses by unnecessary wire-resistance, by Foucault-currents, and by induced currents in the rotating armature were avoided, as much as 90 per cent., or even more, of the power communicated to the machine were realised in the form of electric energy, and that *vice versâ* the reconversion of electric into mechanical energy could be accomplished with similarly small loss. Thus, by means of two machines at a moderate distance apart, nearly 80 per cent. of the power imparted to the one machine could be again yielded in the mechanical form by the second, leaving out of consideration frictional losses, which latter need not be great, considering that a dynamo-machine had only one moving part well balanced, and was acted upon along its entire circumference by propelling force. Jacobi had proved many years ago that the maximum efficiency of a magneto-electric engine was obtained when

$$\frac{e}{E} = \frac{w}{W} = \tfrac{1}{2}$$

which law had been frequently construed by Verdet ("Théorie Mécanique de la Chaleur") and others to mean that one-half was the maximum theoretical efficiency obtainable in electric transmission of power, and that one-half of the current must be necessarily wasted or turned into heat. The lecturer could never be reconciled to a law necessitating such a waste of energy, and had maintained, without disputing the accuracy of Jacobi's law, that it had reference really to the condition of maximum work accomplished with a given machine, whereas its efficiency must be governed by the equation

$$\frac{e}{E} = \frac{w}{W} = \text{nearly } 1.$$

From this it followed that the maximum yield was obtained

[1] Abstract of lecture given at the Institution of Civil Engineers on March 13 by Dr. C. William Siemens, F.R.S., M.Inst.C.E. Revised by the author.

when two dynamo-machines (of similar construction) rotated nearly at the same speed, but that under these conditions the amount of force transmitted was a minimum. Practically the best condition of working consisted in giving to the primary machine such proportions as to produce a current of the same magnitude, but of 50 per cent. greater electromotive force than the secondary; by adopting such an arrangement, as much as 50 per cent. of the power imparted to the primary could be practically received from the secondary machine at a distance of several miles. Prof. Silvanus Thompson, in his recent Cantor Lectures, had shown an ingenious graphical method of proving these important fundamental laws.

The possibility of transmitting power electrically was so obvious that suggestions to that effect had been frequently made since the days of Volta, by Ritchie, Jacobi, Henry, Page, Hjorth, and others; but it was only in recent years that such transmission had been rendered practically feasible.

Just six years ago, when delivering his presidential address to the Iron and Steel Institute, the lecturer had ventured to suggest that "time will probably reveal to us effectual means of carrying power to great distances, but I cannot refrain from alluding to one which is, in my opinion, worthy of consideration, namely, the electrical conductor. Suppose water-power to be employed to give motion to a dynamo-electrical machine, a very powerful electrical current will be the result, which may be carried to a great distance, through a large metallic conductor, and then be made to impart motion to electro-magnetic engines, to ignite the carbon points of electric lamps, or to effect the separation of metals from their combinations. A copper rod 3 inches in diameter would be capable of transmitting 1000 h.p. a distance of say 30 miles, an amount sufficient to supply one quarter of a million candle-power, which would suffice to illuminate a moderately-sized town." This suggestion had been much criticised at the time, when it was still thought that electricity was incapable of being massed so as to deal with many horse power of effect, and the size of conductor he had proposed was also considered wholly inadequate. It would be interesting to test this early calculation by recent experience. Mr. Marcel Deprez had, it was well known, lately succeeded in transmitting as much as 3 h.p. to a distance of 40 kilometres (25 miles) through a pair of ordinary telegraph wires of 4 mm. diameter. The results so obtained had been carefully noted by Mr. Tresca, and had been communicated a fortnight ago to the French Academy of Sciences. Taking the relative conductivity of iron wire employed by Deprez, and the 3-inch rod proposed by the lecturer, the amount of power that could be transmitted through the latter would be about 4000 h.p. But Deprez had employed a motor-dynamo of 2000 volts, and was contented with a yield of 32 per cent. only of the power imparted to the primary machine, whereas he had calculated at the time upon an electromotive force of 200 volts, and upon a return of at least 40 per cent. of the energy imparted. In March, 1878, when delivering one of the Science Lectures at Glasgow, he said that a 2-inch rod could be made to accomplish the object proposed, because he had by that time conceived the possibility of employing a current of at least 500 volts. Sir William Thomson had at once accepted these views, and with the conceptive ingenuity peculiar to himself, had gone far beyond him, in showing before the Parliamentary Electric Light Committee of 1879, that through a copper wire of only ½-inch diameter, 21,000 h.p. might be conveyed to a distance of 300 miles with a current of an intensity of 80,000 volts. The time might come when such a current could be dealt with, having a striking distance of about 1·2 feet in air, but then, probably, a very practical law enunciated by Sir William Thomson would be infringed. This was to the effect that electricity was conveyed at the cheapest rate through a conductor, the cost of which was such that the annual interest upon the money expended equalled the annual expenditure for lost effect in the conductor in producing the power to be conveyed. It appeared that Mr. Deprez had not followed this law in making his recent installations.

Sir William Armstrong was probably first to take practical advantage of these suggestions in lighting his house at Cragside during night-time, and working his lathe and saw-bench during the day, by power transmitted through a wire from a waterfall nearly a mile distant from his mansion. The lecturer had also accomplished the several objects of pumping water, cutting wood, hay, and swedes, of lighting his house, and of carrying on experiments in electro-horticulture from a common centre of steam-power. The results had been most satisfactory; the whole of the management had been in the hands of a gardener and of labourers, who were without previous knowledge of electricity, and the only repairs that had been found necessary were one renewal of the commutators and an occasional change of metallic contact brushes.

An interesting application of electric transmission to cranes, by Dr. Hopkinson, was shown in operation.

Amongst the numerous other applications of the electrical transmission of power, that to electrical railways, first exhibited by Dr. Werner Siemens, at the Berlin Exhibition of 1879, had created more than ordinary public attention. In it the current produced by a dynamo-machine, fixed at a convenient station and driven by a steam-engine or other motor, was conveyed to a dynamo placed upon the moving car, through a central rail supported upon insulating-blocks of wood, the two working-rails serving to convey the return current. The line was 900 yards long, of 2-feet gauge, and the moving car served its purpose of carrying twenty visitors through the Exhibition each trip. The success of this experiment soon led to the laying of the Lichterfelde line, in which both rails were placed upon insulating sleepers, so that the one served for the conveyance of the current from the power station to the moving car, and the other for completing the return circuit. This line had a gauge of 3 feet 3 inches, was 2500 yards in length, and was worked by two dynamo-machines, developing an aggregate current of 9000 Watts, equal to 12 h.p. It had now been in constant operation since May 16, 1881, and had never failed in accomplishing its daily traffic. A line half a kilometer in length, but of 4 feet 8½ inch gauge, was established by the lecturer at Paris in connection with the Electric Exhibition of 1881. In this case two suspended conductors in the form of hollow tubes with a longitudinal slit were adopted, the contact being made by metallic bolts drawn through these slit tubes, and connected with the dynamo-machine on the moving car by copper ropes passing through the roof. On this line 95,000 passengers were conveyed within the short period of seven weeks.

An electric tramway 6 miles in length had just been completed, connecting Portrush with Bush Mills in the north of Ireland, in the installation of which the lecturer was aided by Mr. Traill, as engineer of the Company, by Mr. Alexander Siemens, and by Dr. E. Hopkinson, representing his firm. In this instance the two rails, 3 feet apart, were not insulated from the ground, but were joined electrically by means of copper staples and formed the return circuit, the current being conveyed to the car through a T iron placed upon short standards, and insulated by means of insulite caps. For the present the power was produced by a steam-engine at Portrush, giving motion to a shunt-wound dynamo of 15,000 Watts = 20 h.p., but arrangements were in progress to utilise a waterfall of ample power near Bush Mills, by means of three turbines of 40 h.p. each, now in course of erection. The working-speed of thi line was restricted by the Board of Trade to 10 miles an hour, which was readily obtained, although the gradients of the line were decidedly unfavourable, including an incline of 2 miles in length at a gradient of 1 in 38. It was intended to extend the line 6 miles beyond Bush Mills, in order to join it at Dervock station with the north of Ireland narrow-gauge railway system.

The electric system of propulsion was, in the lecturer's opinion, sufficiently advanced to assure practical success under suitable circumstances—such as for suburban tramways, elevated lines, and above all lines through tunnels, such as the Metropolitan and District Railways. The advantages were that the weight of the engine, so destructive of power and of the plant itself in starting and stopping, would be saved, and that perfect immunity from products of combustion would be insured. The limited experience at Lichterfelde, at Paris, and with another electric line of 765 yards in length, and 2 feet 2 inches gauge, worked in connection with the Zaukerode Colliery since October, 1882, were extremely favourable to this mode of propulsion. The lecturer however did not advocate its prospective application in competition with the locomotive engine for main lines of railway. For tramways within populous districts the insulated conductor involved a serious difficulty. It would be more advantageous under these circumstances to resort to secondary batteries, forming a store of electrical energy carried under the seats of the car itself, and working a dynamo-machine connected with the moving wheels by means of belts and chains.

The secondary battery was the only available means of propelling vessels by electrical power, and considering that these batteries might be made to serve the purpose of keel ballast,

their weight, which was still considerable, would not be objectionable. The secondary battery was not an entirely new conception. The hydrogen gas battery suggested by Sir Wm. Grove in 1841, and which was shown in operation, realised in the most perfect manner the conception of storage, only that the power obtained from it was exceedingly slight. The lecturer, in working upon Sir William Grove's conception, had twenty-five years ago constructed a battery of considerable power in substituting porous carbon for platinum, impregnating the same with a precipitate of lead peroxidised by a charging current. At that time little practical importance attached, however, to the subject, and even when Planté, in 1860, produced his secondary battery, composed of lead plates peroxidised by a charging current, little more than scientific curiosity was excited. It was only since the dynamo-machine had become an accomplished fact that the importance of this mode of storing energy had become of practical importance, and great credit was due to Faure, to Sellon, and to Volckmar, for putting this valuable addition to practical science into available forms. A question of great interest in connection with the secondary battery had reference to its permanence. A fear had been expressed by many that local action would soon destroy the fabric of which it was composed, and that the active surfaces would become coated with sulphate of lead preventing further action. It had, however, lately been proved in a paper read by Dr. Frankland before the Royal Society, corroborated by simultaneous investigations by Dr. Gladstone and Mr. Tribe, that the action of the secondary battery depended essentially upon the alternative composition and decomposition of sulphate of lead, which was therefore not an enemy, but the best friend to its continued action.

In conclusion, the lecturer referred to electric nomenclature, and to the means for measuring and recording the passage of electric energy. When he addressed the British Association at Southampton, he had ventured to suggest two electrical units additional to those established at the Electrical Congress in 1881, viz., the Watt and the Joule, in order to complete the chain of units connecting electrical with mechanical energy and with the unit-quantity of heat. He was glad to find that this suggestion had met with favourable reception, especially that of the Watt, which was convenient for expressing in an intelligible manner the effective power of a dynamo-machine, and for giving a precise idea of the number of lights or effective power to be realised by its current, as well as of the engine power necessary to drive it: 746 Watts represented 1 h.p.

Finally the Watt-meter, an instrument recently developed by his firm, was shown in operation. This consisted simply of a coil of thick conductor suspended by a torsion wire, and opposed laterally to a fixed coil of wire of high resistance. The current to be measured flowed through both coils in parallel circuit, the one representing its quantity expressible in Ampères, and the other its potential expressible in Volts. Their joint attractive action expressed therefore Volt-Ampères or Watts, which were read off upon a scale of equal divisions.

The lecture was illustrated by experiments, and by numerous diagrams and tables of results. Measuring instruments by Professors Ayrton and Perry, by Mr. Edison and by Mr. Boys were also exhibited.

FAUNA AND FLORA OF THE ALEUTIAN ISLANDS

THE last number of *Naturen* contains an interesting report by Dr. Leonhard Stejneger of the result of his six months' observations of the fauna and flora of the Kamschatkan coast and of the so-called Kommandorski Islands, which form the western group of the Aleutian archipelago between Behring's Sea and the Pacific, in 50°–55° N. lat. The Kommandorski group consists of two islands, one of which is known as Mednoj Ostrov, Copper Island, from the large amount of the pure metal found there; while the other, which was the scene of Behring's shipwreck and death, bears his name. Both islands are geologically allied to Kamschatka, and excepting at the north of Behring's Island, where the gradual subsidence of the sea has left raised beaches, terraces, and tabulated rock-formations, the islands consist generally of deep narrow valleys separated by rocky barriers, which rise precipitously to a height of from 1000 to 2000 feet above the level of the sea. The islands, which were uninhabited before their annexation by Russia, are now occupied by about 700 persons, in the employment of a Russo-American fur company, which has been attracted to the spot by the enormous numbers of sea-bears (*Callorhinus ursinus*) and sea-otters (*Enhydra lutris*) which frequent the coasts. The climate is foggy, and the vegetation stunted and sparse, while in the neighbouring Kamschatkan territory the blue of the summer sky, the stillness of the sea, and the softness of the air, are almost Italian in character. The flora, moreover, is so exuberant that numerous plants, which in Norway never exceed two or three feet, here attain the height of a tall man. Next to the birch (*Betula ermanni*), alders, willows, and roans (*Sorbus Kamschaticus*), are the most frequent trees, the berries of the last-named, and those of *Lonicera cærulea*, possessing a sweetness which brings them into great request among strangers as well as natives. Some flowers also, as the wild, indigenous, dark red rose, several rhododendrons, and native lilies, are equally remarkable for exceptional fragrance. Among wild flowers, some of the geraniums, potentillas, taraxacums, &c., are almost identical with those found in Norway. Besides a large whale, and a specimen of the walrus (*Rosmarus obesus*), which had been killed near Avatscha Bay, Dr. Stejneger could find no trace of any mammal but a small specimen of *Arvicola œconomus*. Of birds there is, however, an enormous variety, some of which, as *Calliope Kamschatica*, *Carpodacus Erythrinus*, and a kind of sedge-warbler, provisionally named by the author "*Acrocephalus dybowskii*," combine an almost tropical brilliancy of colouring with a sweetness of song equal to that of our own nightingale or thrush. Besides these melodious warblers, Kamschatka harbours large numbers of *Locustella lanceolata*, whose grasshopper-like cry is heard when all else is still. *Cuculus canorinus* represents our common cuckoo. Pipits, chats, and wagtails abound; *Larus capistratus* is commoner than any other gull, and the osprey is not unfrequent. Mosquito-like gnats of vindictive nature swarm in such numbers as to make the pursuits of the field naturalist almost impracticable. The fauna, generally, is palæarctic in character, with a scarcity of American forms which is very remarkable when we consider the vicinity of the western continent.

PHYSICAL HISTORY OF THE DEAD SEA, THE JORDAN VALLEY, AND PALESTINE

PROF. E. HULL, LL.D., F.R.S., delivered an interesting lecture on the above subject on March 2, in the Theatre of the Royal Dublin Society's premises, Kildare Street. Prof. Hull said:—"There is no country which possesses for us an interest equal to that which I have to treat of this evening. Its religious and historical associations stand alone amongst those of all nations, and will ever maintain in the history of the world an undying import. But while this is true as regards the religious and social aspects of Palestine, I hope to show that in its physical aspect it possesses points of interest which render it unique amongst all countries, and which have attracted to it the attention of naturalists during a lengthened period down to the present day. Probably no country has been so often described. Its physical features have attracted the attention of observers of natural phenomena from Strabo downwards to the recent admirable work of M. Lartet and the Duc de Luynes, to which I am largely indebted. In more recent times we have the observations of Humboldt, of the late Dr. Hitchcock, of Lieut. Lynch of the United States Navy, who carried out a systematic series of soundings over the bed of the Dead Sea, and more recently of the Rev. Dr. Tristram, of Prof. Roth, Burkhardt, and others, including the Survey made by the officers of the Royal Engineers. It is curious however that the remarkable physical phenomenon which renders the Holy Land unique amongst all countries (regarded in its physical aspect) was not discovered till the year 1836-37, when Heinrich Von Schubert and Prof. Roth determined by barometric observations that the surface of the Dead Sea lies no less than 1300 feet below the level of the Mediterranean, a fact not suspected by previous observers. It is the deep depression of the Jordan Valley, deeper by far than any river valley elsewhere, which is the key to the physical history of the whole country; and in endeavouring to trace out its origin I shall reproduce in as general a manner as I can the successive phases through which the region bordering the Mediterranean, and extending eastwards towards the Euphrates and southwards to the Dead Sea, has passed. The fundamental basis of the geological formation of Palestine is the gneissic granite, of Archæan age and metamorphic origin, which rises into the mountains of Idumea, and is the rock from which the huge

monoliths of Egypt have been hewn, such as Cleopatra's Needle' the obelisk of Luxor, and the columns which adorn the Piazza of Venice. This foundation rock formed part of a continental area down to the Carboniferous period, when it was submerged, and a great sandstone formation was spread over it known as "the Nubian sandstone." After another interval of time the sandstone itself was overspread by limestone deposits of Cretaceous and Tertiary age, deposited over the floor of the ancient sea, and down to the close of the Eocene period the waters of the sea overspread the greater portions of Asia Minor, Palestine, and the adjoining districts of the Asiatic and African continents. The first appearance of Palestine and the adjoining districts as a land surface dates from the succeeding Miocene period, when the bed of the sea was upraised into dry land, and at the same period a great fissure corresponding with the line of the Jordan valley was produced. Along this fissure, which has been traced from the Lebanon southwards towards the Gulf of Akaba —the strata on the eastern (or Moabite) side have been relatively elevated: those on the western relatively depressed;—so that the strata on the opposite sides of the Jordan valley and the Dead Sea do not correspond with each other. This great fissure is the key to the physical formation of the whole region, because it gave origin to a river which once flowed down from the mountains of Lebanon—southwards through the Gorge of Arabah (discovered by Burkhardt)—into the Red Sea in a remarkably straight line running north and south for a distance of over 250 miles. This is now the Jordan. The depression of the valley continuing through the succeeding Pliocene epoch, the district of the Ghor and the Jordan valley was conveyed into a lake, which Prof. Hull considered ultimately extended from the southern end of the Dead Sea, northwards nearly to the Lake Merom, and included the Sea of Galilee. This lake would then have had a length of 160 miles and an average breadth of ten miles. During "the Pluvial period," which succeeded "the Glacial," the waters probably reached their maximum elevation, and continued to flow southwards through the Gorge of Arabah and the Gulf of Akaba into the Red Sea; but from the increasing dryness of the climate they gradually decreased, and the surface of the lake became contracted, and ultimately reduced to its existing limits. During this lowering of the surface, the remarkable terraces noticed by most travellers were formed. Dr. Tristram has taken the barometric level of several of these above the Dead Sea. They range up to 750 feet, and even higher. They appear to be undoubtedly old lake margins, and indicate the successive levels at which the lake stood. The 750-foot terrace very closely corresponds to the summit-level of the Gorge of Arabah. When the waters were reduced so low as not to pass through the Gorge of Arabah, they became brackish, and ultimately salt—the salinity increasing as the area became diminished. All lakes not having an outlet become saline; and the contrast of the waters of the Sea of Galilee and those of the Dead Sea form a striking illustration of the law just stated. The saline ingredients in the surface waters of the Dead Sea are 24·57 lbs. in 100 lbs. of the water, while that of the Atlantic only contains 6 lbs. in the same quantity. The Dead Sea water is therefore over four times as strongly impregnated with salts as that of the ocean, and in the deeper waters the salinity amounts to saturation, as saline deposits are forming over the floor of the Dead Sea. This remarkable inland sea had assumed somewhat of its present contracted dimensions, and was known as "the Salt Sea" as far back as the time of the Patriarch Abraham. Near its borders stood the doomed cities of Sodom and Gomorrah—not beneath its waters, as was often supposed—but near its upper margin. With the call of Abraham the political and religious history of Palestine begins, and the narrative of the physical historian ends.

SCIENTIFIC SERIALS

American Journal of Science, March.—The selective absorption of solar energy, by S. P. Langley.—New locality of the green turquois known as chalcuite, and on the identity of turquois with the callais or callaina of Pliny, by W. P. Blake.—On portions of the skeleton of a whale from gravel on the line of the Canada Pacific Railway near Smith Falls, Ontario, by J. W. Dawson.—The cobwebs of Uloborus, by J. H. Emerton.—Glacial drift in the Upper Missouri River region, by C. A. White.—Late observations concerning the molluscan fauna and the geographical extent of the Laramie group, by the same.—The Sphingidæ of North America, by A. R. Grote.—"Rotational coefficients" of various metals, by E. H. Hall.—Recent exploration of the volcanic phenomena of the Hawaiian islands, by C. E. Dutton.

Journal of the Russian Chemical and Physical Society, vol. xiv. fasc. 9.—On several ethylenic hydrocarbons, and on their action on water, by M. A. Eltekoff. Of the compounds of the series $C_nH_{2n}O$, the oxides are the least known, and it still remains in doubt as to those described by MM. Bauer, Würtz, Jekyll, and Clermont being true oxides and not ketones; M. Eltekoff studied, therefore, the action on water of seven compounds of this series. He arrives at the conclusion that the characteristic features of oxides do not disappear, as seemed formerly to be the case, in more compound oxides containing even as much as six equivalents of carbon. Their capacity of entering in direct compounds with water diminishes, however, in proportion as the molecule becomes more complicated.—On the oxidation of sulphur used for covering the vineyards, by M. A. Bazaroff.—On the evaporation of liquids, by M. Sreznewsky. Evaporation of benzol, ether, ethyl-alcohol, chloroform, and sulphur of carbon at different temperatures. The paper will be continued.—On the critical temperature and pressure of water, by M. O. Strauss. The average of a series of observations gives for the critical temperature of water 370°, with a probable error of 5°. The critical pressure would be 195·5 atmospheres.—Historical sketch of the work accomplished by the Physical Society during its ten years' existence, by M. N. Hesehus.—On the temperature of the absolute vaporisation of liquids, by M. Nadejdin.—On the spheroidal state of liquids, by M. D. Diakonoff.—Minutes of proceedings.

Rivista Scientifico-Industriale e Giornale del Naturalista, January 15.—The glossograph of S. Gentilli.—Influence of ozone in agriculture, by S. Zimo.—The radiometer and school experiments, by C. Rovelli.—Fossil elephants in the district of Parma.—Simple holohedral forms of the rhombohedral system, by M. de Lupo.

Reale Istituto Lombardo di Scienze e Lettere. Rendiconti, vol. xvi. fasc. i.—Meteorological *résumé* of the year 1882, calculated on observations made at the Royal Observatory of Brera, by E. Pini.—The frost of 1882 considered in its agrarian and meteoric aspect, by E. Ferrario.—Results of observations on the amplitude of diurnal oscillation of the declination-needle made during 1882 at Brera Observatory, by G. Schiaparelli.—On the action of metallic iodide on leucine and other like substances, by G. Körner and E. Menozzi.

Fasc. ii.—Property of a class of functions with more variables than are presented in dynamics in the case of permanent motion, by C. Formenti.—On some plane involutions, by E. Bertini.—Generalisation of a theorem on the analytical representation of substitutions, by A. Grandi.

Schriften der Physicalisch-Ökonomischen Gesellschaft zu Königsberg. 1880, first part; 1881, first and second parts.—Geological investigation of the North German level country, especially East and West Prussia, in the years 1878–80, by A. Jentzsch.—Contributions to a knowledge of the Silurian Cephalopoda found in the East and West Prussian diluvial formations, by H. Schröder.—Rugous corals in the same formation, by G. Meyer.—The scales of our fishes, by B. Benecke.—On some diluvial and alluvial diatom-layers of North Germany, by P. T. Cleve and A. Jentzsch.—The underground portion of the North German level country, by A. Jentzsch.

Verhandlungen der Naturhistorischen Vereines der Preussischen Rheinlande und Westfalens, 1882 (first half).—Further observations on fertilisation of flowers by insects, by H. Müller.—On the various systems of measurement of electric and magnetic quantities, by R. Clausius.—The lower Devonian strata of Olkenbach, by O. Follmann.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, March 8.—"Notes on the Absorption of Ultra-Violet Rays by various Substances," by Professors Liveing and Dewar.

These notes contain some records of ultra-violet absorptions in addition to those which have been examined by Soret,

Hartley, M. de Chardonnet, and other investigators. For these observations the spark of an induction coil, with Leyden jar, between iron electrodes, was generally used as the source of light. The lines of iron are so multitudinous, and so closely set in a large part of the ultra-violet region of the spectrum, that they form almost a continuous spectrum, at the same time there are amongst them a sufficient number of breaks and conspicuous lines to serve as points of reference. The optical train used was wholly of quartz, and the spectra were all photographed.

Chlorine in small quantity shows a single absorption band extending from about N (3580) to T (3020). As the quantity of chlorine is increased this band widens, expanding on both sides, but rather more rapidly on the less refrangible side. Different quantities of chlorine produced absorption from about H (3968) to wave-length 2755, from wave-length 4415 to 2665, and from wave-length 4650 to 2630. With the greatest quantity of chlorine tried, the absorption did not extend above wave-length 2550.

Bromine vapour in small quantity absorbs light up to about L (3820), and is quite transparent above that. With larger quantity the absorption increases, gradually extending with increase of bromine vapour from L to P (3360); and at the same time there is a gradually increasing general absorption at the most refrangible end of the spectrum beginning at about wave-length 2500; so that the denser bromine vapour is transparent for a band between wave-length 2500 and 3350.

Liquid bromine in very thin film between two quartz plates is transparent for a band between wave-length about 3650 and 3400, shading away on both sides, so that below M on one side and above P on the other the absorption seems complete. The transparency of the liquid film ends on the more refrangible side just where that of the vapour begins.

Iodine vapour tolerably dense cuts off all within the range of our photographs below wave-length 4300, and its absorption gradually diminishes from that point up to about wave-length 4080; from that point it is transparent. Denser vapour produces complete absorption up to 4080 and partial absorption above that point.

Comparing the absorptions of the three haloid elements, the principal band shifts towards the less refrangible side with increasing atomic weight, as Lecoq de Boisbaudran has noticed in the case of lines corresponding to one another in the spectra of groups of similar metals.

Iodine dissolved in carbon disulphide is transparent for a band between G and H, cutting off all above and below. It is not possible to tell how much of the light above M (3727) is absorbed by iodine in such a solution, inasmuch as carbon disulphide is opaque for rays more refrangible than M.

Iodine dissolved in carbon tetrachloride when the solution is weak shows only the absorption due to the solvent, described below. More iodine increases the absorption until it is complete above P (3360), with shading edge as far down as about wave-length 3400.

Sulphurous acid gas produces an absorption band which is very marked between R (3179) and wave-length 2630, and a fainter absorption extending on the less refrangible side to O (3440), and on the other side to the end of the range photographed, wave length 2300.

Sulphuretted hydrogen produces complete absorption above wave-length 2580. Below that a partial general absorption.

Vapour of carbon disulphide in very small quantity produces an absorption band extending from P to T, shading away at each end; no absorption in the higher region. With more vapour the absorption band widens, extending from about wave-length 3400 to 3000, and a second absorption occurs beginning at about wave-length 2580, and extending to the end of the range photographed.

Carbon tetrachloride liquid produces an absorption band with a maximum about R, extending, but with decreasing intensity, up to Q (3285) on one side, and to *s* (3045) on the other. In the higher region there is a second absorption sensible about wave-length 2600, and increasing in intensity up to about wave-length 2580, beyond which point it is complete.

Chlorine peroxide gives a succession of nine shaded bands, at nearly equal intervals, between M and S, with faint indications of others beyond. In the highest region this gas seems quite transparent.

A slice of chrome-alum a quarter of an inch thick is transparent between wave-lengths 3270 and 2830; its absorption gradually increases on both sides of those limits, but rather more rapidly on the more refrangible side than on the other, and becomes complete below about wave-length 3360 and above wave-length 2730.

A very thin plate of mica shows absorption beginning about S (3100), rapidly increasing above U (2947), and complete above wave-length 2840.

A thin film of silver precipitated chemically on a plate of quartz transmits well a band of light between wave-length about 3350 and 3070, but is quite opaque beyond those limits on both sides.[1]

A thin film of gold similarly precipitated merely produces a slight general absorption all along the spectrum.

The difference between the limits of transparency of Iceland spar for the ordinary and extraordinary rays, inferred from theory, was found to be very small, and hardly to be detected without using a considerable thickness, three inches or more, of the spar.

The authors had expected to be able to apply the well-known photometric method by means of polarised light to the comparison of intensities of ultra violet rays. Ordinary Nicol's prisms are not applicable to ultra-violet rays on account of the opacity of the Canada balsam, with which they are cemented, so Foucault's prisms were used. Upon taking photographs of the spectrum of the iron spark through this pair of prisms at various inclinations between the planes of polarisation of the two prisms, it was found that for the whole range between the position of parallelism and the inclination of 80° there was no sensible difference of effect upon the photographic plate, though the length of exposure was in all cases the same. For inclinations between 80° and 90° there was a sensible and increasing diminution in the photographic effect as the planes of polarisation of the polariser and analyser were more nearly at right angles to one another. It seems to follow from this that the full photographic effect on the dry gelatine plates used ensues when the intensity of the light reaches a certain limit, but that for intensities of light beyond that limit there is no sensible increase in the effect until the stage of solarisation is reached.

Chemical Society, March 15.—Dr. Gilbert, president, in the chair.—Dr. Gilbert will resign the presidential chair at the end of the session.—The Council have proposed Dr. W. H. Perkin to fill the vacancy, and Mr. J. Millar Thomson to be Secretary.—The following papers were read:—On some condensation-products of aldehydes with aceto-acetic ether and with substituted aceto-acetic ethers, by F. E. Matthews. The author has studied the following reactions: condensations of aceto-acetic ether with isobutylic aldehyde, valeric aldehyde, chloral furfurol, acrolein; of benzoic aldehyde with aceto-diethylacetic ether, aceto-dichloracetic ether, and aceto-benzilidene-acetic ether, and of benzoic aldehyde with aceto-monoethylacetic either.—Contribution to the chemistry of "Fairy Rings," by Sir J. B. Lawes, Dr. Gilbert, and Mr. Warington. The authors have analyzed samples of the soil inside the ring, on the ring, and outside the ring. The soil inside is much poorer in organic carbon and nitrogen than the soil outside the ring; the soil at the ring itself is intermediate in character as to carbon and nitrogen, but contains a larger quantity of nitrates. The fairy ring fungi seem to derive and assimilate nitrogen from the soil; this nitrogen is eventually deposited as manure at the ring, and becomes available to the associated herbage, which thereby acquires the characteristic dark-green colour.—On lines of no chemical change, by Dr. Mills and Mr. D. Mackey. The authors have investigated the strength at which sulphuric acid ceases to attack zinc at certain temperatures.—On homologous spectra, by W. N. Hartley. The author has photographed and mapped the spectra of various elements belonging to the same homologous series, *e.g.* magnesium, zinc and cadmium, calcium, strontium and barium, &c., especially with a view to finding out whether the striking similarity in such spectra was due to harmonic vibrations of a common fundamental vibration. The author concludes that the data contained in the paper support the view that elements whose atomic weights differ by a constant quantity, and whose chemical character is similar, are truly homologous, or in other words, are the same kind of matter in different states of condensation.

[1] Cornu ("Spectre Normal du Soleil," p. 23, *note*) mentions that such films of silver are transparent for rays about $\lambda = 270$, which is a good deal too high. Chardonnet (*Comptes Rendus*, February, 1883) states that the band extends from O to S. W. A. Miller (*Phil. Trans.* 1863) noticed that a silver reflector failed to reflect a band in the ultra-violet.

Linnean Society, March 1.—Sir John Lubbock, Bart., president, in the chair.—The following gentlemen were elected Fellows of the Society:—W. B. Barrett, L. J. K. Brace, J. B. Bridgman, W. O. Chambers, W. E. Clarke, W. Godden, F. H. H. Guillemard, J. C. Havers, T. M. Hocken, C. H. Middleton Wake, James Stirling, and Rev. P. W. Wyatt.—Two pieces of North American yellow pine were exhibited for Mr. R. M. Middleton, which displayed on their surface a great number of depressions like fine shot holes. These were doubtfully supposed to be produced by insect depredations.—Mr. W. T. Thiselton Dyer called attention to and made remarks on the dried leaves and rind of the fruit of oranges from the Bahamas, partially destroyed by the *Mytilaspis citricola*, Packard.—Mr. R. F. Towndrow showed examples of a new variety of *Rosa stylosa*, obtained at Madresfield, near Malvern, by Mr. A. D. Melin. This variety is evergreen, and its fruits ripen in the second year.—Mr. Alfred W. Bennett read a paper on the constancy of insects in their visits to flowers.—Then followed a communication on the methodic habits of insects when visiting flowers, by Mr. R. M. Christy, see notice (p. 498).—The Secretary, Mr. G. J. Romanes, read some observations on living Echinodermata. He stated that star-fish possess a sense of smell which is not localised in any particular organs, such as the ocelli, but is distributed over the whole of the ventral surface. The function of the Pedicellariæ was shown by some further experiments, corroborative of those already published by him in the *Philosophical Transactions*, to be that of seizing upon and arresting the movements of fronds of seaweed in order to give the pedicels time to establish their adhesions. It was also shown that the righting movements of echinus, when inverted on its aboral pole (which are performed by means of the pedicels) are due to central coordination proceeding in part from the pentagonal nerve-ring surrounding the mouth, and in part from central nerve-matter distributed along the course of the radial nerve-trunks. One of the experiments whereby the fact of such central coordination (depending on a sense of gravity) was proved consisted in rotating an inverted echinus upon a wheel moving in a vertical plane. It was found that whatever phase in the righting manœuvre the echinus might have attained at the moment when the rotation commenced was maintained so long as the rotation continued, but the manœuvre was resumed so soon as the rotation was allowed to cease. The paper concluded with an account of the effects of the various nerve poisons on the Echinodermata.—There followed in abstract the 17th part of the Rev. R. Boog Watson's memoir on the mollusca of the *Challenger* expedition; therein he deals with the family Pyramidellidæ, describing twenty-three new species of the genus *Eulima*, and one of the genus *Stylifer*.

Geologists' Association, March 2.—Mr. W. F. Stanley read a paper upon the possible causes of the elevation and subsidence of the earth's surface. In this he offered an hypothesis that both the rising and sinking of land was entirely due directly or indirectly to the action of our great common motor, the sun. But most particularly for the greatest effects to the elevation of aqueous vapour, and to its after deposition as snow about the poles of the earth. The deposition of snow was assumed at the present time to reach a considerable altitude at the south pole, and in this position by its gravity to react as a pressure upon the interior mass of the earth, which was assumed to be in a highly heated viscous or semi-liquid state, and to be surrounded by a somewhat rigid crust of 200 miles or so in thickness. The crust was assumed to offer a certain amount of resistance to internal and external pressures, beyond which it was deflectable upon or from the viscous interior. The pressures from continued accumulation of snow at the poles acting as an hydraulic pressure upon the interior mass were assumed to be distributed in such a manner as was evident by elevation of land in volcanic and plutonic action, so that the earth could remain approximately under the conditions present, a symmetrical spheroid whose outward figure would constantly represent a natural resultant of the action of gravitation upon all its parts, and of the tangential force of such parts in revolution. It was argued that the stability of the land-surface was entirely due to *permanent elevation* by volcanic and plutonic action, and that if this did not exist the effects of atmospheric denudation would reduce the land surface within moderate geological time to a nearly level swampy plane. It was further discussed that if the interior of the earth is metallic, which has been reasonably inferred from its high specific gravity (about 5·6), then it would consist of a heat-conducting material, so that, beyond the non-conducting coating, which we term the crust, a certain degree of heat would be reached which might henceforth remain uniform throughout the interior mass. The crust would therefore be that portion of the exterior which was oxidised into a non-conducting coating in which the interior heated mass would conserve its heat with little loss. It was further argued that if the interior were a *viscous* mass the reaction of hydraulic pressure upon it, as from great accumulation of ice at either pole, would be made most evident about the most deflectable parts of the crust, so that the central mass might remain static, and if this was assumed by the presence of enormous pressure to form a practically incompressible semi-liquid, it would in this state possess enormous rigidity. Mr. Stanley further discussed the conditions of continuity of volcanic action throughout all time that the earth has existed as a cooling globe with a solid crust accumulating ice at either of its poles, and that the periods of greatest glaciation at either pole would be the periods of greatest volcanic eruption and elevation. Dr. Croll's theory of displacement of the earth's centre by polar glaciation was shown not entirely to coincide with observation, in that the coast of Greenland was sinking, and the coast of Norway, in the same latitude, was rising whereas by this theory of displacement of the earth's centre, the present accumulation of ice at the south pole should cause both of these parts to be rising equally. Mr. Stanley held that the cause of the coast of Greenland sinking was the weight of the present accumulation of ice upon that continent, which represented on a small scale a polar pressure system such as he had discussed.

Royal Horticultural Society, March 13.—Sir J. D. Hooker, K.C.S.I., in the chair.—*Potato-disease*: Dr. Masters read a portion of a paper on this subject forwarded to him by Mr. A. Stephen Wilson, and having especial reference to the "sclerotia" which Mr. Wilson has discovered in nearly all the organs of the adult plant, as well as in the seedlings and tubers. The sclerotia are supposed to germinate and lie in a state of incubation in the haulm. Ultimately they give rise to the conidial threads. The conidia form, according to circumstances, either (1) zoospores, (2) plasm granules, or (3) secondary conidia. These are succeeded by oospores and a non-parasitic mycelium, from which latter, as it creeps through the soil, are thrown out "floats" and specks of the seminal plasm. The seed-tuber comes into contact with the plasm in the soil, which is absorbed and becomes developed in the shape of sclerotia, and thus the life-cycle is completed. From the tuber or seed to the conidia is the parasitic arc. From the conidia to the tuber is the non-parasitic arc. The author illustrated his position by what happens in the case of cereals, wherein the plasm, say, of smut or rust, is absorbed by the cells of the scutellum or cotyledon, passes through a period of gestation and then germinates. Mr. G. Murray observed that a microscopical examination did not clearly reveal any organic connection between the sclerotia and the peronospora mycelium, and thought that possibly they might prove to be glandular bodies of some kind, and belonging to the potato itself. Moreover they could not be true sclerotia in the fungoid sense, as the latter are a compact mycelium.—*Retinospora pisifera* and *R. plumosa*: Mr. Noble sent a specimen exhibiting sprays of both of these supposed species on the same plant. Dr. Masters remarked that the latter is the young form, while the former is the adult, and that a microscopical examination showed a correspondingly different distribution of the stomata, being more numerous in *R. plumosa*.—*Juniperus Chinensis*: He also sent a male spray taken from a female plant; the sexes in this species being normally quite distinct.—*Garrya elliptica grafted on Aucuba Japonica*: Mr. Noble forwarded a specimen showing the stock and the graft united. Mr. Henslow observed that this was an instance where physiological affinity corroborated the morphological; in that while Endlicher had placed Garrya between the hop and the plane, Bentham and Hooker assigned its position in the "Gen. Plantarum" next to Aucuba; but the discovery of its power of grafting on Aucuba was purely accidental, having been made by a gardener in Mr. Veitch's nurseries.—*Carica*, hybrid: Mr. Green, gardener to Sir G. Macleay, sent ripe fruits and foliage of a plant grown from seed furnished by M. Van Volxem of Brussels. It is a hybrid of the second generation, the first being raised from *C. erythrocarpa*, impregnated with the pollen of *C. cundinamarcensis* (from Colombia). From the fruit of this cross seedlings were raised, which were impregnated with pollen from the last named species, or from the hybrid itself. Some of the fruits supplied by Mr. Green contained apparently good seed. Mr. Henslow has tried the effect of the foliage on meat, that of the "Papaw,"

C. papaya, having the well known property of rendering it tender. He wrapped a piece of steak in a leaf for twenty-four hours, and it was quite effectual in softening it, and when cooked was pronounced excellent, though some thought there was a somewhat peculiar flavour as compared with a similar piece not wrapped up.

MANCHESTER

Literary and Philosophical Society, January 9.—H. E. Roscoe, F.R.S., &c., president, in the chair.—Dr. Joule said that he had, in December, 1882, made a fresh determination of the freezing-point in a sensitive thermometer constructed thirty-nine years ago. During that time the point had risen about 1° Fahrenheit, and although now rising very slowly, was not even yet quite stationary, having risen 1/40 of a degree Fahrenheit since November, 1879.

January 23.—J. P. Joule, F.R.S., vice-president, in the chair.—Remarks on the simultaneous variations of the barometer recorded by the late John Allan Broun, by Prof. Balfour Stewart, F.R.S.—A paper was read entitled "Jeremiah Horrox and William Crabtree, the Observers of the Transit of Venus in 1639," by Mr. John E. Bailey, F.S.A.

February 6.—Prof. Balfour Stewart, F.R.S., in the chair.—Note on the vapours of incandescent solids, by Henry Wilde.—Remarks on Prof. Osborne Reynolds' paper on isochronous vibrations, by Robert Rawson, Hon. Member. Assoc. I.N.A., Mem. of the London Mathematical Society.

February 20.—H. E. Roscoe, F.R.S., &c., president, in the chair.—Mr. R. D. Darbishire, F.G.S., read a note upon the Mammoth Cave, by Mr. G. Darbishire.

BERLIN

Physiological Society, February 23.—Prof. Du Bois Reymond in the chair.—Prof. Lucae, induced by the perception of a low noise when, in the open country, a strong wind blew against his ear, has long experimentally studied this phenomenon, investigating sounds and noises which arise on blowing into the external auditory meatus. He observed in normal ears which were closed with a sound tympanic membrane a moderately high noise, the pitch of which could not exactly be determined. When the tympanic membrane was stretched, the noise was somewhat higher and piping; when, on the other hand, the tympanic membrane was broken through or was absent, so that in the experiment the large air-space formed by the middle ear with the large cellular air-spaces beyond was blown into, he then heard a very deep noise. This great difference between the proper tone of the external auditory meatus and that of the large irregularly-formed air-space behind the membrane Prof. Lucae has verified both in all suitable patients and in dead bodies. An estimate of the relation of resonance of the ear cavities was obtained when, upon a spherical resonator which gives the tone c_i on blowing, a short open cylinder was placed, which, blown into separately, gives the tone c_{ii}; when this combination was jointly blown into, the considerably deeper tone *H* was heard. When, however, between sphere and cylinder, a stretched membrane of caoutchouc was introduced, and the system blown into, there was heard again a higher tone, f_i. The influence here exercised by the degree of tension of the membrane could not be determined. To bring this schema of the air-spaces of the ear still nearer to the natural conditions, dry sponge was placed in the spherical resonator, the cavities of this material corresponding to the bone cells communicating with the middle ear; the pitch of the tone on blowing was not thereby much altered. The determination of the proper tone of the tympanum and the influence of these conditions on audition are further engaging the author's attention.—Dr. Pohl Pincus had explained at a previous meeting of the Society that in the non-vascular frog heart two groups of muscular fibres with different action must be distinguished. The one class of fibres surrounds the fissures of the heart-wall, which perform the function of the vessels and admit the nutritive liquid to the tissue (vessel-muscles); the others, by their regular contractions and dilatations, act in the way of moving the blood (proper heart-muscles). The contraction of the first kind of muscles closes the fissures and produces paleness of the heart-wall, and their dilatation opens the fissures, lets the blood penetrate into the substance of the heart, and reddens the heart-wall; while the action of the second group of muscles produces systole and diastole of the heart. Now the actions of these two kinds of muscles—the heart-vessel muscles and the proper heart-muscles—are not simultaneous and similar under the influence of local stimuli, removal of the brain, section of the spinal cord in different places, and poisons; sometimes the heart-walls were observed to be pale in diastole and deep red in systole, and there were various other local differences of behaviour. This led the author to seek also an anatomical difference of the two groups of muscles, and he found one such on microscopical examination, for the proper heart-muscle fibres were cross-striped throughout and had long cell nuclei, whereas in the others the cross-striping did not comprise the whole width of fibres, and the nuclei were oval. With this anatomical difference the different mode of reaction of the two kinds of muscles and their different function is intelligible.

VIENNA

Imperial Academy of Sciences, January 4.—The following papers were read:—G. Haberlandt, on the physiological anatomy of milk-tubes.—T. Wiesner, on the entering of the winter-buds of creeping blackberry-shoots into the soil, and on the mechanical cause of this process.—F. Rathay and B. Haas, on Phallus and Caprinus.—A. v. Obermayer, on diffusion of gases (third paper).

January 11.—F. Enrich, on the action of bile acids on albumen and peptones, and on their antiseptic effects.—T. Haubner, on the logarithmic potential of an uninsulated elliptic plate.—A. Lieben and S. Zeisel, on the products of condensation of propion-aldehyde and its derivatives.—F. Anton, determination of the orbit of the Cassandra planet (114).—T. Ehrmann, on the formation of adipose tissue by the fat-organs, named winter-sleep-glands.

January 18.—C. Rabl, contribution to the history of development of Prosobranchiata.—F. Brauer, systematic studies based on the Diptera-larvæ, with a description of new species (third part).—R. Andreasch, on the oxidation of bases obtained by the action of halogen-compounds on thio-urea.—T. Freydl, note on the dry distillation of tartaric and citric acid with an excess of lime.—C. Pelz, on the determination of the outlines of warped screw-planes.—G. Goldschmidt, on the products of decomposition of the anhydrides of salicylic acid by distillation.—F. N. Dafert, on amylbenzol.

February 1.—W. Biedermann, contributions to general nerve and muscle physiology (tenth communication); to the knowledge of secondary contraction.—A. Belohoubek, on crystallised potassium hydroxides.—T. Blaas, contributions to the knowledge of natural water containing double sulphates.—T. Hepperger, determination of the orbit of Schmidt's nebulæ.—M. Kretschy, on the oxidation of kynurine and kinurenic acid.

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THURSDAY, APRIL 5, 1883

FIRE-FOUNTAINS

Fire-Fountains; the Kingdom of Hawaii, its Volcanoes, and the History of its Missions. By C. F. Gordon Cumming. In two vols. 8vo. (Edinburgh and London: William Blackwood and Sons, 1883.)

MISS GORDON CUMMING has, in the work before us, given a most lively and interesting account of the Sandwich Islands. The large amount of experience which she has gained during five years of almost continual travel among the islands of the Pacific has enabled her to make careful comparisons between the physical features, the productions, and the populations of the different groups. In her two previous works, "A Lady's Cruise in a French Man-of-War," and "At Home in Fiji," our authoress has given us her impressions of Tahiti and the Fiji Islands respectively.

It is evident that Miss Gordon Cumming's first sentiments on arriving in the islands were those of disappointment. In productiveness, in the picturesque character of their scenery, in the beauty of their coral reefs, and in richness of flora, the Hawaiian Islands must certainly yield the palm to the Archipelagos of the Pacific. Even Kilauea itself failed to satisfy the traveller's expectation, for at the time of her first visit the fires of Halemaumau seemed to be almost extinct. Fortunately these first feelings of disappointment were to some extent removed by what the authoress subsequently witnessed during her long sojourn in the country.

The title of "Fire-Fountains" may perhaps lead a geologist to anticipate a more than usually exact account of the volcanic phenomena of these interesting islands. The extreme liquidity of the Hawaiian lavas enables them—as Dana, Brigham, Coan, and others have so well shown—to be thrown up into actual "fountains," and such jets have been witnessed both in Kilauea and Mauna Loa, rising to the height of several hundred feet. Any expectations of scientific accuracy in the account of the volcanic phenomena are, however, dispelled when we turn to the work itself. Miss Gordon Cumming's descriptions are wonderfully graphic, and a small amount of geological training would have enabled her to avoid popular errors, and employ accurate instead of misleading terms, thus making them valuable records of the phenomena she witnessed. Unfortunately, as in so many similar cases, this small amount of previous training was wanting.

The first part of the work consists of descriptions of the physical features of the group and of the characteristics of the inhabitants, and here the authoress largely relies upon her own observation, and furnishes us with many instructive comparisons with Tahiti and Fiji.

The second part of the book, which contains a history of the islands and of missionary enterprise in them, is of course compiled from published works, the information thus acquired being supplemented by facts derived from independent sources, such as letters and conversations.

The visit to Kilauea has been so often described that it may seem difficult to understand how any ordinary traveller can find anything new to say on the subject. But Miss Gordon Cumming had the good fortune (though she does not seem to have appreciated it at the time) to see the crater under somewhat exceptional conditions, as the following account will show (vol. i. pp. 164, 165):—

"After traversing three miles of this strangely varied lava-bed we reached the base of that inner circle of crags which within the last few months have been thrown up all round the central crater—*i.e.* the Halemaumau. So rapidly have they been upheaved, that they now form a ring 600 feet in height; and up this steep ascent we had to climb in order to look into the Lake of Fire.

"It was a toilsome ascent over very brittle lava; but Roback kept cheering me by telling me what a grand sight awaited me, and that he had never seen the lake in finer action than last week. So we climbed over coils of huge hollow vitreous lava-pipes, which constantly broke beneath our weight, and over ridges which looked to me like gigantic sugarsticks pulled out and twisted—and at last we gained the summit, and looked eagerly for the much-described Lake of Fire.

"THERE WAS NONE! at least nothing worth speaking of, in the first instance. I turned to look at my guide, and he stood staring in stupefied, bewildered amazement. He could not believe his own eyes. Only a few days had elapsed since he had led a party of Americans to the very spot where he now stood beside me in speechless wonder at the change.

"They had watched the blood-red waves dashing in scarlet spray against the cliffs on the farther side of the lake of molten fire, then rushing back to form a mad whirlpool in its centre, and thence, as if with a new impulse, flinging themselves headlong into a great cavern which undermined the lava-terrace just below the spot where I was now standing."

This was written on October 29, 1879, but three days afterwards the authoress has a very different state of things to chronicle (vol. i. pp. 186-189):—

"*November 1st.*

"Last night was Hallowe'en—the great fire-festival of our ancestors—and here it has been celebrated in right royal style, for the fire-spirits have broken loose and are holding high revel.

"The flow is increasing rapidly and is magnificent. The fire has burst out at so many points together that it has formed a new lake in the outer crater, in which fire-jets are spouting and molten lava thrown high in mid-air, great masses of red-hot solid lava being tossed to a height of from forty to fifty feet, while from the overflowing rim, or from weak points in the sides of the lake-basin, flow rivers of lava, forming a network of living, rushing fire, covering fully two square miles of the very ground over which I was walking only two days ago. It is a scene of marvellous beauty and is inexpressibly fascinating.

"From the edge of the crater-wall I have watched each stage in the growth of this strange new lake. I have seen it gradually rise higher and higher, till at last it overflowed in glowing streams, like rivers of golden syrup, but brighter far—an indescribable colour. The centre of the lake is oftenest of a silvery grey, only crossed by zigzag lines of flame colour and deep rosy red; but all round its shores it is continually surging and upheaving great crested billows, which break in fiery surf and toss up clouds of fire-spray. Sometimes the whole lake appears to be in a tremendous commotion—heaving and trembling as if acting obedient to some pressure from the furnace below.

"About a dozen cones have formed in and around the lake, each a distinct fire-fountain, yet all flameless—only merrily flinging about the molten metal: a bouquet of rare fireworks.

"These cones are miniature volcanoes—spouting liquid lava in the most sportive manner, playing gracefully like true fountains—spouting like intermittent geysers, and

falling in showers of red hail—sometimes silently, sometimes with puffing and spluttering, v ried with a roar like an angry bull; then a hush, followed by low moaning sobs.

"Some of these explosive forces have not built themselves chimneys, or, if they have, the lake has melted them, for they only betray their existence by suddenly bursting beneath the surface, like torpedoes, and tossing up red rockets.

"From the crag above I looked down upon a heaving, restless expanse of dull red almost entirely coated over with a silvery-grey scum, intersected by flowing rivers of red gold. The ceaseless movement beneath the surface kept up a glancing, gleaming play of white and red light, glistering like quicksilver in motion. Sometimes there came a swirling eddy, like the rush of a Highland stream.

"Then, again, the lava seemed to writhe and twist as if in agonised contortions, and then commenced a violent boiling and bubbling preparatory to its bursting into active fire-fountains. These play sometimes singly, sometimes alternately, sometimes a dozen burst into simultaneous action—like some marvellous display of rockets, flinging their fiery rain on every side, then dying away altogether, till the silvery coating spreads so evenly over the surface of the lake, that, but for the sulphureous exhalations and columns of smoke, it might almost be mistaken for some cool refreshing pool. In truth, the white vapours which play so eerily among those black rock-masses, might well be morning mists floating upward from a quiet mountain-tarn.

"This, however, is a delusion not to be cherished for long, especially towards sunset; for then the lake appears in its true glory, and all the wonderful chemical colours which were lost in the full light of day reveal themselves, the difference of the scene before and after sundown being that of any huge smelting works, as seen by day or by night, only magnified ten thousand times. Then the scale of colour varies from deepest chocolate, crimson, and scarlet, to orange, yellow, and primrose tints, and the silvery grey becomes tinged with pink and violet, while the solid rocks become ever more intense in their blackness; and the many-tinted sea plays around them, and dashes over them, and from time to time detaches some huge fragment, which falls with thunderous crash, reverberating from crag to crag.

"As the twilight faded away, my kind landlord rigged up blankets and lanterns to make me a snug sketching-point on the hill above this house, whence I could watch the glory undisturbed, and attempt to preserve notes in colour, which may give you and others an idea, however faint, of the amazing scene before me. A full moon added its cool, pure light to the lurid crimson glow, which was reflected on all the overhanging clouds, as well as on the column of white steam which for ever rises from the Halemaumau itself; and these clouds, being visible at a distance of many miles, must have declared plainly to our friends in Hilo that there was unusual activity at Kilauea."

The authoress of this work did not reach the summit crater of Mauna Loa, but at the end of her book she has collected from various sources a tolerably complete account of the great outbursts of 1880 and 1881.

The details given in this volume concerning the aboriginal inhabitants and their manners and customs—or rather, we should say, of the total want of the former and the utter "beastliness" of the latter—is interesting to the anthropologist. The judgments of the authoress upon historical questions are by no means unfair, and if she does not follow American writers in treating Capt. Cook's visit as an act of piracy and his fate as a just retribution, she clearly points out that the death of the great navigator followed as a natural consequence of the sad misunderstanding between the English and the natives. From the traditions of the natives we can now fill in many details of the story, and explain certain matters which Cook, in his total ignorance of the language of the people, could scarcely guess at. In this and in the subsequent transactions between the English under Capt. Vancouver, and the Hawaiians, it must be confessed that the natives were treated with but scant justice at the best, and in too many instances with wanton cruelty and tyranny.

The admirable illustrations of this work constitute one of its most valuable features. They are reproduced by the autotype process from the sketches of the authoress. The frontispiece, showing the low rounded dome of Mauna Loa, with Kilauea on its flanks, is one of the best representations of this most wonderful district which we remember to have met with. The indefatigable traveller who has now become an acknowledged favourite with the public may be heartily congratulated upon the success of thi latest production of her busy pen and pencil.

OUR BOOK SHELF

Africana, or the Heart of Heathen Africa. By the Rev. Duff Macdonald. 2 vols. (London: Simpkin, Marshall, and Co., 1882.)

NOTWITHSTANDING a large amount of professional commonplace, this work rises considerably above the level of ordinary missionary productions. The author, who administered the Church of Scotland Mission at Blantyre, south of Lake Nyassa, during the years 1878-81, applied himself diligently to the study of his dusky flock, and has embodied his experiences chiefly in the first volume, devoted to the "native customs and beliefs." The second is occupied more specially with "mission life," and with the inevitable difficulties and troubles entailed upon the writer in consequence of his accepting a position which from the first he felt to be untenable.

Since his enforced retirement from active work, Mr. Macdonald has usefully occupied his time in arranging for publication some of the rich materials collected during his stormy missionary life. Most of these materials, being the result of original observation in a new field not yet disturbed by contact with Europeans, possess great scientific value. The descriptions of the native manners, customs, beliefs, superstitions, and traditions are as interesting as they are trustworthy, and they are supplemented by two appendixes, which may be specially commended to the attention of all lovers of folk-lore. These comprise numerous selections of original "native tales" and "cosmical tales," literally translated from the author's manuscript collection of tales, songs, enigmas, &c., the whole of which it is to be hoped he will be induced to publish. Some of the tales accounting for natural phenomena have at least the merit of brevity, as, for instance, that about the wind: "A great man had a daughter, and she said, 'Father, in this country I am hot, I sweat.' Then her father said, 'Come here, my child, I have pity, I will blow with my breath.' So he blew, and thence came wind" (i. 283).

It is sad to learn that trial by ordeal and torture is still as universally practised as it was in Europe during mediæval times. "When a Magololo suspects his wives, he places a stone in a jar of boiling water or oil, and orders them to fetch it up with their bare arms. He then judges of their guilt by the amount of injury they sustain. When a woman is thus convicted, he makes her confess who seduced her. In vain does the helpless creature protest that she is innocent. Notwithstanding that her arm is severely scalded, she is subjected to the most cruel

torture by a kind of thumbscrew (*mbanilo*), which is applied to her head. A small tree is partly divided along the middle, the skull of the poor woman is inserted as if it were a wedge for splitting the tree still farther. Great pressure is exerted by forcing the halves of the tree together with the aid of pulleys" (i. 201). This of course has the wished-for effect, and as in the "processus inquisitorii," the wretched victims "dum propria sua confessione contra sui pugnare coguntur sui ipsius proditores tortæ constituuntur."

A. H. K.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Natural Selection and Natural Theology

I READ with interest, in NATURE, vol. xxvii. p. 362, the reply made by Dr. Romanes to a letter of mine which, although not originally addressed to a scientific organ, found hospitable reception in your columns. It was not much out of place there, for it was essentially an inquiry whether certain inferences may or may not *scientifically* be drawn from certain premisses. I am not wholly without hope of making it clear that the criticisms which I ventured to bring forward are grounded in reason; and confining my rejoinder strictly to the issue joined, I may hope not to be long nor very tedious. Let me trust that no curtness of statement will imply any want of the great respect which I entertain for an able investigator and writer, whose view may be imperfectly apprehended, or may bear an interpretation I should accede to.

The issue is a narrow one, and there is no need to widen it. Dr. Romanes is understood to derive from scientific premisses the conclusion that evidence of design is not legitimately derivable from the structure and adaptations of plants and animals, and, more particularly, that the theory of natural selection has destroyed the evidence of special design in organic nature, so that now the facts of organic nature furnish no other and no better evidence of design than do the facts of inorganic nature.

The first of these conclusions was derived from the proposition that there is no point of logical contact between natural science and the idea of design, wherefore no inference can legitimately be carried from the facts of the one to the conceptions of the other. I suggested that the maintainer of that position could not consistently argue that a particular scientific theory has annihilated an inference admittedly beyond its logical range. The reply is that, "If a man believes that there is no logical connection between one thing and another, I do not understand why he should be deemed inconsistent because he endeavours to show the fictitious character of the logical connection which has been erroneously supposed to exist." But the point of the objection was that, while insisting that any inference from the one to the other was invalid from the nature of the case, he actually inferred that certain scientific facts and theories completely overthrow and destroy the theory of particular design in organic nature. This may be. Only one would think that whatever may be legitimately overthrown may be as legitimately supported.

Moreover, if I rightly understand, there was not long ago a legitimate ground of inference (whether scientific in the narrower sense or philosophical need not here be inquired) from organic nature to design. "For it would be proof positive of intelligent design if it could be shown that all species of plants and animals were created"; and therefore proof presumptive while the theory of special creation was accepted and probable. At least—and this is the point—the argument from structure and adaptation to design was then admissible and even cogent.

Now, from the scientific side, upon which we are standing, special creation means only that the forms were scientifically inexplicable, and to be taken as original; their adaptations to their surroundings and their relations of means to ends in themselves equally as primary endowments. And whatever evidences of intellectual origination these manifested, *were seen in the things themselves*, and we suppose are to be seen there still. The inference was not one from an intellectual originator to design in the organic world, but from marks and operations in the latter which indicated design to an intellectual originator. The inference to most minds was convincing; at least it was legitimate. The recognised laws and operations of nature—a better knowledge of which has destroyed so many crude notions—were not thought to interfere with it.

It used to be so, but we have changed all that. How?

First, by the declaration of the principle that the facts of organic nature, in all their multiplicity and variety, yield no other and no better indications of design than do any of the facts of inorganic nature. That is to say, a stratum no more than a structure, a crystal than a chrysalis, living things and their responses than lifeless things simply acted upon, things which are intelligible only when contemplated as means and ends, no more than things of which ends are predicable, if at all, only by remote implication. Not only is the one as good as the other, but any one is said to be as good as all. Because of "the universal prevalence of laws and sequences of cause and effect, . . . they are not really or logically strengthened by a mere enumeration of particular instances. . . . The so-called law of causation as a whole being known, and its universality recognised, its true argumentative value to the theory of theism is not influenced by the explicit formulation of any number of its specific cases."

Here "law of causation," or the way how something comes to pass, is mixed up with "evidence of design," or what it was for. And we are to conclude that the immense variety and multiplicity of adaptations of particular means which accomplish particular ends in organic nature bring no contributory and cumulative evidence as to there being any design in them. In palliation of the charge of "damnable iteration," to which the teleologists are thus exposed, it may be pleaded that, although possibly one good witness or one good observation may be as convincing as many for certifying a fact, surely the more and the more varied the better for proving an underlying intervention—of which the evidence must always be circumstantial, and the conclusion a judgment or belief.

The old belief that adaptation of means to ends in plants and animals gives evidence of intellectual origination, had not been seriously unsettled by the scientific belief of the universality of the law of causation. It remains to be seen whether it will survive the establishment of the belief that the forms in which these adaptations are recognised have themselves been slowly evolved and diversified in a way that is partly explained by the doctrine of natural selection; and this is the gist of the question.

Dr. Romanes thinks that we have, in natural selection, "a cause other than intelligence competent to produce the adaptations," one which supersedes intelligence by working gradually. For, "if the adaptations have been effected gradually, and by *the successive elimination of the more favourable variations by a process of natural causation*, we clearly have a totally different case to contemplate, and one which is destitute of any evidence of special design." "The progressive adaptations of structures to functions by such a purely physical cause as natural selection, when once clearly revealed, must destroy all special or particular *evidence* of design, even supposing such design to exist." This phrase, "such a purely physical cause as natural selection," and the preceding phrase italicised by its author as specially significant and as being its equivalent, show that the term is used in its strict sense. So the substitute for intelligence, that which is said to account for all the adaptations in living nature, is the successive destruction of the less favourable variations by natural causes, leaving the most favourable to survive! Here "we clearly have a totally different case to contemplate, and one which is destitute of any evidence of special design,"—equally destitute, one would say, of any pretensions to act as its substitute until it is explained how the physical destruction of a part should have set the rest into varying at all, into varying advantageously, and into varying into the very special ways they have done. Not till this, or something like it, is done, can natural selection pure and simple claim to give scientific explanation of the adaptations and the forms at whose birth it has assisted.

When I before insisted that "to make the purely physical explanation tenable it must be shown that natural selection scientifically accounts for the adaptation," and that it has not done this, that no reasons have been given why the organisms

must have responded in the ways they do, or have responded at all to the environment, I meant only that the theory ought to fulfil the conditions which other physical theories are bound to satisfy, *i.e.* to account for the principal facts of the case. I had no reference to any subsidiary hypothesis which might help the matter. Dr. Romanes rightly says that it lies not with the evolutionist to show that variations may not have been intellectually planned or guided. But when he assigns the whole results to known physical causes and discards the factor of intelligence, he is bound to render their adequacy at the least conceivable.

It may now be seen, I trust (and the context might have made it clear), that, in asking Dr. Romanes if he was quite sure that any other cause than intelligence could adapt organisms to their environment gradually, I was not inviting him to guesses "about the possibilities of supernatural creation," but to a reconsideration of his antithesis between special (and as he will have it, sudden) creation, requiring intelligence, and gradual evolution, which might dispense with it; and I was intimating that he had not shown how the latter could dispense with it. The problem was: Given plants and animals with certain structures and certain adaptations to their environment, to be changed into other forms with other structures equally well adapted to a more or less changed environment, how to do this solely by the action of said environment. Answer: By the killing out of all which have not somehow or other acquired the particular structure and adaptation they needed.

But now comes an important qualification: "The evolutionist may freely admit that natural selection has probably not been the only physical cause at work, and even that the variations supplied to natural selection may not have been wholly fortuitous, but may have occurred along favourable lines as responses of the organisms to their physical surroundings"; and Dr. Romanes calls my attention to a statement of his that it may be so in an essay which I regret that I have not read. He continues, however: "But such admissions would make no change in the logical aspect of the case; for, however many supplementary causes of this kind we may choose to imagine as possible, the evolutionist is bound to regard them as all alike in this: that they are of a physical or natural kind."

"Physical or natural kind." The agency which explained away all implication of design was in the strict sense physical, being the action of the environment on the organisms. It is now extended to whatever is *natural*, that is, to whatever occurs in the course of nature, presumably under established laws; and it is assumed that whatever so occurs is thereby void of all *evidence* of intellectual intention (we need not regard the difference—if any there be in such relations—between general and special design, the question being wholly one about the grounds of any *evidence* of design in nature). To me it is wholly probable that existing species and their special adaptations became what they are in the course of nature. And my argument is that, if "such a purely physical cause as natural selection" leaves these adaptations still unaccounted for, whatever implication of designed origination there formerly was still holds, and may hold, although the series of natural causes be practically endless.

Then as to such causes being all of a piece, so that pure physics may explain all biology. Doubtless in a certain sense all nature is of a piece. But in another sense—the very one we are concerned with—it is of at least two pieces; no matter how it came to be so. One of them is pervaded by an element of its own—that of *direction of action to ends*—which is more and more manifested as we rise in the scale of being, but is characteristic of all organisms. That seems to lay a foundation for a difference in the quality of the "inference which can be drawn by the human mind [*quoad* design] from the province of natural science" This difference might have made Dr. Romanes hesitate to draw, from scientific premisses, the downright conclusion that "the facts of organic nature present no evidence of design of a quality other or better than any of the facts of inorganic nature."

Here lies our whole contention. We agree that natural science leaves aside the question whether evolution and design in nature are compatible or not, this being only a phase of the enigma which was as puzzling before evolution was dominant as it is now. We suppose, too, that the difficulty of conceiving how design can coexist with the natural evolution of organisms is fairly balanced by the difficulty of conceiving how the phenomena of organic nature can be accounted for without it. The point which we have laboured over is that, if science has no call to settle the question, it has none to prejudge it. It was only because Dr. Romanes seemed to me unwittingly to have done so, that I ventured the criticisms which opened this discussion.

Cambridge, Mass., U.S. ASA GRAY

P.S.—A brief note upon Mr. Hannay's letter, NATURE, vol. xxvii. p. 364, referring to my supposition of successive generations slowly changing, "*yet always so as to be in compatible relations to the environment.*" He remarks, this "is just such a statement as 'Design' would require, but cannot be held by scientific evolutionists, otherwise why are there so many extinct species?" Surely it could be held by the soundest of evolutionists, for it is of the very essence of Darwinism. Are not the individuals which compose the present fauna and flora in compatible relations to the environment, and is not the extinction of species going on? In human society do we consider that the unmarried and the childless members of the community are not in compatible relations to their surroundings? Is there any reason to suppose that the individuals of a flora of earlier times—say of the Miocene—were not on the whole in as orderly and compatible relations as the existing flora is? It is not *chaos* but *cosmos* that true Darwinism has in mind, common though the contrary impression be. A. G.

PROF. ASA GRAY is kind enough to remark that he has read my reply to his previous communication with interest. I should like to say, *in limine*, that I have read his reply to me not only with interest but with profit; for it is not often that one meets with an argument so carefully thought out and so clearly presented. Therefore, if I seek to meet his further criticisms, it is not in any spirit of controversy that I do so, but solely for the sake of endeavouring to help, so far as I am able, in determining the true logical position of an important question.

This question, as Prof. Gray observes, is a narrow one, and I shall keep to it. Without therefore trespassing upon the wider question of Theism as a whole, our discussion is confined to "an inquiry whether certain inferences may or may not *scientifically* be drawn from certain premisses."

First, I have to meet the dilemma which is put to me when I am told that, having said there is no point of logical contact between natural science and natural theology, I ought not forthwith to say that natural science is competent to destroy an inference belonging to natural theology. But in stating it as my opinion that natural science had shown the inference previously drawn to be invalid, I did not myself, as my critic asserts, draw any inference (even of a negative kind) from natural science to natural theology; I merely endeavoured to point out that an inference previously drawn from the one to the other was illegitimate, that inasmuch as the inference proceeded from natural science it was liable at any time to be overturned by natural science, and that it had now actually been overturned. Whether or not, therefore, I was right in saying that there is no point of logical contact between natural science and natural theology, at least I did not myself endeavour to institute such contact.

But I am told, you admit that long ago the inference in question was valid, and even cogent. Well, I answer in one sense it was, but in another and a truer sense it was not. For its cogency arose from the hypothesis of special and sudden creation on which it rested; grant this hypothesis, and the inference from organic adaptation to intelligent design becomes not only cogent but inevitable. The hypothesis, however, was not one that really belonged to natural science, and it was just this hypothesis that constituted the "fictitious logical connection" alluded to in the passage which Prof. Gray quotes from my previous letter. The facts presented by science remain, of course, very much the same as they were; but it does not follow that, in the absence of the special creation hypothesis, "whatever evidences of intellectual origination these manifestly were *seen in the things themselves*, and we suppose are to be seen there still." Let us take an illustration. In the last issue of NATURE there is a letter from Prof. Darwin describing the formation of mudballs by a suitable and rare combination of natural causes. He and his brother did not see these balls in process of formation, and therefore he says, "On seeing the first one or two, they looked to us like the handiwork of some boy with an enthusiasm for mud pies"; but their number and the constancy of their situation on the slopes of hills—*i.e.* further knowledge of the inferred conditions of their origin—afterwards disposed of the teleological hypothesis in favour of a physical one. Now here it is equally true that "whatever evidences of intellectual

origination these manifested were *seen in the things themselves*," and after the hypothesis of their physical origin had been arrived at, were "to be seen there still." Yet we should have deemed the brothers Darwin very unworthy representatives of their family if, after having arrived at the physical hypothesis, they had continued to argue in favour of a teleological enthusiasm for mud pies, on the ground that "the inference was not one from an intelligent originator to design in the (in-)organic world, but from marks . . . in the latter which indicated design to an intelligent originator." In other words, a change in the hypothesis concerning the *origination* of the mudballs entirely changed the logical cogency of the teleological inference.

Now I have purposely chosen this illustration because it is of so simple a character, and therefore serves in a clear manner to show how greatly a teleological inference may be modified by a change of hypothesis concerning the mode of origin of a structure, even though the structure remains the same; if there had been no evidence of a purely physical mode of origin in this case, it might truly have been said of the teleological interpretation, "the inference to most minds was convincing; at least it was legitimate." Of course in organic nature the apparent marks of design "in the things themselves" are much more numerous, varied, and complex than any that we meet with in inorganic nature; but no matter how numerous, varied, and complex such marks of design may be, if we see good reason to conclude that they have *all been produced by physical causes*, they are no more available as *evidences* of special design than are the mudballs—although both they and the mudballs, being alike formed under an orderly system of causation, may be due to a general design pervading the cosmos. And here I understand that Prof. Gray is in agreement with me, for he says that when I assign the whole results to known [or unknown] physical causes and discard the factor of intelligence, I am bound to render their adequacy at least conceivable. This appears to show that Prof. Gray is at one with me in holding that physical causes as such do not constitute other or better evidence of design in the organic than in the inorganic world; and it is only because he cannot conceive how such causes are adequate to produce the results observed in the former that he deems these results unique as evidence of "the factor of intelligence." In other words, supposing for the sake of argument that all these results have been due to purely physical causes, and supposing further that all these causes were as perfectly well known as the less complicated physical causes of the inorganic world, then I take it Prof. Gray would agree with me in saying that under such circumstances the former would constitute no other or better evidence of design than the latter.

If so, our only difference resolves itself into a difference in the estimate which we respectively form of the probable adequacy of purely physical causes to produce all the results which are observable in organic nature. To me the probability appears overwhelming that in respect of method "all nature is of a piece," and therefore that the terms "physical" and "natural," when applied to causation, are logically, as well as etymologically, convertible. To Prof. Gray, on the other hand, the probability appears to be that such is not the case, but that, when we meet with the "*direction of action to ends*," we have special evidence of "the factor of intelligence," which therefore makes nature "of at least two pieces," and so makes the term "natural" to mean more than the term "physical."

Supposing that I am right in understanding this as the only difference between us, I may point out that if, while following my ideas of probability, I have erred on the side of rashness in drawing "the downright conclusion" that the facts of organic nature present no other or better evidence of design than the facts of inorganic, Prof. Gray, in following his ideas of probability, can scarcely be able to shut out the suspicion (more especially in view of abundant historical analogies) that, in resorting to "the factor of intelligence" as a hypothesis wherever physical causation is found to be complex or obscure, he may be merely supplementing our present ignorance of such causation by an inference which is at least as rash as my statement.[1] And here I should like to observe, with special reference to the natural or physical causes summed up in the term "natural selection," that although I speak with all the respect which I sincerely feel for so distinguished a naturalist and so able a dialectician, I am not able to follow Prof. Gray in his understanding of this subject. For he says of the theory of natural selection that it is destitute of any pretensions to act as the substitute of the theory of special design, "until it is explained how the physical destruction of a part should have set the rest into varying at all, into varying advantageously, and into varying into the very special ways they have done." But surely it is no part of the theory of natural selection to suppose that the *physical destruction* of unfit organisms is, or has any need to be, the *cause* of advantageous variations arising in other and allied organisms. The theory merely supposes that variations *of all kinds and in all directions* are constantly taking place, and that natural selection seizes upon the more advantageous. Therefore, so far as this theory is concerned, there is no call to explain why promiscuous variation occurs; it is simply a fact that it does occur, though not necessarily *made to occur* by the destruction of other organisms. Neither is there any call to explain why the variations occur in special and advantageous ways, for they are not supposed to occur in special and advantageous ways, but only to appear to do so on account of all other variations being eliminated, while those which happen to occur in the specially advantageous ways are preserved. Again, Prof. Gray says in his postscript that the theory of natural selection supposes successive generations to be slowly changing, "yet always so as to be in compatible relations to the environment." Now it is true that where the changes in the environment are gradual, and the variations of specific type are being slowly accommodated to them, each generation is, on the whole, in compatible relations with its environment. But it is not true that such continuous compatibility in itself points to design; it only points to the plasticity of the varying type, which, if not sufficiently plastic to meet the new demands upon it in this respect, simply becomes extinct.

In conclusion, I agree that "natural science leaves aside the question whether evolution and design in nature are compatible or not," and I agree that, "if science has no call to settle the question, it has none to prejudge it." But I do not agree that I have prejudged this question by saying that in my opinion the theory of evolution, in supplanting the theory of special creation, has necessarily removed the special evidence of design in organic nature, by showing that in respect of causation organic nature and inorganic nature are one. GEORGE J. ROMANES

[1] I suppose it will be admitted that the validity of an inference depends upon the number, the importance, and the definiteness of the things or ratios known, as compared with the number, importance, and definiteness of the things or ratios unknown, but inferred. If so, we should be logically cautious in drawing inferences from the natural to the supernatural; for although we have the entire sphere of experience from which to draw an inference, we are unable to gauge the probability of the inference when drawn—the unknown ratios being confessedly of unknown number, importance, and degree of indefiniteness: the whole orbit of human knowledge is insufficient to obtain a parallax whereby to institute the required measurement or proportion between the terms known and the terms unknown. Or, otherwise phrased, we may say—As our knowledge of a part is to our knowledge of a whole, so is our inference from that part to the reality of that whole. Who, therefore, can say, even upon the supposition of Theism, that our inferences or "ideas of design" would have any meaning if applied to the "All-Upholder," whose thoughts are not as our thoughts?

The High Springs of 1883

THE high springs of the present year, consequent upon the excessive rainfall of the past winter, are an event that ought not to pass unrecorded in the pages of NATURE. I can speak only of phenomena which I have observed upon my native chalk hills of Hampshire, but I doubt not that similar facts have attracted attention elsewhere.

The Candover, a confluent of the Itchen from the north, burst forth this year in a field near Preston Candover, where it has not been known to rise for the last fifty years, and has flooded the road between Preston Candover and Chilton Candover. The Itchen itself rose in the valley above Cheriton beyond its recognised source, and has flooded fields on the road to Kilmeston, where no one recollects to have seen water before.

The Hampshire tributaries of the Thames have acted in exactly the same manner. The Whitewater has issued forth in the valley just below Upton Grey, far above its usual origin even in the highest springs, and has flooded the whole road between Bidden and Greywell. Another branch of the same stream has risen in the fields on the left of the main road from Odiham to South Warnborough, where spring water has never been known within the memory of the oldest inhabitant. In like manner the Wey, which, in wet seasons, takes its rise in the meadows adjoining Chawton House, has issued forth this year at a much higher level in the fields below Farringdon.

These facts are the more worthy of notice because it has been generally believed that, in the Hampshire hills at least, owing to more efficient drainage and other causes, the springs were

getting lower every year, and would never again attain the level that they once had according to the traditions of past generations. It should be added that the springs were at their highest about the commencement of this month, and are now gradually falling. P. L. SCLATER

Hoddington House, Odiham, March 31

Scorpion Suicide

I AM sorry that my experiments on scorpion suicide has given pain to some of your correspondents. Allow me to explain in a few words the object of my investigation. It is commonly believed in this colony and elsewhere that scorpions commit suicide; Dr. Allen Thomson, in a letter to NATURE, lent the weight of his scientific name to this view; and Dr. G. J. Romanes, in his "Animal Intelligence," treats it as an open question. Now if this habit of committing suicide be an established fact, we have in scorpions a highly persistent type of creature that inherits a habit detrimental alike to the individual and the species. *Scorpion suicide, therefore, if a fact, is one of the strongest individual cases against the Theory of Evolution by Natural Selection that is presented to us in the animal kingdom.* It seemed to me that the only way of settling this question was by the direct appeal to experiment. But is the Theory of Natural Selection of sufficient importance in its bearing upon human life and human progress to justify the infliction of pain upon, say, sixty scorpions? I am one of those who believe that it is. I am one of those who believe that the theory of evolution has enormously influenced human thought and action, and is destined to influence it in a constantly increasing degree. I believe that much of the moral and intellectual progress of our race is indissolubly associated with this theory of evolution. I may be wrong in that opinion, but that is the opinion I hold. And holding that opinion it became to me a duty to do something towards settling a question which seemed to me to be of great importance in its bearing on the evolution theory. And it was my object to do the work, as far as I could, thoroughly and once for all. I believed that if I could show that even under torture scorpions do not commit suicide, the view that they do so when irritated by the bright light of a candle-flare became highly improbable. To establish a negative in the face of positive assertions is, however, difficult, and I considered it necessary to experiment upon a number of individuals. *Hinc illæ lachrymæ!* One of my friends, however, protested as follows: "The theory of evolution," he said, "is now so strongly established, that scorpion suicide is *a priori* impossible." But I hold it to be dangerous in the extreme, in the present position of science, to set up the theory of evolution as a doctrine from which to draw deductions, *unchecked by an appeal to nature where such appeal is possible.* C. LLOYD MORGAN

Rondibosch, March 12

Nesting Habits of the Emu

I AM able fully to confirm Prof. Moseley's statement of the habits of the emu in nesting at Blenheim. Some years ago my father was very successful in rearing these birds at his place at Brockham Lodge, near Dorking. The first egg was usually laid shortly after Christmas; the total number of a brood being from fifteen to twenty, laid usually at intervals of about forty-eight hours. Some time before the full number was laid the cock bird would commence the incubation by carefully drawing them under him. When the hen bird was ready to add to their number she would sit down by his side, produce the egg, and her mate would then carefully draw it under him with his foot. As soon as the number was completed, it became necessary to seclude the hen bird, as she was from this time "vicious" towards her mate and towards her own eggs; and the seclusion continued until the young birds had attained a considerable size, as she showed every disposition to destroy them. The number of eggs laid was often too large for the cock bird to get comfortably under him. Still during several years that my father kept the birds a considerable number of eggs were annually hatched, and the young birds reared to the breeding age. No brood from native birds was, however, obtained. They showed no disposition to change the breeding season from January to July. In captivity the birds strikingly exhibited their singular inquisitive propensities. They were not unusually vicious, except during the breeding season, but were very easily frightened.

London, March 31 ALFRED W. BENNETT

The Recent Cold Weather

THE excessively severe and prolonged cold weather of the month of March has hardly a parallel in this century. It appears to have been felt throughout Europe, and has even reached the shores of Africa. Frost, snow, and wintry gales we expect at a season proverbial for its fitful severity, but the scarcely interrupted sweep of the frigid atmospheric waves which have overwhelmed us for three successive weeks is an experience of weather so remarkable that I conceive the record will probably interest some of your readers.

In position, altitude, and in its freedom from the sheltering influence of large towns, this station may be accepted as favourable for giving an accurate account of the weather in the centre of England. Our instruments are on a proper meteorological stand, and are by Negretti and Zambra. I may add that, in its blighting influence on vegetation stimulated into activity by a mild and moist period in February, this weather has proved more destructive to early fruit blossoms, certain shrubs and plants accepted as hardy, than from any weather previously experienced in March in other years; but apart from vegetation, and acting on the upturned fallows and soddened clods of clay, the penetrating winds, frequent frosts and falls of snow have pulverised the land, so that it falls before the plough or harrow like calcined limestone, and in respect to the preparation of land the weather has had a beneficial action.

Record of Weather, March, 1883, at Belvoir Castle, Leicestershire

March.	Min.	Max.	Grass.	Wind.	Rain.	Snow.
4	27	50	27	S. to N.	—	—
5	27	51	20	N.	—	—
6	33	52	29	N.	—	0″·2
7	26	40	22	N.	—	0″·2
8	24	41	24	N.	—	0″·25
9	20	35	14	N.	—	0″·12
10	9	37	4	N.	—	0″·5
11	20	38	10	N.	—	—
12	25	39	23	N.W.	—	0″·2
13	25	39	20	W.	—	0″·1
14	29	40	22	W.	—	—
15	27	39	20	N.	—	0″·5
16	26	38	19	W.S.W.	—	—
17	28	38	24	S.W.	—	0″·9
18	25	40	20	S.	—	0″·1
19	28	42	21	N.	—	0″·1
20	31	40	31	E.N.E.	—	0″·31
21	32	37	31	N.E.	—	—
22	28	35	27	E.	—	—
23	28	35	26	N.E.	—	—
24	18	42	5	W.	—	—
25	26	45	16	N.W.	0″·4	—
26	26	41	19	N.W.	0″·5	—
27	27	40	18	N.	—	—
28	26	43	16	N.W.	—	—
29	24	41	12	S.	—	—
30	35	48	35	S.	0″·3	—
31	30	55	24	S.W.	0″·11	—

Belvoir Castle Gardens WILLIAM INGRAM

Sap-Flow

A REMARKABLE instance of the strong up-rush of sap in trees at this time of the year occurred here during the late severe weather. The boughs of a sycamore overhanging a road were trimmed on the 21st of this month during a very keen frost, and next day icicles of frozen sap, varying in length from a couple of inches to a foot, were hanging from the severed ends. The icicles were semi-opaque in appearance and slightly opalescent, like the sheen on the moonstone, and, when put in a bottle and melted, the product was pure sap.

The sycamore, being one of the earliest trees to develop leaves, had its sap rising, notwithstanding the intense cold and late season; while a beech, which is much later in coming out, and an ash, which is usually latest of all, whose boughs had also been lopped, showed no signs of bleeding, and the cuts remained dry and bare.

The icicles have been melted, reformed, and melted again since the 21st, and still the sap is dropping from the cuts.

Highfield, Gainsborough, March 28 F. M. BURTON

Foamballs

To artificial snowballs and mudballs will you permit me to add an experience of foamballs. We were staying at Biarritz in early spring, and one morning on going down to the beach we found it covered with such balls. A strong wind was blowing off the bay, which caught the wave-crests, and threw off little masses of foam. These, though quite small at first, accumulated, and in some cases, conglomerated as they rolled inland, until they gradually attained a size of two to three feet in diameter; and as many of these balls of various sizes were drifted along by the wind, they presented a most singular appearance. This was made more curious by some of the town dogs catching sight of the objects, and taking to chevying them along the sand, until a sort of steeplechase was established. Every now and then a dog would overtake and dash into a flying sphere, only to find it, to his manifest disappointment, of a very unsubstantial character. The beach was covered far and wide with the debris of the broken balls.

Guildown, March 31 J. RAND CAPRON

Meteor; the Transit; the Comet

As you have on previous occasions deemed it of sufficient interest to record notices of striking meteors observed, I send you an account of a singularly brilliant and unusual form which appeared here about half-past 8 p.m. on the 29th inst.

I happened to be looking at a portion of the sky a little below the constellation "Orion," that is to the southward and eastward, when suddenly a brilliant meteor became apparent. Unlike ordinary meteors, it did not move, at least to my vision; it simply increased in size and brilliancy, till it appeared like a fine "Roman candle" or "blue light," intensely blue, and emitting rays at about two hundred yards' distance. It appeared to illuminate the country with a pale blue light.

It disappeared as suddenly as it came. Could its stationary appearance and increasing brightness have been caused by its *approaching* me in a direct line? I have thought so.

I saw the transit of Venus splendidly from my hilltop, through my binocular, an ordinary hand-telescope, and even with the naked eye, protected of course in each instance by coloured glass.

The comet also was a glorious object for several weeks. It was first seen here on September 23. I noticed very plainly the dark line near the right edge of the tail, as if there had been a *fold* in the luminous substance; that was the idea that the appearance gave to me. Fig. 3, p. 610, vol. xxvi. of NATURE, most resembles what we saw here, but the shadow, or dark part, from the V-like incision at the end, should be longer and darker.

Not being a scientific observer, I did not trouble you with any notices of either, feeling sure you would have plenty.

British Consulate, Noumea, January 31 E. L. LAYARD

Ticks

CAN none of your readers be prevailed on to take up the study of the Ixodes (Ticks), of which there are several British species? I feel sure their life-history, if fully worked out, would prove both interesting and instructive, and might throw some light on a mysterious and deadly disease amongst cattle and sheep, which prevails extensively in Scotland, and in some districts in England. It is a curious fact that Ixodes are almost invariably, if not always found infesting sheep where this disease prevails, and it becomes an important question whether their presence is merely a coincidence, from the rough coarse natural grasses forming a congenial habitat, or whether they are not the carriers or inoculators of vegetable or other poison. I should be very glad to give further information to any one disposed to take up the study. W. E. L.

Ignition by Sunlight

"M." MAY like to have the following case:—I went once at sunrise (at Kishnagar, Bengal) into my coachhouse, which opened east. I saw smoke ascending from the tops of the two carriage lamps. I jumped hastily to the conclusion that my syce (groom) had been using the carriage candles illegitimately, and taxed him. His defence obliged me to examine closer, and to see that the two wicks had been ignited to smouldering point by the horizontal rays of the sun condensed by the parabolic reflectors at the backs of the lamps. A notable enough example of Indian heat, was it not? W. J. HERSCHEL

Collingwood, March 31

WHEN driving along the Beaumaris Road on Tuesday last at half-past three, I observed smoke issuing from the top of one of the carriage lamps. I stopped to examine the cause, and found that the reflector had concentrated the sun's rays on the wick of the candle lamp and caused it to smoulder.

Rhianva, Bangor, April 2 EDMUND H. VERNEY

Mimicry

REFERRING to Mr. Stokoe's letter in NATURE, vol. xxvii., p. 508, and to his remarks on the defective vision of the Teleostei as proved by the very poor imitations of insects which are sufficient to entrap them, have not bats and swallows—animals of certainly more than normal acuteness of vision—been hooked on several occasions by the flyfisher? H. J. MORGAN

Exeter, March 31

Braces or Waistband?

CAN you or any of your readers answer the following:—Which method of suspending the trousers is the least interference with *nature*—their suspension from the *hips* or from the *shoulders*, the wearing of braces, or a tight waistband? R. M.

March 16

SINGING, SPEAKING, AND STAMMERING[1]

II.—SPEAKING

IN the first lecture the musical and emotional side of human utterance; in the second, the colloquial and intellectual aspect of speech was adverted to. Speaking in modern times, and in England especially, is a more neglected art than singing. Even in Shakespeare's days there must have been a state of things not very dissimilar; for he makes Dogberry, who always manages to state the wrong proposition, say, "Readin' and writin' come by nature," and there is a quaintly satirical passage in that graceful and ethereal play, the "Midsummer Night's Dream," which goes straight to the point. Theseus, in commenting on the Clown's blunders of diction, says:—

> "Where I have come, great clerks have purposed
> To greet me with premeditated welcomes;
> Where I have seen them shiver and look pale,
> Make periods in the midst of sentences,
> Throttle their practised accents in their fears,
> And in conclusion dumbly have broke off,
> Not paying me a welcome."

It cannot be too often reiterated that speech is essentially an acquirement, and that it must be learned. At first, indeed, it is picked up by imitation in early childhood, and later on in life is commonly neglected and left to take its chance; though much can be done with little labour to correct defects both of this and of the handwriting, the two first things by which a man's intellectual status is judged of. It is unlike singing, in that pleasant and articulate speaking does not require the gift of a musical organ, but is open to all alike. There exists, however, in some quarters a prejudice against fluent speaking. Ineffableness is held to indicate grasp of thought; taciturnity to be the cloak of profundity. This would be correct if fluency were to supersede accuracy; but such an antagonism is by no means necessary, or it would reach its natural limit in the case of the sailor's parrot, which "could not talk, but thought the more."

Some other hindrances to correct speech require passing comment. In the first place its acquirement is too much mixed up with recitation and dramatic representation. Neither exaggeration nor servile imitation produce good speaking, the one salient feature of which is natural-

[1] Abstract by the Author of three Lectures at the Royal Institution, by W. H. Stone, M.B., F.R.C.P. Continued from p. 510.

ness and spontaneity. Elocutionary teaching has also been hindered by an over-cultivation of poetical rhythm, which tends to reduce speech to a kind of singsong. The same may be said of punctuation, which is not elocutionary but grammatical; though the absurd rule has been formulated to "pause one for the comma, two for the semicolon, three for the colon, and four for the full stop." It is sufficient to test this pedantic error by reading any piece of nervous or pathetic English on the system, and thus to show its full absurdity.

It has been said above that whereas in singing the musical note is predominant, in speaking it is secondary and subsidiary to the words; but it still exists, and its function is well described by Cicero in his treatise, "De Oratore." He says, "Est in dicendo etiam quidam cantus obscurior." An appreciation of this fact is of the greatest value to the public speaker, since the imperfect regulation of the laryngeal element often renders the voice indistinct and even inaudible. Many speakers drop their voices with a descending inflection, and from want of musical ear fail to raise it again: others err from excess of noise, and in their anxiety to be audible, shout and labour, with the result of enveloping the significant sound in an overwhelming mass of heterogeneous and meaningless vibration.

It has several times been attempted to reduce speech to a definite musical notation like that of singing. To a certain extent this was done in the Ecclesiastical Plainsong; but it was carried to its extreme limit in a work of the last century, the "Prosodia Rationalis" of Joshua Steele. It is sufficient to glance at the vague and complicated symbols there employed to realise its practical uselessness.[1] Indeed, so far from being an advance, it is really a step of retrograde character. Mr. Deacon, in "Grove's Dictionary of Music," gives very clearly the four chief differences between song and speech:—1. The isochronism of vibration is never present long enough to make a musical note. 2. Little more than the lower third of the singing voice comes into play in speech. 3. In singing short syllables do not exist. 4. Singing tends to preserve intact purity of language; speaking, to split it up into dialects and idiosyncracies.

A common defect in speaking in large buildings is inability to catch the keynote or resonance vibration of the inclosed space. All large areas have such resonance notes, and in some it is very marked: Westminster Abbey, for instance, consonates to G sharp, and intoning on this note is much more audible than on one a semitone above or below it. Personally the lecturer prefers the use of an open chest-voice as little vocalised as may be. It is less laborious, less liable to accidents, less liable to develop the affection commonly known as "clergyman's throat," and, by removing the sensation of effort, more easy and sympathetic.

He then proceeded to analyse the constituents of a good delivery; and first, pauses. Haste is one of the commonest faults in speech. It has two defects; the one in overtaxing the complex muscular mechanism of the speaker; the other in adding to the intellectual labour of the listener. The former would be considered in the third lecture; the latter needed a few words. The rapidity of reception of ideas through the ear differs materially in different persons, even excluding those distinctly "hard of hearing." It is not great among the uneducated, whence it had been paradoxically said that all illiterate persons are deaf. But they do require a longer time to arouse them to a state of attention than the more cultivated. Naval officers had defended the practice of swearing, or as it was euphemistically termed, "shotting their speech," with sailors; the expletive rousing attention and preparing the mind for the succeeding command. Mr. Hullah had on a similar ground explained the refrains or fal-lal-las of the older music, in that they dilute the too concentrated sense of the words, and give time for the perception of the music.

When the great actor Salvini was in this country in 1875, the lecturer made some experiments on this point. Salvini's voice was one of the most remarkable ever heard for its power of travelling; even suppressed phrases coming up to the distant gallery with perfect clearness. He spoke on a note about D in the bass, from the chest, and in a sort of recitative; there were distinct periods from accent to accent, and the inflections were very large, running over an interval of more than a fifth. The individual words came about one a second, and the pauses were astonishingly long. They frequently amounted to four, several times to five, and at the two great crises of the play to seven continuous seconds. And yet there was no sense of delay or of interruption, but quite the reverse. The lecturer incidentally noted another thing, which the recent development of Wagner's musical theories had invested with additional interest. In the play "Il Gladiatore," the four principal characters, a young Christian virgin, a Roman matron, the hero a Roman officer, and the gladiator, formed an unintentional though perfect vocal quartett of soprano, contralto, tenor, and bass. At times the alternations of dialogue produced a distinctly musical effect, an observation which to his mind strongly corroborated the views of the great musician lately deceased, that dramatic music, instead of being conventional, should be the outflow of passion and emotion, and that this result could be attained as well from the elocutionary as from the strictly melodic side.

Pronunciation, under which is included respiration as well as vocalisation, was then spoken of, schemes of the vowels and consonants by Dr. Bristowe and Melville Bell being distributed among the audience. The latter being unfamiliar in this country, may be reproduced in this abstract.

General Vowel Scheme. — Melville Bell.

Lingual.	Labio-Lingual.	Labial.
1. Eel	Ü (German)	Oo ...
2. In	U (French)	O (Provincial)
3. Ale	Û (French)	Old
4. Ill (Scotch)	Zur (Provincial)	Ore
5. Ell	Eu (French)	Awe
6. An	Er Ir (English)	Urge (Scotch)
7. Ask	Er Ir (variety)	Urge
8. ,,	Ah	,,

Articulations or Consonants.

			Oral.		Nasal.
Obstructive. Complete contact.	... ...		P	B	M
			T	D	N
			K	G	Ng
Continuous	Approximation	Firm ...	Ph	Bh	
			Rh	R (smooth)	
			Ch	Gh	
			Wh	W	
			S	Z	
			Sh	Zh	
			Yh	Y	
		Relaxed	KRh	Gr (burr)	
			Rh	R (rough)	
	Partial contact		F	V	
			Th	Th	
			Ll (Welsh)	L	
			L (Gaelic)		

The aspirate was briefly described as being no fixed

[1] "King's College Lectures on Elocution." Plumptre p. 162.

articulation, but simply a vowel sound first whispered and then pronounced aloud. Accent has for its object to make one syllable or several more prominent than those around. The English language tends to throw it as far back in a word as is practicable. A long word may have one strong, and one or even two weaker accents in it.

Inflexion is either rising, falling, or a compound of these. As a rule, rising tones appeal, falling tones assert, compound tones suggest; a complete balance of the two is the antithesis, which can be heard in such a remark as "It was not so much what you said—as your manner of saying it, which struck me." The contrasted effect of the two accents may be reproduced by reading this sentence aloud and intelligently.

When inflexion is applied in this way to sentences, three cases occur: the sentence either asserts, asks, or orders, and the nature of the inflexion depends on the relative circumstances of the speaker and listener.

Delivery and modulation are combinations of pausing and of pitch. The conversational pitch being taken as a medium, all below this denotes sadness or solemnity; all above it joy or levity. Force, expression, and sentiment, thus developed, are infinite in their variety.

Emphasis can only be attained and regulated by a full perception of the point to be brought out; as a rule it marks the predicate of a logical expression. False emphasis is the foundation of many quaint stories in common currency. Speaking generally, new, contrasted, or antithetical ideas are marked by emphasis.

In conclusion, the lecturer gave three general rules by which any one can speak. The first, in the words of Horace: "Dicendi rectè principium est sapere, et fons;" that is, "Know exactly what you are going to say." The second, "Endeavour to forget yourself." This frame of mind had been formulated by old elocutionists as "Have a contempt for your audience." He preferred to state it in a less obnoxious way as "Consider yourself one of your audience." The third, "Be natural and unaffected."

By bearing in mind these simple injunctions any man free of congenital or acquired defects, though he might not be a brilliant, could hardly fail in being an agreeable and sympathetic speaker.

PROFESSOR SCHIAPARELLI ON THE GREAT COMET OF 1882

READERS of NATURE will be glad to have a full report of the interesting popular lecture which Prof. Schiaparelli, the well-known Italian astronomer, gave in Milan on February 4, on the great comet of 1882. Referring to the national misfortune which had given origin to his and other lectures, he began by showing that while a connection between the comet and the inundations which wasted, in October 1882, many Venetian provinces, was not absolutely impossible, it was at least very improbable, both because the comet was yet a great distance from the earth when the floods rose, and from the difficulty of understanding why the supposed influence of the comet should have acted only on that little part of the globe. After this preamble M. Schiaparelli gave the public a rapid and elementary account of our planetary system, and of the comet's trajectory during its passage near the sun and planets. The orbit of the comet, in the position which could be subjected to astronomical measurement, is parabolic, in a plane inclined 30° or 40° to planes of the solar system. The greater portion of the orbit is in the southern regions; for in the austral hemisphere the comet was sooner and better observed than in the boreal, where it never was very high above the horizon. The vertex of the parabola is very near the sun, and only when the comet was approaching to this position with an extraordinary rapidity, astronomers could perceive it,—at Auckland (September 2), at the Cape of Good Hope, in Australia, the Argentine Republic, and Brazil. The direction of its movement was perhaps towards the sun; but the inconceivable rate which the comet acquired in its falling towards the sun (480 km. in a second, sixteen times the mean velocity of the earth in its orbit), and the lateral rush coming from it, were enough at that time to overcome the attractive power of the sun, and to hinder the great luminary from swallowing it. The attraction of the sun failed not to produce its effect, slackening successively its flight; but being animated by this great velocity, the comet could escape in security to where the sun's action is very feeble, and whence it will not return for many years.

The Cape astronomer had the opportunity of witnessing this rare spectacle of a heavenly body which, rushing headlong from extraplanetary depths, went directly on the sun, as if it would fall in, and notwithstanding, in a few hours delivered itself, changing completely its direction of motion. At that time the earth was placed very obliquely in respect to the arc described by the comet about the sun, so that astronomers observed it with a great foreshortening of perspective. In those hours the comet, being exposed to an extraordinary heat, swelled and became so luminous, that the Cape astronomers, and afterwards some in Europe, could see it near the sun. They could make the absolutely new observation of a comet's transit before the solar disk, thus satisfying an ancient desire of astronomers, who have wished to know if in those bodies' head, which often appears as a very bright star, is hidden an obscure perceptible nucleus, and to judge of the density of the shining atmosphere whose splendour produces the star's appearance. In this case it was not possible to be deceived by an illusion, as happened in 1819. Messrs. Finlay and Elkin, at the Cape, saw the comet gradually approach the sun's limb, touch it, and disappear; so that their searches to find the comet in the place where it obviously was were vain. The comet then was so thin and clear, that the most slender cloud would more obscure the sun: its solid nucleus (if it had a nucleus, as was very likely) was so small that the observer's telescope could not perceive any spot or shade. After it left the neighbourhood of the sun, the effects of the enormous heat began to appear in the development of that splendid tail, which everybody could see in the morning hours of October and November.

The orbit of the comet (continued the Professor) is not easily deducible from the very little portion which we know. Both because to assign a trajectory observed in a small branch is very difficult, and sometimes impossible, and because exact and definitive calculations will not be undertaken before the vanishing of the comet; the notes which at present can be given are only approximative. On observations of last September, October, and November, it was stated that the period of the comet is included between eight and nine centuries, and the aphelion is nearly six times farther than Neptune from the sun (175 times the earth's mean vector radius), the rate of velocity in aphelion and perihelion being as 1 to 23,000.

On the brightness of the comet, M. Schiaparelli observed that it could be attributed to three causes: the strong illumination of the sun, its own light, and electrical discharges, which take place continually in similar bodies, in the opinion of expert physicists. Those causes united to make that very splendid appearance of a matter clearer and less dense than the rarefied air of our best pneumatic engines. The density of the tail was so small that an astronomer estimated it at no more than a few kilogrammes, while its dimensions were larger than were ever before observed in comets. It is true that other comets (that of 1861, for example) showed an apparently longer tail, their position in respect to the earth being more favourable to observation; but in the annals of

astronomy we have never found a comet's tail really as long as this.

I leave out the detailed description of the comet's aspect, because NATURE has given full accounts and sketches, and I come to the most interesting part of M. Schiaparelli's lecture, on the production of those magnificent phenomena. I translate literally from Signor Schiaparelli's manuscript.

The proper nucleus of the comet is a solid or liquid body so small as rarely to be seen : in the greater number of comets, as in this, it seems to be not large enough to be visible even in powerful telescopes. It seems also that in some comets there are several nuclei, very small and close, whose particular atmospheres in their development at last unite in one. As long as such a body (or system of bodies) remains far from the sun, in extraplanetary regions, where temperature is less than $-140°$ C. (according to the most moderate estimates), and where the sun has perhaps no power to heat it, the matter must be wholly solid or at least liquid ; and, if a small quantity is gaseous or vaporous, it must have a great density and a small volume. The progressive approach to the sun by its descending orbit will obviously swell the enveloping atmosphere, or give rise to one if it does not yet exist, with materials generated by the surface. Shortly the nucleus begins to appear surrounded by a blaze of light, feeble at first, but afterwards more and more brilliant, which is the star or head of the comet. Many comets do not go beyond this first phase, both because they have not matter enough to make an atmosphere, and because they do not come near enough to the sun to be subject to a great heat. Some comets do not enter the earth's orbit, others cannot reach that of Mars, and we know that the comet of 1729 got only a little way into the orbit of Jupiter. The most part of those comets, being exposed very moderately to the solar influence, cannot increase, and remain telescopic ; and it is very probable that a large number stop at Jupiter or Saturn's orbit (or even further) in their descent upon the sun : none of those are seen, and we can speak of them only by conjecture.

When a comet, however, as the present, pierces through the interior part of the planetary system, it is in the best condition to develop its atmosphere if it contain matter enough to do so. But the sun, while attracting to himself the nucleus, has the property to repel some of the matter of the atmosphere. It is not well known how and why this matter is repelled, and to expound the various hypotheses on this point would take too long a time. The effect of repulsion is nevertheless undoubted, and manifested by the fact that those parts of the cometary atmosphere, under the sun's impulse, almost as if under a gale blowing from it, leave the nucleus and fly in an opposite direction away from the sun, producing the tail, which, nourished successively by incessant evaporations of the nucleus, more and more increases in length, till the atmosphere of the nucleus, wholly repelled, overflows into the tail, and thus exhausts itself. This happens usually when the nucleus, after the perihelion, receding from the sun and being then exposed again to the cosmical cold, is no longer able to supply with new evaporations the part of the atmosphere which the tail absorbs. Deprived thus of its former envelopes, and unable to engender others, the nucleus is reduced again to itself, and the comet disappears.

The tail of the comet consists then of matters repelled by the sun with a mysterious power. But, during the above described period of conflagration, other interesting events occur in the comet. It is so much swollen and convulsed by solar heat that the little nucleus is not able sometimes to keep together the fragments by its own very feeble attraction. Violent eruptions take place at the surface, so that pieces of nucleus are raised and thrown out of the principal body's attraction. Those fragments then pass through the heavens as independent bodies, and their orbits are not very different from the orbit of the nucleus. Sometimes one of the broken pieces is great enough to engender another separate comet : that is, the several times observed phenomenon of a divided comet. But most generally it seems that separated pieces are very small and numerous, like the sparks of a piece of salt thrown on the fire ; and extend along the trajectory of the nucleus like a current or projection of corpuscles, which gradually invade all the orbit of the comet. Many comets (probably all) engender in their course a similar retinue ; and the planetary intervals are peopled by those corpuscles produced by a comet's partial disintegration. When the earth in its yearly revolution passes through one of these processions it meets with several pieces, which get inflamed by contact with the terrestrial atmosphere, and burn in a very short time, producing a falling star.

An example of such a process of separation was given by the present comet. In effect, a little before October 15 M. Schmidt, the astronomer at Athens, observed an irregular and very feebly shining thin cloud leaving the comet, withdrawing, and finally disappearing. It was more dense and luminous in some places than in others, but it looked not like a comet, having rather the aspect of a mass of corpuscles exploded by the principal nucleus. The atmosphere also enveloping the principal nucleus offered analogous phenomena, being not round and symmetrical, but lengthened spindle-fashion, with several more luminous centres of different intensity spread in an oval cloud. We have, besides, reason to believe that another little comet, which was observed in the beginning of 1880 in the austral regions of the earth, running in an orbit very similar, was previously separated from our great comet.

M. Schiaparelli passed afterwards to another question, on the chemical constitution of comets, explaining the principle of spectrum analysis and its application to celestial chemistry. He remarked that the present and Wells's comet only, by their coming so near the sun, could present the lines of sodium, whilst all the comets before observed gave only lines of hydrocarbons to the spectroscope ; and it is very probable, according to modern theories, that comets contain also some matters which are made apparent in falling stars and in aërolites, as iron, nickel, silicium, magnesium, aluminium, and others. This confirms the induction as to the similarity of their chemical composition to that of the earth ; and the common origin of comets in the planetary system is evidently proved by their accompanying the sun in its progressive movement towards the constellation of Hercules. It seems that comets belonging to the solar system would have the function of continually dissipating matter in space, as a compensation to the attractive power of the greatest centre, the sun.

Pressure of space obliges me to leave out the very eloquent conclusion of this lecture, in which the lecturer refuted the apprehensions as to the shock of a comet with the earth, and its probable consequences, discussing the great moral importance of these studies as an antidote to the fears and superstitions of ignorant people. Referring to Anaxagoras and Galileo, he concluded with these words: "A science which suffered such noble condemnations, and is able to awake such noble hopes, cannot be considered as futile and idle ; it will always be dear to the friends of truth ; dear to every one who is convinced that man lives not by bread alone."

FRANCIS PORRO

R. Observatory of Brera in Milan

THE SOARING OF BIRDS

THE recent correspondence in NATURE upon this subject ought not to close without some reference to a possible explanation of *soaring* which does not appear to have been yet suggested.

I premise that if we know anything about mechanics it is certain that a bird *without working his wings* cannot, either in still air or in a uniform horizontal wind, maintain his level indefinitely. For a short time such maintenance is possible at the expense of an initial relative velocity, but this must soon be exhausted. Whenever therefore a bird pursues his course for some time without working his wings, we must conclude either (1) that the course is not horizontal, (2) that the wind is not horizontal, or (3) that the wind is not uniform. It is probable that the truth is usually represented by (1) or (2); but the question I wish to raise is whether the cause suggested by (3) may not sometimes come into operation.

In NATURE, vol. xxiii. p. 10, Mr. S. E. Peal makes very distinct statements as to the soaring of pelicans and other large birds in Assam. The course is in large and nearly circular sweeps, and at each lap some 10 or 20 feet of elevation is gained. *When there is a wind*, the birds may in this way "without once flapping the wings" rise from a height of 200 to a height of 8000 feet.

That birds do not soar when there is no wind is what we might suppose, but it is not evident how the existence of a wind helps the matter. If the wind were horizontal and uniform it certainly could not do so. As it does not seem probable that at a moderate distance from the ground there could be a sufficient vertical motion of the air to maintain the birds, we are led to inquire whether anything can be made of the difference of horizontal velocities which we know to exist at different levels.

In a uniform wind the available energy at the disposal of the bird depends upon his velocity *relatively* to the air about him. With only a moderate waste this energy can at any moment be applied to gain elevation, the gain of elevation being proportional to the loss of relative velocity squared. It will be convenient for the moment to ignore the waste referred to, and to suppose that the whole energy available remains constant, so that however the bird may ascend or descend, the relative velocity is that due to a fall from a certain level to the actual position, the certain level being of course that to which the bird might just rise by the complete sacrifice of relative velocity.

For distinctness of conception let us now suppose that above and below a certain plane there is a uniform horizontal wind, but that in ascending through this plane the velocity increases, and let us consider how a bird sailing somewhat above the plane of separation, and endowed with an initial relative velocity, might take advantage of the position in which he finds himself.

The first step is, if necessary, to turn round until the relative motion is to leeward, and then to drop gradually down through the plane of separation. In falling down to the level of the plane there is a gain of relative velocity, but this is of no significance for the present purpose, as it is purchased by the loss of elevation; but in passing through the plane there is a really effective gain. In entering the lower stratum the actual velocity is indeed unaltered, but the velocity relatively to the surrounding air is *increased*. The bird must now wheel round in the lower stratum until the direction of motion is to windward, and then return to the upper stratum, in entering which there is a second increment of relative velocity. This process may evidently be repeated indefinitely; and if the successive increments of relative velocity squared are large enough to outweigh the inevitable waste which is in progress all the while, the bird may maintain his level, and even increase his available energy, without doing a stroke of work.

In nature there is of course no such abrupt transition as we have just now supposed, but there is usually a continuous increase of velocity with height. If this be sufficient, the bird may still take advantage of it to maintain or improve his position without doing work, on the principle that has been explained. For this purpose it is only necessary for him to descend while moving to leeward, and to ascend while moving to windward, the simplest mode of doing which is to describe circles on a plane which inclines downwards to leeward. If in a complete lap the advantage thus obtained compensates the waste, the mean level will be maintained without expenditure of work; if there be a margin, there will be an outstanding gain of level susceptible of indefinite repetition.

A priori, I should not have supposed the variation of velocity with height to be adequate for the purpose; but if the facts are correct, some explanation is badly wanted. Mr. Peal makes no mention of the circular sweeps being inclined to the horizon, a feature which is essential to the view suggested. It is just possible, however, that the point might escape attention not specially directed to it.

However the feat may be accomplished, if it be true that large birds can maintain and improve their levels without doing work, the prospect for human flight becomes less discouraging. Experimenters upon this subject would do well to limit their efforts for the present to the problem of gliding or sailing through the air. When a man can launch himself from an elevation and glide long distances before reaching the ground, an important step will have been gained, and until this can be done, it is very improbable that any attempt to maintain the level by expenditure of work can be successful. Large birds cannot maintain their levels in still air without a rapid horizontal motion, and it is easy to show that the utmost muscular work of a man is utterly inadequate with any possible wings to allow of his maintenance in a fixed position relatively to surrounding air. With a rapid horizontal motion, the thing may perhaps be possible, but for further information bearing upon this subject, I must refer to a paper on the resistance of fluids published in the *Philosophical Magazine* for December, 1876.

March 22 RAYLEIGH

PHILIP CHRISTOPH ZELLER

ENTOMOLOGY has just sustained an irreparable loss by the death of Prof. Zeller, which took place at Grünhof, near Stettin, on March 27, suddenly, from heart disease. Zeller was born on April 9, 1808, at Steinheim, in Württemburg. For many years he was attached to official educational establishments in Germany, especially at Glogau in Silesia, and Meseritz in Posen. While at the former place the honorary title of Professor was bestowed upon him by the Government on account of his eminent scientific researches, and some time afterwards he retired from official duties, and settled near Stettin, where much of his leisure was devoted to the Entomological Society that has its headquarters in that town, of which he was acting secretary, and of which Dr. C. A. Dohrn is president. Zeller's fame as an entomologist is more especially based upon his publications on *Lepidoptera*, more particularly of Europe, and chiefly on the smaller moths. His first recorded paper appeared in Oken's *Isis* for 1838, and consisted of a critical determination of the *Lepidoptera* in Réaumur's "Memoirs," a prize essay, in which the author took first place. From that time a continuous stream of valuable papers by him appeared, and on the day of his death he was engaged in scientific work. It is utterly impossible to give here even the titles of his more important works. It is with regret that we are obliged to admit that the title of "entomologist" does not always enable us to take for granted that the entomologist is also a naturalist. Zeller was both, in the fullest acceptation of the terms. While his purely descriptive work is of the highest character, his investigations into the natural history of his subject were persistent, and he never ceased to deprecate the "slop-work" so painfully evident in the writings of some entomologists. For many years he made almost annual excursions in pursuit of his favourite science, especially in the Alps of Central

Europe, and so long ago as 1844 a more extended tour in South Italy and Sicily. In this country he was so well known that British entomologists will feel that in his death they have lost one of themselves; it is nearly thirty-five years since he was elected an Honorary Member of the Entomological Society of London, and he was one of the editors of Mr. Stainton's magnificent "Natural History of the Tineina." There are those amongst us in this country who in Zeller's death have lost one of their dearest friends. Scientific entomology has lost one of its most shining lights. R. McLACHLAN

THE GREAT INTERNATIONAL FISHERIES EXHIBITION

HER MAJESTY THE QUEEN has recently appointed the 12th of May for the opening of the International Fisheries Exhibition, which an influential and energetic committee, under the active presidency of the Prince of Wales, has developed to a magnitude undreamt of by those concerned in its early beginnings. This magnitude is perhaps as great a matter of agreeable surprise to Mr. Birkbeck and its other Norwich founders as it will be to those who have very naturally become accustomed to class all specific exhibitions together upon a standard formed by the unfortunate annual exhibitions of which the public has, not without reason, grown weary.

The idea of an *international* Fisheries Exhibition arose out of the success of the show of British fishery held at Norwich a short time ago; and the president and executive of the latter formed the nucleus of the far more powerful body by whom the present enterprise has been brought about.

The buildings are well advanced towards completion, and will be finished long before the opening day; the exhibitors will, it is hoped, support the executive by sending in their goods in time, and thus all will be ready for the 12th proximo.

The plan of the buildings embraces the whole of the twenty-two acres of the Horticultural Gardens: the upper half, left in its usual state of cultivation, will form a pleasant lounge and resting-place for visitors in the intervals of their study of the collections. This element of garden accommodation was one of the most attractive features at the Paris Exhibition of 1878.

As the plan of the buildings is straggling and extended, and widely separates the classes, the most convenient mode of seeing the show will probably be found in going through the surrounding buildings first, and then taking the annexes as they occur.

On entering the main doors in the Exhibition Road, we pass through the Vestibule to the Council Room of the Royal Horticultural Society, which has been decorated for the reception of marine paintings, river subjects, and fish pictures of all sorts, by modern artists.

Leaving the Fine Arts behind, the principal building of the Exhibition is before us—that devoted to the deep sea fisheries of Great Britain. It is a handsome wooden structure 750 ft. in length, 50 ft. wide, and 30 ft. at its greatest height. The model of this, as well as of the other temporary wooden buildings, is the same as that of the annexes of the great Exhibition of 1862.

On our left are the Dining Rooms with the Kitchens in the rear. The third room, set apart for cheap fish dinners (one of the features of the Exhibition), is to be decorated at the expense of the Baroness Burdett Coutts, and its walls are to be hung with pictures lent by the Fishmongers' Company, who have also furnished the requisite chairs and tables, and have made arrangements for a daily supply of cheap fish, while almost everything necessary to its maintenance (forks, spoons, table-linen, &c.), will be lent by various firms.

The apsidal building attached is to be devoted to lectures on the cooking of fish.

Having crossed the British Section, and turning to the right and passing by another entrance, we come upon what will be to all one of the most interesting features of the Exhibition, and to the scientific student of ichthyology a collection of paramount importance. We allude to the Western Arcade, in which are placed the Aquaria, which have in their construction given rise to more thoughtful care and deliberation than any other part of the works. On the right, in the bays, are the twenty large asphalt tanks, about 12 ft. long, 3 ft. wide, and 3 ft. deep. These are the largest dimensions that the space at command will allow, but it is feared by some that they will be found somewhat confined for fast going fish. Along the wall on the left are ranged twenty smaller or table-tanks of slate, which vary somewhat in size; the ten largest are about 5 ft. 8 in. long, 2 ft. 9 in. wide, and 1 ft. 9 in. deep.

In this Western Arcade will be found all the new inventions in fish culture—models of hatching, breeding and rearing establishments, apparatus for the transporting of fish, ova, models, and drawings of fish-passes and ladders, and representations of the development and growth of fish. The chief exhibitors are specialists, and are already well known to our readers. Sir James Gibson Maitland has taken an active part in the arrangement of this branch, and is himself one of the principal contributors.

In the north of the Arcade where it curves towards the Conservatory, will be shown an enormous collection of examples of stuffed fish, contributed by many of the prominent angling societies. In front of these on the counter will be ranged microscopic preparations of parasites, &c., and a stand from the Norwich Exhibition of a fauna of fish and fish-eating birds.

Passing behind the Conservatory and down the Eastern Arcade—in which will be arranged Algæ, Sponges, Mollusca, Star-fish, worms used for bait, insects which destroy spawn or which serve as food for fish, &c.—on turning to the left, we find ourselves in the Fish Market, which will probably vie with the Aquaria on the other side in attracting popular attention. This model Billingsgate is to be divided into two parts, the one for the sale of fresh, the other of dried and cured fish.

Next in order come the two long iron sheds appropriated respectively to Life-boats and Machinery in motion. Then past the Royal Pavilion (the idea of which was doubtless taken from its prototype at the Paris Exhibition) to the southern end of the central block, which is shared by the Netherlands and Newfoundland; just to the north of the former Belgium has a place.

While the Committee of the Netherlands was one of the earliest formed, Belgium only came in at the eleventh hour; she will, however, owing to the zealous activity of Mr. Lenders, the Consul in London, send an important contribution worthy of her interest in the North Sea fisheries. We ought also to mention that Newfoundland is among those colonies which have shown great energy, and she may be expected to send a large collection.

Passing northward we come to Sweden and Norway, with Chili between them. These two countries were, like the Netherlands, early in preparing to participate in the Exhibition. Each has had its own Committee, which has been working hard since early in 1882.

Parallel to the Scandinavian section is that devoted to Canada and the United States. While the American Government has freighted a ship with specimens expected daily, the former has entered heart and soul into the friendly rivalry, and will occupy an equal space—ten thousand square feet.

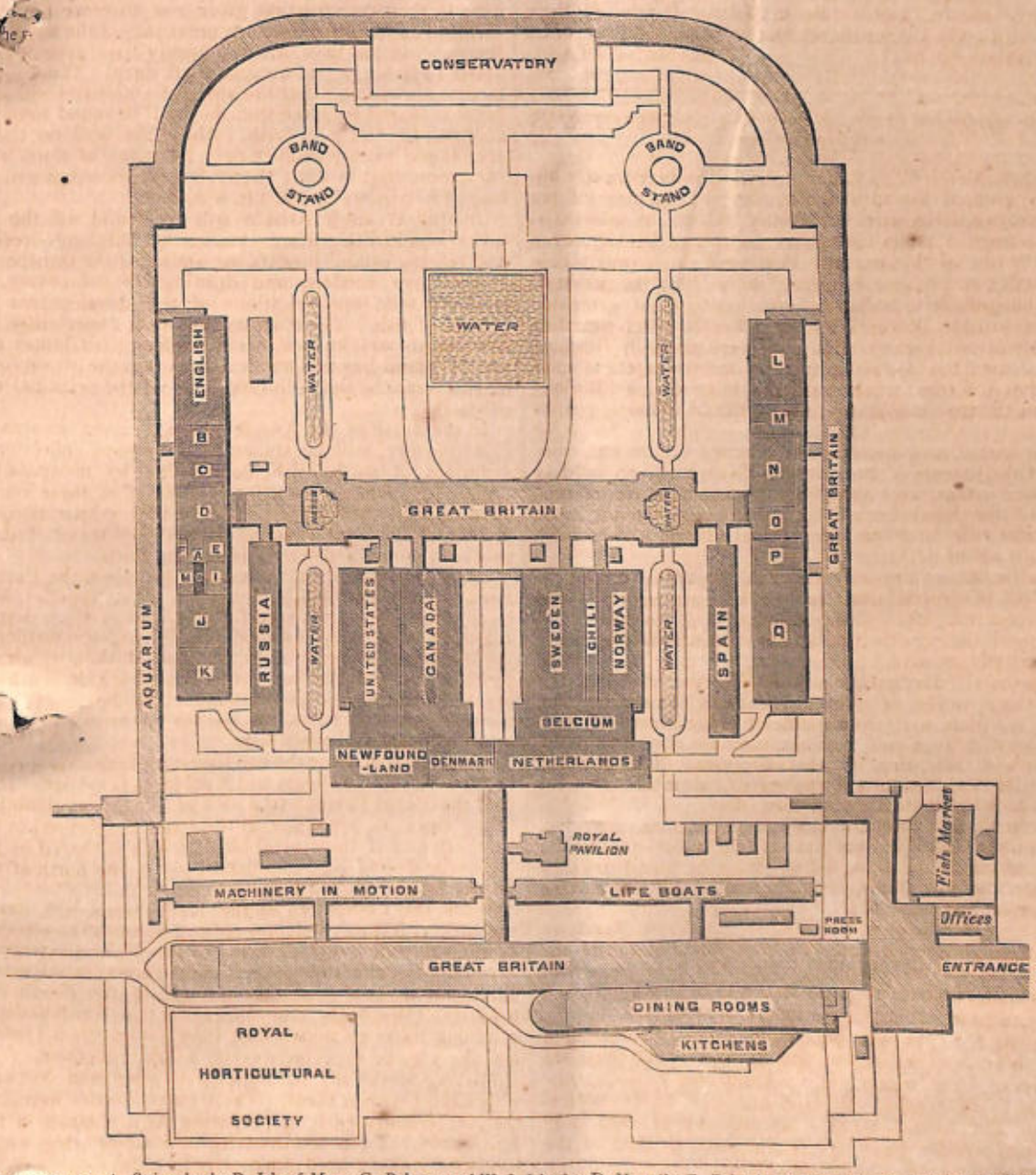
In the Northern Transept will be placed the inland fisheries of the United Kingdom. At each end of the building is aptly inclosed a basin formerly standing in the gardens: and over the eastern one will be erected the

daïs from which the Queen will formally declare the Exhibition open.

Shooting out at right angles are the Spanish annexe, and the building shared by India and Ceylon, China and Japan, and New South Wales: while corresponding to these at the western end are the Russian annexe and a shed allotted to several countries and colonies. The Isle of Man, the Bahamas, Switzerland, Germany, Hawaii, Italy, and Greece—all find their space under its roof.

After all the buildings were planned, the Governments of Russia and Spain declared their intention of participating; and accordingly for each of these countries a commodious iron building has been specially erected.

The Spanish collection will be of peculiar interest; it



BLOCK PLAN.—A. Switzerland; B. Isle of Man; C. Bahama and W. I. Islands; D. Hawaii; E. Poland; F. Portugal; G. Austria; H. Germany; I. France; J. Italy; K. Greece; L. China; M. India and Ceylon; N. Straits Settlements; O. Japan; P. Tasmania; Q. New South Wales.—Scale, 200 feet to the inch.

has been gathered together by a Government vessel ordered round the coast for the purpose, and taking up contributions at all the seaports as it passed.

Of the countries whose Governments for inscrutable reasons of state show disfavour and lack of sympathy, Germany is prominent; although by the active initiative of the London Committee some important contributions have been secured from private individuals: among them, we are happy to say, is Mr. Max von dem Borne, who will send his celebrated incubators, which the English Committee have arranged to exhibit in operation at their own expense.

Although the Italian Government, like that of Germany, holds aloof, individuals, especially Dr. Dohrn of the

Naples Zoological Station, will send contributions of great scientific value.

France, the other day only, consented to the official appointment of her Consul to look after the interests of the oyster cultivators who are contributing an important feature.

In the Chinese and Japanese annexe, on the east, will be seen a large collection of specimens (including the gigantic crabs) which has been collected, to a great extent, at the suggestion of Dr. Günther of the British Museum.

It is at the same time fortunate and unfortunate that a similar Fisheries Exhibition is nowbeing held at Yokohama, as many specimens which have been collected specially for their own use would otherwise be wanting; and on the other hand, many are held back for their own show.

China, of all foreign countries, was the first to send her goods, which arrived at the building on the 30th ultimo, accompanied by native workmen, who are preparing to erect over a basin contiguous to their annexe models of the summer-house and bridge with which the willow-pattern plate has made us familiar; while on the basin will float models of Chinese junks.

Of British colonies, New South Wales will contribute a very interesting collection placed under the care of the Curator of the Sydney Museum; and from the Indian Empire will come a large gathering of specimens in spirits under the superintendence of Dr. Francis Day.

Of great scientific interest are the exhibits, to be placed in two neighbouring sheds, of the Native Guano Company and the Millowners' Association. The former will show all the patents used for the purification of rivers from sewage, and the latter will display in action their method of rendering innocuous the chemical pollutions which factories pour into rivers.

In the large piece of water in the northern part of the gardens, which has been deepened on purpose, apparatus in connection with diving will be seen; and hard by, in a shed, Messrs. Siebe, Gorman & Co. will show a selection of beautiful minute shells dredged from the bottom of the Mediterranean.

In the open basins in the gardens will be seen beavers, seals, sea-lions, waders, and other aquatic birds.

From this preliminary walk round enough has, we think, been seen to show that the Great International Fisheries Exhibition will prove of interest alike to the ordinary visitor, to those anxious for the well-being of fishermen, to fishermen themselves of every degree, and to the scientific student of ichthyology in all its branches.

The economic question of the undertaking we have left untouched.

NOTES

It will be seen from a communication in another column that the Council of the British Association have virtually decided that that body is bound to hold its meeting in Canada in 1884. From Sir A. T. Galt's letter it is evident that our Canadian fellow-subjects have already arranged to give the Association a hearty and generous welcome; and now that Canada seems inevitable, we hope that as many members as possible will make up their minds to be present. The expenses for visitors will be reduced to a minimum, and the travelling expenses of officials, to the number of fifty, to *nil*. A magnificent programme for three weeks' excursions has been sketched, and the expenses connected with them will be confined to hotel charges, carriages, &c., the railway companies having handsomely offered to convey members free of charge.

The Academy of Sciences held its Annual Meeting on April 2, M. Jamin in the chair. He pronounced the *éloge* of the three Academicians who died last year, viz., MM. Liouville, Bussy, and Decaisne. M. Blanchard, filling the room of M. Dumas, who, although present, was unable to deliver any speech, read the list of laureates. The number of prizes offered for public competition is yearly enlarging; not less than three of them—Monti, Machedo, and Francœur—were delivered for the first time. The number of verdicts which the commission had to render was thirty-three. In nine cases the commission declared no memoir was worthy to take a prize; the competitions were in general adjourned to 1885, and a certain sum of money was given to some semi-successful candidates. In two instances the merit of the candidates was acknowledged so great that two prizes were delivered instead of one. These two cases were in statistics and mathematics; the question put was to give a theory of the partition of numbers in five squares. Amongst the prizes lost is included the famous Prix Breaux, for the cure of cholera. The interest was divided amongst four pupils of M. Pasteur's. The Poncelet Prize has been taken by M. Clausius, and the Voltz Prize by Mr. Huggins and M. Crüls, a Brazilian, for their spectroscopic work.

It was announced at the above-mentioned meeting that the great mathematical prize of the French Academy had been awarded to the late Prof. H. J. S. Smith for his dissertation on the representation of a number as the sum of five squares. The subject for the prize was announced in the *Comptes Rendus* of the Academy in February of last year, and, according to custom, the essays were to be sent in before June 1—each dissertation bearing a motto and being accompanied by a sealed envelope having the motto on the outside and the writer's name inside. The envelopes of the unsuccessful candidates are destroyed unopened. Prof. Smith's dissertation bore as its appropriate motto :—

"Quotque, quibusque modis possint in quinque resolvi
Quadratos numeri pagina nostra docet."

There were three candidates, and the value of the prize is 3000f. The theory of numbers, to which the prize subject related, is one to which Prof. Smith had devoted the greater part of his life, and in which he occupied an almost unique position; with the exception of Prof. Kummer of Berlin, there is no one whose contributions to the science could be compared to his, and this posthumous mark of the appreciation on the Continent of the value of his work is all the more satisfactory as the great prize has never before, we believe, been awarded to an English mathematician. The complete solution of the important problem proposed by the French Academy had been obtained by Prof. Smith sixteen years ago as part of a far more general investigation, and the results were published by him in the *Proceedings of the Royal Society* in 1868, but without demonstration. These researches seem, however, to have escaped the notice of the French mathematicians. When the subject of the prize was announced last year, Prof. Smith extracted from his manuscript books the demonstrations of the propositions relating to the five-square problem, and it is to the dissertation so formed that the prize has been awarded. No more striking instance of the extent to which Prof. Smith had carried his researches, or of his great mathematical genius, could be given than is afforded by the fact that a question considered by the French Academicians of so much importance to the advancement of mathematical science as to be chosen for the subject of the "Grand Prix" should have been completely solved by him as only a particular case in the treatment of a general and even more intricate problem. In 1868 Prof. Smith won the Steiner Prize of the Berlin Academy, so that had he but lived till now he would have been "laureate" of the Academies of both Paris and Berlin.

The removal of the natural history collection from Great Russell Street to its new quarters at South Kensington, on the

site of the Great Exhibition of 1862, has been proceeding gradually during the last two years, and is now rapidly approaching completion. Several of the rooms, formerly stocked with birds, fishes, &c., have been already emptied.

LIEUT. SAMUEL W. VERY, U.S.N., and Dr. Orlando B. Wheeler, the two principal members of the expedition sent by the United States Government to Santa Cruz, Patagonia, to observe the recent transit of Venus, arrived in Liverpool on Friday, by the Pacific Steam Navigation Company's mail steamer *Patagonia*. Lieut. Very, who had charge of the expedition as chief astronomer, states that the expedition arrived off the mouth of the Santa Cruz River on November 2. The weather during the first fourteen days was very encouraging, but this was succeeded by nine days of overcast, disagreeable weather, with frequent and sharp showers of hail and rain. Fine weather again followed until the eventful morning of December 6, which broke cloudy and hazy. By half-past seven a.m., however, the clouds began to weaken, half an hour later the sun shone out dimly, and as the day advanced the weather improved, so that when it was time to take up stations for the first contact, the sun was almost entirely clear. All four of the contacts were observed both by Lieut. Very, with the large equatorial, and by Mr. Wheeler, with a smaller one; and during the transit 224 photographs were taken, with a continuous improvement in the results. By sunset the weather changed again for the worse, and the sun was not seen, except at intervals, for four or five days, during which time Lieut. Very was looking anxiously for observations for rating his chronometers. While the expedition was in camp the temperature changed to the extent of 19° in the course of every twelve hours. In the daytime the heat occasionally was oppressive, while at night the air was very cold, and the party had to sleep with double blankets and heavy clothing upon them. The Lieutenant speaks in the highest terms of the kindness and consideration shown to him by the Pacific Steam Navigation Company and the Customs authorities, both of whom, when they were informed of his business, put all possible facilities in his power.

THE next ordinary General Meeting of the Institution of Mechanical Engineers will be held on Wednesday, April 11, and Thursday, April 12, at 25, Great George Street, Westminster. The chair will be taken by the president, Percy G. B. Westmacott, at three o'clock on Wednesday afternoon, and at ten o'clock on Thursday morning. The following papers will be read and discussed:—On the strength of shafting when exposed both to torsion and end thrust, by Prof. A. G. Greenhill, of Woolwich; On modern methods of cutting metals, by Mr. W. Ford Smith, of Salford; On improvements in the manufacture of coke, by Mr. John Jameson, of Newcastle-on-Tyne; On the application of electricity to coal mines, by Mr. Alan C. Bagot, of London.

THE 21st meeting of the delegates of the French Learned Societies took place last week at the Sorbonne. M. Ferry, the French Premier, presided over the final meeting on March 31, and delivered, as is customary, an address. The Minister dwelt much on the circumstance that he had added to the four sections in existence a fifth, devoted to political economy, so that the meeting of the Learned Societies included every subject in human knowledge. He praised the Trustees of the British Museum for their fair dealing towards France in the matter of the Ashburnham manuscripts, and eulogised the French Government for their zeal in the promotion of knowledge, declaring that 60 millions of francs had been already spent for the rebuilding of French universities, and that 40 millions were to be spent shortly for the same purpose. The presidents of the several sections omitted to deliver their reports, and the proceedings terminated somewhat abruptly. The address was well received, but the unexpected silence of the presidents has taken the public by surprise, and has been unexplained as yet.

M. HERVÉ MANGON, President of the Bureau Central of French Meteorology, opened the Session of the Congress of Meteorologists on March 29 by reading a report on the working of the institution. This document states that, from a comparison made by the Bureau, its forecasts have been acknowledged good 83 times in each 100; that for the warning of tempests 207 had been sent to the seaports, out of which 100 had been fulfilled entirely, 65 partly, and 42 had not been warranted by the event. The president, who is a member of the French Legislative Assembly for La Manche, announces the intention of asking from Parliament an augmentation of credit.

MR. MUYBRIDGE has issued a prospectus of "a new and elaborate work upon the attitudes of man, the horse, and other animals in motion." As the expense of conducting these experiments is very great, Mr. Muybridge naturally waits until he obtains a sufficient number of 100-dollar subscriptions before entering upon them. Each subscriber of the sum will receive a large album containing the photographic results of the experiments. Their scientific and artistic value is so great that we trust Mr. Muybridge will receive sufficient encouragement to put his plan into execution. His address is Scovill Manufacturing Company, Publishing Department, 419-421, Broome Street, New York.

THE Warwick Museum has been enriched by the very valuable collection of local Liassic and Keuper fossils formed by the late Mr. J. W. Kirshaw, F.G.S., which it is intended to keep as a separate collection. The whole of the collection in the Museum has lately been classified and arranged by Mr. R. Bullen Newton, of the Natural History Museum, South Kensington.

HARTLEBEN'S "Elektrotechnische Bibliothek" has been further augmented by three volumes. They consist of two little books by Dr. Alfred von Urbanitzky, viz. "Die elektrischen Beleuchtungs Anlagen" and "Das elektrische Licht," and one by Herr W. P. Hauck, "Die galvanischen Batterien, Accumulatoren, und Thermosäulen."

ACCORDING to latest accounts, the eruption of Mount Etna is resuming activity. Enormous quantities of gas are thrown out, and slight shocks are again felt in the neighbourhood of Nicolosi.

THE second number of *Timehri*, the journal of the British Guiana Agricultural and Commercial Society is to hand; it completes the first volume. Among the contents we note the following:—Forest Conservancy in British Guiana, by M. McTurk, G. M. Pearce, and the Hon. W. Russell; Mount Russell in Guiana, by the Editor, Mr. Im Thurn; The Aspect and Flora of the Kaieteur Savannah, by G. S. Jenman; Notes on West Indian Stone Implements, by the Editor, with several coloured illustrations; British Guiana Cave-Soils and Artificial Manures, by E. E. H. Francis. There are also several interesting notes, and the reports of the Society's meetings. Among the notes is a letter from Dr. R. Schomburgk, of Adelaide, giving some interesting autobiographical details. Stanford is the London agent.

WE have received the first number of the new American monthly, *Science*, to which we heartily wish all success.

WHILE Western Europe enjoyed a mild autumn, very severe weather was experienced on the Ural. At Ekaterinburg the average temperature of October was four degrees lower than the average for forty five years, that is, $-3°{\cdot}9$, instead of $+0°{\cdot}9$, the lowest temperatures in October witnessed since 1836 having been but $-2°{\cdot}4$ and $-3°{\cdot}2$. For nineteen days the thermometer did not rise above zero, and it fell as low as $-19°{\cdot}2$ and $-17°{\cdot}9$.

ENTOMOLOGISTS generally, as well as those more particularly interested from their geographical position, will be pleased to learn that the long-expected Yorkshire List of Lepidoptera—on which Mr. Geo. T. Porritt, F.L.S., of Huddersfield, has for some time past been engaged—is now completed, and that the MS. is now being set up for the *Transactions of the Yorkshire Naturalists' Union*. Mr. Porritt, who has been assisted by the leading entomologists of the county, and who has also paid attention to the literature of the subject, has written what will probably be regarded as one of the best county catalogues of Lepidoptera extant. The diversity of soil and climate, geological and physical conformation, for which Yorkshire is famous, is once more illustrated by the richness in species which the lepidopterous fauna shows, 1344 out of the 2031 species recognised as British finding places in Mr. Porritt's catalogue.

THE following occurrence is worth notice:—The Weymouth and Channel Islands Steam Packet Company's mail steamer *Aquila* left Weymouth at midnight on Friday for Guernsey and Jersey on her passage across Channel. The weather was calm and clear, and the sea was smooth. When about one hour out the steamer was struck violently by mountainous seas, which sent her on her beam ends and swept her decks from stem to stern. The water immediately flooded the cabins and engine-room, entering through the skylights, the thick glass of which was smashed. As the decks became clear of water, the bulwarks were found to be broken in several places, one of the paddle-boxes was considerably damaged, the iron rail on the bridge was completely twisted, the pump was broken and rendered useless, the skylight of the ladies' cabin was completely gone, and the saloon skylight was smashed to atoms. The cabins were baled out with buckets, while tarpaulins were placed over the skylights for protection. Fives minutes after the waves had struck the steamer the sea became perfectly calm. Several of the crew were knocked about, but none were seriously injured.

AT 10 p.m. on March 27 an earthquake occurred in and around the town of Miskolcz, Hungary. There were two separate shocks, and so distinctly were they felt that in the theatre, where the performance was going on, a panic ensued, the entire audience rising and rushing in terror towards the outlets. Many persons were injured, but, happily, no lives were lost. An earthquake was observed on March 12 in various parts of Italy. Reports now state that it was principally noticed in the Pellice valley, in the Po district, at Gessi, Varcita, Stura, and Coni. The direction of the shocks was from N.E. to S.W. In the plains the shocks were far less severe than in the mountains, where the foundations of the houses were shaken. Nobody, however, was hurt.

AN interesting discovery has been made at St. Pierre Quiberon (department of Morbihan). It consists of a new dolmen, one of those stone monuments of grey antiquity. It contained several entire human skeletons, besides a number of skulls, stone axes, a bronze pin, and some fragments of vessels.

THE large gold Cothenius medal, which the Imperial "Leopoldinisch-Carolinische" German Academy of Naturalists at Halle awards every year, has this time been given to Prof. F. Eilhard Schulze of Graz.

THE Berlin Mining Academy has purchased for the Mineralogical Museum of this Institution a so-called lightning tube or fulgurite, which was recently found near Warmbrunn. It measures nearly 2 metres in length. It is specially interesting, inasmuch as it shows a branch formation, about 30 centimetres from its end, measuring half a metre in length. The fulgurite was found after a severe thunderstorm in a sandhill and in a vertical position.

A BRILLIANT meteor was observed at Carlsruhe on March 5 at 8.9 p.m. It was about twice as bright as Venus at her greatest brilliancy. Its direction was S.S.W. to N.N.E.; it left a trail of yellowish red colour and of several degrees in length. The phenomenon finally disappeared in the constellation of Cassiopeia, developing little cloudlets at its disappearance.

AT Salez (canton of St. Gallen) some sixty bronze hatchets have been found imbedded in the ground only one metre deep. Their age is stated to be at least 2500 years.

THE additions to the Zoological Society's Gardens during the past week include an Arabian Baboon (*Cynocephalus hamadryas* ♀) from Arabia, presented by Mr. F. E. Goodner; a Sharp-tailed Grouse (*Tetrao phasianellus*) from North America, presented by Mr. Henry Nash; two Sea Mice (*Aphrodite aculeata*) from British Seas, presented by Mrs. A. Browning; an Olive Weaver Bird (*Hyphantornis capensis*) from South Africa, presented by Mr. Edward Ling; two Bonnet Monkeys (*Macacus radiatus* ♂ ♀) from India, deposited; a Red-vented Parrot (*Pionus menstruus*) from South America, a Sordid Parrot (*Pionus sordidus*) from Venezuela, purchased; a Long-eared Fox (*Otocyon lalandii*) from South Africa, received on approval; a Sambur Deer (*Cervus aristotelis* ♀), an Axis Deer (*Cervus axis* ♀), born in the Gardens.

OUR ASTRONOMICAL COLUMN

THE GREAT COMET OF 1882.—Herr Stechert has continued his ephemeris of this comet from the elliptical elements by Dr. Kreutz, which still agree pretty nearly with observations. We extract as follows:—

At Berlin, Midnight

	R.A.	Decl.	Log. distance from Earth.	Log. distance from Sun.
	h. m. s.	° ′		
April 9	5 59 32	−8 43·1	0·5973	0·5787
11	6 0 30	8 29 5		
13	− 1 30	8 16·5	0·6084	0·5843
15	− 2 33	8 3·9		
17	− 3 37	7 51·9	0·6191	0·5898
19	− 4 43	7 40·4		
21	− 5 50	7 29·3	0·6294½	0·5952
23	− 6 59	7 18 8		
25	− 8 10	7 8·7	0·6393	0·6005
27	− 9 22	6 59·1		
29	− 10 35	6 50·0	0·6487	0·6057
May 1	6 11 50	−6 41·3		

Assuming the intensity of light = 1, on February 8, when Prof. Schmidt last saw the comet with the naked eye at Athens, the intensity on April 9 will be 0·234, and on April 29, 0·163.

From September 8, the date of the first accurate observation at the Royal Observatory, Cape of Good Hope, to the middle of last month, the comet had described a heliocentric or orbital arc of 339½°; no other comet since the celebrated one of 1680 has passed over so large an arc of its orbit while under observation. Between Kirch's first observation on the morning of November 14, 1680, and the last observation by Sir Isaac Newton on March 19, 1681, that body traversed a heliocentric arc of 345°.

VARIABLE STARS.—Mr. G. Knott has observed three more minima of Ceraski's variable U Cephei. The resulting times of minima are—

	h. m.		
1883, March 12,	11 49	G.M.T. ...	Magnitude 9·4
,, 22,	11 10	,, ...	,, 9·3
April 1,	10 29	,, ...	,, 9·45

Mr. Knott remarks that the star is not a very easy one to observe, and it is not therefore an easy matter to disentangle errors of observation from real irregularities in the light curve.

On March 31 and April 1 he found the variable star R Coronæ Borealis very visible to the naked eye, and nearly equal to π Coronæ. "It has presumably brightened up further since Schmidt's observations towards the end of last year" (*Ast. Nach.* No. 2491). π Coronæ is a sixth magnitude according to Argelander and Heis. The variability of this star was established by Pigott in 1795, but its fluctuations are exceedingly irregular.

Schönfeld in his last catalogue gives, as the limits of variation, 5·8m. and 13m. The actual position is in R.A. 15h. 43m. 45s., Decl. + 28° 31′·0. Schmidt found that a star which precedes R Coronæ by 2 seconds, and 7½ minutes N. varies from 11m. to 13·12m. in a period of perhaps 1½–2 months (see *Ast. Nach.* No. 1915).

Bradley 396 has been so discordantly rated in our catalogues that variability appears highly probable, and the period may not be a long one. The estimates are from 4·5m. to 7m. It is Groombridge 580, Fedorenko 473, and B.A.C. 906. The position for 1883·0 is in R.A. 2h. 53m. 40s., Decl. +81° 1′·0.

Prof. Pickering reports that a careful study of the fluctuations of Sawyer's variable by Mr. Chandler shows that it belongs to the Algol class, and has a period of little over 20 hours. A long series of observations of the light curve and successive minima gives 20h. 7m. 41·6s. ± 1·3s.

THE LATE TRANSIT OF VENUS.—Prof. Pickering has published the results of contact observations in the transit of Venus made at the observatory of Harvard College; the times are as follow:—

	h.	m.	s.		
First external contact ...	2	4	32	G.M.T. by 3	observers.
,, internal ,, ...	2	24	43	,, by 4	,,
Last internal ,, ...	7	47	40	,, by 6	,,
,, external ,, ...	8	7	52	,, by 6	,,

These differ from the times given by the equations of reduction inserted in this column by + 58s., + 11s., + 22s., and −25s. respectively, a very close accordance, considering that observations of the first external contact are less certain than the others.

GEOGRAPHICAL NOTES

AT a recent meeting of the Geographical Society of Copenhagen, Capt. Irminger in the chair, Dr. Oscar Dickson was present to give an account of the proposed Swedish expedition to Greenland. The chairman referred to Dr. Dickson as the Mæcenas who enabled Nordenskjöld to carry out his ideas, while both had an ardent supporter in King Oscar. Of the Arctic expeditions, which wholly or partly owed their origin to Oscar Dickson, he mentioned the following:—The expedition of 1868 to Spitzbergen was almost entirely paid for by him; the expedition of 1870 to Greenland was entirely paid for by him; the expedition of 1872–73, which wintered at Spitzbergen, was partly paid by him, while the great deficiency subsequently arising was covered by him; the expedition of 1875 to the Yenissei was entirely paid by him; the expedition of 1876 to the same river, by sea and by land, was chiefly paid by him; the *Vega* expedition of 1879–80 was paid to the extent of one-third by him, and if the vessel had not succeeded in rounding Asia he would have borne the entire cost of this expedition; and eventually the cost of the Swedish expedition of 1883 would be borne by him. It should also not be forgotten that, at the time when the despatch of the *Dijmphna* expedition was nearly frustrated for the want of 20,000 kr. (1150*l.*), Oscar Dickson came forward to supply the deficiency, and although it was most generously contributed by Mr. Gamél, every lover of geographical discovery ought to appreciate his noble action. Dr. Oscar Dickson next addressed the meeting. He began by stating that the King of Denmark had sanctioned the new expedition. Nordenskjöld had not desired that the programme of the expedition should be made public too soon, as he was much occupied with preparations for his journey and his duties as a senator, and if his plans should be questioned by *savants*, he would have no time for discussing them. He next referred to the oldest accounts of Greenland, its colonisation from Iceland, and "Esquimauxising" from America. After this, Greenland was for a time forgotten, until the west coast was rediscovered. The speaker then mentioned the achievements of Hans Egede, and the founding of a commerce. The west coast was one of the best known Arctic countries, both geographically and ethnologically; but not so the east coast. In spite of several expeditions and researches, only the southern portion was known. The interior was a *terra incognita*. These tracts were, however, too important to remain unknown. He then referred to the wanderings of Nordenskjöld and Lieut. Jensen on the inland ice. From these expeditions it was impossible to infer that the interior of Greenland was entirely covered with ice, while in the constant advance of the glaciers and their melting off he (the speaker) found a corroboration of this theory. By the geographical appearance of Greenland, and more especially by the circumstance that the country gradually rose in the interior, it was more than probable that the interior was not entirely covered with ice. Even in the temperature and moistness of the air there seemed a proof that the country would answer to its name. In any case the exploration of the interior of this country was most important, and it was for this purpose that the expedition would make its researches. To these belonged the ascertaining of the extent of the drift ice between Cape Farewell and Iceland, the study of the inland ice, the fossil remains, and the cosmic dust in the island. Eventually it was hoped that, while Nordenskjöld made his expedition across the ice, another party of the members would pay a visit to the west coast, where there were some very peculiar blocks of ironstone. The expedition would possess a complete staff of scientific specialists. The expedition had also one more object in view, viz. to settle the question as to where the Österbygd had been. Every one who read without prejudice the oldest accounts could but come to the conclusion that its remains must be found on the east coast. After excursions on the inland ice, it was the intention to attempt to penetrate northwards along the east coast. In May next the expedition would start in a well-equipped steamer, and, if the state of the ice would permit, first land on the east coast; but as this was not expected to be the case the party would land on the west coast, and when the researches here were at an end they would proceed along the east coast in a channel between the land and the drift ice. In September, next the expedition would return.

THE changes of level of the Caspian are still a puzzling problem for Caucasian geographers. It is known that the late M. Khanikoff was of opinion that the level of this sea has been rapidly falling during our century. After having been, in 1780, 13 feet above the level of 1852, and 10 feet in 1820, he said, it was only 3·3 feet higher in 1830, and has almost regularly decreased since. Sokoloff maintained that it had risen and fallen at irregular intervals since 1744, but was 10 feet lower in 1830 than in 1780. Lenz admitted that it had fallen about 10 feet during the years 1816 to 1830. In any case, for the benefit of subsequent measurements he made permanent marks at Baku showing the level of the sea in 1830, and since that time measurements of level were carried on at Baku. But their results were unsatisfactory—as it appears from M. Filipoff's paper in the last number of the Caucasian *Izvestia*—and the only sure result is that on May 30, 1853, the level of the Caspian was 2 feet 1·3 inches lower than in March, 1830. In September, 1854, at high water it already had risen 1 foot 6 inches *above* the mark of May 30, 1853. On June 4, 1882, that is, at high water, it was also higher than in 1830 by 10·5 inches, so that it may be admitted, according to M. Filipoff, that since 1830 the level of the Caspian, although subject to fluctuations (such as a rapid rise after 1847), has not sensibly fallen during the last fifty years.

ACCORDING to the recent explorations of M. Yadrintseff, the situation of the aborigines throughout Northern and Middle Siberia is very precarious. The Bakaharians and Tartars, who were formerly a privileged class of merchants, and number at present 43,670 souls in Middle Siberia, are decreasing, and belong to the poorest population of the country. The Voguls in the Government of Tobolsk number 6070, and their increase is insignificant. As to the 23,070 Ostyaks and Samoyedes, they are in the worst imaginable position; the rate of increase is very low, while in other parts they obviously decreasing. They are accustomed now to eat bread, but have no means to provide it in necessary quantities owing to its high price. As to the southern Tartars, who have maintained their pasture lands, they are in a better position; those of Barnaoul and Biysk, who are agriculturists, and those of Kuznetsk, living on trade, are on the increase; and M. Yadrintseff quotes an instance of ten families who have maintained their land and occupy now seven villages, making a total of 1270 souls. The dying out of these aborigines is the more regrettable, as M. Yadrintseff proved by numerous instances that they display a high degree of intelligence, and might adapt themselves to new conditions.

IN the April number of *Petermann's Mittheilungen* there is a full account, by Dr. Rink, of recent Danish researches in Greenland,—on the geography of the interior, the ice-formation and glaciers, geology and mineralogy, botany and archæology; accompanying the paper is a map of the west coast between Godhavn and Pröven, coloured geologically. Baron von Richthofen discusses the value of the copy of "Marco Polo" recently discovered in the royal library of Stockholm.

It is reported that Dr. Emil Holub is at last about to start again for the dark continent. As before, so will Dr. Holub now go to Africa without one penny State assistance; and the only support he could obtain is that a special train will carry his cases to the Austrian frontier, and, if the German Government permit, to Hamburg, where they will be embarked for South Africa. The money for his expedition he acquired himself by lecturing in Vienna, Berlin, London, &c. He will leave Austria after he hears that his cases have arrived in Africa, in about two months, and he contemplates remaining on the African continent about four years. The 150 cases and about 100 other packages which he takes contain all that is necessary for a scientific expedition, including scientific instruments which the Austrian War Ministry lends him. He has also a transportable iron cart, which can be taken to pieces, and an iron boat on Stanley's celebrated models, both gifts of Austrian manufacturers or private persons. The remainder of the cases are filled with utensils, arms, stuffs, cotton goods, &c., for the natives, and all other necessaries.

The Museum for Commercial Geography was opened at Berlin in the Architekten House on April 1.

The Imperial Geographical Society of St. Petersburg has awarded its highest distinction, viz. the Constantine medal, to Dr. Hermann Abich of Vienna, for his work, "Geological Researches in the Caucasus."

FACTS AND CONSIDERATIONS RELATING TO THE PRACTICE OF SCIENTIFIC EXPERIMENTS ON LIVING ANIMALS, COMMONLY CALLED VIVISECTION[1]

[Issued by the Association for the Advancement of Medicine by Research]

§ 1. MEDICINE, as the art of preventing and curing disease, depends first, upon Anatomy and Physiology, or knowledge of the structure and working of the human body in health; secondly, upon Pathology, or knowledge of the origin, course, and results of disease; and thirdly, upon knowledge of the effects of various mechanical, physical, or chemical means which prevent or modify diseased processes, and are thus available for preventive or curative Treatment.

As in every other practical art, the application of scientific (that is to say, exact and general) knowledge to particular cases must be checked and controlled by practical experience. But the history of medicine abundantly proves that experience is productive only in so far as it is guided by the habit of scientific inquiry and quickened by physiological knowledge. The foundation of efficient medicine was laid by the discoveries of the sixteenth century in anatomy, and of the seventeenth century in physiology, and its rapid progress in modern times has been chiefly the result of discoveries in physics, in chemistry, and in general biology.[2]

Medicine then, including Hygiene, or preventive medicine, and Therapeutics, or curative medicine, whether it acts by operative and mechanical measures,[1] by the administration of drugs, or by other means, does not depend upon arbitrary dogmas, or upon the theories of one or another school; it depends upon accurate knowledge of the structure and functions of the body in health and disease, and of the effects of various agents upon it, applied in each case by the aid of bedside experience—*καθ' ἕκαστον γὰρ ἰατρεύει.*

The relation of medicine to physiology and pathology is the same as that of navigation to astrometry and meteorology, or of engineering to applied mathematics, or of dyeing and other manufactures to chemistry. A seaman may safely direct a vessel who is ignorant of the construction of a quadrant; a bridge may be built without knowledge of theoretical mechanics, and a watch may be "cured" or a musical instrument "tuned" by a workman who is unacquainted with mathematics or acoustics. In the same way many men are useful practitioners of medicine who are imperfectly acquainted with the scientific basis of their practice. But it is only the most ignorant of sailors who sneer at natural science, and the most presumptuous of watchmakers who rail at mathematics.

§ 2. The knowledge of the functions of the body in health, or Physiology; the knowledge of the origin and course of diseases, or Pathology; and the knowledge of the action of remedies, or Pharmacology, like other branches of natural science, depend entirely upon observation and experiment. Mere observation at its best is but careful noting of such experiments as natural laws or accident may present; experiment, or observation of events under intentionally varied conditions, is absolutely necessary in addition.[2] Indeed, it would be as unreasonable to expect the "Institute of Medicine" (as physiology and pathology are rightly called) to advance without laboratories and experiments on animals, as to hope for progress in chemistry or physics by allowing only observation upon metals and gases and forbidding the performance of experiments.

It is true that there are special difficulties in the study of the natural laws of living bodies. The conditions are far more complicated than those of the inorganic world, and observations and experiments must be proportionately numerous, well-devised, and cautiously interpreted. Fallacies of observation and deduction are difficult to avoid, and often results are seemingly contradictory until their true meaning is perceived by help of fresh experiments and more careful reasoning. But the great and assured results which have been already obtained prove that these difficulties are far from insurmountable. All our present knowledge has been achieved in spite of them, and thereby the path to future discoveries has been cleared. No reasonable person would disparage experimental inquiry into the functions of plants and the cultivation of crops, because the laws of vegetable life are more complicated and obscure than those of mineralogy: or would call the experiments of the botanist useless because they are difficult.

That experiments on living creatures, like all other experiments made by fallible persons, have sometimes misled, is an obvious truth. Many errors attended the first application of the stethoscope, of the microscope, and of chemical analysis to medicine, so that impatience and ignorance pronounced that each of these valuable methods of investigation was useless.

§ 3. The future progress of medicine, in the widest sense of the word, of the art which prevents disease, promotes health, relieves sickness and prolongs life, depends upon the same cause which has led to its present position—upon more complete acquaintance with the laws of health and disease. These laws have been, and can only be, successfully investigated by observations and experiments.

This conclusion is not only the inevitable result of reasoning,

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[1] The term "Vivisection" is open to objection. As a question-begging epithet, it produces an unfounded prejudice against experiments, of which the majority are painless, and of which the object is to relieve the sufferings of both man and brutes. Moreover, the term is at once too narrow and too wide: too narrow, since it excludes painful experiments which do not involve cutting, such as exposure to disease; and too wide, since it includes painful procedures upon animals for other than scientific or humane objects, for food, as in preparation for the table, for convenience, as in horse and cattle breeding, or for amusement, as in certain sports. The same operation which, if performed for the acquirement of knowledge, is called a vivisection, is not called a vivisection when performed for a less worthy object.

[2] Some otherwise well-informed persons have expressed doubt as to the reality of the great progress of medicine during the present century. This doubt arises partly from an arbitrary separation of what is called internal medicine from surgery (la médicine opératoire) and from preventive medicine. The world fully appreciates such triumphs of medicine as the cure of Aneurism and prevention of Small-pox, the discovery of Anæsthetics and the success of Ovariotomy, the results of Antiseptic surgery, the vastly decreased mortality after operations, and the protection of cattle from pestilence by inoculation. But in the treatment of fevers, inflammations, and other internal diseases, conventionally called medical, progress is less striking, because, being more obscure, these maladies have not yet been brought under the complete influence of scientific investigation.

In proof, however, that the scientific spirit of modern medicine has not failed to advance the treatment of even the more obscure diseases, and that practical advance in medical treatment has not been limited to operative surgery, may be adduced as instances: the greatly lessened mortality in Fevers, owing to physiological observations and scientific treatment, the improved diagnosis and more successful results in cases of paralysis and other diseases of the Nervous System; the far shorter and less painful course of acute Rheumatism; the advance in treatment of Diabetes, Consumption, Dropsies, and affections of the Heart, and the successful cure of numerous forms of disease now proved to be due to animal or vegetable parasites.

"Looking back over the improvements of practical medicine and surgery during my own observation of them in nearly fifty years (writes Sir James Paget) I see great numbers of means effectual for the saving of lives from the detection, prevention, or quicker remedy of diseases and physical disabilities, all obtained by means of knowledge, to the acquirement or safe use of which experiments on animals have contributed. There is scarcely an operation in surgery of which the mortality is now more than half as great as it was forty years ago; scarcely a serious injury of which the consequences are more than half as serious; several diseases are remediable which used to be nearly always fatal; potent medicines have been introduced and safely used; altogether, such a quantity of life and working power has been saved by lately-acquired knowledge as is truly past counting."

[1] "Forasmuch as the Science of Physick doth comprehend, include, and contain the knowledge of Chirurgery as a special member and part of the same."—Statute 32 Hen. VIII. c. 40.

[2] "L'observateur écoute la nature, l'expérimentateur l'interroge."—Cuvier.

but is also enforced by the unwavering testimony of those best qualified to judge,—not only of scientific workers themselves, but of the medical profession in all civilised countries. There is not the smallest danger that the ninety-nine hundredths of the medical profession who are engaged in the daily effort to prevent or relieve disease will undervalue practical medicine in comparison with the more abstruse branches of experimental physiology and pathology; the danger is the other way. With few exceptions physicians and surgeons are not themselves experimenters in physiology or pathology. Their business is to prevent disease and to relieve their patient's sufferings: but they know the benefits which their art has derived from the work of the laboratory, and understand the nature and value of experiments. They are thus at once the most disinterested and the most competent witnesses, and their constant and unanimous testimony ought to be conclusive.[1]

The International Medical Congress of 1881, where upwards of 3,000 physicians and surgeons assembled in London, among whom were the ablest and most respected leaders of the profession in the three kingdoms, in America, and in foreign countries, passed, without a dissentient voice, the following resolution:—

"That this congress records its conviction that experiments on living animals have proved of the utmost service to medicine in the past, and are indispensable to its future progress. That, accordingly, while strongly deprecating the infliction of unnecessary pain, it is of opinion, alike in the interest of man and of animals, that it is not desirable to restrict competent persons in the performance of such experiments."

§ 4. A moral question, however, arises, from the fact referred to in the resolution just quoted, that some of the necessary experiments of physiology and pathology involve the infliction of pain or of death upon certain of the lower animals.

The better informed opponents of experimental medicine do not dispute its scientific and practical value, but assert that no probable benefit to man or animals justifies the infliction of pain.

No one would succeed in closing the laboratories of the chemist, or the observatories of the astronomer, however strong his disbelief in the experimental method of inquiry might be, however cordially he disliked or dreaded the advance of science, or however obstinately he persisted that the useful arts do not depend on scientific data.[2] It is obvious, however, that the fact of pain or death being inflicted in the course of experiments cannot alter their scientific importance and necessity; it only imposes on us the duty of making a comparison between the injury to a sentient creature and the probable benefit to mankind, or to others of its own species. This comparison we will attempt to make.

Happily, the amount of pain inflicted in the course of scientific experiments need only be small, and the destruction of life insignificant. That, from carelessness or want of forethought, experiments have been performed which were "cruel," because the pain produced was excessive and unnecessary, may be admitted. In many countries consideration for the brute creation is still little developed, and the vice of cruelty lightly regarded; even in England, until comparatively lately, the torture of harmless animals was thought an innocent pastime. Men of science have not always risen above the average humanity and moral enlightenment of their age and country. But speaking of this country, and of modern times, it may safely be said that no charge of wanton, needless, or excessive sacrifice of animals can be, or indeed has been, seriously alleged against the small number of experimental physiologists and pathologists at work in the three kingdoms.[1] Science has herself provided the means by which pain is reduced to a minimum. The beneficent discovery of anæsthetics is one cause of the great difference between the sufferings inflicted by Harvey, Boyle, Hales, Haller, Hunter, Magendie and Bell, and the generally painless experiments of a modern laboratory. These may be classified as follows, with reference to the suffering inflicted:—

(1) Many physiological experiments are entirely unaccompanied by pain, and can therefore be performed, according to convenience, either upon animals or upon man himself. Such are many experiments upon vision, taste, smell and touch; experiments on the value of different kinds of food, experiments on the effect of exercise, temperature, and other conditions on the excretions; many experiments on bodily heat, on the pulse, and on respiration.

(2) In still more numerous cases, observations and experiments can be made on the tissues and organs after the death of an animal: *e.g.*, the relative tenacity of the different textures, the mechanical effects of violence upon the bones, the action of the heart (which in cold-blooded creatures continues long after their death) and the whole of a long and important series of experiments on the functions of muscles and nerves, which cause no pain, since they are performed on the tissues of a dead organism.

(3) Next, but far less in number, comes a third class of experiments which are performed on animals rendered insensible by various anæsthetic agents. These can be, and were, by the practice of physiologists long before legislative sanction was added, carried out without any pain or even discomfort to the animal, which being killed before awakening, is deprived of life in probably the most painless manner possible.

(4) There are, however, certain observations, for which it is necessary to allow an animal to recover from insensibility, and to live for a longer or shorter time. In such cases the severest pain, that of the operation, is abolished, and the subsequent suffering is sometimes quite insignificant, usually that of a healing wound, and occasionally that of inflammation, colic, or fever. In many of these experiments the initial pain is so trifling that it would be absurd to give an anæsthetic; such are acupuncture and inoculation. It would be unreasonable to give a rabbit chloroform for such observations as bleeding, vaccination, or pricking with the needle of a subcutaneous syringe, for which no human being would take it.

(5) There remain a small number of experiments in which anæsthetics would be impracticable. These are chiefly the experimental production of various diseases, such as tubercle, glanders, cattle-plague, where the pain is that of the subsequent

[1] It would be invidious to dwell upon the very few exceptions to this almost universal testimony. One only deserves special mention. Sir William Fergusson was one of the most skilful and successful operators, but he had no authoritative claim to give an opinion upon the sources or the methods of surgical science, and even he in his evidence before the Royal Commission admitted the use of experiments on animals.

The testimony of the late Professor Claude Bernard has been often adduced against that of all other physiologists because he once wrote, "Nous venons les mains vides, mais la bouche pleine de promesses légitimes." This phrase occurs in an elaborate exposition of the necessity of experiments on living animals not only for knowledge but for use. Bernard well understood the bearing of experiments upon medicine, but he foresaw future developments of scientific treatment, in comparison with which his own eminent services would appear insignificant. The following quotation shows that his evidence on the whole question did not differ from that of other competent witnesses:

"On voit que la physiologie, ou médecine scientifique, comprend à la fois ce qu'on a artificiellement séparé sous les noms de physiologie normale, de physiologie pathologique, et de thérapeutique. Au point de vue pratique, c'est certainement la thérapeutique qui intéresse au plus haut degré le médecin; or, c'est précisément la thérapeutique que doit le plus de progrès à la physiologie expérimentale."—*Leçons de Physiologie Opératoire*, p. 20.

[2] On the other hand, it is almost as clear that no serious obstacle would be put in the way of even painful experiments in the cause of science, if all their opponents were convinced of their utility, and were acquainted with the methods of science in general, or the facts of medical science in particular. This seems to follow from the very moderate opposition to, or tacit acquiescence in, the infliction of pain for desirable objects which obviously cannot be otherwise attained, such as more delicate food, more docile horses, increased wealth and comfort, or the pleasurable excitement of chasing and killing animals.

[1] The following extract is taken from the Report of the Royal Commission, which was drawn up after a prolonged and patient examination of witnesses and documents, and was signed by all the Commission—Lord Cardwell (Chairman), Lord Winmarleigh, the Rt. Hon. W. E. Forster, the late Sir John Karslake, Professor Huxley, Mr. Erichsen, and Mr. R. H. Hutton:—

"That the abuse of the practice by inhuman or unskilful persons, in short, the infliction upon animals of any unnecessary pain, is justly abhorrent to the moral sense of your Majesty's subjects generally, not least so of the most distinguished physiologists and the most eminent surgeons and physicians."

The imputation of cruelty which has always been indignantly repudiated, has not been substantiated by a single authentic instance. In their evidence, given before the Royal Commission, the Royal Society for the Prevention of Cruelty to Animals state, through their Secretary, that they do not know a single case of wanton cruelty.

On the occasion of the present Act (39 and 40 Vict. cap. 77) being passed, all teachers of physiology, in a memorial addressed to the House of Commons, said:

"We repeat the statement which most of us have made before the Commission, that within our personal knowledge, the abuses in connection with scientific investigation, against which in this Bill it is proposed to legislate, do not exist, and never have existed in this country." Signed by the late Prof. Sharpey (University College, London); Dr. William Carpenter, C.B. (formerly Lecturer on Physiology at the London Hospital); Professor G. Humphry (Cambridge); Professor Rutherford (Edinburgh); Dr. Pavy (Guy's Hospital); Dr. M. Foster (Trinity College, Cambridge); Dr. Burdon Sanderson (University College, London); Dr. Robert McDonnell (Dublin); Prof. Redfern (Belfast); Prof. Cleland (Galway); Prof. Charles (Cork); Prof. McKendrick (now of Glasgow); Dr. Pye-Smith (Guy's Hospital); Prof. Yeo (King's College, London); Mr. Charles Yule (Magdalen College, Oxford); Prof. Gamgee (Owens College, Manchester).

disease, and more justly described as discomfort than as torture; and the trial of certain modes of treatment, as inoculation, and of various drugs, where the suffering produced is less than the familiar effects of corresponding remedies in human beings. Probably the most painful scientific experiments ever performed have not been vivisections at all. Such are those of ascertaining the effect of starvation, carried out abroad many years ago; observations of great value and importance, but happily not needing repetition.

Vivisections in the popular sense of the word, experiments comparable to surgical operations, involving cutting and irritation of sensitive parts, can, with few exceptions, be performed without the slightest pain. Hence the results of acutely painful experiments, comparable with the pain endured by rabbits and weasels caught in ordinary traps, by young animals being gelded, by wounded birds, or by rats poisoned with strychnine or phosphorus, are not to be found in our physiological laboratories.

That the utmost possible limitation of the infliction of pain has always been the object and practice of scientific workers in England,[1] is sufficiently proved by a Report which was drawn up by a Committee of the Physiological Section of the British Association for the Advancement of Science, in 1871, several years before the appointment of the Royal Commission.

While the suffering caused to animals by scientific experiments has been enormously exaggerated, both absolutely and relatively, no one denies that both pain and death are and must be inflicted thereby. Otherwise there would be no more reason for licensing and inspecting the physiologist's laboratory than that of the chemist. The whole question is one of justification for causing the pain or death of brutes. Few who compare the extent of suffering and of slaughter thus caused with that generally recognised as right in other cases by enlightened Christian morality, or who compare the objects for which animals commonly suffer pain and death (for food, for dress, for profit, for convenience, or for amusement) with those of the scientific observer (for advance of knowledge and for relief of human suffering) will hesitate to conclude that so long as the principles and practice of scientific men in this country continue what they now are, their investigations should rather be fostered than impeded.

But any possible danger of abuse is prevented by the Act passed in 1876, by which not only are all physiological laboratories placed under the inspection of the Home Office, and exist only by its license, but, in addition, no experiment involving pain can be performed without a special, elaborate, and carefully guarded certificate. Indeed, so stringently has the law been administered that more than one investigation of great practical value has been prevented, others have been injuriously hampered or delayed, and a serious check has been given to medical science in England. In two instances eminent members of the profession found it necessary to go abroad in order to carry out investigations of great importance. The object of one was to decide a question in relation to treatment of wounds; that of the other was to determine the action of certain new drugs.

This was certainly not the intention of the Royal Commission in recommending, or of Parliament in passing, an Act for the purpose of preventing possible abuses without hindering scientific and useful work. What is now needed is such an expression of opinion in Parliament as will permit the Act to be worked in the spirit in which it was framed and loyally accepted, and according to its strict provisions. It may be remarked that attempts have been made, by the same methods of agitation, to check physiological research by legislation in Germany, Denmark, Sweden, and the United States: but in each case the humane and enlightened judgment of the country has refused to impede researches of which the usefulness is beyond dispute.

§ 5. It has been imagined that students of medicine perform operations upon living animals in order to gain manual dexterity; such a practice would be as useless as it would be reprehensible, and has never, we believe, been thought of. For our veterinary surgeons it would be quite unnecessary, and they have always reprobated the practice.

It has also been supposed that students might, for amusement, perform physiological experiments upon living animals. This would be practically impossible, since not only are knowledge and skill necessary, but a properly equipped laboratory and suitable appliances.[1] If, however, any ill-disposed person without scientific object or training should be guilty of cruelty most alien from the practice and the training of the profession, there is no doubt that every member of it, teacher or student, would help to detect and punish such conduct.[2] The case has never arisen; if it did, it could be efficiently dealt with under the law known as "Martin's Act."

§ 6. The real objects of scientific experiment on living animals are briefly as follows:—

i. *To extend, correct, and define our knowledge of the functions of the living body.*

Even apart from ulterior advantage to medicine, physiology must be held to be a branch of science of at least equal importance with chemistry or geology; and to be successfully cultivated, it must be cultivated for its own sake, without perpetual or premature inquiry as to the immediate and material results which increased knowledge of the laws of Nature will bring. In physiology, as in other natural sciences, the investigator must have primarily in view the discovery of truth; for, in the words adopted by the Royal Commissioners, "if in the pursuit of science he seeks after immediate practical utility, he may generally rest assured that he will seek in vain." There must be, to quote the words of an older authority, "light-bearing," as well as "fruit-bearing experiments."

As examples of this first kind of experiment, and of their success in extending useful knowledge, we may refer to the following:—

(1) The great discovery of the circulation of the blood by Harvey, the firstfruits of the experimental method.[3] Upon this as the foundation depends all the subsequent progress in the surgical treatment of hæmorrhage and of aneurisms, and the recognition and treatment of diseases of the heart, the arteries, and the veins.

(2) The discovery of the effects of electricity on animals by Galvani and Volta, from which have resulted not only the development of one great branch of electrical science, but also important means of diagnosis and treatment in cases of paralysis.

(3) Artificial respiration, invented and improved in the case of animals with purely scientific objects by Vesalius, Hooke, Lower, and others, and long afterwards applied with complete success to resuscitation from drowning.

(4) The experiments of the Rev. Dr. Hales on pressure of the blood in the arteries.

(5) Those of Boyle, Hooke, Mayow, and other natural philosophers on respiration.

(6) Transfusion of blood from one animal to another, accomplished by Sir Christopher Wren and others of the early Fellows of the Royal Society in the seventeenth century, but only recently, owing to fresh physiological knowledge, applied with success to the saving of human life.

(7) Experiments by a Committee of Physicians at Dublin, in 1835; showing the way in which the sounds that attend the action of the heart are produced, and enabling physicians to judge of the condition of the organ by the alterations of the sounds.

(8) The discoveries of reflex action and of the separate endowments of motor and sensory nerves, on which much of our present knowledge of the functions and disorders of the nervous system is founded.

(9) The discovery of vasomotor nerves.

[1] The following quotation, from a Manual of Physiological Experiment by a well-known German physiologist, will serve to show that humane consideration for animals is not confined to this country:—"An experiment involving vivisection should never be performed, especially for purposes of demonstration, without previous consideration whether its object may not be otherwise attained:" and, as a second rule, "Insensibility by chloroform or other drugs should be produced whenever the nature of the experiment does not render this absolutely impossible."—Cyon, *Physiologische Methodik*, p. 9.

[1] It is obvious that this sound general principle admits of exceptions when the skilled person with suitable appliances must, from the nature of the case, carry out his researches on board ship, as for instance for investigation into the functions of jelly-fish, or the electric torpedo; or in the open fields, as in inquiries into means of protection from epidemic diseases of cattle.

[2] For the real sentiments of medical students, see Dr. Pavy's evidence before the Royal Commission, *Blue Book*, p. 214.

[3] Some persons have ventured to deny that Harvey's discoveries were due to vivisection, on the faith of a reported statement of his to the Hon. Robert Boyle (another eminent vivisector), and in contradiction to Harvey's express words. Others have denied that the circulation was proved by vivisection, because Harvey having proved all but one point by a series of experiments on living animals, Malpighi completed the demonstration by another experiment on another living animal. The full account of the matter is contained in Harvey's own treatise, "De Motu Cordis et Sanguinis." It is briefly referred to in the article *Harvey* of the "Encyclopædia Britannica," and in the evidence of Professor Turner, of Edinburgh, before the Royal Commission (Blue Book, pp. 157, 158); where also are given the account of the discovery by vivisection of the great system of lymphatic vessels, by Aselli and Pecquet, and of the discovery of motor and sensory nerves by the same means by Bell and Magendie.

ii. *To obtain direct and exact knowledge of the processes of disease.*

The following examples may be cited :—

(1) Experiments relating to the nutrition of the body and the maintenance of its constant temperature constitute the basis of the existing knowledge of fever.

(2) Experiments relating to the mechanism of the circulation, and to the influence of the nervous system thereon, have served to explain the nature and mode of origin of the various forms of dropsy

(3) Experiments as to the effect of plugging arteries (Embolism) have afforded explanations of diseased processes previously not understood, and in particular of many obscure cases of sudden death.

(4) Experimental investigations of the functions of the liver and other secreting glands have materially advanced our knowledge of diabetes and of the affections known as Bright's disease.

(5) Knowledge gained from experiments relating to the mode of action of the muscles, and of the nervous system which regulates them, constitutes the basis of the pathology and diagnosis of convulsive and paralytic diseases.

(6) Experiments on animal grafting and as to the nature of the processes by which wounds are healed and injured parts restored. Among the best known are those which relate to the mode of repair of fractured or otherwise injured bones, particularly the researches of Duhamel (1740), Sir Astley Cooper (1820), and Syme (1831). In recent times such inquiries have been pursued much more completely by Ollier and others, and with practical results of ever-increasing value.

(7) The dangerous form of blood poisoning after operations has been investigated by strictly physiological experiments, with the result of almost complete protection from it.

(8) Researches into the origin and nature of inflammation, by Redfern, Cohnheim, Von Recklinghausen, and others, have been of necessity conducted by means of experiments on animals, and have proved of great practical value.

(9) Our recently extended knowledge of the locality of diseases of the brain, and of their accurate diagnosis and treatment, has been due, partly to clinical observations, partly to pathological investigations, but also, and not least, to direct experiments upon the lower animals.[1]

iii. *To test various remedial measures directly.*

The utility of the greater number of the older remedies and methods was first learnt empirically: but many of them were not applied to the best purpose until they have been investigated by observations on the lower animals. As regards the remedies and appliances of modern times, they have, in almost every instance, been investigated first and brought into use afterwards. For example :—

(1) Subcutaneous injection was used in the laboratory for years before it was applied in practice.

(2) The useful property of the well-known anodyne chloral hydrate was first investigated in the laboratory, and then introduced into practice.

(3) Pepsin and pancreatin were known for years as physiological agents before they were applied in practice.

(4) The action and mode of administration of such important new drugs as nitrate of amyl, physostigma, and the anæsthetic, methylene, were discovered entirely by physiological experiments.

(5) The better appreciation and more useful application of some of the most valuable remedies were gained by experiments, such as those by Traube on digitalis, by Magendie on strychnia, and by Moreau and others on saline purgatives.

(6) The application of various practically useful methods of checking hæmorrhage was tested upon animals before being tried on human beings, with the result of saving innumerable lives.[2]

(7) Similar preliminary trials of subcutaneous and other operations, especially those of tenotomy, have helped in the relief of numerous deformities; while the trial of such formidable operations as excision of the kidney and tentative improvements in ovariotomy have led to some of the most brilliant results of modern surgery.[3]

In cases where new drugs are to be introduced, or new operative methods tried, the first experiments must be made either upon living animals or upon living men. Where circumstances excluded the former alternative, members of our profession have not hesitated to make themselves the subject of often hazardous experiments: but happily, in most instances, the sacrifice of a few guinea-pigs or frogs will suffice to help in saving human life.

iv. *To ascertain the means of checking contagion, and preventing epidemic disease both in man and in brutes.*[1]

An experiment of this kind, inoculating the udder of a cow so as to produce a vaccine pustule, was one of the links in the great discovery of Jenner. Among more recent examples may be mentioned :—

(1) The experimental investigations of the last fifteen years, as to the origin and nature of the infective diseases which spring from wounds and injuries (pyæmia and septicæmia), the results of which constitute the basis of antiseptic surgery.[2]

(2) The discovery by experiments of the infective nature of tuberculosis (1868), of its relation to chronic inflammation, and finally of the dependence of its infectiveness on a living parasitic organism (1881).

(3) Discovery of the mode of origin, and consequently of the prevention, of various parasitic entozoa (hydatids, trichina) which infect the human body, by inference from investigation of their development in the bodies of animals.

Among diseases of animals may be mentioned :—

(1) Silkworm disease, which has been brought completely under control by the experimental discoveries of Pasteur.

(2) Small-pox of sheep, against which preventive inoculation has been long used.

(3) Cattle-plague, the prevention of which is entirely founded on the knowledge of its mode of spreading gained by experiment.

(4) Pleuro-pneumonia of cattle, and foot and mouth disease, of which, although experiment has not as yet yielded a satisfactory mode of prevention, it has furnished exact knowledge as to the method of its propagation.

(5) Splenic fever of cattle, and the analagous diseases of horses, sheep, and other animals, against which experiment has recently furnished a mode of prevention, now successfully used in countries in which this disease has most fatally prevailed, particularly in France.

(6) Farcy and glanders, the early detection and prevention of which has been greatly promoted by experiments.

v. *For instruction.*

It is not necessary to insist on the well-known difference between book-learning and demonstration. Like chemistry, physiology must be taught practically if it is to be taught well, and it is necessary that all students of medicine to whom the care of the human body will be intrusted should have a practical and thorough familiarity with the most important functions of that body. For this purpose no painful experiments are necessary, and none are performed in our medical schools and colleges. Most of the demonstrations of what is called "practical physiology" are demonstrations of the microscopical structure of the tissues, or of their chemical properties and processes, or of their physical endowments, and the remainder apply to the organs of insensible or recently killed animals. Whether the occasional repetition of an experiment of great importance, and involving very little pain, would be morally justifiable may admit of question; but, as a matter of fact, it is not and cannot be done. Apart from the provisions of the Act, this question was decided long before by the resolution quoted above.

vi *For the detection of poisons.*

The fact that some of the most subtle and dangerous poisons cannot be certainly identified by ordinary testing (*i.e.* by recognition of their physical and chemical properties), is well known. In such cases the physiological test, or the effect of the poison upon the lower animals, is the only means by which the guilt of murder can be brought home to a criminal, or the innocence of a wrongfully accused person established. This, like many other scientific facts, has been disputed by ill-informed persons: but it is beyond serious question.[3]

[1] See an article in NATURE, vol. xxv., p. 73.

[2] See an article in the *Nineteenth Century* for December 1881, p. 926.

[3] See a paper by Mr. Spencer Wells, *Trans. Internat. Med. Congr.* vol. ii. p. 226.

[1] For details on this part of the subject, see the Address by Mr. Simon, C.B., F.R.S., entitled "Experiments on Life as fundamental to the Science of Preventive Medicine." ("Transactions of the International Medical Congress, 1881.")

[2] For details, see a paper in the *Nineteenth Century* for March, 1882, by Mr. George Fleming, President of the Royal College of Veterinary Surgeons: "Vivisection and Diseases of Animals."

[3] See on this subject a paper by Prof. Gamgee, of Owens College, "The Utility of Physiological Tests in Medico-Legal Inquiries."

It was found necessary to insert a clause in the Act allowing a judge to order any needful experiments by a medical jurist. But this may cause, and has already caused, injurious delays, and it would be desirable for each person engaged in this department of scientific work to take out the necessary license beforehand.

§ 7. The above is only a brief enumeration of some of the more striking and illustrative cases in which the objects proposed by experiments on animals have been attained. In some of these success has been brilliant and complete, in others comparative and needing fuller development. In some the results have been the direct and exclusive consequence of the experiments, in others they have been due to these either as confirming or correcting previous conjectures, or as guiding clinical research, or as suggesting fruitful investigations by other methods.

Without exaggerating its extent and cogency, the evidence is ample to show, what no one conversant with the subject doubts, that the great strides made in the practice of medicine during the last fifty years have been chiefly due to the exact scientific experimental inquiries of this epoch. In fact, experience fully bears out what reason demonstrates and authority confirms, that medicine rests chiefly upon physiology, and that physiology cannot advance without experiments.

The prejudices excited by the account of long past or distant abuses of the right and duty of experiment will, it may be hoped, be dispelled (as in many cases they have been) by increased knowledge of the facts; while those which have been raised by reckless misstatement will subside on candid investigation. If any fear remain that evils which do not now exist may possibly arise in future, it may be dispelled by a consideration of the stringent regulations of the existing law, even if carried out with the utmost desire not to obstruct demonstrably useful scientific work.

But it is on the scientific investigator himself that the responsibility must ultimately rest of determining what is the best method of accomplishing a given scientific result, and by what means *the greatest possible result may be obtained at the least possible cost of suffering*. If restrictions are supposed to be necessary to control the conduct of careless individuals, let them be continued; but so long as scientific men exercise their responsibility in the humane spirit which has hitherto guided investigation in this country, they have a right to ask that no unnecessary obstacles should be placed in their way.

It is therefore hoped that such a decided and influential expression of opinion will be made in Parliament as will not only rebuke ill-advised attempts to totally abolish one of the most important methods of natural knowledge, and an indispensable method for the improvement of medicine; but will also strengthen the hands of the Government in administering the law, so as not to interfere with the just claims of science and with the paramount claims of human suffering.

THE BRITISH ASSOCIATION AND CANADA

THE following circular is being sent out by the British Association for the Advancement of Science:—

"22, *Albemarle Street, London, W., March* 19

"SIR,—We have been instructed by the Council of the British Association to communicate to you the accompanying letter from Sir A. T. Galt, G.C.M.G., High Commissioner for Canada. This letter was written in reply to one addressed by us to him, making certain inquiries with reference to the invitation to visit Montreal in 1884, which was accepted by the General Committee at the Southampton meeting last year. In that letter it was our endeavour to obtain information as accurate as possible concerning the probable expense of the journey to and from Montreal, including a stay of a fortnight or three weeks in Canada (in addition to the period of the meeting), and excursions to some of the more interesting localities. From the statements in Sir A. T. Galt's letter, the members will be able to form an opinion as to the probable cost of the expedition, the amount of which must obviously, to a considerable extent, depend upon the length of time which they are willing to devote to the visit.

"It is obviously most important to secure that the Montreal meeting should be attended by a strong and thoroughly representative body of members, so that the gathering may be both creditable to the Association and gratifying to our Canadian hosts. Further, many arrangements must be made prior to the meeting, and these must be settled considerably in advance of the usual dates. It will therefore greatly aid the Council and those who will have to carry out their instructions in detail, if you will be so good as to state your intention concerning the visit to Montreal by filling up the annexed form and returning it as addressed before April 14.

"We remain, sir, your obedient servants,

"C. W. SIEMENS, President
"A. W. WILLIAMSON, General Treasurer
"DOUGLAS GALTON, } General
"A. G. VERNON HARCOURT, } Secretaries
"T. G. BONNEY, Secretary"

"9, *Victoria Chambers, London, S.W., March* 3, 1883

"Dear Sir,—I have to refer you to your letter of November 28 on the subject of the visit of the British Association for the Advancement of Science to Montreal in 1884, in accordance with the decision of the general committee, at their meeting at Southampton on August 28 last, and to inform you that I have received a communication from the Chairman of the Montreal Invitation Committee (T. Sterry Hunt, M.A., LL.D., F.R.S.), containing some detailed information on the different matters you mentioned to me.

"It is my pleasant duty to state that the inhabitants of the city of Montreal received with satisfaction the intimation that the Association had decided to honour them with a visit, and much public spirit has already been manifested in the desire that everything should be done to make the occasion worthy of the illustrious body and of the country. Committees on invitation, on finance, and on conveyance have already been formed, and a guarantee fund opened very satisfactorily; while the Government of the Dominion, in view of the widespread interest which the matter has awakened, will ask Parliament during its present session to vote a considerable sum ($20,000) as a contribution to the funds that will be subscribed by the public. Montreal, I may add, is not without experience of the requirements of an important meeting of the kind, having twice been favoured with visits from the American Association, the last occasion being in 1882, when an attendance of more than 900 members and associates was registered, and the Association, with its nine sections, found ample accommodation in the buildings of McGill University.

"I propose to answer your questions in the same order as that in which they were placed in your communication, but it will not be possible for me to do so in such full detail as I should like so far in advance of the time of their application, especially in regard to the cost of conveyance and the various expeditions to be arranged. I trust, however, the following information will be sufficient for the purpose of giving to the members of your distinguished Association an idea of the probable expenses they may be called upon to defray during their stay in Canada.

"(I.) 'The cost of the journey to and from Montreal to one who makes it as a member of the Association or as the near relative of a member.'

"Dr. Sterry Hunt desires me to say that the committee will arrange fifty free passages for the conveyance of the officers of the Association whose attendance is indispensable at its annual meetings. The funds at the disposal of the committee will also enable it to negotiate with the steamship companies for the reduction of the ordinary ocean passages in favour of *bonâ fide* members of the Association. Two courses are open in which this can be done.

"(1) To arrange for a number of passages to be offered at the single rate for the double journey—say 15*l.* 10*s.*

"(2) For a general reduction, so far as the funds will permit.

"Either of these plans can be adopted, but the steamship companies, although fully disposed to entertain the matter, do not care to make any definite engagements so far in advance, which will, I am sure, be readily understood. I am to state, however, that the committee is prepared, with the aid of the Government grant, to devote 3000*l.* to these purposes alone.

"(II.) 'The cost of board and lodging per head per diem for the above during the week of the meeting at Montreal.'

"I cannot do better than quote a paragraph from Dr. Sterry Hunt's letter, in regard to this inquiry:—'In reply to Prof. Bonney's question as to the expenses of board and lodging for members of the British Association during the meeting in Montreal, the committee will give assurance that free entertainment will be provided for at least 150, and probably for all other members who may attend.'

"I may amplify this by stating for your information that the tariff of the Montreal hotels ranges from $2 50c. to $4 per day inclusive, and that private accommodation can be obtained at much lower prices than in England.

"(III.) 'A scheme of expeditions which would occupy from two to three weeks subsequent to the meeting, and the cost of each of them.'

"T. Sterry Hunt says:—'As to the proposed excursions, we are prepared to say that the Grand Trunk, the Canada Pacific, and the Intercolonial Railways will furnish free transportation over their lines throughout the dominion of Canada from Nova Scotia to the North-West. The Canada Pacific will also arrange an excursion to the Rocky Mountains, and the Grand Trunk one to the Great Lakes (note: this will include Niagara) and Chicago; while the South-Eastern Railway will do the same for the White Mountains and Portland and Boston. For an excursion of this kind, occupying three or four weeks, tourists should be provided with, say, 20*l.* in money for hotels, carriages, and other incidental expenses, though it is possible that a less sum than this would be needed.'

"I am inclosing a copy of a circular that has been prepared by the Montreal committee. It contains interesting information, and it will be seen that the arrangements are in the hands of representative and eminent men.

"I believe from the information that reaches me that the Association will receive the addition of a considerable number of associates in Canada, and that the visit will give an impetus to scientific research in the Dominion such as it has not experienced before. It is confidently anticipated also that the American Association will hold its meeting in 1884 at a convenient time and place, affording an opportunity for scientific intercourse that I imagine does not often occur.

"I will gladly supply any further information you may require if it is in my power to do so, and shall readily cooperate in any measures having for their object the success of the meeting of the British Association for the Advancement of Science at Montreal in 1884.

"I am, dear Sir, your obedient servant,

"A. T. GALT,

"High Commissioner for Canada, and Vice-Chairman of the Montreal Citizens' Committee

"Prof. T. G. Bonney, M.A., F.R.S., F.G.S., &c.,

"22, Albemarle Street, W."

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—The Savilian Professorship of Geometry in the University is vacant, and an election to the office will be held before the end of Trinity Term (July 7, 1883). A Fellowship in New College is now annexed to the Professorship. The duty of the Professor is to lecture and give instruction in pure and analytical geometry. The combined emoluments of the office from both sources will be, for the present, 700*l.* a year, but may possibly hereafter be increased to an amount not exceeding 900*l.* a year. The qualifications required in candidates for the Savilian Professorships by the existing Statutes of the University are as follows:—"Hos Professores sive lectores, prout voluit fundator, statuimus et decernimus fore perpetuis temporibus eligendos ex hominibus bonæ famæ et conversationis honestæ, ex quacunque natione orbis Christiani, et cujuscunque ordinis sive professionis, qui in mathematicis instructissimi sint, et annos ad minimum sex et viginti nati; et, si Angli fuerint, sint ad minimum Artium Magistri." Candidates are requested to send to the Registrar of the University their applications, and any documents which they may wish to submit to the Electors, on or before Thursday, May 31.

VICTORIA UNIVERSITY.—At a meeting of the University Court on March 30, Vice-Chancellor Greenwood laid on the table the supplementary charter, dated March 20, 1883, enabling the University to confer degrees and distinctions in medicine and surgery. After some discussion it was resolved that the Council be empowered and instructed to appoint external examiners in medicine and surgery for a limited period, and to appoint certain lecturers of the University to act as University examiners; also to prepare, after a report from the General Board of Studies, a statute or statutes and regulations relating to degrees in medicine and surgery for the consideration of the Court, and also to report of the subsequent appointment of external examiners in medicine and surgery, in accordance with the recommendation of the University Council. The Council were instructed to ascertain whether the University charter would allow of the same facilities that had been given to Owens College students being extended to the students of other colleges when those colleges sought admission to the University. The Council were of opinion that such facilities should certainly be given.

SOCIETIES AND ACADEMIES

LONDON

Linnean Society, March 15.—Frank Crisp, treasurer and vice-president, in the chair.—Dr. T. S. Cobbold read a paper on *Simondsia paradoxa*, and on its probable affinity with *Sphærularia bombi*. Thirty years ago Prof. Simonds discovered a remarkable parasite within cysts in the stomach of a wild boar which died in the Zoological Gardens, London. Prof. Simonds regarded the worm as a species of *Strongylus*, but Dr. Cobbold in 1864 suggested its affinities might probably be nearer the genus *Spiroptera*, then naming it *Simondsia*. The original drawings unfortunately were lost, and only quite lately, along with the specimens, they have turned up and have enabled Dr. Cobbold to investigate them more closely. He arrives at the conclusion that *Simondsia* is a genus of endoparasitic nematodes in which the female is encysted and furnished with an external and much enlarged uterus, whose walls expand into branches terminating in cæca. The male is ½ inch and the female $\frac{5}{8}$ inch long. Moreover, it is now found that what was at first regarded as the head turns out to be the tail, so that supposed Strongyloid character is incorrect. Taking into account what is known of *Sphærularia bombi* as interpreted by Schneider, and whose views are universally accepted, it appears that *Simondsia*, though unique, yet approaches towards *Sphærularia* in respect of the enormously developed female reproductive organ, which in both lies outside the body proper. Until Sir J. Lubbock's memoir on *Sphærularia* appeared, the so-called male had never been indicated; but, judged by Schneider's interpretation of that genus, the male is still unknown. Dr. Cobbold points out that the so-called rosette in *Simondsia* is morphologically a prolapsed uterus furnished with two egg-containing branches; he regards the external branched processes as homologous with the sphærules of *Sphærularia*, whilst the ultimate cæcal capsules have nothing comparable to them in nature.—A paper was read on the moths of the family Urapteridæ in the British Museum, by Arthur G. Butler. The author, basing distinctions on wing neuration and other characters, redistributes the family, and indicates the following new genera:—*Tristrophis*, *Gonorthus*, *Sirinpteris*, *Nepheloleuca*, *Thinopteryx*, *Xeropteryx*, and *Æschropteryx*.—The eighteenth contribution to the mollusca of the *Challenger* Expedition, by the Rev. R. Boog-Watson was read, in which the author treats of the family Tornatellidæ, therein describing six new species of the genus *Actæon*.

Geological Society, March 7.—J. W. Hulke, F.R.S., president, in the chair.—Messrs. Thomas Gustav Hawley, Richard Lydekker, and J. O'Donoghue were elected Fellows, and M. F. L. Cornet, of Mons, a Foreign Correspondent of the Society.—The following communications were read:—On Gray and Milne's seismographic apparatus, by Thomas Gray, B.Sc., F.R.S.E. Communicated by the President. This apparatus was stated to have for its object the registration of the time of occurrence, the duration, and the nature, magnitude, and period of the motions of the earth during an earthquake. The instrument was made by Mr. James White, Glasgow, and is to be used by Prof. John Milne in his investigations in Japan. In this apparatus two mutually rectangular components of the horizontal motion of the earth are recorded on a sheet of smoked paper wound round a drum, kept continuously in motion by clockwork, by means of two conical pendulum-seismographs. The vertical motion is recorded on the same sheet of paper by means of a compensated-spring seismograph. In details these instruments differ considerably from those described in the *Philosophical Magazine* for September, 1881, but the principle is the same. The time of occurrence of an earthquake is determined by causing the circuit of two electromagnets to be closed by the shaking. One of these magnets relieves a mechanism, forming part of a time-keeper, which causes the dial of the timepiece to come suddenly forward on the hands and then move back to its original position. The hands are provided

with ink-pads, which mark their positions on the dial, thus indicating the hour, minute, and second when the circuit was closed. The second electromagnet causes a pointer to make a mark on the paper receiving the record of the motion. This mark indicates the part of the earthquake at which the circuit was closed. The duration of the earthquake is estimated from the length of the record on the smoked paper and the rate of motion of the drum. The nature and period of the different movements are obtained from the curves drawn on the paper.—Notes on some fossils, chiefly Mollusca, from the Inferior Oolite, by the Rev. G. F. Whidborne, M.A., F.G.S.—On some fossil sponges from the Inferior Oolite, by Prof. W. J. Sollas, M.A., F.G.S. Some fossil sponges have been described from the Inferior Oolite of the Continent, but hitherto none have appeared in the lists of fossils from this formation in British localities. The collection of sponges described by the author was made by the Rev. G. F. Whidborne. The author described eleven species (six of which he identified with those already described from Continental localities) belonging to nine genera, and concluded his paper with some general remarks. These sponges are calcareous, but are considered by the author to have been originally siliceous, replacement of the one mineral by the other having taken place as already noticed by him. The beds in which these sponges are found bear all the appearance of being comparatively shallow-water deposits.—On the Dinosaurs from the Maastricht beds, by Prof. H. G. Seeley, F.R.S., F.G.S.

EDINBURGH

Royal Society, March 5.—The Right Hon. Lord Moncrieff, president, in the chair.—Prof. Turner, in a paper on bicipital ribs, described two examples which he had recently come across in the human subject. In both of these cases, one of which closely resembled a specimen in the Anatomical Museum of the University which Knox had explained as due to the fusion of a cervical with a thoracic rib, the real cause was the union of the two first thoracic ribs. That the former explanation was the true one in certain instances was demonstrated by other specimens; and the distinctive peculiarities of each kind of fusion were pointed out.—Sir William Thomson read two papers on gyrostatics and on oscillations and waves in an adynamic gyrostatic system. The papers were in great part experimental illustrations of the theorems regarding gyrostatic stability which are laid down in Thomson and Tait's "Natural Philosophy" (second edition, vol. i. part i. § 345). It was thus demonstrated to the eye that a system when under gyrostatic domination is stable in positions for which, statically considered, the system is unstable as regards an *even* number of degrees of freedom; so that, to take a particular case, a gyrostat which is unstable, because statically unstable as regards one mode, is rendered stable by making it statically unstable as regards two modes. Hence also an ordinary spinning top is stable because it is statically unstable in two of its degrees of freedom. The curious behaviour of a gyrostat resting horizontally on gimbals with its axis of rotation vertical was also shown, viz. its instability as soon as the framework on which it rested was moved in the opposite rotational sense to the spin of the gyrostat. The author then proceeded to point out that all phenomena of elasticity which are ordinarily treated by assuming forces of attraction or repulsion between parts or stresses through connections can be as readily explained by the assumption of connecting links subject only to gyrostatic domination. The gyrostatic hypothesis led to other consequences which the ordinary dynamic assumption did not involve; but it had not been found as yet that elasticity had properties corresponding to these.—Sir William Thomson also communicated a paper on the dynamical theory of dispersion, which was virtually an application of the principle of forced vibrations to a molecular structure, each molecule forming the nucleus of a region whose density increases gradually from without inwards. As bearing upon the same kind of problem, a model was shown illustrating Prof. Stokes' dynamical theory of fluorescence, which is that, if the first of a connected chain of elements is disturbed by a periodic disturbance having no close relation to the free vibration periods of the chain, the disturbance does not pass along the chain, but has its energy stored up in the first few elements, to be given back again when occasion offers.

PARIS

Academy of Sciences, March 19.—President, M. E. Blanchard.—The following communications were read:—Summary description of a new system of equatorials and its installation at the Paris Observatory, by M. M. Lœwy.—Observations of the Swift-Brooks comet made at the Paris Observatory, by M. Périgaud.—Graphic proof of Euler's theorem on the partition of pentagonal numbers, by Prof. Sylvester.—Observations on blue milk (second part), by M. J. Reiset.—On the second edition of the "Pilot of Newfoundland," of Admiral Cloué, and on a question of atmospherical optics, by M. Faye.—Function of the lymphatic vessels in the production of certain pathologies phenomena, by M. Alph. Guérin.—The following memoirs were presented:—On the possibility of increasing the irrigation waters of the Rhone, by means of reserves to be established in the lakes of Geneva, Bourget, and Annecy, by M. Ar. Dumont.—Determinations of longitudes effected at Chili, by the Transit of Venus Expedition, by M. de Bernardières.—On the number of the divisions of an entire number, by M. T. O. Stieltjes.—On the equations to the partial derivatives, by M. G. Darboux.—On the application of the elliptic and ultra-elliptic intervals to the theory of unicursal curves, by M. Laguerre.—Table of reduced positive quaternary quadratic forms of which the determinant is equal or inferior to 20, by M. L. Charve.—Method of obtaining the formula giving the general integral of the differential equation—

$$x^n \frac{d^n y}{dx^n} + A_1 x^{n-1} \frac{d^{n-1} y}{dx^{n-1}} + A_2 x^{n-2} \frac{d^{n-2} y}{dx^{n-2}} + \ldots + A_n y = f(x),$$

by means of a definite multiple integral, by Abbé Aoust.—New equations relative to the transmission of force, by M. Marcel Deprez.—The transmission of force by batteries of electrical apparatus, by M. James Moser.—On the maximum yield which a steam motor may attain, by M. P. Charpentier.—Influence of tempering on the electrical resistance of glass, by M. G. Foussereau.—On a modification into the bichromate of potassium pile to adapt it for lighting, by M. Trouvé.—On the calories of combination of the glycolates, by M. D. Tommasi.—On mononitrosoresorcine, by M. A. Févre.—Contributions to a study of the plastering of wires, by M. P. Picard.—Physiological effects of coffee, by M. J. A. Fort.—On salmon-breeding in California, by MM. Raveret-Wattel and Bartel.—On the solenoconchal molluscs of the deep sea, by M. P. Fischer.—Ovogenesis among the Ascidians, by M. Ad. Sabatier.—Influence of the wind on meteorological phenomena, by M. E. Allard.—On the hail-storm of March 9 at the Hyères Salines, by M. Le Goarant de Tremelin.—The Alfianello meteorite, by M. Denza.

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THURSDAY, APRIL 19, 1883

THE SCOTCH UNIVERSITIES BILL

THE long expected Scotch Universities Bill has at last made its appearance. As no explanation of its provisions has yet been offered in Parliament, and the Scotch newspapers have shown the caution characteristic of their country in declining to commit themselves to an opinion about it till they learn what its authors have to say in its favour, it may be interesting to our readers to know what the Bill proposes to do and how it proposes to do it. So much at least can be stated in a few sentences. The Scotch Universities derive a considerable portion of their revenues from Parliamentary grants. The Bill proposes to give them a sum which is estimated at about 8000*l.* a year, or 25 per cent., more than they now get; to remove the whole of their payment from public moneys from the annual estimates to the Consolidated Fund; to settle this sum of 40,000*l.* on them "in full discharge of all claims past, present, and future," and to cut them adrift. They now get really about 28,000*l.* annually, the other 4000*l.* going to two institutions—the Royal Observatory in Edinburgh, and the Botanic Garden there, which are in future to be handed over to the University of Edinburgh and to be maintained by it out of the portion of the 40,000*l.* to be allocated to it. The allocation of this sum as between the Universities is to be made once and for ever by a new Executive Commission, with whose judgment, except in the form of a somewhat complicated and expensive appeal to Her Majesty in Council and the usual formal laying of their ordinances on the table of Parliament, the State will not farther concern itself.

The second main provision of the Bill is that these Commissioners are directed to make ordinances, subject only to the same appeal, regulating everything in or concerning these Universities, and in particular fixing anew the *constitution* and functions of all the various University bodies and officers, such as the University Court, the University Council, the Senatus Academicus, the chancellor, the rector, the assessors, and all other University officers. They are directed in only two particulars. They are to institute a first examination which is to be compulsory on all persons who intend to graduate in Arts or in any other Faculty, and to institute if they think fit, in any or all of the Universities, a new Faculty of Science, subject to these particular directions: they are to "regulate the manner and conditions in and under which students shall be admitted, the course of study and manner of teaching, the amount and exaction of fees, the length of the academical session or sessions, and the manner of examination."

The next important duty imposed on the Commissioners is to report within twelve months whether in their opinion it is no longer possible for the University of St. Andrews, which is the oldest and by far the least numerously attended of the four, "in consequence of the want of sufficient endowments," to "continue to perform its functions with advantage," and in the event of their so reporting they are to make "suggestions for dissolving that University and its Colleges, and creating a new corporation to which the funds and property of the University and Colleges shall be transferred."

There is another curious provision, which we mention only from the interest which will generally attach to it, not because we should venture in this place to express any opinion about it, in one way or another. Like all the Universities in the kingdom, except London and the new Victoria University, the Scotch Universities have a Faculty of Theology. This has been hitherto in direct connection with the Scotch Established Church, and the Professorships can only be held by clergymen of that Church. It is well known that the Nonconformist denominations in Scotland prescribe a professional course of their own for students preparing for their ministry, and the two great Presbyterian nonconforming bodies have each of them Colleges and Professors, whose lectures their students must attend. The Bill provides that from this time forward no test of any kind shall be applicable to the University Chairs of Theology, which may therefore either be held by clergymen of any persuasion or by laymen. Should this provision become law, it will be most interesting to watch what may be the tendencies and character of the new scientific theology which will develop itself in Scotland after it has been freed from the trammels of any creed. It is to be feared, indeed, that the first effect may be that the students who now attend the University Chairs of Theology may be directed elsewhere to new Colleges or Halls of Presbyterian theology taught from the point of view of the Established Church, and that the rising clergymen of the nation, who are generally of opinion that they do enough when they do all that their licensing bodies require of them, may not sit in great numbers at the feet of the occupants of the new scientific Chairs. There is another provision which illustrates in a singular way the jealousy with which a lay State can scarcely help regarding theology, even after it has become scientific, and "in the abstract." Whatever happens, whoever may benefit by the 25 per cent. of increased emolument to be made over to the Scotch Universities, it is expressly provided that the scientific theologians are never to get any of it.

The most interesting question to our readers is how the new Bill will influence the progress of science in the Scotch Universities. The obvious and only answer is that nobody can tell. The Commissioners may make provision for a Faculty of Science, and in the three younger and more numerously attended Universities they will probably do so. In Edinburgh they could certainly do so without requiring to create new Chairs. In Glasgow there is not at present a Chair of Geology, though that subject is taught in an old-fashioned alliance with zoology, by the single Professor of Natural History. There is no Professor of Geology or of Astronomy or of Engineering in Aberdeen. The foundation of new Chairs on these subjects may possibly be thought necessary before a Faculty of Science is instituted; and there are medical Chairs, like that for Pathological Anatomy, which are not established in Glasgow. A great deal will depend, in fact, on the extent to which the free balance of 7500*l.* or thereabouts may be found sufficient to meet the more urgent and immediate demands which will be made on it from all quarters. Glasgow and Aberdeen have no Chair of Modern History. In Aberdeen one Professor teaches English Literature and Logic, and there is no Chair of Political Economy. In the University of Adam Smith

Political Economy is only taught by the incumbent of the Chair of Moral Philosophy. The recommendations of the Inquiry Commissioners stated urgent wants of the Universities five years ago which would amount to much more than the added 25 per cent. now to be given to the Commissioners to settle upon the Universities for ever. It is true that Scotland is now both a rich and a liberal country, and that much may be expected in future from the direct contributions of her people. But experience has abundantly shown that private benevolence is never organised benevolence, and that sums which might in the aggregate be sufficient to meet all the most urgent wants of the Universities are not to be expected to be provided by voluntary contributions where or when they are most wanted. To reorganise the Scotch Universities, a liberal provision of public money is probably necessary, and it seems strange to throw the burden of such a provision on a fixed and moderate sum, which is declared to be for ever incapable of increase. It is not for us of course to consider whether Parliament would act wisely in placing the grants for the Scotch Universities on the annual estimates, where they are always open to comparison and challenge, or on the Consolidated Fund, where they are practically liable neither to increase nor to diminution. But it seems a strange policy to declare beforehand that the grants for objects which are admitted to be of national importance shall never exceed a severely limited sum. The demands of science alone are continually increasing in pecuniary severity, and we say no more than every one will admit when we add, that it is not for the public advantage that the natural teaching of science should be hindered in any of the three kingdoms by a too rigid or mechanical economy. It is not placing her in her true position to compel her to an undignified struggle with a host of other claimants for her fair share of a moderate allowance which cannot be increased. If the Scotch Universities had great College estates and ample revenues like Oxford and Cambridge, her claims might be met from time to time as they have been in England. Until of late years they have been very moderately provided institutions, and there are no available funds for the extensions of the future but the freewill offerings of her people. The State is, in our opinion, reasonably expected from time to time to organise, or to help to reorganise them in the interests of the nation. We should like to see it ready to do something more in that way than to offer to cut them adrift with a little extra money, and to provide Commissioners to whose absolute discretion is to be intrusted the reconstructive duties which naturally devolve on Parliament. The Oxford and Cambridge Act of 1877 gave the Commissioners then appointed very extensive powers, but it was in marked contrast, in the precision and fulness of its enacting clauses, and in the checks under which the Commissioners were to exercise their functions, to the Scotch Universities Bill of 1883.

THE SCHEME OF THE GROCERS' COMPANY FOR THE ENCOURAGEMENT OF ORIGINAL RESEARCH IN SANITARY SCIENCE

THE relation of man, whether savage or civilised, to his surroundings is one of constant exposure to influences which are hostile to his bodily well-being. Some of these are dangerous chiefly by reason of their insidiousness; others, although not concealed, are in their nature unavoidable: others, though both known and avoidable, are yet for various reasons not avoided. All of them, of whichever class, are subjects of earnest study to the pathologist, one part of whose science is for this reason called etiology as relating to the *causes of disease*, the other being concerned with the disturbances which these causes induce inside the living human organism. "Sanitary Science" in so far as it is a science, is identical with etiology, and is therefore a branch of pathology. In this sense it is the science on which the art of preventing disease, or "Preventive Medicine," as it is commonly called, is founded, and it will be admitted that, whatever doubt may exist as to the utility of exact knowledge of the nature of disease for its cure, there can be none as to its direct applicability to prevention.

The Grocers' Company, one of the oldest and most distinguished of the City Guilds and second to none in the liberality with which it has always bestowed its funds for the general good, has thought fit to create an endowment, or rather a system of endowments, for the encouragement of "Original Research in Sanitary Science." This it defines as relating to the "causes of important diseases and the means by which the respective causes may be prevented or obviated." The endowments which the Company have created are of two kinds. The one is intended "as maintenance for work in progress in fields of research to be chosen by the worker," the other as reward for actual discovery; the former intention being carried into effect by the establishment of three "Research Scholarships," each of 250*l.* a year, the latter by the appointment of a "Discovery Prize" of 1000*l.*, to be given once in every four years. With a special view to the promotion of pathological study in the United Kingdom and its dependencies, the Scholarships are limited to British subjects who must be under thirty-five years of age; but in all other respects they are entirely open to persons *cujuscunque ordinis sive professionis*. Candidates are expected to state precisely the researches they propose to undertake, and are invited to refer, in support of their applications, to any work they may have in progress or may have published in the same or in any kindred field of study. In the appointment to Scholarships preference will be given to those candidates whose researches are judged likely to result in increase of knowledge of the "*Causation or Preventability of some important Disease or Diseases.*" It is further provided that towards the close of his year of scholarship each scholar shall publish the result of his research or researches either in print, or, if desired, in a lecture to be delivered at Grocers' Hall or elsewhere.

The Quadrennial Discovery Prize is intended to reward original investigations, irrespectively of the country in which they may have been made, which shall have resulted in important additions to exact knowledge in particular (previously defined) subjects. The subject for the first Discovery Prize will be announced in May next, and the award will be made in May, 1887, when a further subject for investigation will be proposed. Any treatise which the candidate may have published, whether in England or in any other country, at any time during the period allowed, will be accepted as a competition-treatise,

provided that the author has duly declared himself a candidate. Every treatise must be in print and in the English language, and must bear the name of its author. It seems to be contemplated that some definite problem will always be involved in the subject announced, the solution of which will be considered as the essential condition of success. But if it shall appear that although this has not been accomplished any candidate has made valuable progress towards its accomplishment, or has even incidentally made some discovery of practical importance, the merits of such candidate will be recognised by the award of a part of the sum offered.

Such is the scheme; we think it will be generally regarded as well adapted for the accomplishment of the end proposed. The objections to which it is liable are exclusively those which are applicable to all similar schemes for the encouragement of research by pecuniary endowment.

To the Discovery Prize we attach less value than to the other. In the natural sciences discoveries are usually made by men to whom the prospect of a reward, however munificent, would not be a sufficiently strong reason to induce them to change the course or purpose of their investigations. It is the consideration of this fact, no doubt, which has led the Company, we think very wisely, to determine to accept published researches in competition; but here the difficulty at once arises, for the discovery must relate to a particular question previously announced. How will the selection of this question be made?

It is clearly desirable that on each occasion the problem selected should be one which will certainly meet with its solution during the next four years—and therefore one as to which investigation is already in progress. To anticipate what will and must soon be discovered is half way toward discovery, and consequently demands, on the part of the individuals intrusted with the selection, powers at least equal to those which it is proposed to recognise in the bestowal of the prize. Nothing could be better than that Prof. Tyndall, Mr. Simon and the other scientific advisers of the Company should have the opportunity given them, or rather the duty imposed upon them, of publishing these forecasts of the probable progress of knowledge in relation to the causes of disease, for, even if their prognostications serve no other purpose, they will at least be of use in directing inquiry into the most promising channels.

The more important division of the scheme—that which relates to the scholarships—is open to no objections of the kind referred to above. Its purpose is simple, and the way in which it is proposed to carry it out effectual. It is of course quite as impossible to make a worker of a man by giving him a scholarship as to make a discoverer of him by offering him a prize, but there is this difference between the two cases, that the endowment *enables*, the prize only *rewards*. The scholarships are limited to candidates under thirty-five. Among men of this age who are now working at pathology in this country we may be sure that there are some who are doing so, if one may so express it, at the cost of life, for they are devoting to investigations which certainly will not pay, time which could otherwise be spent with direct advantage to themselves; and that there are among such men some at least who are fitted by nature to undertake the work of investigation, and have the additional qualifications afforded by training in scientific methods. Their number is no doubt very inconsiderable, for pathology as a science is of very recent birth. It is the offspring of physiology, and has only just arrived at such a stage of development as to claim an independent position. By reason of its being in this evolutionary condition it happens in pathology, as in all other sciences during the initial stage of their growth, that the more work is done the more is required—the completion of each bit of research only preparing the way for fresh investigations. New methods, new applications of physical, chemical, or physiological knowledge to the problems which relate to the causes of disease, are being brought within reach of the pathological worker every year, but all of these require *work* to make them fruitful. There is therefore not the least reason for apprehending that there will be any difficulty in finding subjects for future inquiries. It is far more doubtful whether the men possessing the qualifications which have been already indicated will be forthcoming. At first, if we are not mistaken, the choice will be very restricted, but each year will bring an accession of strength to the ranks of the competitors, so that if in the first instance the Company should be advised for want of suitable applicants to allow one or more of their scholarships to remain vacant, they will act wisely in delaying the appointment.

We do not think that the difficulty will arise, for the tide has already turned. Practical medicine, which has hitherto been strangely indifferent to the science on which it professes to be founded, is awakening to the importance of scientific investigation of the cause and nature of diseases. Among indications of the change may be mentioned the origin and successful progress of the new "Association for the Advancement of Medicine by Research," which has begun its function by devoting its funds to an inquiry into the etiology of tuberculosis. Another fact of equal moment as indicating the recognition of pathology as a special subject of study, is the intended establishment of a Professorship of the science in the University of Cambridge—an example which will no doubt soon be followed by the sister University. When this shall have been accomplished it may be hoped that the great educational institutions which are attached to Guy's, Bartholomew's, and St. Thomas's Hospitals may be also induced to follow the example of the Worshipful Company of Grocers, by doing something more than they have done hitherto to encourage and provide for "the making of exact researches into the causes of important diseases and the means whereby these causes may be prevented or obviated."

ELEMENTARY METEOROLOGY

Elementary Meteorology. By R. H. Scott, M.A., F.R.S., Secretary to the Meteorological Council. (London: Kegan Paul, Trench, and Co., 1883.)

MR. SCOTT'S aim in this text-book of meteorology is to explain the conditions required for the successful prosecution of the science, and to show in some detail the more prominent of the results which have already been arrived at. The various instruments are

figured and described, and the methods of observing detailed at length; and emphasis is laid on the necessity of securing accurate observations, and of paying attention, in making arrangements for observing, to the few simple and obvious principles which underlie the science. An account is then given of the geographical distribution of temperature, pressure, and the other phenomena of meteorology, particularly those which are usually comprised under the heads of climate and weather. The book is a highly successful one, and evinces a full and ready knowledge of the work which has been done by the meteorologists of this and other countries down to the present time, and we must not omit to add that there is an earnest endeavour manifested throughout to give the fullest credit to the first discoverers of the more important facts and principles.

The following extracts, in explanation of hill and valley winds and the distribution of rain and weather on the two sides of a mountain-chain, show the general style of the book:—

"The day wind brings up moisture to the upper strata of the atmosphere, and this is condensed, forming caps on the mountain-tops, and often giving rise to thunderstorms. The night wind, a descending current, carries the moisture with it, and so the highest peaks are often clear in the early morning. The reasons of this rhythmical change in air motion are to be sought for in the action of heat. In the daytime the air in the valleys and on the lower slopes of the mountains becomes heated and expanded. The isobaric surfaces over such districts rise, and the air so raised has a tendency to flow towards the mountains and up the upper valleys as long as the heat action over the lowlands is maintained. At night the temperature in the valleys falls, and the air lying in them contracts, producing a partial vacuum. This causes the air above to descend, so that a downward current is generated, which lasts all through the night. . . .

"When wind coming in from the sea, and therefore charged with moisture, meets a mountain-chain, it is forced to rise; it is cooled by rising, and made to give up much of the vapour it brings with it in copious rains. The result is that the air is rendered dry and cold. If now the average height of the cols of the chain above the plain country beyond be 4000 feet, the air in its descent may receive an increment of temperature of over 20°·0, and as at the same time its capacity for containing moisture will be increased, it will be felt as a dry hot wind. This is the explanation of the characteristics of the Föhn of Switzerland."

This gives the true explanation of the increased humidity observed during the hottest hours of the day on the Faulhorn and similar elevated situations.

A long extract is given (pp. 269-275) from Laughton's "Physical Geography," summarising the broad features of atmospherical circulation as exemplified by the trades and anti-trades, in which it is stated that in both hemispheres to the north or south of the parallel of 30° or 40° a strong westerly wind blows with great constancy all round the world;—and that, alike in the Atlantic and Pacific; in North America, west of the Rocky Mountains; in the Eastern States; in European Russia and Germany, and in Northern Asia, there is found the same predominance of westerly winds. A more decided objection might have been made to the above view than by stating that the winds of the temperate zone and of the higher latitudes seem to be regulated by the distribution of pressure. Laughton's statement might possibly be accepted if we had before us little more than Horsburgh and the other directories of the navigator. In the north-west of Iceland observations show on the mean of the year 212 days when the wind blows from some easterly point, and only 71 days when it blows from any westerly point, and these prevailing winds of Iceland are essentially typical of the winds of an extensive region of the north. The cyclonic and anticyclonic systems of winds observed on the surface of the earth in connection with the well-known seasonal areas of low and high pressure are not merely surface winds, but extend to a considerable height in the atmosphere. This is evident from the consideration that in winter, pressure is 1·115 inch higher in Siberia than in Iceland, and in summer 0·860 inch higher in the Atlantic than in the south-west of the Punjaub in the same latitudes, and that great disturbances of the equilibrium of the atmosphere must necessarily obtain to very great heights. It therefore follows that over large portions of the northern hemisphere gradients for prevailing westerly winds cannot be formed within many thousand feet of the earth's surface.

Mohn's happy classification of thunderstorms into heat thunderstorms and cyclonic thunderstorms is adopted and illustrated. The former is the type which predominates in summer and in hot climates, while the latter are characteristic of our Atlantic coasts, Iceland, and Norway, and are a not infrequent accompaniment of cyclonic disturbances. Cyclonic thunderstorms have their maximum period in winter, and though they occur at all hours of the day, yet long-continued observations show a distinct diurnal period having the maximum during the night; and in some regions so strongly marked is this phase that, of the twenty-three cyclonic thunderstorms which occurred in Iceland in fourteen years, only three took place at an hour of the day when the sun was above the horizon. On the other hand, heat thunderstorms are most frequent during the hottest period of the day, or during the early afternoon.

Sheet lightning and the so-called summer or heat lightning are stated to be nothing else than the reflection of, or the illumination produced by, distant electrical discharges. This opinion, so long and generally entertained, is not supported by observation. At Oxford, during the twenty-four years ending 1876, the following are the number of times which thunder, with or without lightning, and lightning unaccompanied with thunder, have been recorded during May, June, July, and August, from 3 p.m. to 4 a.m.:—

	Thunder.	Lightning.		Thunder.	Lightning.
3–4 p.m.	17	0	10–11 p.m.	12	13
4–5 ,,	27	1	11–mid.	8	30
5–6 ,,	29	0	mid.–1 a.m.	7	27
6–7 ,,	21	0	1–2 ,,	11	27
7–8 ,,	21	2	2–3 ,,	12	10
8–9 ,,	8	1	3–4 ,,	3	3
9–10 ,,	7	7			

Thus at Oxford the hours of maximum occurrence of thunder is from 4 to 6 of the afternoon; but the hour of maximum occurrence of sheet lightning or heat lightning is delayed till about midnight. These different times, but above all the larger number of cases of heat lightning over thunder about midnight, which are nearly as 7 to 2, proves that a large proportion of these cases of heat lightning at Oxford were not the reflection of distant

electrical discharges or thunderstorms, and this conclusion is amply confirmed by similar observations made in other parts of the globe. A very large number of these cases of sheet lightning at Oxford are, as suggested by Prof. Loomis in 1868, due to the escape of the electricity of the clouds in flashes so feeble that they produce no audible sound, and they occur when the air being very moist offers just sufficient resistance to the passage of the electricity to develop a feeble light.

SALVADORI'S PAPUAN ORNITHOLOGY

Ornitologia della Papuasia e delle Molucche, di Tommaso Salvadori. Parte terza. 4to. pp. 597. (Torino, 1882.)

THE completion of the third and concluding portion of Count Salvadori's great work upon the Birds of New Guinea and the adjoining Islands is an event that should be duly chronicled. We have already spoken of the plan of this great undertaking, and of the excellent way in which it has been carried out, in our notices of the preceding volumes (see NATURE, vol. xxiii. p. 240, and vol. xxiv. p. 603). We will now say a few words upon the general results arrived at.

The ground covered by the present work embraces, it must be recollected, the whole of the northern portion of the great Australian region. The mainland of this district is New Guinea, but it also contains the islands of the Moluccan Archipelago up to "Wallace's Line," besides various groups situated to the east and south-east of New Guinea, and extending as far as the Solomon Islands. In the "Papuan Sub-region," as it is generally called, thus constituted, it will be evident that variation must necessarily play a much more important part than in the solid continent of Australia. Not only do the species isolated in the different islands obtain a better chance for the exaggeration of their peculiarities (as has been so well shown by Mr. Wallace in his "Island Life"), but in the mainland of New Guinea we find mountains reaching to such an altitude as to cause the presence of a very different fauna from that of the adjoining lowlands. From these two causes it would be naturally expected that the ornithology of the Papuan Sub-region would be more rich in species than that of Australia proper. And such, indeed, is shown to be the case by the completion of Count Salvadori's work, whereby the first summary has been effected of the Papuan Ornis, since recent researches have revealed to us its luxuriance. In Mr. Gould's great work upon the Birds of Australia little more than 700 species of birds are given as inhabitants of the whole of that great continent. By Count Salvadori's volumes, we find that 1028 species are already known to us from the Papuan Sub-region, and, as we all know, a very large portion of New Guinea and many of the adjacent islands are still *terra incognita*. Much therefore remains to be added to the Papuan Avi-fauna, whilst in Australia the subject is comparatively exhausted.

Taking a general survey of the forms of Papuan bird life, we see at once how nearly akin it is to that of Australia. Recent researches especially have shown that nearly all the peculiar forms of the Australian Ornis have their representatives in the Papuan Sub-region. Some of these forms, however (for example, the Paradise-Birds and the Cassowaries), are much better represented in the Papuan Islands than on the Australian Continent, and the Papuan Islands must be regarded as their original home, whence they have sent forth stragglers into the Southern Continent.

Such general facts as regards the distribution of bird life in the Australian Region may be easily gathered from an inspection of the contents of the present work. But our author, we are glad to see, promises us to put them forward in his own shape, in an "Introduction to the Ornithology of Papuasia and the Moluccas," which he is now preparing. In this supplementary volume will be likewise given chapters on the history and bibliography of the subject, and a chart to illustrate its somewhat complicated geography. Count Salvadori is evidently determined to spare no trouble in order to render complete the results of his eight years' hard labour on the Birds of Papuasia and the Moluccas.

OUR BOOK SHELF

Cutting Tools Worked by Hand and Machine. By Robert H. Smith, M.I.M.E. (London: Cassell, Petter, Galpin, and Co., 1882.)

STUDENTS of mechanical engineering, and more especially those who study machine tool construction, have up to the present time found it very hard to obtain a suitable text-book relating to the theoretical part of the subject; hitherto almost the only books relating to it have been published in Germany.

This work comes to hand at a time when the want of such a work is much felt, and students attending mechanical engineering classes will find that it will help them considerably in understanding the construction, theoretically and practically, of the machines dealt with. The author in his preface states distinctly that he does not intend the book to be a descriptive treatise on tools, nor does he refer to all the different cutting tools in use, but he has happily chosen the more important machines, and gives a very full description and illustration of each. The subject of driving power is dealt with and fully explained, and results of experiments carried out by the author on the subject are carefully arranged in tables.

In the first chapters cutting tools for wood are discussed, the wedge action of any cutting tool being clearly described and illustrated; also the method of grinding and setting edge tools, frequently a very difficult task for beginners to accomplish. He also gives the results of experiments carried out by himself on the power required to be exerted through certain tools when doing a fixed amount of work, an interesting subject from a theoretical point of view.

The chapter on chipping-chisels and hand-planes fully explains the action and construction of the several tools, the different angles of the cutting-edge of cold chisels are shown, and the author points out the reasons for varying the angle according to the quality of the metal. The whole chapter goes into the subject practically, the explanations being clear and to the point. The next chapters deal more especially with wood-working machinery. The variety of teeth used in the different kinds of saws, including inserted teeth, are amply illustrated, the important matter of setting the teeth being fully explained, with experiments showing the power absorbed in driving the different saws, this also being usefully arranged in tables; after which the author goes on to explain the different machines used in working the metals, milling machinery having its full share of the text. The cutting speed and rate of feed for milling-cutters is gone into, and in the latter part of the chapter the milling-cutters themselves are dealt with.

Chapter V. relates to the various methods of planing

metals, and concludes by dealing with the behaviour and action of the larger machine tools, including planing, shaping, and slotting machines, all of which are illustrated.

The following chapters go fully and minutely into the construction and working of the lathe, perhaps the most important of all the machines in a workshop; describe its various uses, both for hand-turning and wood, and the mechanical slide-rest for metals. A screw-cutting and surfacing lathe is illustrated, and all its different motions explained. It is impossible here to do justice to these chapters on the lathe and turning in general. The student will find the time well employed if he studies them carefully, the author evidently being well acquainted with the practical working of this all-important machine.

The remaining chapters are occupied with drilling, boring, and the necessary machines for carrying out the same; a variety of drills are shown, and their different uses explained. The drilling machine, its construction, and various arrangements of feed gear are illustrated and concisely shown.

The book concludes with shearing and punching machines. Various illustrations are given, including Whitworth's driving gear for the same, and an illustration of Tweddle's hydraulic shearing and punching machine.

As a treatise on cutting-tools, for wood and iron, this work will be found extremely useful to engineers generally; moreover, there is a good deal of original information that will be found interesting to experienced tool-makers. The classification of the machinery is decidedly good, and the descriptions are so simple as to be easily understood by the uninitiated. It is impossible to study the book without at once finding that the author is completely master of his subject. Of course there is no doubt that practical working is essential to perfection in any branch of engineering; yet the student who is unable to attain such practical knowledge will obtain a good insight into the construction and the uses of the various machines and tools in their connection.

The author certainly seems to have omitted a matter of great importance in tool-making, namely, that of tempering and hardening the cutting-tools. There is little doubt that most of the failures arising in wood and iron-working machinery are due to tools not being properly hardened. A chapter or two devoted to the subject would have been of great service.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Metamorphic Origin of Granite.—Prehistoric "Giants"

I HAVE for some time intended to send you a few notes on two matters, both connected with geology, though very different in kind. In NATURE, vol. xxvii. p. 121, there was an interesting paper by Mr. Geikie on the metamorphic origin of granite and on the crystalline schists. Last autumn I became satisfied of a conclusion which I had long suspected—that the large granitic district in the Ross of Mull, adjacent to the Island of Iona, is a great mass of granite formed by the metamorphism of an old stratified deposit belonging to crystalline schists. It is well known to geologists that the Isle of Mull consists almost entirely of the series of (Tertiary) volcanic rocks which have been admirably described by Prof. Judd. These traps, tuffs, and lavas rest in some places on chalk, and where the chalk had been previously denuded they rest on Oolitic and Liassic beds. Older rocks, belonging, I think, to the Cambrian series, appear at one place subjacent to the traps. But the limit of all these volcanic rocks to the south-west is sharply defined by the deep bay and harbour of Bunessan, called Loch Laigh. As we enter that loch in a boat, we have on our left the trap headland of Ardtun, where I found the Tertiary leaf-bed many years ago, and on the right a headland of massive red granite. But the shores at the end or head of the bay, including all the hills above the village of Bunessan, are neither trap nor granite, but are composed of the regular crystalline mica schists which constitute the great bulk of the county of Argyll. This is the only part of Mull, so far as I know, where these rocks appear. They stretch right across the long promontory of the Ross to the southern shore. At the head of Loch Laigh they are more highly crystalline than in most parts of the mainland of the county. Very fine crystals of tourmaline have been found above Bunessan, and the schists near the new pier are highly micaceous and in some places full of coarse garnets. These schists dip at a high angle, and indeed are in some places nearly perpendicular. On the southern coast of the promontory (which is here very narrow) they occupy a considerable space between the traps which terminate on the farm of Scoor, and the granite which begins on the farm of Ardalanish. The point of contact between these schists and the granite is obscured at the head of Loch Laigh, and I have not visited it on the southern shore. But the point of most interest will be found in the granitic headland which forms the south-western shore of Loch Laigh. Along part of this shore the granitic masses at the top of the hill have all the appearance of standing upon legs. These legs at a little distance seem granitic, and although they have a suspicious appearance of tilted strata, I had passed them over and over again under a general impression that they were nothing but granite divided by unusually narrow lines of cleavage. On examining them, however, carefully, in August, 1882, I found that they are (in my judgment) beyond all doubt crystalline bedded schists exhibiting the phenomena of metamorphism in the most curious and instructive form. The metamorphic action has often segregated the mineral constituents of the old sedimentary rock in bands transverse to the line of bedding, so that in one stratum we have bands of pure quartzite and of hornblende gneiss, between bands of granitoid and of pure granitic composition. These beds pass up without a break into the amorphous granite of the great bulk of the hill; and how pure and typical that granite will be acknowledged when I add that the columns of the memorial to the Prince Consort in Hyde Park are made of it. Since discovering this passage I have found some other spots on the coast where the relation of the two rocks to each other is well seen. The best is on the deeply-indented shore of the farm of *Knockvoligan*, behind the Island of "Gilan Giraid," on which the Northern Light Commissioners have placed their establishment in the Sound of Iona. Boats can be hired at Iona, and at high tide there is a beautiful passage behind Gilan Giraid to the shore I refer to. There a dark hornblendic gneiss will be seen underlying, involved in, and passing into granite in every form of complication and variety. An interesting question arises as to the horizon to which this hornblendic rock belongs. As Iona belongs unquestionably, as I believe, to the Laurentian series, and the Bunessan schists to the metamorphosed Silurian, the sub-granite gneiss which intervenes may be assigned to either the one or the other. My impression is that it represents some of those gneissose beds of the Silurian series which are highly developed in Sutherland, and lie high above the "fundamental" or Laurentian gneiss, so well known in that county. I should be very glad if some competent geologist could investigate this district of the Ross of Mull, and could confirm or check my observations.

Turning now to the other subject. I have been surprised to see in the English scientific journals no notice taken of the very remarkable discovery reported from the Californian Academy of Science in a paper communicated to that body by Charles Drayton Gibbs, C.E., on the discovery of a great number of (apparently) human footprints of a gigantic size in the State of Nevada. It appears that in building the State Prison, near Carson City, the capital of that State, there was occasion to cut into a rock composed of alternate layers of sandstone and clay.

On several of the clay floors exposed in this operation great numbers of tracks of all sorts of animals have been exposed. These tracks include footprints of the mammoth or of some animal like it, of some smaller quadrupeds apparently canine and feline, and of numerous birds. Associated with these are repeated tracks of footsteps, which all who have seen are agreed can

be the footsteps of no other animal than man, and the engravings and photographs which accompany the paper leave no doubt on the mind of any one who sees them. The most remarkable circumstance characterising them is their great size. In one case there are thirteen footprints measuring 19 inches in length by 8 inches wide at the ball, and 6 inches at the heel. In another case the footprints are 21 inches long by 7 inches wide. There are others of a smaller size, possibly those of women. One track has fourteen footprints 18 inches long. The distance between the footprints constituting a "step" varies from 3 feet 3 inches to 2 feet 3 inches and 2 feet 8 inches, whilst the distance between the consecutive prints of the *same foot* constituting a "pace" varies from 6 feet 6 inches to 4 feet 6 inches. In none of the footprints of the deposit are the toes or claws of animals marked. As regards the beasts, this is probably due to the "slushy" state of the mud when the tracks were made. But in the case of the human footprints it is probably due to the use of some kind of shoe or mocassin.

I need not say that so far as the geological horizon is concerned this discovery does not carry the existence of man beyond the Quaternary Mammalia, with which it has long been pretty clear that he was associated in prehistoric times. Nevertheless it is, if confirmed, a highly remarkable discovery, especially as connected with the curious intimation so concisely made in the Jewish Scriptures, "And there were giants in those days." Hitherto, so far as I know, the remains of prehistoric man, so far as hitherto discovered, have not revealed anything abnormal in point of size. It is just possible that the slippery and yielding nature of the muddy lacustrine shore on which the tracks were made may have partly occasioned the apparent size. But the photographs and engravings exhibit them as very sharp and "clean cut." Professional Indian trackers have been employed to examine the tracks, and none of them seem to have the smallest doubt as to the footprints being human. ARGYLL

Cannes, April 14

P.S.—The paper was sent to me by my son, the Governor-General of Canada, a few weeks ago.

"The Ether and its Functions"

In NATURE vol. xxvii. pp. 304, 328, is a reprint of a lecture delivered by Dr. Oliver Lodge in December 28, 1882, at the London Institution, on "The Ether and its Functions." As this happens to be a subject to which I have devoted special attention, I would beg to offer a few remarks, also as my name is alluded to in the article.

The repudiation of the assumption of "action at a distance" in the first part of the lecture, coupled with the ingenious arguments by which its baselessness is exhibited, will no doubt be encouraging to all those who favour the advance of knowledge. But that portion of the lecture dealing with the constitution of the ether (and which assumes it to be non-molecular) is to my mind disappointing, as it looks like a step backwards to suppose the ether to be something essentially different from ordinary matter, while on all sides the simple opinion of the "unity of matter" has been making progress. I will quote the passage more especially relating to this point, viz.:—

"As far as we know, it (the ether) appears to be a perfectly homogeneous incompressible continuous body, incapable of being resolved into simple elements or atoms; it is in fact continuous, not molecular. There is no other body of which we can say this, and hence the properties of ether must be somewhat different from those of ordinary matter" (p. 305).

It will be admitted that clearness is a first desideratum in a theory. It appears difficult to see how an "*incompressible*" body is to transmit waves.[1] A remark of Maxwell's in his paper "On the Dynamical Theory of Gases" has some bearing on this point, viz.: "The properties of a body supposed to be a uniform plenum [*i.e.* not molecular] may be affirmed dogmatically, but

[1] For it seems apparent that an incompressible, non-molecular, "frictionless" liquid could not have wave-energy imparted to it at all, or a hot substance could not emit light or heat in such a medium. Moreover let us (in a spirit of fair argument) take a representation of one of the most commonplace effects in physics, say an explosion of gunpowder. Then the assumption of "action at a distance" being rejected, there is (admittedly) no more playful building of castles in the air out of "force," or no store of phantom energy to get the motion ("explosion") from. The motion therefore must inevitably come from the matter of space, or from a set of particles or atoms already in motion in space in their normal state. How is a non-molecular, *frictionless* liquid to lay hold (as it were) or act upon the molecules of gunpowder and put them in motion? Is not the objection conclusive though elementary?

cannot be explained mathematically" (*Phil. Trans.*, 1867, p. 49). Moreover, it seems hard to reconcile the fact that Sir W. Thomson's theory of the constitution of matter is apparently adopted or favoured in the lecture, and yet at the same time the molecular or atomic nature of the ether is repudiated. But is not Sir W. Thomson's theory of matter essentially an atomic theory? The incompressible fluid outside the vortex atoms cannot serve as the ether, or this seems an impossibility. Maxwell, for example, remarks in relation to this point, viz.: "The primitive fluid [*i.e.* the fluid exterior to the atoms] entirely eludes our perceptions"[1] (see "Encyc. Brit.," article "Atom," p. 45). The ether, however, does not entirely elude our perceptions, but is very distinctly felt in the beating of the waves of light upon the eye. It appears, therefore, that if the ether is to affect the senses at all it must consist of atoms or molecules (doubtless very much smaller than those of gross matter, a difference in *degree* but not a difference in *kind*). It may be noted in passing here how often notoriously has the error of mistaking a mere difference in *degree* for a difference in *kind or essence* been made in the history of science, and the correction of this error with the correlation and simplication of views attendant on its removal marks one of the chief stages of our progress. The theory of evolution abolished this error in regard to the animal world; its abolition in regard to the universe of matter is equally demanded. One satisfaction that Sir W. Thomson's theory of matter brings, consists perhaps in the fact that it does not overthrow our old conceptions as to the atomic constitution of matter so firmly built up by the able reasoners of the past, including Lucretius and Newton—and which has produced such great results for science. The Thomsonian view goes rather to confirm the atomic theory and to establish its truth by explaining in addition how an atom can be *elastic* and yet indestructible. Let us not deviate from the well-tried ground of the atomic constitution of matter, already won with so much labour, unless we are forced to do so, and let us work towards the great generalisation of the Unity of Matter and of Energy.

London, April S. TOLVER PRESTON

P.S.—My views regarding the Matter of Space (the result of many years of thought and study) are contained in various scattered papers, references to the chief of which may be conveniently given here, viz. *Philosophical Magazine*, September and November, 1877, February, 1878; NATURE, January 15,[2] 1880; March 17, 1881; March 20, 1879; *Philosophical Magazine*, August, 1879, November, 1880: April and May, 1880, &c., &c. Also a little book, "Physics of the Ether" (E. and F. N. Spon), was published in 1875 as a first imperfect essay on the subject. The above papers include an atomic theory of the ether, capable of affording a simple and natural explanation of gravitation without the aid of "ultramundane corpuscules" [*i.e.* without the supply of any energy or matter at all from outside the bounds of the visible universe]. Dr. Lodge seems to admit that his premises cannot explain gravitation. But is not the elucidation of gravitation (which may be called the primary physical effect in the universe) one of the first requirements of any theory of the constitution of the Matter of Space? A more concise summary of my views (with additions and developments, the work of recent years) regarding the relations of the Matter of Space to ordinary matter and to the local fluctuating changes taking place in the universe, may be found in the forthcoming volume of the *Transactions of the Vienna Academy of Sciences*, to which they have been communicated by Prof. Ludwig Boltzmann of Graz.

"Krao"

SOME two months ago there appeared in NATURE (vol. xxvii. p. 245) certain statements about "Krao," the Siamese hairy child, which with your leave I would venture to correct. Krao's parents are both Siamese, not Laos; they are both still living in this city; neither of them presents any special peculiarity; they have other children still living, and also showing no special peculiarities; Krao, it is true, was not born in Bangkok, but in a village between this and the sea, her parents having a little time before her birth run away from their master, but coming back after the event. Siamese is of course Krao's native language;

[1] A clear and able exposition of the relation of Sir W. Thomson's theory of matter to the old-established atomic theory may be found in a paper on "The Atomic Theory of Lucretius," by an anonymous author in the *North British Review* for March, 1868.

[2] This paper includes a corpuscular theory of light consistent with the main principles of the undulatory theory—*not* therefore an *emission* theory.

in her short journey up country with Mr. Bock, she, being an intelligent child, picked up a few words of Laos; the joints of her arms and fingers possess, it is true, according to European ideas great flexibility, but really they have it to no greater degree than those of ordinary Siamese; it is also true that she is able to use her toes, grasping things between the big toe and the next one in a way that is surprising and amusing to Europeans, but this is a faculty which all Siamese, being a barefooted people, possess to a greater or less degree; the child was looked upon here as even a greater natural curiosity than she is considered to be in England, her parents being in the habit of taking her about and showing her for a small reward, and the price they obtained for her (in native currency equal to 60*l.*) being twice that of an ordinary child of the same age. A strange mistake has been made about the child's name, "Krao" being merely the Siamese name for whiskers, a very natural nickname for the child to obtain. As far as I can ascertain from those who knew the child well, she is endowed with the average intelligence of Siamese children of her age and class, and beyond her abnormal hairiness presents no peculiarity.

To sum up, "she is," as you rightly remarked, "merely a *lusus naturæ*, or a sport, possessed rather of a pathological than an anthropological interest. I may add that I have carefully verified all the foregoing statements. A RESIDENT

Bangkok, Siam, March 3

[From information that has since reached me I am able fully to confirm the particulars here supplied by "A Resident."—A. H. K.]

Singing, Speaking, and Stammering

IN NATURE, vol. xxvii. p. 532, in the report of Dr. Stone's lecture on "Singing, Speaking, and Stammering," there appears a *Classification of Vowels*, which is described as an abstract of Mr. Melville Bell's scheme. I shou'd like, however, to point out that the system which Mr. Bell has advocated for the last fifteen years is hardly represented in the *Classification* referred to. On turning to p. 63 of Mr. Bell's "Sounds and their Relations," which is a new exposition of "Visible Speech," it will be seen that the vowels *Ir* and *Ah* are not described as labio-lingual, and that the threefold arrangement of the vowels as lingual, labio-lingual, and labial is abandoned as incorrect. The lecturer does not appear to have mentioned the phonetic researches of Mr. A. J. Ellis and Mr. Henry Sweet. In many important points, however, they supplement the system of Mr. Bell, and their works cannot be overlooked in the scientific study either of etymology or pronunciation. The student of language can hardly do better than begin with Mr. Ellis's "Speech in Song," and Mr. Sweet's "Handbook of Phonetics."

JAMES LECKY

5, Alexandra Road, Wimbledon, S.W., April 10

THE classification of vowels to which Mr. Lecky refers is taken from Mr. Melville Bell's "Principles of Elocution," which I obtained with much difficulty from a publisher in Salem, Massachusetts. It is dated 1878, and may, I suppose, be held to represent the author's system at that date. I am well acquainted with the other works named by Mr. Lecky. W. H. STONE

As an illustrative instance of the peculiarities akin to stammering, referred to in Mr. Stone's lecture in last week's NATURE (p. 559), I may mention the case of an old Scotch lady whom I knew some years ago, and who was in the habit of interpolating at frequent intervals in her talk the wholly irrelevant words "This that here there ye ken." She herself evidently made use of the words with perfect unconsciousness of their irrelevancy; indeed I doubt whether, if challenged, she would have admitted using them at all. K.

A Curious Case of Ignition

"A CURIOUS case of ignition," quoted in NATURE, vol. xxvii. p. 509, reminds me of a similar circumstance that came under my own observation when serving in H.M. despatch vessel *Psyche*, 1862-66. We were moored "head and stern" in Port Napoleon, Marseilles, on a bright summer day. A strong smell of burning was traced to the saloon skylight. On bursting open the door of the saloon it was found that a scuttle glass (a plano-convex lens) through which the solar rays were admitted and focused on a rep curtain (which was smouldering) had been substituted for a broken one, but through an oversight had not been ground on the plane surface (as is usual). The case was reported by letter, and an order issued to insure all scuttle glasses used in men-of-war for the purpose being ground.

BERTRAM GWYNNE

Fibreballs

I HAVE seen balls of vegetable fibre, such as those referred to by Mr. G. H. Darwin in his letter of March 23 (NATURE, vol. xxvii. p. 507), in great abundance on the sea-beach at Cannes; there however they are not spherical like those described by Sir A. Musgrave, but cylindrical, two or three inches in length, finely and closely matted, and all wonderfully similar in appearance. In one place they had been collected and employed, if I remember rightly, to form a kind of wall. Some balls of a similar kind, but more nearly spherical and much coarser in texture, were found, on draining a pond, by Dr. Fitton, and sent by him to Sir J. Herschel, these were three or four inches across, and looked almost like small hedgehogs rolled up. J. H.

Benevolence in Animals

MR. GEO. J. ROMANES, in a lecture delivered in Manchester, March 12, 1879, on "Animal Intelligence," points out the following emotions which resemble human intelligence as occurring in animals below the human species, namely: fear, affection, passionateness, pugnacity, jealousy, sympathy, pride, reverence, emulation, shame, hate, curiosity, revenge, cruelty, emotion of the ludicrous, and emotion of the beautiful, and gives some remarkable instances in support of his statement. To this I can add benevolence on the part of our household cat, who was observed to take out some fish bones from the house to the garden, and, being followed, was seen to have placed them in front of a miserably thin and evidently hungry stranger cat, who was devouring them; not satisfied with that, our cat returned, procured a fresh supply, and repeated its charitable offer, which was apparently as gratefully accepted. This act of benevolence over, our cat returned to its customary dining-place, the scullery, and ate its own dinner off the remainder of the bones, steak in bit with additional zest. OSWALD STICH

Woodend, Fortis Green, N., April 12

The Zodiacal Light (?)

LAST Friday evening about 7 p.m. my attention was called to a peculiar appearance in the western sky. The sun had set not long before. No clouds were visible but one long thin streak, and there were the usual mists near the horizon. Above where the sun might be, a pillar of light faintly red in colour, with soft edges, but fairly well defined, rose vertically from near the horizon to the height of perhaps a few degrees. It did not look like an illuminated cloud nor like rays of light shot up through a cloud, nor like anything local; in fact I am told that it moved northwards with the sun. Was this the zodiacal light, or merely some sunset effect? It began to grow dim about 7.10 p.m., but was visible later than this. J. W. B.

New Kingswood School, Lansdown, Bath, April 10

Braces or Waistband?

THE writer has for the last thirty years dispensed with the use of either braces or a belt, having had his waistcoats made with short elastic straps attached inside and with holes to button on to the trousers like braces, one on each side and a third in front.

They answer as well as braces in conjunction with the ordinary waistband and buckle of the trousers, and the wearer is saved the feeling of strain across the shoulders or round the waist connected with the use of braces or a belt. G. H.

April 13

THE TEACHING OF ELEMENTARY MECHANICS[1]

AT the recent Annual Meeting of the Association for the Improvement of Geometrical Teaching held, as has already been noted in our columns, at University

[1] Association for the Improvement of Geometrical Teaching, Ninth General Report, January, 1883.

College, on January 17, the following statements of work done by the Committees were presented. In *Solid Geometry* no progress had been made in consequence of the serious illness of the secretary (Mr. Merrifield), but the President in his subsequent address remarked that it was hoped that the Committee would meet at an early date and work out, upon the basis of what Mr. Merrifield had done, a syllabus of propositions corresponding to the 11th and 12th Books of Euclid and the simpler geometry of the sphere. In *Higher Plane Geometry* the Committee had revised about half of the syllabus issued in 1879 and had added chapters on the geometry of the triangle and on geometrical maxima and minima (copies were distributed amongst the members present, and have been subsequently circulated). In *Geometrical Conics* the former syllabus had also been revised and continued to the end of the hyperbola (this syllabus is also in the hands of members). In *Elementary Plane Geometry* the proofs of Book I. of the *Syllabus* had been revised, the proofs of Book II. drawn up, and a collection of Exercises on Books I. and II. had been added (the motion in connection with the adoption of these proofs, which was down in the President's name, had to be postponed in consequence of copies not having been circulated before the meeting).[1] Gratifying testimony to the success of the Association's efforts was afforded by the fact recorded in the Council's Report that the copies of the syllabus were all disposed of, and that it was in contemplation to bring out at once a revised edition of the work in accordance with the changes made in the books of proofs.

In addition to the usual routine business, the President closed the morning sitting with some remarks on the teaching of arithmetic. This is a subject the claims of which upon teachers he has at many previous meetings pressed upon his hearers, it being his desire that the teaching should be put upon a sounder footing than it at present in most cases occupies. A true disciple of his old master, De Morgan, he insisted strongly upon the more frequent appeal to reason than to rule. "It seemed to him to be the wrong order to give first the rule and then the reason. Teachers should take particular examples, and work them out with reasons for every step. They should lead up to a rule by a series of examples worked out from common sense, and only when these have been thoroughly grasped should the rule be introduced as a convenient embodiment and summing up of the results attained by the application of reason and common sense." A common habit with boys is to ask, "How am I to do this?" "In his own practice he never answered that question, but he said, 'What does it mean? If you will only find out what it means, then you will know how to do it.'" Another principle he advocated was "that all arithmetical processes should follow the order of thought, according to which numbers are grouped in language. . . . . The order of thought in the expression of numbers was from the higher group to the lower, hundreds to tens, tens to units, &c." In this connection he referred to a lecture by the late Mr. Bidder. A reform on the principles he (the President) advocated would, he believed, be very valuable in teaching and in the practical operations of arithmetic. In the natural sciences arithmetic is applied to cases where approximate data only are employed. "Hence it was becoming more and more important that methods of approximation should be carefully and distinctly taught." This led him to enter a protest against the practice, frequent amongst University examiners, of setting in papers for schoolboys, "among the questions on decimal fractions, some examples to be done only by reducing recurring decimals to vulgar fractions, and then working out the result by vulgar fractions. To give prominence to such examples was simply to destroy the notion which a good teacher would have been endeavouring to instil into a boy's mind, that decimal fractions are useful only in general for approximate results. He did not wish to say anything against recurring decimals rightly used and in their proper place." A final point was that he would substitute Horner's process for the extraction of any roots for "the awkward and almost useless special processes usually given for extracting square and cube roots. This he would teach simply as a process; but of course with fair warning to the boy by telling him that he was for once giving him a process which would lead to the desired result, and that it would be a reward of his future mathematical attainments if he could get to the reason of it."

The novel feature, however, in this year's proceedings was the holding of an afternoon sitting, which was wholly devoted to the consideration of the subject of elementary mechanics. This meeting was the outcome of the recent extension of the Association's sphere of action, and proved to demonstration that the said extension had met with the approval of many of our most able physicists. The papers read were three in number: (1) The Teaching of Elementary Mechanics, by Mr. W. H. Besant, F.R.S.; (2) Notes on the Teaching of Elementary Dynamics, by Prof. G. M. Minchin; (3) The Basis of Statics, by Prof. H. Lamb of the University of Adelaide. (1) is remarkable as proceeding from a successful Cambridge "coach," who finds it difficult to emancipate himself "from the ideas and prejudices which are the natural results of an adherence for many years to a special set of books and to a special system of teaching. The fact constantly before us in Cambridge, that mechanics are being studied with a view to success in examinations, tends to make us forget the importance of the practical application to daily life of a knowledge of mechanics, and the temptation is to luxuriate in the flowery and ornamental problems which sometimes form the staple of examination questions," whereas "millions of people must acquire a knowledge of the laws of mechanics, practical or theoretical, or both, who are not going to be tested by a Cambridge examination." It however goes without saying that at present Cambridge methods do exercise a very large influence on the teaching of mechanics throughout the country. In the case of young students and beginners, Mr. Besant considers that the first requisite for a class-room is a set of models and a quantity of machinery (*segnius irritant animos*, &c.). "The handling of systems of pulleys, and experiments with levers and screws, will guide the student, almost unconsciously, to the ideas of the transmission of motions, and of the transmission and multiplication of force. . . . Then, again, experiments with falling bodies, and with an Attwood's machine, will illustrate the ideas of uniform motion and of accelerated motion, and generally of the action of gravity. . . . For many students this kind of experimental teaching will probably be sufficient for the work of their lives, and it will be certainly educationally useful." The Cambridge practice has been to treat the subjects of statics and dynamics separately, and to take statics first; and the teaching is so limited that the ordinary Bachelor of Arts, whose reading has been limited to statics alone, "is sent out into the world without any perception of the laws of motion, and without any knowledge of the elementary deductions from those laws, which are necessary requisites for a true appreciation of a vast range of natural phenomena." Passing next in review the change of nomenclature and of treatment inaugurated by Professors Thomson and Tait, and the late Prof. Clerk Maxwell, Mr. Besant records his opinion that Duchayla's proof is "forced and unnatural," and causes a considerable waste of time. His wish is that the examiners should have greater freedom of action. He would, following the lead of the above-named eminent physicists, commence with a study of the

[1] A special meeting for the purpose of considering the postponed motion was held at University College on the evening of March 20, at which the "proofs" were adopted and their publication sanctioned.

elementary parts of kinematics, to include "the ideas and measures of velocity and acceleration, the parallelogram of velocities, and the parallelogram of accelerations, the motion of a point with a constant acceleration, and the acceleration of a point moving uniformly in a circle." Then would come "falling bodies and projectiles." From these particular cases the student will get a general idea of the action of force, and so be prepared for a study of the laws of motion, and of the deductions from these laws. "One of the first of these is the parallelogram of forces, and I am convinced by actual experiment of the ease with which that mode of proof is appreciated by a beginner. The perception of the physical independence of forces, which is really the qualitative part of the second law of motion, is not a serious difficulty to the majority of beginners in mechanics, and from this principle, with the aid of the parallelogram of velocities, the parallelogram of forces is developed easily and naturally." Next may be taken the mechanical powers and simple cases of equilibrium of bodies and systems of rods, a statement of the laws of friction, and the determination of the centres of gravity of bodies and systems. Mr. Besant also laid great stress upon graphical modes of solution, referring to Mr. Minchin's work on Statics for numerous examples in these methods. "The discussion of the theory of moments of forces would naturally lead up to the idea of a couple and to the transformation and composition of couples." The pupil might then proceed to the impact of balls on each other: "The easiest method for every one . . . is to assume the invariability of momentum, and the constancy of the ratio of the relative velocities before and after impact. The consideration of the action of the forces of compression and of restitution is a more difficult idea, and should be deferred to a later stage of the student's progress. In the discussion of these points the idea of work and of kinetic and potential energy may be introduced and illustrated, gradually leading up to the statement of the general principle of energy." Dwelling upon the wonderful results that accompany the employment of this general principle, "the very concentrated essence of science," which in elementary mechanics "widens the path and shortens the road, and reduces to simple forms of thought many problems which used to be reckoned as belonging to advanced regions of the higher mechanics, and as depending for their solution on the complicated machinery of analytical process," the paper alluded to the fact that it was only as recently as 1877 that this principle of energy appeared in Part I. of the Tripos Examination—a result mainly to be attributed, we believe, to Prof. Clerk Maxwell's advocacy of it. The principle is carefully laid down and discussed in "On Matter and Motion," as well as elaborately discussed in Thomson and Tait's "Natural Philosophy," and in many recent elementary treatises. With such views of its importance, we are not surprised to find Mr. Besant pleading for the introduction of the idea of energy as early as possible, and that every effort should be made to "illustrate the idea by means of simple cases and so to lead the student upwards, by gradual steps, to the conception of the most important principle which lies at the root of all modern science." Another point on which the paper touches is that "ill-chosen technical terms are likely to propagate erroneous ideas and confusion of thought," and reference is made to recent remarks on the use of the word "force." Then coming to the question of a syllabus of mechanics, Mr. Besant remarks that "it will be a matter of supreme importance to discuss the definitions and axioms of the subject," and instances a common definition of the word "vertical"—that it is the line in which a stone moves when let fall. The paper closes with a few general remarks on the value of some branches of scientific study as an education, from which we select the two following extracts:—"The safest and wisest plan seems to be to let every man, who wishes to make research in physics, find out for himself the kinds of tools which he wants, and then learn as much of the use of those tools as may be necessary." "The elementary subjects, such as mechanics and astronomy, are of more educational value to the majority of students than the higher regions of science, and only a select few should be encouraged to spend much of their time in such advanced forms of study."

(2) In this dynamics includes both statics and kinetics. The writer is in favour of continuing the old way of taking statics first, and of then proceeding to kinetics, and argues strongly against the taking the former as a particular case of the latter. Two important advantages, however, of the recent mode of treatment are not ignored, viz. that the student by concentrating his attention on force solely as change of motion, it at once proves for him the fundamental proposition of dynamics, viz. that of the "parallelogram of forces," as an immediate result of the easily admitted parallelogram of velocities ("if for the beginner the choice lay between such proofs as Duchayla's, and none, I should say, 'Assume the proposition'"); and next that "the kinetical method has the very great practical advantage that it makes the student familiar at the outset with the idea of *absolute measures* of force, momentum, energy, &c., such as are used in the C.G.S. system." The notion of *acceleration* is an exceedingly difficult one for beginners, and such a one, as a matter of fact, "is confined to the consideration of acceleration of constant magnitude, and, except in the case of uniform motion in a circle, to the case of acceleration in a constant direction. Thus he gets plenty of exercise in the motion of particles down inclined planes, ... but what idea does our beginner obtain of the acceleration of the motion of a particle revolving uniformly in a circle? Is there not something *primâ facie* very difficult, if not absurd, to him in the statement that any motion which takes place with *uniform velocity* can be accompanied by *acceleration?*" After a few more remarks to the same end, Prof. Minchin says, "So far as my own work in teaching is concerned, I have not a moment's hesitation in saying that the treatment first of kinetics and then of statics as a particular case is to be rejected. So difficult for the mere beginner are the conceptions involved in Newton's second axiom, that three months' work in combating difficulties and removing false impressions would, almost to a certainty, produce a merely negligible amount of positive knowledge." Starting from the kinetical definition of force, and thereby establishing the fundamental proposition ("this does not logically compel us to continue to treat of motion, deducing rest as a particular case"), the writer, after protesting against the too great importance attached to the getting-up of "book-work,"[1] expresses the opinion that the student realises the subject only by incessant application of the principles to particular cases. For this purpose nothing, he believes, is so good as *numerical examples:* and this in contrast to examples dealing with magnitudes as algebraical symbols, and to geometrical examples. "So long as forces are X, Y, Z, and moments are L, M, N, and no particular consideration of the *units* of different quantities is required, they are comfortable enough, but when we have to deal with pounds and foot-pounds, dynes and ergs, the utter unavailability and inutility of their knowledge are made manifest." Another point strongly insisted upon is *the invariable accompaniment of figures constructed accurately to scale with all the examples.* The result should be arrived at by calculations made by means of logarithmic and trigonometrical tables, and also by graphic construction by the aid of the instruments, and on all there should be, when possible, "the perpetual

[1] "I have met students who could write out paragraph after paragraph of general propositions in statics, and who at the same time (although such might appear to *a priori* reasoners impossible) could not make the faintest attempt to discuss any particular question involving the application of the principles of statics."

exercise of a *common-sense check*." Too much weight may be attached to *graphic statics*, "but real utility is gained by making graphic methods a companion to (though not wholly a substitute for) analysis," and Prof. Minchin would assign a more conspicuous place to them in the text-books than they at present occupy. "Their essential merit consists in their furnishing visibly to the student the whole history of a magnitude throughout a series of variations in its circumstances." Prof. Minchin would also banish such "crude" terms as "power," "weight," in the equilibrium of machines: such forces might be called "efforts" and "resistances." Passing over one or two other subjects, we come to remarks on "illogical methods of teaching"; by such a method is here meant *a process which introduces considerations that are not essentially necessary for the purpose aimed at—considerations that can be seen a priori* to be *irrelevant*. The moral is pointed by the discussion of a question of usual occurrence in the text-books. The student should be able to be *critic of his data*, and "he ought to be taught to recognise clearly the object finally aimed at in any problem, and also to see what he must be given, and what he need not be given, in order to arrive at it." For this purpose Prof. Minchin purposely uses with his students some books which, *both in their data and in their methods*, are full of illogicisms. The finale comes in pointing out the desirability of making the student carefully distinguish between the *weight* of a body and its *mass*, and here he "comes down," if we mistake not, on an episcopal writer of works on dynamics, for a "remarkable misuse of language."

(3) Prof. Lamb's object is to suggest a new basis for the science of statics, and in the course of his paper he attacks certain principles and artifices, as "the transmissibility of force," and (in hydrostatics) the solidification of matter, and also "rigid" bodies. The "point of departure" which he suggests is that "the true and proper basis of statics is to be sought for in the principle of linear and angular momentum. Regarding statics as the doctrine of the equivalence of forces, I would define the word 'equivalent,' and say that two sets of forces are 'equivalent' when, and only when, they produce the same effect on the linear and on the angular momentum of any material system to which they may be applied: *i.e.* when they produce the same rate of change of momentum in any assigned direction, and the same rate of change of moment of momentum about any assigned axis." He believes that on examination the objections arising from the supposed difficulty and abstruseness of this mode of treatment, "will disappear, and that *on the whole* the method will be found to be really much simpler than that at present in vogue. The main difficulty is at the outset."

A brief but interesting discussion followed, enlivened as it was by a friendly passage of arms over the term *force of inertia*.

R. T.

THE CHEMISTRY OF THE PLANTÉ AND FAURE ACCUMULATORS

PART V.

1. *Influence of Strength of Acid*

IN the second part of this communication in NATURE, vol. xxv. p. 461, when treating of the charging of the cell, we pointed out that in the electrolysis of dilute sulphuric acid between lead electrodes, two totally different reactions might be obtained. The positive metal becomes thinly coated with lead sulphate when the current employed is of small density, but with lead peroxide when the density of the current is of greater magnitude. This latter action is, of course, what takes place in the ordinary formation of a Planté battery. The chemical change, therefore, which goes on at the positive electrode is to a certain extent dependent upon the strength of the current. It appeared also of both theoretical and practical interest to determine whether the chemical change was also influenced by the strength of the acid employed. Our experiments consisted in passing a current of uniform strength, about 1 ampere, between electrodes of lead, 12 square inches in size, in varying strengths of sulphuric acid, and estimating in each case the amount of oxygen fixed by the positive electrode. We determined this for successive five minutes of time, and as such actions are not always very uniform, we made in each instance more than one experiment. The results are given in the following table:—

Strength of acid.	Expt.	Percentage of oxygen fixed.				
		First 5 mins.	Second 5 mins.	Third 5 mins.	Fourth 5 mins.	Total.
1 to 5	I.	38·1	28·6	28·6	33·3	128·6
	II.	39·5	30·2	25·6	30·2	125·5
1 to 10	I.	43·4	38·7	29·2	34	145·3
	II.	44·1	39·3	29·3	34·9	147·6
1 to 50	I.	48·3	39·6	35·3	22·4	145·6
	II.	46·2	43·9	23	30	143·1
	III.	54	40	35·3	35·5	165
1 to 100	I.	42	38·3	33·9	29·5	143·7
	II.	42·4	40	37·8	35·5	155·7
	III.	51·1	44·2	34·9	34·9	165·1
1 to 500	I.	46·6	32·6	27	27	132·6
	II.	46·4	27	27	18	118·4
1 to 1000	I.	90·6	81·1	76·4	57·5	305·6
	II.	90·8	77	72·3	63·1	303·2

It appears from this that the strong sulphuric acid (1 to 5) is not quite so favourable to the action as the more dilute (1 to 10), but that between this latter proportion and 1 to 500 there is no great difference in the amount of oxygen fixed, and therefore of corrosion of the plate. The appearance of the plate in every instance indicated the formation of only lead peroxide. With sulphuric acid diluted with 1000 parts of water, the amount of oxygen fixed, and therefore of corrosion, was at least doubled, while the chemical action was very different. On parts of the electrode, streaks of a mixture apparently of the yellow and puce-coloured oxides were seen. On other parts a white substance formed and was easily detached, falling in clouds into the liquid. Where this latter action took place, the plate was visibly the most corroded. This white substance gave on analysis SO_4 equivalent to 73·6 per cent. of lead sulphate, suggesting the idea that it was a basic sulphate of the composition $2PbSO_4,PbO$, which would require 73·1 per cent. As the peroxidation of the lead is required, and the corrosion of the plate is to be avoided as much as possible, it is evident that this extremely dilute acid must be avoided. It has already been shown that if the sulphuric acid is entirely removed from solution, as sometimes happens in an accumulator, the lead is simply converted into the hydrated protoxide, and therefore corroded without any good effect.

2. *Function of Hydrogen.*—In the formation of a secondary cell, after the complete reduction of oxide or sulphate to metallic lead, bubbles of hydrogen gas are seen to escape from the lead plate. It has been assumed that a portion of this is occluded by the lead, or in some other way enters into association with it, and it has been

supposed that this hydrogen compound may play an important part in the subsequent production of electromotive force. It therefore appeared desirable to obtain experimental evidence as to whether hydrogen is so absorbed. The process we adopted for this purpose was founded upon the observation of Graham that hydrogen associated with palladium reduced ferri- to ferro-cyanide of potassium, and that generally in the occluded condition the element was more active chemically. We had previously ascertained that hydrogen associated with other elements, as platinum, copper, and carbon, was capable of reducing potassium chlorate to chloride. This method seemed to give trustworthy results, and therefore we applied it in this instance. As the result of several trials, however, we found that the amount of hydrogen associated with the reduced lead was almost inappreciable. Small as this quantity is, however, it is by no means impossible that it may be the cause of the exceedingly high electromotive force observed for the first few moments, on joining up a completely formed cell immediately after its removal from the circuit of the charging current. This, however, may be due, as Planté imagined, to the gaseous hydrogen itself. The principal if not the only function of the hydrogen of the water or sulphuric acid is therefore that of reducing the lead compounds.

By a totally different process Prof. Frankland has very recently come to the same conclusion as ourselves in regard to the exceedingly small amount of occluded hydrogen.

3. *Evolution of Oxygen from the Peroxide Plate.*—Planté noticed a small escape of gas from the negative plate of his cell immediately after its removal from the influence of the charging current. This he attributed to a decomposition of water by means of local circuits between the peroxide and the subjacent lead plate in contact with it.

The explanation we gave in our first paper (NATURE, vol. xxv. p 221) of the local action which goes on at the negative plate does not account for the escape of any gas—either oxygen or hydrogen. We therefore thought it of interest to ascertain the nature, and if possible the origin, of the gas noticed by Planté.

We found that the escape of gas from a Planté negative plate was very slight, and soon ceased; but we observed that it became much more pronounced when the temperature of the electrolytic liquid was raised. In order to get a sufficient quantity of the gas for examination, we prepared a negative plate according to the procedure of Fauré, and then heated it in dilute acid, with an arrangement for collecting the gas as it was evolved. The amount of gas was still very small in comparison with that of the peroxide, but a sufficient quantity was collected to enable us to ascertain that it was oxygen. We next heated some of the electrolytic peroxide apart from the lead plate, and again noticed a similar evolution of gas, which was also found to be oxygen. This shows, therefore, that it was not a result of local action.

The gas has generally some odour of ozone, and, on testing the dilute acid between the plates of a Planté cell, we always found traces of something that bleached permanganate of potassium, and which might be either ozone or peroxide of hydrogen.

The origin of the gas noticed by Planté may be easily attributed to the oxygen which always passes off in quantity from the peroxide plate during the process of "formation." It is only necessary to suppose that some of this becomes condensed on the peroxide, and is gradually eliminated from it when the surrounding conditions are changed. But the matter is capable of another explanation. If peroxide of hydrogen be really formed in the liquid, it will exert its well-known influence on higher oxides, namely, that of reducing them and itself at the same time. As a matter of fact, if peroxide of lead is dropped into peroxide of hydrogen oxygen is evolved.

4. *Temperature and Local Action.*—Planté has recently pointed out that an elevation of temperature facilitates the formation of his secondary cell (*Comptes Rendus*, August, 1882). The character of the chemical changes which took place at the negative plate led us to think it exceedingly probable that this increase in the rate of formation arose from an augmentation in the amount of local action. Experiment showed such to be the case. Pairs of similar negative plates on Planté's model were allowed to remain in repose at 11° C. and 50° C. respectively, and the formation of the white sulphate was visibly more rapid at the higher than at the lower temperature. The same is also true with negative plates prepared by Faure's process. Thus we found that two similar plates kept in repose for an hour, the one at 11° C. and the other at 50° C., formed by local action 2·6 and 7·4 per cent. of lead sulphate respectively. On two other plates the proportions were 7·6 and 9·5 per cent. respectively. These observations of course by no means exclude the idea that an increase of temperature may facilitate the other chemical changes that take place in the formation of a lead and lead-oxide cell.

J. H. GLADSTONE
ALFRED TRIBE

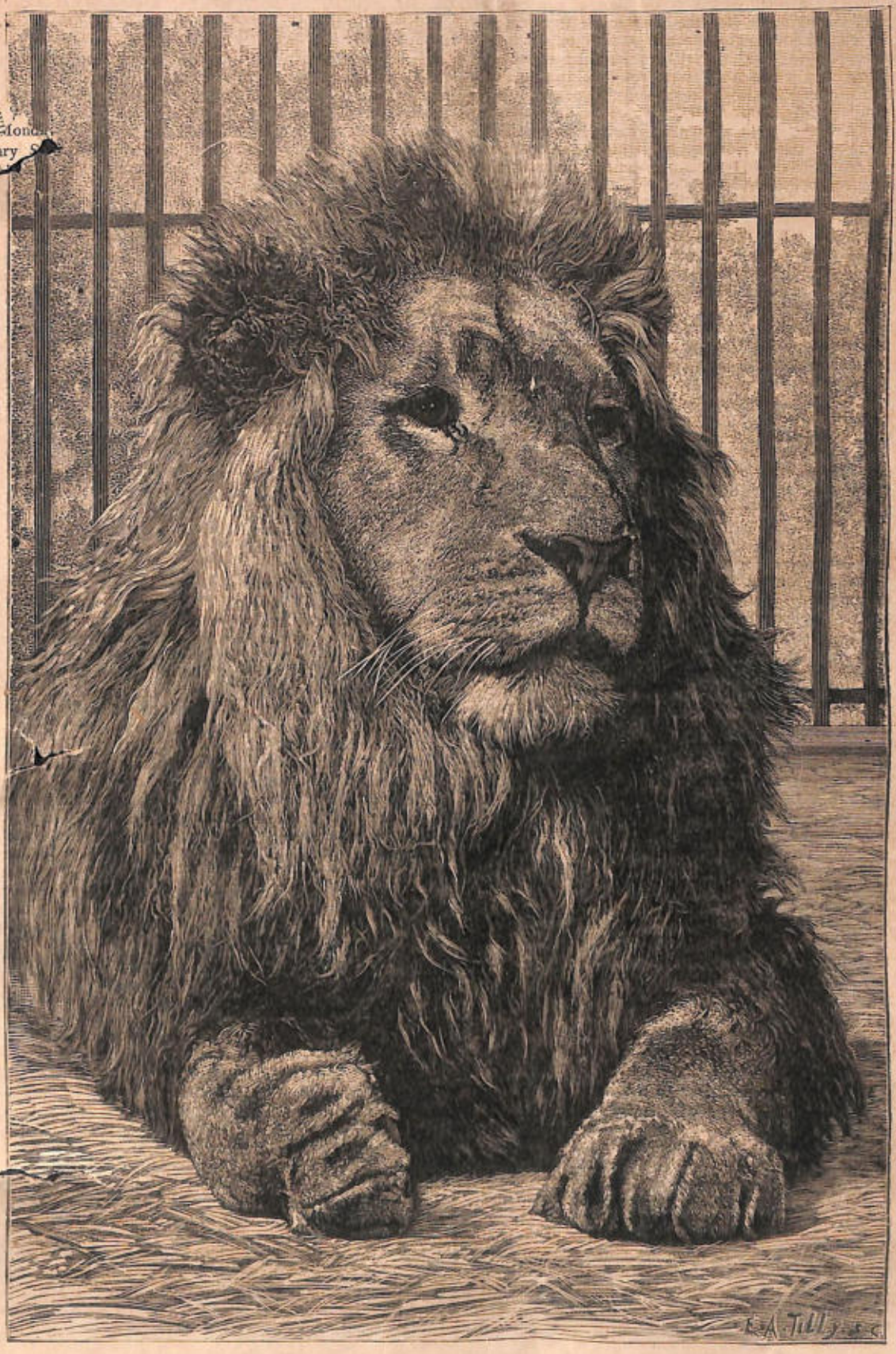
THE LION AT REST

THE illustration which we give on next page, from *La Nature*, is after a photograph of one of the lions in the Zoological Gardens, London. This photograph may be regarded as one of the numerous triumphs of instantaneous photography, valuable both to art and science. The original was rephotographed in Paris directly on wood, by means of a special collodion, at present much used. This has assured a perfectly faithful reproduction of the original, exhibiting all the characteristic details of the lion at rest. The illustration tells its own story.

ON THE RELATIONS OF THE FIG AND THE CAPRIFIG[1]

THE relations of the fig and the caprifig, or the cultivated varieties of fig and the wild form of the Mediterranean region, have been variously explained by different writers, including those recent ones whose works are cited below. Intimately connected with this question is the process of caprification, so often and so circumstantially described by ancient and modern authors, amongst the later of whom we may mention Gasparrini. Graf Solms-Laubach's essay is an elaborate work of upwards of one hundred quarto pages, embodying the results of much research. Not the least interesting part is that treating of caprification, or perhaps we might say the manner in which fertilisation is effected. The author regards the cultivated edible varieties of fig as constituting one race, and the wild caprifig as another race of one and the same species; and the former as having developed from the latter under the influences of cultivation. Gasparrini, on the contrary, described them as distinct genera. Dr. Fritz Müller takes an altogether different view. He says it appears to him far more likely that the fig and caprifig represent, as Linnæus supposed, different forms, the male and the female, belonging together, and not proceeding the one from the other, but which developed side by side, before any cultivation, through natural selection. An examination of the facts adduced by Solms-Laubach himself seems to point to the correctness of Müller's view. But we will set them forth as briefly as possible, leaving the reader to judge for himself. The responsibility of their accuracy rests with the author whom we are quoting. It

[1] "Die Herkunft, Domesticatien und Verbreitung des gewöhnlichen Feigenbaums (*Ficus Carica*, L.)." Von Grafen zu Solms-Laubach. (Göttingen, 1882.)—"Caprificus und Feigenbaum." Von Fritz Müller. *Kosmos*, xi. p. 306.—"Sulla Caprificazione, &c." G. Arcangeli. Processi Verbali della Societh Toscana di Scienze Naturali, November, 1882.



may be well to explain, in the first place, the nature of the fruit of the fig, as it is something more than a seed-vessel of one flower. The fleshy part is a thickened hollow receptacle, closed, except a very narrow aperture at the top, and containing numerous minute flowers crowded together all over the inside of the cavity. Both the fig and caprifig produce three more or less distinct crops of fruit in the course of the year. Each of these crops of fig and caprifig bears a distinctive name; but the three crops of the former do not all reach maturity. In this country only one crop ripens. The varieties of the fig in Naples, whether cultivated or wild, produce fruit at least twice a year, and different varieties exhibit diverse phenomena in the degree of development and maturation of the several crops. In the fig the tissue of the receptacle or inflorescence is fleshy, and the perianth and pedicels of the individual flowers it contains thicken and abound in a sugary juice; whilst the fruit of the caprifig remains hard and milky up to maturity, or only imperfectly softens just at last without any secretion of sugar, and then shrivels and dries up. As long ago as 1770, Colin Milne [1] recorded the fact that the varieties of fig cultivated in England contained only female flowers; and Graf Solms found that male flowers were almost invariably altogether wanting in the varieties cultivated in Naples, and in the very rare exceptional instances in which they were present they were imperfectly developed and abnormal, the anthers being commonly replaced by leafy organs. On the other hand the inflorescence of the caprifig, as observed in Naples, usually contained both male and female flowers, the latter covering the greater part of the surface of the cavity, and the former restricted to a zone, variable in breadth, in the neighbourhood of the apical aperture. It is, moreover, noteworthy that the inflorescence exhibits proterogynous dichogamy in a marked degree. At the time when the female flowers are in a receptive condition the male flowers are still in a very early stage of development. The significance of this will perhaps be better understood after reading the description of caprification—that is if we may assume with Müller that this is really a process of fertilisation, in which there is a mutual adaptation of the inflorescences of the fig and caprifig and the insect which is an agent in procuring fertilisation. Before proceeding to that description, it should be mentioned that a variety of the fig exists in Brittany in which normal male flowers are abundantly produced. Yet, as in the caprifig, the males are not developed until long after the females have passed the receptive stage. The position this variety occupies in relation to other varieties and to the caprifig has not been ascertained. It may be a reversion to an original monœcious condition.

With regard to caprification, it was known to the ancients that an insect inhabits the fruit of the caprifig, and they also discovered that the visits of this insect to the fruit of the fig exercised some beneficial influence, either in accelerating ripening or in hindering the fall of the fruit before it was ripe. Consequently, branches of the caprifig were hung on the fig-trees at a certain season to insure these visits, and effect what was termed caprification. The insect that operates in this manner is a small hymenopter (*Blastophaga grossorum*, Grav syn. *Cynips psenes*, Linn.), the complete annual cycle of development of which takes place within the three crops of fruit of the caprifig, whilst only one generation visits the fig, and that, as will be seen, to no advantage to the insect itself. In order to render what follows easily understood, we will give the present Neapolitan names of the three crops of the caprifig. The fruits that hang through the winter and ripen in April are called *mamme* (*cratitires* of the ancients). These are followed by the *profichi* (*orni*), which ripen in June, and the *mammoni* (*fornites*), which ripen in August and September. If we closely examine the *profichi* when fully ripe in June, we see here and there a black-winged insect emerging from the orifice at the top, its hairy body dusted over with pollen grains that have adhered to it in its passage through the zone of male flowers. And if we cut open one of these fruits, we find a considerable number of these insects, all striving to find the way out. These are females and associated with them are some helpless, sexless males, and very often a number of a slender [illegible] common as well. The female of this generation visits not only the *mammoni*, but also the fruits of the fig, if there are any at hand, in order to deposit her eggs. Now the remarkable fact in connection with this is that she is able to do so effectually in the *mammoni*, but not in the edible fig, though she succeeds in penetrating the fruit far enough to convey pollen to the female flowers, perishing in the act. Furthermore the generation of the insect that develops in the *mammoni* deposits eggs in the *mamme*, and the generation proceeding therefrom finds an asylum for its progeny in the *profichi*. Respecting the reproduction of the *Blastophaga*, Graf Solms claims to have made the important discovery that the eggs must be deposited within the integuments of the ovule itself; otherwise they do not develop. The fertility of the insect is astonishing, a very few of them being able to pierce the numerous female flowers of a fruit of the caprifig. For this purpose the ovipositor is thrust between the branches of the stigma, down the pollen channel of the style into the ovary, and into the solitary ovule itself. This act causes a gall-formation, whilst it does not prevent the development of the ovule into an imperfect seed, which shelters and nourishes the larva that escapes from the egg.

The foregoing condensed extracts are perhaps sufficient to give an idea of the only way in which the female flowers of the fig are fertilised by the male flowers of the caprifig. It seems to be almost certain that seedling figs are unknown in countries where the caprifig does not exist. Where it is found apparently wild it is rather as the remains of cultivation than as plants sprung up from seeds. With regard to the origin of some of the cultivated varieties purporting to have been raised from seeds produced without the intervention of the caprifig, they offer a field for further research and experiment. Possibly they owe their origin to what has been called parthenogenesis, and more recently adventitious embryo-formation. Passing over many other interesting particulars in Graf Solm's essay, we come to one which Dr. Müller regards as strongly in favour of his view. It is this, the seedling offspring of the fig, fertilised by the caprifig, are said to consist of varieties of the fig and the caprifig, pure and simple, without any forms intermediate beween the two parents. On the other hand it is stated that a perfect seed is now and then found in the *profichi*. Prof. Arcangeli, in a later memorandum on the subject, states that he is unable to pronounce judgment in favour of one or the other of these views, and confines himself to recording the following observations on wild and cultivated forms. The *Fico verdino* and the *Fico piombinese* are commonly cultivated varieties in Pisa, yet he had never found a single perfect seed in their fruit, whereas in the fruit of the *Fico biancolino*, which is considered as a wild form, among numerous imperfect seeds he had found some perfect ones, which germinated freely. Whatever light future investigations may throw on this subject, the foregoing facts concerning the life-history of the *Blastophaga* and the fertilisation of the fig are of great interest. In conclusion it may be added that Graf Solms found the same or a closely allied insect in the species of Ficus that are most closely related to *Ficus Carica*, and which inhabit Western Asia, including North-Western India. As Müller suggests, it would be worth while looking into the matter to see whether they offer male and female forms.

W. Botting Hemsley

[1] "A Botanical Dictionary," in the article on "Caprification."

NOTES

THE Queen has been pleased to confer the honour of Knighthood upon Dr. C. W. Siemens, F.R.S.

M. WOLF has been nominated Member of the Academy of Sciences by 32 votes against 21 given to M. Bouquet de la Grye. At Monday's meeting M. Jordan pronounced the *éloge* of Prof. Henry Smith, and M. Bertrand gave an explanation on the double prize, to which we referred last week. He stated that the Commission was aware of the existence of the paper of Prof. Henry Smith, and that it was to oblige Prof. Smith to publish his valuable secret that the prize-subject was selected.

UP to the present date, we understand, there have been received in answer to the official letter of inquiry to the Members of the British Association, as to whether they intended to go to Montreal or not, replies in the affirmative from 340. Among these are a good many who may be said to be really representative of English science, but as might be expected the younger men are present in a larger proportion than the older.

THE Annual Meeting of the Iron and Steel Institute will take place at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Wednesday, May 9th, and two following days. On Wednesday, May 9, the Bessemer Gold Medals for 1883 will be presented to Mr. George J. Snelus and Mr. Sidney Gilchrist Thomas. During the meeting the following papers will be read :—On the Value of Successive Additions to the Temperature of the Air used in Smelting Iron, by Mr. I. Lowthian Bell, D.L., F.R.S., Middlesborough; Comparison of the Working of a Blast Furnace with Blast varying in Temperature from 990° F. to 1414° F., by Mr. William Hawdon, Middlesborough; on American Anthracite Blast Furnace Practice, by Mr. Thomas Hartman, Philadelphia; on the Northampton Iron Ore District, by Mr. W. H. Butlin, Northampton; on Steel Castings for Marine Purposes, by Mr. William Parker, of Lloyds; on the Separation and Utilisation of Tar, &c., from Gas in Siemens' Gas Producers, by Mr. W. S. Sutherland, Birmingham; on Improvements in Railway and Tramway Plant, by M. Albert Riche, London; on the Estimation of Minute Quantities of Carbon by a New Colour Method, by Mr. J. E. Stead, Middlesborough; on the Tin-plate Manufacture, by Mr. Ernest Trubshaw, Llanelly, South Wales; on the Coal-washing Machinery used at Bochum, in Westphalia, by Mr. Fritz Baare, Bochum.

WE regret to announce that Dr. William Farr, C.B., formerly Superintendent of the Statistical Department of the Registrar-General's Office, died on Saturday night. He was born in 1807, at Kenley, in Shropshire, was educated at Shrewsbury, and afterwards proceeded to the Universities of Paris and London. After discharging the duties of house-surgeon of the Infirmary at Shrewsbury for a short time, he continued the practice and teaching of medicine in London, editing the *Medical Annual* and the *British Annals of Medicine*. In 1838 he was appointed Compiler of Abstracts in the Registrar-General's Office, where he organised the statistical department, of which he was made superintendent. In this capacity he assisted in taking the census in 1851, 1861, and 1871. He was author of a large number of articles, contributions to medical journals and papers relating to statistics of health and kindred subjects. He wrote many official reports on Public Health, on the Cholera Epidemic of 1849, and on the Census; and he constructed the English Life Tables, with values of annuities. Dr. Farr was Corresponding Member of the French Institute. It may be remembered that a few years ago considerable disappointment was felt that, when a vacancy occurred in the office of Registrar-General, Dr. Farr was not appointed to the post, with the work of which he had so long been credited.

MAJOR-GENERAL H. G. D. SCOTT, C.B., F.R.S., late Royal Engineers, died on Monday morning at his residence, Silverdale, Sydenham, aged 61. He was educated at the Royal Military Academy, Woolwich, and entered the Royal Engineers in 1840. He acted as instructor in surveying and practical astronomy at Chatham, and also as examiner of military topography for the Military Education Department at the War Office. He retired from the army in 1871 as major-general, and became Director of Buildings at South Kensington, acting as architect to the Royal Albert Hall and Science Schools. He was secretary to the Royal Commissioners of the 1851 Exhibition. He has just finished superintending the construction of the Great International Fisheries Exhibition.

PROF. FRANCIS MARCET, who died a few days ago in London at an advanced age, though English by birth, was a Swiss by adoption and family connection, and spent the greater part of his long life in Geneva. Marcet's achievements in science were numerous and noteworthy, and procured for him the Fellowship of the Royal Society. Some of his discoveries, especially those concerning the boiling point of water, the determination, by freezing, of the specific heat of solids, and, above all, his observations at Pregny on the increase of temperature of artesian wells, are recognised as important. Several of these observations were made in collaboration with his friend, Auguste de la Rive. In conjunction with De Candolle he made a series of researches in vegetable physiology and the action of poison on plants, and his "Manuel de Physique élémentaire," albeit now out of date, ranked forty years ago as the best scientific text-book of the period.

THE French Government are steadily continuing their excellent work of deep-sea investigation. Their vessel, the *Talisman*, is now being equipped and fitted out with the most improved machinery and apparatus, and will leave on June 15 for Morocco, the Canaries, Cape Verd Islands, Azores, and the Sea of Sargasso. Our last expedition of this kind, in the *Challenger*, although highly successful considering the great extent of area traversed by it, might be considered in one respect tentative, and ought to have led to further results. Our own seas have never been sufficiently investigated, while the Americans, Norwegians, Germans, French, and Italians have, especially of late years, been indefatigable in thoroughly exploring their parts of the North Atlantic and Mediterranean.

FROM Monday's debate it is evident that the new Patent Bill will not satisfy everybody, which was just what might be expected. It is certainly a great improvement on the existing law. The provision with regard to the Patent Museum seems to us a step in the right direction. The Bill provides that the control and management of the existing Patent Museum and its contents shall be transferred to and vested in the Department of Science and Art, subject to such directions as Her Majesty in Council may see fit to give. The Department of Science and Art, moreover, may at any time require a patentee to furnish a model of his invention for deposit in the Patent Museum on payment to the patentee of the cost of the manufacture of the model. Another commendable provision is that the Comptroller shall cause to be issued periodically an illustrated journal of patented inventions, as well as reports of patent cases decided by courts of law, and any other information that the Comptroller may deem generally useful or important.

THE Birmingham Natural History and Microscopical Society has established a "Sociological Section," for the study of Mr. Herbert Spencer's system of philosophy. The section originated in a wish to unite, for the purpose of mutual help, those who were already students of Mr. Herbert Spencer's system, but were unknown to each other, and to introduce to the synthetic philosophy those already engaged in some special biological study,

but as yet unfamiliar with the principles common to all departments of natural history. Mr. Herbert Spencer, who is already an honorary vice-president of the Society, has been communicated with, and has expressed his cordial approval of the course of work proposed to be done by the section, adding some valuable suggestions. It is intended to go through the whole of his works, discussing special points as they arise, and where practicable giving illustrations. The president of the section (Mr. W. R. Hughes, F.L.S.) will open the first meeting with a brief address.

SEVERAL contributions to the theory of the microphone have lately appeared. Mr. Shelford Bidwell has communicated to the Royal Society a series of determinations of the changes of resistance of a microphonic contact under different pressures; and comes to the conclusion that the mere fact that a current causes delicately adjusted metal contacts to adhere to each other seems sufficient to account for the superior efficiency of carbon. Mr. Bidwell also thinks that the heat generated at the contact by the current plays an important part, for in carbon this reduces the resistance, whilst in metals it increases it. Mr. Bidwell's experiments on metals were, however, confined to the metal bismuth, which, being both the most fusible and the worst conductor, is the very one which ought to have been avoided. No conclusion of any value as to the metals in general can be drawn from experiments on bismuth alone. Mr. Oliver Heaviside has also experimented on the microphone, and finds the apparent resistance of a contact to vary inversely as the square root of the current. Arguing from these observations he concludes that it is no use to arrange a number of microphones either in series or in parallel. This result is, however, contradicted by experience, for a transmitter such as the Hunnings, with many contacts in parallel, is much more powerful than the single-contact Blake transmitter. Moreover the results attained in Paris lately by M. Moser using a "battery" of microphones arranged partly in series, partly in parallel, disposes of this conclusion of Mr. Heaviside's. It may be remarked that the suggestion to use a battery of microphones was made in 1881 by Prof. Silvanus Thompson. Messrs. Munro and Warwick have lately produced some successful telephonic or microphonic transmitters with metal contacts. These experimenters regard the action of the microphone as due to the existence of a silent discharge of electricity through the thin air stratum at the contact. This view is perhaps sustained by a remarkable observation due to Mr. Stroh, that when a current is passed through a carbon microphone of a peculiar type there is a very minute repulsion observable between the two pieces of carbon, the actual movement being through a distance of ·0005 of a millimetre!

Litera scripta manet is a phrase which is literally true of China. It is generally mentioned in popular books on that country that the respect for paper on which any words are written is so great that scavengers are specially employed to collect it in the streets and preserve it. Whatever doubt existed on this score must now be set at rest, for in a recent issue of the *Peking Gazette* we find a memorial to the throne from the Police Censor of the central division of the capital, reporting that there are in that city over eighty establishments for the remanufacture of waste paper. Paper with characters on it, the memorialist complains, used to be mixed up with the waste paper and defiled by being applied to such base uses. The memorialist and his colleagues published proclamations embodying the sacred edict of the great Emperor Kang-hi, that in heaven and earth there is nothing more precious than written characters. Shopkeepers were forbidden to traffic in printed or written paper, and the manufacturers were ordered to pick out all such paper from among the waste paper purchased by them, and send it to the offices, where a certain amount per pound would be paid for it. Two temples were selected where this paper could be properly burned periodically. The police magistrates on inquiry find that now the manufacturers have some idea of the reverence due to written characters; but some permanent means of supporting the expenses of the purchase and sacred process of destruction should be established, as at present the memorialist has to pay them out of his own pocket. He further suggests that the sale of the house and furniture of a certain escaped criminal, though they will not fetch much, will be sufficient, if put out at interest, to meet these expenses; and he further requests that the sale of written paper to manufacturers be forbidden. The Imperial rescript on this memorial has not come to our notice; but in all probability the escaped criminal's house and furniture are now employed in preventing the defilement of the "*fliegende Blätter*" of Peking.

ACCORDING to the *China Mail* telegraphs in China are likely to receive a most important extension in the shape of a line from Canton to Shanghai. Should this line be constructed, the southern port will then be in direct connection with Tientsin. Lead ore, according to the same authority, has been discovered in Kwantung, the province in which Canton is situated; and it is proposed to work mines of this metal. These movements are stated to be purely Chinese, "and as signs of progress they are worthy of the most attentive consideration."

FROM Mr. J. F. Duthie's "Report on the Progress and Condition of the Government Botanical Gardens at Saharunpur and Mussoorie for the Year ending March 31, 1882," we learn that many additions have been made to the Gardens, of interesting and valuable economic plants, among them the *Cassia marilandica*, L., or North American senna plant, the wax palm of the Andes (*Ceroxylon andicola*, Humb.), upon the trunks of which large quantities of wax are formed and is easily removed by scraping, *Ferula tingitana*, L., the ammoniacum plant of Morocco, *Fraxinus ornus*, L., the manna ash of the Mediterranean region, *Guaiacum officinale*, L., the lignum vitæ of commerce, *Quassia amara*, L., one of the bitterwood trees from the West Indies, *Rheum palmatum*, L., var. *tanguticum*, a native of North-West China, one of the species which yields medicinal rhubarb. Besides these, many new fruits, vegetables, and fodder plants have been under cultivation. Mr. Duthie reports a very important item of cultivation, that of drug-yielding plants for the supply of drugs for the use of the medical department. Extract of henbane and extract of taraxacum have both been made, and Mr. Duthie has prepared a list of other drugs which he proposes to cultivate either in the hills or at Saharunpur. Amongst these may be mentioned aconite, aloes, buchu, calumba root, colchicum, digitalis, gentian, jalap, liquorice, scammony, colocynth, and others. It seems that the cost of maintaining the Saharunpur Gardens much exceeds the income derived from them; but being kept up mainly for scientific purposes they are not expected to prove directly remunerative. It further appears that sanction has lately been given to the closing of the Gardens at Mussoorie and Chajri, which it has been found impossible to work successfully. A new Hill Garden, however, is to be opened at a more eligible site.

THE valuable geological and palæontological collections from Spitzbergen made by Dr. A. Nathorst and Baron De Geer last summer will be distributed between the National Museum and the Geological Museum at Stockholm, while the duplicate specimens will be presented to the museums in Upsala, Lund, and Gothenburg.

THE Second Part of vol. i. of Thomson and Tait's "Natural Philosophy," second edition, is announced for immediate publication, edited for the most part by Prof. F. Darwin. The remaining volume, originally planned, will not be published.

PROF. O'REILLY writes from the Royal College of Science for Ireland, Dublin, that there was visible there, on the night of the 16th, between 10 and 11 o'clock p.m., an aurora appearing as a glow, but without any beams when observed. The wind on the 17th was from the south, but the temperature was still relatively low.

THE opening of the proposed International Horticultural Exhibition and Botanical Congress at St. Petersburg has been postponed to May 5, 1884.

THE Council of the Popular Observatory of the Trocadéro has decided to open a series of Sunday lectures, illustrated by experiments, during the whole of the summer season. The Thursday lectures will be devoted to astronomical topics and delivered in the evening, and will be followed by demonstrations on the sky itself, weather permitting.

DR. DOBERCK, whose appointment to Hong Kong we noted last week, has been attached to Markree and not to Dunsink Observatory.

THE additions to the Zoological Society's Gardens during the past week include a Rude Fox (*Canis rudis*) from Demerara, presented by Mr. G. H. Hawtayne, C.M.Z.S.; an Arabian Gazelle (*Gazella arabica* ♀) from Arabia, presented by Mr. J. Sewell; three Weasels (*Mustela vulgaris*), British, presented by Mr. George Lang; a Wood Owl (*Syrnium aluco*), British, presented by Capt. E. Hall; a Lanner Falcon (*Falco lanarius*) from Eastern Europe, presented by Major J. H. Hussey; a Common Raven (*Corvus corax*), British, presented by the Earl of Eldon; five Mississippi Alligators (*Alligator mississippiensis*) from the Mississippi, presented by Mr. Thos. Baring; two Common Snakes (*Tropidonotus natrix*), British, presented by Lord Londesborough, F.Z.S.; two White-fronted Capuchins (*Cebus albifrons* ♂ ♀) from South America, presented by Mr. H. Smith; a Palmated Newt (*Triton palmipes*), British, presented by Mr. J. E. Kelsall; two Amherst's Pheasants (*Thaumalea amherstiæ* ♂ ♀) from Szechuen, China, deposited; three Lions (*Felis leo* ♂ ♀ ♀) from South Africa, two Reeves's Pheasants (*Phasianus reevesi* ♂ ♀) from China, a Great Black Cockatoo (*Microglossa aterrima*) from New Guinea, a White-backed Piping Crow (*Gymnorhina leuconota*) from Australia, a Common Otter (*Lutra vulgaris*), British, purchased.

OUR ASTRONOMICAL COLUMN

D'ARREST'S COMET.—We last week referred to the discovery of D'Arrest's comet at the Observatory of Strasburg on the 3rd inst., upon the strength of a telegram received at Lord Crawford's observatory from Prof. Krueger, to the following effect:—"Dr. Hartwig discovered on April 3·610 G.M.T. D'Arrest's periodical comet in right ascension 13h. 55m. 24s., declination +8° 16′. Daily motion −44s. in R.A., and + 9′ in declination." This telegram was published in the Dun Echt Circular, No. 76, but in No. 77 issued five days later we read, "Prof. Krueger telegraphs that the object observed by Dr. Hartwig was not D'Arrest's comet but a new nebula." The "daily motion" assigned to the object in the first telegram, notwithstanding its precise accordance in amount and direction with that which the comet would have had in that position, was therefore an illusion. The calculated place of the comet for April 3·610 G.M.T. is in R.A. 13h. 55m. 11s., Decl. +8° 23′·6. During the next period of absence of moonlight for which an approximate ephemeris was given in this column last week, the theoretical intensity of light will be nearly one-third greater than on April 3.

THE GREAT COMET OF 1882.—Prof. Riccò sends us the following observation of this comet made with the 10-inch refractor at Palermo:—

	M.T. h. m. s.	App R.A. h. m. s.	App. Decl. ° ′ ″
April 6 at	8 21 29 ...	5 58 5·93 ...	−9 4 49·2

He states that the comet was a very faint nebulosity with an elongated nucleus containing two or more points. At this time the comet was distant from the earth 3·87, and from the sun 3·75.

In *Bulletino della Società di Scienze Naturali di Palermo* for February 8 we find some remarks by Prof. Riccò on the circumstances attending the passage of the comet through perihelion. On studying the appearance of the sun from twelve to fifteen hours afterwards, he found the prominences were by no means unusual either as regards number or dimensions; there were nine with a greater altitude than 30″, and about as many smaller ones; the highest was one of 85″ on the west-north-west limb, opposite to the part of the disk traversed by the comet, in which no prominences were visible. The comparison of observations made before and after perihelion passage, shows that no very sensible effect was produced upon the motion of the comet in its course through the coronal atmosphere, and Prof. Riccò concludes, on the other hand, that his own observations, made a few hours subsequently, "possono servire a constatare che reciprocamente la cometa non disturbò per nulla il corso degli ordinari fenomeni dell' attività solare."

THE BINARY STAR p ERIDANI.—In a communication to the Royal Society of New South Wales in June, 1880, Mr. Russell, the director of the Observatory at Sydney, suggested, from the measures made since 1856, including his own up to 1880, that this object might not be a binary star at all, but merely afforded an instance of one star passing before another by reason of its proper motion. This opinion is repeated in the volume of double-star results obtained at Sydney, published last year. "In fact," observes Mr. Russell, "a straight line accords better with all the observations made subsequent to Herschel's than any ellipse, and it would appear that the changes are due simply to proper motion; of this I think there cannot be any doubt. . . ." The question has just been very fully and carefully considered by Mr. Downing, of the Royal Observatory, Greenwich, who arrives at an opposite conclusion to that of Mr. Russell, and considers "there is not sufficient evidence to justify us in asserting that p Eridani is other than a binary star." We entirely agree with Mr. Downing in his opinion. If we only compare the measures made by Jacob in 1845-46, with those of Russell and Tebbutt, 1878-80, we get the following expressions:—

$$d \cdot \sin p = -4''{\cdot}361 - [8{\cdot}3894]\,(t - 1850{\cdot}0)$$
$$d \cdot \cos p = +0''{\cdot}122 - [9{\cdot}1017]\,(t - 1850{\cdot}0)$$

showing differences from Herschel's mean measures, epoch 1834·996, of −5°·1 in position, and +0″·82 in distance, which are too large to be tolerated.

This star has been occasionally miscalled 6 Eridani, which would imply that it was one of Flamsteed's stars. Flamsteed, it is true, has a star which he calls 6 Eridani, and which is B.A.C. 926; the binary is B.A.C. 521. The letter p was attached to the star by Lacaille in the catalogue at the end of his *Cælum Australe Stelliferum*. The number 6 is merely borrowed from Bode.

GEOGRAPHICAL NOTES

THE Geographical Society of Lisbon has awarded their gold medal for this year to Mr. Carl Bock, the distinguished eastern traveller, who has also been recently elected Corresponding Member of the Italian Anthropological Society.

THE third German Geographentag was held at Frankfort-on-the-Main on March 29 in the presence of 430 men of science. Prof. Rein (Marburg) delivered the inaugural address, and also opened the geographical exhibition, which comprised 1100 objects of interest. Amongst the most successful addresses we mention the following: Dr. Pechuel Loesche (Leipzig), on the mountain districts of the Congo River, in which he described minutely the mountain chains traversed by the Congo, according to the researches of Oscar Lenz and Güssfeldt. Prof. Ratzel (Munich), on the significance of Polar research with regard to geographical science; he proposed as a resolution, "That the Geographentag recognises that the resumption of Polar research by the German Government is equally in the interest of geographical science and of the German nation." This resolution was adopted unanimously. Dr. Finger (Frankfort), on topography as an introduction to geography. Herr Mang (Baden Baden), on the method of the tellurium and lunarium. Dr. Breusing (Bremen), on the means for the determination of the position of localities at the time of great discoveries. Dr.

Buchner (Munich), on the tribe of Bantu negroes distributed through the whole of South-west Africa. Prof. Günther (Ansbach), on the latest investigations regarding the exact shape of the earth. Lieut. Wissmann, on his journey across Africa with Dr. Pogge. The oldest among the German African travellers now living, Dr. Rüppel, had come to Frankfort to greet Wissmann upon his return. The fourth Geographentag will be held at Munich.

FROM a paper by M. Smicroff on the climate of the Caucasus (published in the Caucasian *Izvestia*, and based on the researches of Dr. Wild on the temperatures in Russia), it is evident that although enjoying a warm climate, still the climate of the Caucasus, especially in the north, is quite continental. Thus, the average mean temperatures of the year are 5°·4 Cels. at Alexandropol, 8°·5 at Stavropol, 12°·6 at Tiflis, and 14°·3 to 14°·5 at Bakou, Lenkoran, Kutais, Poti, and Redut-kaleh; but the yearly range of the average diurnal temperatures is still (with the exception of the two last places) as much as 20 to 30 degrees, while in Central and Southern Russia it varies from 22 to 35 degrees. The highest temperatures observed on the Caucasus vary from 38°·5 at Tiflis, to 34°·4 at Poti; and the lowest from −25°·6 at Stavropol, to −17°·3 at Tiflis, and −6°·6 at Redut-kaleh. It is interesting to compare these temperatures with the +38°·8 and −62° observed at Yakutsk, and −63°·2 at Verkhoyansk. Altogether, it is only in Southern Transcaucasia that localities are found where the temperature does not fall below −10°, and the southern limit of the region beyond which temperatures lower than −20° are no longer found, runs from the Crimea to the Caucasus range, and along the northern slope of this last, towards Khiva, Tashkend, and Peking. The whole range of temperatures observed at Caucasian stations is 60°·4 at Stavropol, 55°·8 at Tiflis, 45°·9 at Bakou, 42°·1 at Poti, and 41°·6 at Redut kaleh. Of course it is nothing in comparison with the range at Yakutsk, where the inhabitants must be accustomed to experience differences of temperature ranging a little more than 100° (from −62° to +38°·8). But still it is large enough, especially for the places situated on the plateaux. High-level meteorological stations are established at Goudaur (2156 metres above the sea-level) and Kvi am (2362 metres). Their average yearly temperatures respectively are 4°·1 (−8° in February and 14°·3 in August) and 1°·1 (−14° in January and 12°·3 in August).

THE last number of the *Izvestia* of the Russian Geographical Society contains an elaborate paper, by M. Malakhoff, on the anthropology of the Vyatka region; a description of inscriptions on rocks on the Yenisei, with drawings, by M. Schukin; a note on old Russian geography, by M. Arsenieff; an account on M. Balkashin's researches into the Kirghiz, being a most valuable addition to our very imperfect knowledge of them. The author comes to the conclusion that the Kirghiz are not a separate nation, but a federation of several nomad tribes who inhabited Southern Russia, the Go i, the neighbourhoods of Dalay-nor, the sources of the Black Irtish, and the shores of the Baikal, who were mingled together by Genghiz Khan and his successors. M. Grigorieff contributes a note in which he shows that Henriette Island, which was discovered by the *Jeannette*, is only the land which was sighted by Hedenström and Sannikoff from New Siberia in 1810, and that Bennett Island was seen by Sannikoff from the northern coast of New Siberia in 1811. There can be no doubt also that the land discovered by Sannikoff to the north-west of the northern extremity of the Kotelnyi Island exists in reality, but is more distant than Sannikoff supposed. This land, which was shown in dotted lines on older maps, but disappeared since Wrangell and Anjou's journeys, ought to be reintroduced on our maps. The same number contains a note on the map of Bokhara of the Greek Vatalsi, a necrological notice of M. Tchoupin, several notes, and a new edition of the complete bibliography of the Amoor, by M. Bousse. One of the notes contained new astronomical determinations and hypsometrical measurements on the Yu-tschou, by Dr. Fritsche; the Shio-Utai-shan proved to be only 9500 feet high, instead of the 11,452 feet given by Mellendorf.

ACCORDING to intelligence from Tashkend, dated March 31, received by the *Cronstadt Courier*, it is in contemplation to send two Russian Exploring Expeditions into Central Asia during the coming summer. The ostensible object of one is to survey part of the Pamir Steppe and fix certain points astronomically, with the object of connecting the Russian surveys with the English. The purpose of the other is to determine the astronomical position of places on the Oxus from the points of passage in its upper course down as far as Khiva, in fact the whole course of the river.

FROM M. De Lesseps's examination of the ground through which it is proposed to let the waters of the Mediterranean into the Tunisian and Algerian Chotts, he concludes that the scheme is perfectly practicable, and that there will be no difficulty as to boring and excavating. The size of the proposed inland sea will be fourteen times that of the Lake of Geneva.

THE SOARING OF BIRDS[1]

THE circling, soaring flight of birds on stiff, outspread wings appears to me a much more complex problem, and less easy of explanation, than that of motionless hovering (poising). At the same time it has certain definite and characteristic features, which must depend upon and connote certain definite aërial conditions, and which should therefore afford us so many hints toward the solution. The whole phenomenon has been very clearly described in NATURE (vol. xxiii. p. 10) by Mr. S. E. Peal, who appears to have had grand opportunities of observing it at Sapakati in Assam. [The explanation which he gives is, however, insufficient, because he does not show how the bird in falling with the wind can acquire greater "impetus" *relative to the air* than it would if the air were still. But such greater "impetus" is necessary if the bird is to rise to a greater height than it would reach in still air.]

The most typical instance that I have observed was on January 8, 1882, near King's Lynn, in Norfolk. The whole country for many miles round is flat, broken only by trees, buildings, and sea walls or river embankments. The wind was strong from the south. The birds (large gulls) were drifting away northwards towards the Wash, circling as they drifted on stiff, outspread wings at a height of 200 or 300 feet, and apparently rising higher. The level nature of the land forbade the notion that the wind had received an upward throw from any fixed obstacle in its path (though I shall show below that there may be upward currents in the air without the presence of a fixed obstacle).

The circling appears to begin about 100 or 200 feet above the ground. A strong wind is a constant and (presumably) necessary condition. The bird descends with the wind, and then circles round to right or left, and rises against the wind to a greater height than it had before. Now if the whole mass of air were moving together horizontally with the same velocity throughout, this action would be wholly inexplicable, for the bird would feel no more wind in one direction than in another, and indeed would have no evidence of the existence of any wind at all except in glancing at objects on the earth. The fact that the earth is slipping away under the air in a certain direction does not affect the bird's relation to the air, and gives no reason why the bird should fall or rise at one phase of its circle more than at another. Still less does it furnish an explanation of the bird's progressive ascent. We may therefore infer as highly probable that the air in which the birds are circling does not move in a mass, but that there is some differential movement in it which makes a great difference to the bird, whether it rises or falls with or against the wind.

I think there are at least two types of differential movement in the upper air which admit of demonstration, and which should be tested in turn to see if either of them can give the meaning of the phenomenon of circling.

(1.) First, there is almost always *a greater velocity in the higher strata* of the air than in the lower. The lower strata are delayed by friction on the earth's surface, and the higher strata outrun them; just as the water of a brook is delayed by friction against its bank, but flows faster in mid-stream.

(2.) Secondly, where currents override or run past one another there is generally some *rolling* between them. This may be seen near the edge of any stream of water if the surface is smooth enough to exhibit the little swirls and whirlpools that are formed between the swifter and slower currents. In the air it may be seen on a grand scale on almost any windy day when there are separate floating clouds in the sky. Looking at right angles to the direction of the wind, each cloud is seen to have a

[1] Lord Rayleigh's valuable letter on this subject (NATURE, vol. xxvii. p. 534) gives me confidence in offering the following considerations, which I had prepared last February, and have submitted to two or three mathematical friends. I congratulate myself on finding my own views in such close agreement with Lord Rayleigh's.—H. A.

systematic movement within itself: the rearward skirts of the cloud are climbing up its sloping back, often with little rolling curls: the top of the cloud is rolling over like a breaker in the act of tumbling on the beach. Each cloud is in fact the scene of a travelling vortex in the air revolving round a more or less horizontal axis. And the persistency with which such a cloud will preserve its individual identity indicates the persistency of the vortex. In the comparatively lower regions of the air it may be difficult to demonstrate the presence of such vortices, but similar causes are present, and it cannot be doubted that similar effects are produced and that such vortices, possessing a proportionate degree of persistency, are generated in those regions of the air which are within the range of the habitual flight of circling birds.

Let us see what effect these conditions (1) and (2) separately would have upon the circling flight of a bird.

(1.) First, let us take horizontal currents increasing in velocity the higher they are above the earth; and suppose a bird at the highest point of one of its gyrations, when it has mounted against the wind and is wheeling to one side or the other, preparatory to the descent with the wind which is to give it sufficient velocity for another rise (but which could not enable it to rise to the same height as before if the air had no internal movement, for there would be no self-renewing force to neutralise the ever-new force of gravity and the perpetual friction of the air). Let us regard the air at the level of the bird, at this turning-point, as *still*. Then, relative to this point, the lower strata of air have a horizontal velocity in the opposite direction to the wind (as perceived on earth); and the bird in falling apparently down the wind will really be meeting stronger and stronger adverse currents, and when it has reached the lowest point of the "circle," it will have a greater horizontal velocity relative to the air at that level than if the whole air through which it has fallen had been still. Therefore, in virtue of its greater horizontal velocity relative to the air (which is accompanied by increased air-resistance), the bird will be subject to a greater force upon its wing-surface, and will therefore be able to mount higher (*cæteris paribus*) than if it had fallen through still air. But (instead of "*cæteris paribus*") suppose the bird, as it rises, wheels gradually round and faces the wind. Then, in rising, it will enter successive strata of air having successively greater and greater velocity relative to itself (the bird) than if the air had no internal movement, and therefore the air-resistance, which is the lifting force, will ever be greater than that due to the height gained by the bird if in still air; and therefore the bird will be able to rise yet higher. But this manœuvre of wheeling to face the wind in rising will cost some time, during which gravity ceases not to act; it will also cost some friction and a slight loss of horizontal velocity, and the question is whether these losses are sufficient to destroy the advantage above described. This is a problem for the mathematicians to solve.

It seems difficult to imagine that within the narrow limits of the actual rise and fall of the bird at the different phases of its circle, there should be sufficient difference of velocity of upper and lower air-currents, to account for such a gain of elevation as Mr. Peal mentions (from 10 to 20 feet at each lap). We require, however, to know the vertical height of the bird's fall and subsequent rise. I have not seen any estimate of this, but, judging from Mr. Peal's diagram, the bird's fall appears no greater than its gain of elevation (10 or 20 feet).

Still it appears from the foregoing considerations that the bird will gain support by falling with the wind and rising against it, when the upper wind is stronger than the lower.

This result suggests that a bird might with like effect make use of two collateral currents of different velocity. Suppose two currents, fast and slow, side by side, flowing in the same direction. The bird may fall with the slow current, and so acquire a certain horizontal velocity. Then let it wheel round against the swift current, and it will be able to rise against it to a height due to the greater horizontal velocity between bird and air. Having reached full height, let it again wheel round into the slow current and recover by a sloping descent therein the horizontal velocity it has lost, which, when recovered, will enable it to mount again against the fast current.

Thus it would appear that a bird can take advantage of alternate fast and slow currents, whether collateral or superposed, rising against the fast and falling with the slow, to maintain itself in the air, while partaking in the general drift of the wind, without flapping its wings.

(2.) In the next case to be considered, we have to deal not with horizontal currents, but with the rotatory currents of rolling masses of air. A mass of air rolling about a horizontal axis will have descending currents in its front, and ascending currents in its rear. The former can be of no use to the bird for the purpose of support. The bird must keep in the rear of the roll, where it will find an upward slanting current. In a high wind this current would probably be strong enough to support the bird in motionless poise (relative to the earth), but this could only be for a few seconds, because the whole vortex is travelling rapidly with the wind (of which it forms a part) and would speedily pass and leave the poised bird behind at the mercy of the downward currents in the van of the next advancing vortex. How then is the bird to remain in the upward current, and at the same time to maintain a high velocity relative to the air in which it moves? It can only be done by circling. The bird must face the current in rising, and as it approaches at once the outskirts of the current and the limits of its own momentum (relative to the air) it must wheel round (—indeed it must have begun to wheel while rising—) and fall down the wind, for the double purpose of recovering its spent velocity and of overtaking the receding vortex.

In falling down the wind, the bird will pass out of a stronger into a weaker current, and will be able to take advantage of the difference (regarded horizontally), just as in the case (already considered) of horizontal currents of different velocity. But regarded vertically the descent into the weaker current will be a disadvantage. However, it is clear that under these conditions there will be no difficulty about the bird's support in air by a circling flight without stroke of wing.

But there is still a difficulty with regard to the progressive ascent of the bird. Mr. S. E. Peal (NATURE, vol. xxiii. p. 10) testifies that the pelican, adjutant, vulture, and cyrus rise circling from 100 or 200 to as much as 8000 feet. Can it be supposed that a rolling vortex of air would have equal range or climb to such a height? Swirls formed at the edge of a deep stream of water are seen to be drawn obliquely away from the side towards mid-stream, and I suppose that an aërial vortex with horizontal axis will in like manner be drawn obliquely upwards into the more rapid air. Moreover I remark that Mr. Peal's observations were made on the coast, and that his diagram represents the birds as rising on a wind blowing up the country towards the hills. Such a wind would have a general upward slant, and any rolling of the air would have the same slant to begin with and to rise from, so that a bird keeping to the (supposed) vortex would rise with it to the same height.

The same principles which we have found useful in dealing with the regular and rhythmical phenomenon of circling flight will, I think, help us to understand the general case of irregular sailing flight, like that of the albatross following a ship, as described by so many writers (*e.g.* the Duke of Argyll, "Reign of Law," fifth edition, pp. 153-4). This general case may be accounted for by (1) irregular alternations (either in strength or direction) of horizontal air-currents; or (2) irregular upward currents.

Currents alternating in strength are equivalent, in relation to any intermediate point, to currents alternating in direction.

To take an extreme, almost imaginary, case: let us suppose a bird on outspread wings exposed alternately to the force of exactly opposite winds. To each in turn the bird will offer the sloping under-surface of its wings, and by each in turn it will be at once uplifted and pushed back, but each will counteract the backward push of the other, while each will reinforce the other's uplifting effort. The result will be that the bird will rise in a wavy line without any effort of its own beyond what is required to keep its wings rigid, and to present them favourably to the alternate winds.

Now suppose the whole air to be travelling horizontally in a direction at right angles to the two opposite currents. This supposition will not affect the lifting power of those opposite currents, but it will make it necessary for the bird (if it is not to be swept away by the travelling air) to sacrifice some of the height it might gain for the sake of making head against the general drift of the wind. This is no longer an extreme or imaginary case, but one of very frequent occurrence. It is simply that of oscillating gusts in a high wind. The air is full of sidelong rushes of wind (probably parts of neighbouring vortices). See how the vane of a weathercock oscillates. A sidelong rush means fresh velocity relative to the bird in a new direction. The bird by a tilt of the wing can instantly convert that fresh air-pressure into a lifting force and rise upon it. And if these rushes of wind come alternately (as in an irregular

fashion they are sure to do) from right and left, the bird can take advantage of their alternation to rise higher and higher, or at least to remain floating, without more effort than that which is required to give the due slope to its wings to make the most of every gust.

Next suppose the whole air with its two alternate opposite currents (as above) to be travelling horizontally in the same direction as one of the two opposite currents. Whether this supposition represents a possible state of things I hardly know, but it would correspond in some measure with the commonly observed phenomenon of a succession of alternate gusts and lulls in the wind. Under these conditions, if the air-movement be all horizontal, it is difficult to see how the bird can turn the alternate gusts to advantage, unless it can alternate its own direction accordingly, stemming the gust and wheeling round to fall back with the lull. The bird then would either circle or would follow a wavy course oblique to the direction of the wind. But I imagine that alternate gusts and lulls (as felt, say, at the top of an observatory) are generally caused by a succession of vortices, of which only one phase at a time is present to the observer. These vortices will be infinitely various in the direction of their axes and currents, and it is useless to try and imagine their relative positions. Probably the sea-birds, with their aeons of inherited experience, have acquired an instinctive perception of the probable sequences and correlations of air-streams and air-swirls, and are thereby guided so to steer their course, selecting the upward and avoiding the downward currents, as to gain the greatest possible advantage of lifting force that those currents can afford, to the great economy of their muscular strength, which would otherwise have to be spent in the labour of the wing.

In reading of the way in which albatrosses and other large sea birds will follow a ship at sea with little or no flapping of the wings, it has occurred to me that the great obstacle which the ship herself offers to the wind must of necessity give the wind an upward throw and originate a vortex in the air, possibly large enough and persistent enough to be useful to the birds. If the ship be a steamer, the drift of smoke from the funnel will indicate approximately the path of the retiring vortex. It is long since I have had any opportunity of observing, but I well recollect that the gulls used often to be seen in close relation to the smoke that drifted to leeward of the steamer. It is true that any chance morsels of biscuit, &c., thrown from the steamer would probably be thrown to leeward, and this might help to determine the position of the expectant gull.

Again, at sea, the ocean waves themselves, such as roll in from the Atlantic to the Land's End, must throw the wind into rolling vortices, which would afford slant upward currents. The slant, though very flat, might well be sufficient for the purpose of support to the long-winged sea-birds that know how to use it.

On land, countless obstacles impede the lower wind and tend to throw the air into a roll.

Bearing in mind, then, the perpetual variation in strength and direction of current in a high wind, the whirls and gusts, and veering flaws, and seeing how it is possible for the bird to utilise every such variation (except a downward current) to the purpose of its bodily support, we may, I think, obtain some insight into the agency whereby the birds accomplish their marvellous feats of soaring and sailing, upborne upon stiff-strained, motionless wings.

Further observations however are required for the full solution of the problem which I have here only tentatively approached.

HUBERT AIRY

Woodbridge, February 28

SOME POINTS IN ELECTRIC LIGHTING[1]

THE science of lighting by electricity was divided by the lecturer into two principal parts—the methods of production of electric currents, and of conversion of the energy of those currents into heat at such a temperature as to be given off in radiations to which the eye was sensible. The laws known to connect together those phenomena called electrical, were essentially mechanical in form, closely correlated with mechanical laws, and might be most aptly illustrated by mechanical analogues. For example, the terms "potential," "current," and "resistance," had close analogues respectively in "head," "rate of flow," and "coefficient of friction" in the hydraulic transmission of power. Exactly as in hydraulics head multiplied by velocity of flow was power measured in foot-pounds per second, or in horse-power, so potential multiplied by current was power and was measurable in the same units. Again, just as water flowing in a pipe had inertia and required an expenditure of work to set it in motion, and was capable of producing disruptive effects if that motion were too suddenly arrested, so a current of electricity in a wire had inertia: to set it moving electromotive force must work for a finite time, and if arrested suddenly by breaking the circuit the electricity forced its way across the interval as a spark. Corresponding to mass and moments of inertia in mechanics there existed in electricity coefficients of self-induction. There was, however, this difference between the inertia of water in a pipe and the inertia of an electric current—the inertia of the water was confined to the water, whereas the inertia of the electric current resided in the surrounding medium. Hence arose the phenomena of induction of currents upon currents, and of magnets upon moving conductors—phenomena which had no immediate analogues in hydraulics.

The laws of induction were then illustrated by means of a mechanical model devised by the late Prof. Clerk Maxwell.

In the widest sense, the dynamoelectric machine might be defined as an apparatus for converting mechanical energy into the energy of an electrostatic charge, or mechanical power into its equivalent electric current through a conductor. Under this definition would be included the electrophorus and all frictional machines; but the term was used in a more restricted sense, for those machines which produced electric currents by the motion of conductors in a magnetic field, or by the motion of a magnetic field in the neighbourhood of a conductor. The laws on which the action of such machines was based had been the subject of a series of discoveries. Oersted discovered that an electric current in a conductor exerted force upon a magnet; Ampere that two conductors conveying currents generally exerted a mechanical force upon each other: Faraday discovered—what Helmholtz and Thomson subsequently proved to be the necessary consequence of the mechanical reactions between conductors conveying currents and magnets—namely, that if a closed conductor moved in a magnetic field, there would be a current induced in that conductor in one direction, if the number of lines of magnetic force passed through the conductor was increased by the movement; in the other direction if diminished. Now all dynamoelectric machines were based upon Faraday's discovery. Not only so; but however elaborate it might be desired to make the analysis of the action of a dynamo-machine, Faraday's way of presenting the phenomena of electromagnetism to the mind was in general the best point of departure. The dynamo-machine, then, essentially consisted of a conductor made to move in a magnetic field. This conductor, with the external circuit, formed a closed circuit in which electric currents were induced as the number of lines of magnetic force passing through the closed circuit varied. Since, then, if the current in a closed circuit was in one direction when the number of lines of force was increasing, and in the opposite direction when they were diminishing, it was clear that the current in each part of such circuit which passed through the magnetic field must be alternating in direction, unless indeed the circuit was such that it was continually cutting more and more lines of force, always in the same direction. Since the current in the wire of the machine was alternating, so also must be the current outside the machine, unless something in the nature of a commutator was employed to reverse the connections of the internal wires in which the current was induced, and of the external circuit. There were then broadly two classes of dynamoelectric machines—the simplest, the alternating-current machine, where no commutator was used; and the continuous-current machine, in which a commutator was used to change the connection with the external circuit just at the moment when the direction of the current would change. The theory of the alternate-current machine was then explained, and it was proved that two independently-driven alternate-current machines could not be worked in series, but that they might be worked in parallel circuit, and hence were quite suitable for distribution of electricity for lighting without the necessity of providing a separate circuit for each machine.

It was easy to see that, by introducing a commutator revolving with the armature, in an alternate-current machine, and so arranged as to reverse the connection between the armature and the external circuit just at the time when the current would

[1] Abstract of lecture delivered at the Institution of Civil Engineers on Thursday evening, April 5, by Dr. John Hopkinson, F.R.S., M.Inst.C.E.

reverse, it was possible to obtain a current constant always in direction; but such a current would be far from constant in intensity, and would certainly not accomplish all the results obtained in modern continuous-current machines. This irregularity might, however, be reduced to any extent by multiplying the wires of the armature, giving each its own connection to the outer circuit, and so placing them that the electromotive force attained a maximum successively in the several coils. A practically uniform electric current was first commercially produced with the ring armature of Pacinotti, as perfected by Gramme. A dynamo-machine was not a perfect instrument for converting mechanical energy into the energy of electric current. Certain losses inevitably occurred. There was the loss due to friction of bearings, and of the collecting-brushes upon the commutator; there was also the loss due to the production of electric currents in the iron of the machine. When these were accounted for, there remained the actual electrical effect of the machine in the conducting wire; but all of this was not available for external work. The current had to circulate through the armature, which inevitably had electrical resistance; electrical energy must therefore be converted into heat in the armature of the machine. Energy must also be expended in the wire of the electromagnet which produced the field, as the resistance of this also could not be reduced beyond a certain limit. The loss by the resistance of the wires of the armature and of the magnets greatly depended on the dimensions of the machine. To know the properties of any machine thoroughly, it was not enough to know its efficiency and the amount of work it was capable of doing; it was necessary to know what it would do under all circumstances of varying resistance or varying electromotive force; and, under any given conditions, what would be the electromotive force of the armature? Now this electromotive force depended on the intensity of the magnetic field, and the intensity of the magnetic field depended on the current passing round the electromagnet and the current in the armature. The current then in the machine was the proper independent variable in terms of which to express the electromotive force. The simplest case was that of the series-dynamo, in which the current in the electromagnet and in the armature was the same, for then there was only one independent variable. The relation between electromotive force and current might be most conveniently expressed by a curve.

When four years ago the lecturer first used such a curve (since named by Deprez the "characteristic curve") for the purpose of expressing the results of his experiments on the Siemens dynamo-machine, he pointed out that it was capable of solving almost any problem relating to a particular machine, and that it was also capable of giving good indications of the results of changes in the winding of the magnets, or of the armatures of such machines. The use of the characteristic curve was illustrated with reference to charging accumulators and Jacobi's law of electric transmission of power.

When the dynamo-machine was not a series-dynamo, but the current in the armature and in the electromagnet, though possibly dependent upon each other were not necessarily equal, the problem was not so simple. In that case there were two variables, the current in the electromagnet and the current in the armature; and the proper representation of the properties of the machine would be by a characteristic surface, of which a model was exhibited. By the aid of such a surface any problem relating to a dynamo-machine could be dealt with, no matter how its electromagnets and its armature were connected together. Of course in actual practice the model of the surface would not be used, but the projections of its sections.

The properties of a machine depended much upon its dimensions. Suppose two machines alike in every particular, excepting that the one had all its linear dimensions double that of the other. The electrical resistances in the larger machine would be one-half those of the smaller. The current required to produce a given intensity of magnetic field would be twice as great in the larger machine as in the smaller. The comparative characteristic curves of the two machines when driven at the same speed were shown in a diagram. The two curves were one the projection of the other, having corresponding points with abscisses in the ratio of one to two, and the ordinates in the ratio of one to four. At first sight it would seem that the work done by the larger machine should be thirty-two times as much as that which would be done by the smaller. Practically, however, no such result could possibly be attained for many reasons. First, the iron of the magnets became saturated, and consequently, instead of eight times the electromotive force, there would only be four times the electromotive force. Secondly, the current which the armature could carry was limited by the rate at which the heat generated in the armature could escape. Again, the larger machine could not run at so great an angular velocity as the smaller one. And lastly, since in the larger machine the current in the armature was greater in proportion to the saturated magnetic field than in the smaller one, the displacement of the point of contact of the brushes with the commutator would be greater. Shortly, the capacity of similar dynamo-machines was pretty nearly proportionate to their weight, that was to the cube of their linear dimensions; the work wasted in producing the magnetic field was directly as the linear dimensions; and the work wasted in heating the wires of the armature was as the square of the linear dimensions.

A consideration of the properties of similar machines had another important practical use. Mr. Froude was able to control the design of ironclad ships by experiments upon models made in paraffin wax. It was a much easier thing to predict what the performance of a large dynamo-machine would be, from laboratory experiments made upon a model of a very small fraction of its dimensions. As a proof of the practical utility of such methods, the lecturer stated that by laboratory experiments he had succeeded in greatly increasing the capacity of the Edison machines without increasing their cost, and with a small increase of their percentage of efficiency, remarkably high as that efficiency already was.

The electric properties of the electric arc were experimentally illustrated; in particular it was shown that the difference of potential between the carbons was nearly independent of the current.

When a current of electricity passed through a continuous conductor it encountered resistance, and heat was generated, as shown by Joule, at a rate represented by the resistance multiplied by the square of the current. If the current was sufficiently great, heat would be generated at such a rate that the conductor would become incandescent and radiate light. Attempts had been made to use platinum and platinum iridium as the incandescent conductor. But these bodies were too expensive for general use, and besides that, refractory though they were, they were not refractory enough to stand the high temperature required for incandescent lighting, which should be economical of power. Commercial success was not realised until very thin and very uniform threads or filaments of carbon were produced and inclosed in reservoirs of glass, from which the air was exhausted to the utmost possible limit. Such were the lamps made by Mr. Edison with which the Institution was temporarily lighted. The electrical properties of such a lamp were examined, and in particular it was shown that its efficiency increased and its resistance diminished with increase of current.

The building was lighted by about 230 lamps, each giving sixteen candles light, produced each by 75 Watts of power developed in the lamp. To produce the same sixteen candles' light in ordinary good flat-flame gas-burners, would require between 7 and 8 cubic feet of gas per hour, contributing heat to the atmosphere at the rate of 3,400,000 foot-pounds per hour, equivalent to 1250 Watts, or nearly seventeen times as much heat as the incandescence lamp of equal power.

At the present time, lighting by electricity in London must cost something more than lighting by gas. What were the prospects of reduction of this cost? Beginning with the engine and boiler, the electrician had no right to look forward to any marked and exceptional advance in their economy. Next came the dynamo, the best of these were so good that there was little room for economy in the conversion of mechanical into electrical energy; but the prime cost of the dynamo-machine was sure to be greatly reduced. Hope of considerably increased economy must be mainly based upon probable improvements in the incandescence lamp, and to this the greatest attention ought to be directed. It had been shown that marked economy of power could be obtained by working the lamps at high pressure, but then they soon broke down. In ordinary practice, from 140 to 200 candles were obtained from 1 horse-power, developed in the lamps, but for a short time he had seen over 1000 candles per horse-power from incandescence lamps. The problem, then, was so to improve the lamp in details, that it would last a reasonable time when pressed to that degree of efficiency. There was no theoretical bar to such improvements, and it must be remembered that incandescence lamps had only been articles of commerce for about three years, and already much had been done. If such an improvement were realised, it would mean that it

would be possible to get five times as much light for a sovereign as could be done now. At present electric lighting would succeed commercially where other considerations than cost had weight. Improvements in the lamps were certain, and there was a probability that these improvements might go so far as to reduce the cost to one-fifth of what it now was. He left the meeting to judge whether or not it was probable, nay, almost certain, that lighting by electricity was to be the lighting of the future.

HARDENING AND TEMPERING STEEL

ONE of a series of lectures to the Liverymen and Apprentices of the Company of Cutlers of London was delivered on Thursday last by Prof. W. Chandler Roberts, F.R.S., "On some Theoretical Considerations connected with Hardening and Tempering Steel."

The Master of the Company, Mr. J. Thorne, presided, and the Lecturer observed that the phenomena with which they had to deal, although admittedly as interesting and remarkable as any in the whole range of metallurgy, are but little understood.

If the fact that steel can be hardened had not been known, the whole course of our industrial and even political history would probably have been widely different, and the dagger, which occupies so prominent a place in the armorial bearings of the City of London, would have represented a survival of implements made, not of steel, but of copper hardened with tin.

It has long been known that there are extraordinary differences between the properties of wrought iron, steel, and cast iron, but our knowledge that these differences depend upon the presence or absence of carbon is only a century old, for it was not until the year 1781 that Bergman, Professor in the University of Upsala, showed that wrought iron, steel, and cast iron, when dissolved in certain acids, leave amounts of a graphitic residue, varying from $\frac{1}{10}$ to $2\frac{1}{2}$ per cent., which are essential to the constitution of these three varieties of metal. Bergman's work led many early experimenters, notably Clouet in 1796, to attempt to establish the importance of the part played by carbon, and Clouet converted pure iron into steel by contact at a high temperature with the diamond, which was the purest form of carbon he could command. Prof. Roberts said that this experiment had been repeated by many other observers with varying success, as in all the earlier work the furnace gases, which had not been excluded, might have converted the iron into steel without the intervention of the diamond. It remained for a distinguished Master of the Cutlers' Company, Mr. W. H. Pepys, to repeat Clouet's fundamental experiment under conditions which rendered the results unequivocal, by employing electricity as a source of heat. This experiment, which had been communicated to the Royal Society in 1815, was performed in the way Pepys had indicated.

It was then shown that in soft, tempered, and hardened steel respectively the carbon has a distinct "mode of existence," as is indicated by the widely different action of solvents on the metal in these three states.

The evidence as to whether carbon in steel is *combined* in the chemical sense, or is merely *dissolved*, was then considered at some length, special reference being made to the results obtained by various experimenters, from Berzelius and Karsten to Sir Frederick Abel of the War Department.

Prof. Roberts stated that the researches of Troost and Hautefeuille afforded strong evidence that in "white cast-iron" and steel the carbon is merely dissolved, a view which he adopted, as he did not consider it to be at all in opposition to the facts recently established by Sir Frederick Abel, who had shown that the carbon may be left by the slow action of solvents on soft steel as a carbide of iron.

The various physical, as distinguished from the chemical theories that had been propounded from the time of Réaumur, (1722) to that of Akerman (1879), to account for the "intimacy of the relation" of carbon and iron in hard as compared with soft steel, were then described at some length, and the remarkable experiments of Réaumur, who cooled steel slowly in a Torricellian vacuum in order to show that the absorption of gas did not take place during cooling, was illustrated.

In recent years much importance has been attached to the physical evidence as to the peculiar constitution of steel, and it has been shown that there is a remarkable relation between the amount of carbon contained in different varieties of steel and their electrical resistance. Some of the very interesting experiments of Prof. Hughes on this point were then exhibited and described, and Prof. Roberts concluded by saying that the value of the early work by Bergman and Réaumur had rather been lost sight of in recent discussions, Bergman's work being specially remarkable, as he attempted, by thermometric measurement, to determine the heat equivalent of the phlogiston he believed iron and steel to contain.

The importance of the degree of carburisation of steel from the point of view of its technical application was illustrated by reference to a series of curves, and it was incidentally mentioned that, in the case of the variety of steel used for the manufacture of coinage-dies, the presence of $\frac{1}{10}$ per cent. of carbon more or less than a certain standard quantity makes all the difference in the quality of the metal.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—The new Board of the Faculty of Natural Science has issued its first list of lectures this term. The lectures are divided under the following heads:—Physics, Chemistry, Animal Morphology, Geology, and Botany. No lectures are scheduled this term under Mineralogy or Physiology.

In Physics Prof. Clifton lectures on "Instruments and Methods of Measurement employed in the Study of Optics." These lectures are given in the Clarendon laboratory, where practical instruction in Physics is given by the Professor, assisted by Messrs. Stocker and Heaton. At Christ Church Mr. Baynes lectures on Electrokinematics and Electrodynamics, and gives practical instruction on Electric and Magnetic Measurements. At Balliol Mr. Dixon gives a course of experimental lectures on Elementary Heat and Light.

In Chemistry Dr. Odling lectures at the Museum on the Composition of Air and Water; Mr. Fisher lectures on Inorganic Chemistry; and Dr. Watts on the Cyanogen Series. At Christ Church Mr. Harcourt has a class for Quantitative Analysis, and Mr. Dixon a class for Gas Analysis.

In Animal Morphology Prof. Moseley lectures on Comparative Anatomy, and gives practical instruction to his class after each lecture; Mr. Hickson lectures on the Development of the Chick, Mr. Hatchett Jackson on Mammalian Osteology and the Principles of Embryology, Mr. Poulton on the Distribution of Animals, and Mr. Lewis Morgan on the Vertebrate Exoskeleton and on Human Osteology.

In Botany Mr. Chapman gives practical instruction on Vegetable Morphology at the Botanic Gardens.

In Geology Prof. Prestwich will give a series of lectures on Friday afternoons on the strata and fossils to be visited on his Saturday excursions.

On June 19 an examination will be held in common by Magdalen, Merton, and Corpus Christi Colleges for electing a Scholar in Physical Science at each College. At Merton and Corpus the chief subjects will be Chemistry and Physics.

Jesus College offers a Welsh Scholarship in Natural Science. The examination will be held on June 14.

Examinations for the degree of Bachelor of Medicine (both First and Second) will be held this term. Candidates are to send in their names before May 1.

CAMBRIDGE.—Prof. Huxley's Rede Lecture at Cambridge University will be given on June 12, at 3 p.m., in the Senate House. The subject is not yet announced.

Dr. Michael Foster leaves the Lectures on Elementary Biology for this term in the hands of Dr. Vines and Mr. Sedgwick, and will hold Catechetical Classes in Physiology for the Natural Sciences Tripos.

Dr. F. Darwin will give six Demonstrations on the Physiology of Plants (Growth, Movement, &c.) at the Physiological Laboratory on Saturdays at noon, beginning April 21.

Prof. Liveing will lecture on the Chemistry of the Heavenly Bodies, beginning May 1.

LONDON.—Mr. A. H. Keane has been appointed to the Hindustani Lectureship at University College.

THE Winter Session at the College of Agriculture, Downton, near Salisbury, ended on Monday, when the certificates and prizes were presented to the successful students by Archdeacon Sanctuary. The certificate of membership, obtainable on examination after completion of the two years' course of study, was granted to Mr. Arthur Herbert Kerr, Crookham, Farnham,

and to Mr. Henry Blair Mayne, Brantridge Park, Balcombe. The Scholarship, open to first-year students, was awarded to Mr. Robert Alan Benson, Clifton.

SOCIETIES AND ACADEMIES

LONDON

Mathematical Society, April 12.—Prof. Henrici, F.R.S., president, in the chair.—The Chairman announced that Prof. Rowe, of University College, London, had been elected a Member of the Council in the room of the late Prof. Henry Smith, F.R.S.—The following communications were made:—Equations of the loci of the intersections of three tangent lines and of three tangent planes to any quadric $u = 0$, Prof. Wolstenholme.—Investigation of the character of the equilibrium of an incompressible heavy fluid of variable density, Lord Rayleigh, F.R.S.—On the normal integrals connected with Abel's theorem, Prof. Forsyth.—Spherical functions, Part 1, Rev. M. M. U. Wilkinson.—Calculation of the equation which determines the anharmonic ratios of the roots of a quintic, Prof. M. J. M. Hill.—On simultaneous differential equations, with special reference to (1) the roots of the fundamental determinant, (2) the method of multipliers, Mr. E. J. Routh, F.R.S.

Chemical Society, April 5.—Dr. W. H. Perkin, president, in the chair.—It was announced that a ballot for the election of Fellows would take place at the next meeting of the Society (April 19).—The following papers were read:—On the estimation of hydrogen sulphide and carbonic anhydride in coal-gas, by L. T. Wright. The coal-gas, dried and freed from ammonia, is passed through two weighed U-tubes, the first containing precipitated cupric phosphate dried at 100° and calcium chloride, the second, soda lime, slightly moist, and calcium chloride. Three cubic feet of clean coal-gas are first passed through the U-tubes to "saturate" the reagents. The increase of weight of the first U-tube, after the passage of the crude coal-gas, then gives the hydrogen sulphide, and the increase in weight of the second the carbonic anhydride.—Some compounds of antimony and bismuth containing two halogens, by R. W. Atkinson.—On the theory of a molecular combination, when antimonious chloride is mixed with potassium bromide and antimonious bromide with potassium chloride, two distinct compounds should be produced. The author finds that but one is formed, the two compounds being identical in composition as well as in colour, crystalline form, and other physical characters. This body has the formula $SbCl_3.BrK + 3H_2O$. An attempt to form the corresponding bismuth compound was not successful.—Contribution to the chemistry of the cerite metals, by B. Brauner. The author has determined the atomic weight of didymium with the greatest care, and fixes it at 145·4; the higher numbers previously obtained were due to the presence of a metal having a higher atomic weight; this metal is proved by the author to be samarium, the atomic weight of which he calculates to be 150. The author also proves that the principal gadolinite earths—yttria, terbia, erbia, &c.—are present in cerite, but not in large quantities.

Institution of Civil Engineers, April 3.—Mr. Brunlees, president, in the chair.—The paper read was "On the Summit-Level Tunnel of the Bettws and Festiniog Railway," by Mr. William Smith, M.Inst.C.E.

April 10.—Mr. Brunlees, president, in the chair.—The paper read was on "The Introduction of Irrigation into New Countries, as illustrated in North-Eastern Colorado," by Mr. P. O'Meara, M.Inst.C.E.

EDINBURGH

Royal Society, April 2.—Mr. John Murray in the chair.—Dr. Gibson, in a communication on some laboratory arrangements, described and exhibited a modification of Bunsen's method of filtration. The modification consisted essentially in placing the vessel which received the filtrate inside a bell-jar, which was connected with the exhausting apparatus and perforated above so as to admit the funnel through which the liquid filtered. By a suitable three-way stopcock arrangement the adjusting of the internal partial vacuum was kept quite under the control of the experimenter. A contrivance for the more convenient use and better preservation of sulphuretted hydrogen water was also described and shown.—Prof. Tait, in a short note on the thermoelectric position of pure cobalt, described recent experiments which fully bore out results formerly obtained with other specimens. The cobalt line runs nearly parallel to the iron line, but far down on the diagram below palladium and nickel. Prof. Tait also indicated the solution of certain problems of heat conduction in heterogeneous bodies as affected by the Peltier and Thomson effects.—Prof. George Forbes read a paper on transmission of power by alternate currents, in which he pointed out the value of alternate current machines as electromotors, especially in cases in which perfect isochronism was of importance.—Prof. Herdman, in a paper on the so-called hypophysis in the Tunicata, described the structure of the neural (hypophysal) gland and the dorsal tubercle in various Ascidians, and suggested that possibly the connection of the neural gland (and also of the vertebrate hypophysis cerebri) with the pharynx might be a secondary modification caused by one or more of a series of primitive lateral excretory ducts, opening either upon the exterior of the body or into the peribranchial cavity, having come to open into a lost sense organ, in the Stomodæum represented by the dorsal tubercle. These lateral ducts are found in *Ascidia mammillata*, in some cases existing along with a median duct opening into the pharynx at the dorsal tubercle, and in other cases without this connection with the supposed sense-organ.—Prof. Tait presented a paper on the quaternion expression for the displacements of a rigid system, by Dr. G. Plarr.

Mathematical Society, April 13.—Mr. A. J. G. Barclay, M.A., in the chair.—Mr. J. S. Mackay, president, read a paper on the triangle and its six-scribed circles, adding historical notes on the discovery of the various properties enumerated. The name *medioscribed* circle (il circolo medioscritto) was suggested for use instead of nine-point circle, as had been proposed twenty years ago by G. B. Marsano, "Considerazioni sul Triangolo Rettilineo," Genova, 1863, p. 11.

BERLIN

Physiological Society, March 9.—Prof. Du Bois Reymond in the chair.—Dr. Wernicke gave a short sketch of the illness of a patient who fell sick, exhibiting all the symptoms of a cerebral tumour except epileptic attacks, and who manifested a disturbance of speech that was characterised by Dr. Wernicke as a "sensorial aphasia," and by others as "word-deafness." A sensorial aphasia consists, according to Dr. Wernicke, in the fact that the patients, though in possession of a large vocabulary, no longer understand the meaning of words, that they use these confusedly, and so that their speech is quite muddled; moreover they do not understand what one says to them at all, so that it is impossible to arrive at an understanding with them. The patient in question soon succumbed to an intercurrent disease, and it was possible to make a thorough dissection of the brain, which exhibited a bilateral affection of the cerebral cortex at the first temporal convolution. An accurate dissection of the ears showed that the deafness that had been observed during life was not brought about by any disease of the sound-conducting apparatus, but that it was rather to be regarded as a central deafness conditioned by the disease of the cortex of the first spheno-semporal convolution in which, as Dr. Wernicke made probable so long as ten years ago, the terminal expansion of the acoustic nerve has its seat. Now the local disease of the brain-cortex and the consequent observed disturbances in hearing and speech correspond to the manifestations of "soul-deafness" that were experimentally produced by Dr. Munk in animals by extirpation of the auditory sphere (*Hörsphäre*), and consequently establish the results of experiments on animals as true for man also. The total deafness of the patient had only set in at a later period towards the end of the disease, when the affection of the brain had passed from the cortex into the deeper structures and had destroyed the acoustic fibres. The physiological import of the above case consists in the clearly proved limitation of the disease to the first temporo-sphenoidal convolution in a case where the clinical phenomena corresponded accurately to those of "soul-deafness."—Dr. J. Munk had found in previous experiments that the function of neutral fats in nutrition can just as well be performed by the fatty acids. Animals manifested absolutely no disturbance of nutrition when supplied with fatty acids instead of fats; the fatty acids were made into an emulsion, and absorbed by the villi in precisely the same fashion as the fats, and afterwards the chyle-vessels were found just as densely filled with a milky fluid as after a meal of fat. The examination of the chyle had, however, shown that the fatty acids that were supplied were no longer to be found, but only neutral fats, and hence Dr. Munk had assumed that a synthesis of neutral fats took place as the fatty acids passed out of the intestinal villi into the chyle, and that the glycerine was supplied by the animal body, probably by the breaking down of albu-

men. Dr. Munk has only just lately been able to advance a proof of the truth of this assumption of a synthesis of neutral fat out of fatty acids after it had been shown by other observers that heterogeneous neutral fats could be taken up by animals and also be deposited as such in the body. Dr. Munk now fed a dog, which had been greatly reduced in weight by prolonged starvation, with large quantities of the fatty acids of mutton and with a little lean meat. The animal very soon increased considerably in weight upon this diet, and after fourteen days had deposited 1100 grms. of fat in various organs under the skin, in the mesentery, in the heart, and in the liver. Analysis of this fat elicited the fact that it consisted of at least 96 per cent. of neutral mutton fat. And in the dog it is evident that the mutton fat could only have arisen by a synthesis of the fatty acids of mutton that were eaten.

PARIS

Academy of Sciences, April 9.—M. Blanchard in the chair.—The following papers were read:—On charbonous vaccination, by M. Pasteur. Some Turin professors having found that vaccinated as well as unvaccinated sheep died after virulent inoculation, M. Pasteur made inquiry, and came to the conclusion that the blood used for such inoculation was septic as well as charbonous (the sheep was dead twenty-four hours before its blood was taken). He challenges the Turin men to a test of this.—Description of a means of obtaining a wholly automatic action of the sluice with oscillating liquid columns, without cataract; experimental realisation of this system during the emptying of the sluice of l'Aubois, by M. de Caligny.—Units of mechanics and of physics, by M. Ledieu.—The salt lands of the South-East, by M. de Gasparin. The problem of freeing such ground from salt is (unlike the formation of a *polder*) an indeterminate one, and may be insoluble; many years' submersion and drainage may be ineffectual.—Report on electrodynamic machines applied to the transmission of mechanical work, by M. Marcel Deprez. The dynamometric return (viewed apart from the mechanical motor) was over 48 per cent.—On surfaces with *nil* mean curvature, on which may be limited a finite portion of the surface by four straight lines situated on the surface, by M. Schwarz.—A letter of invitation to the Institute to the second session of the Royal Society of Canada at Ottawa, on May 22, was read.—Observation of the transit of Venus at Punta-Arenas (Straits of Magellan), by M. Cruls. The four contacts were observed under excellent conditions.—Observations of the Swift-Brooks comet, by M. Périgaud.—Observations of comet II., 1882, at Algiers Observatory, by M. Trépied.—On uniform functions affected by sections, and on a class of linear differential equations, by M. Appell.—Law of periods, by M. de Jonquières.—Remarks on the primitivity of groups, by M. Dyck.—Determination of arithmetical progressions, whose terms are only known approximately, by M. Lucas.—On a theorem of M. Stieltjes, by M. Cesaro.—On an improvement applicable to the Jonval turbine, by M. Léauté.—On the radiation of silver at the moment of solidification, by M. Violle. The radiation decreases at first, more or less rapidly; then the decrease slackens, and when solidification begins at the border of the vessel, there is a slight increase: till solidification reaches the central part the radiation of the liquid remains constant, then there is slight increase, followed by rapid decrease.—On several optical apparatuses for testing plane surfaces, parallel, perpendicular, and oblique, by M. Laurent.—Very powerful direct-vision spectroscope, by M. Zenger. By adding to the dispersion parallelipiped a light crown glass prism, he gets a dispersion of 150° (A to H); this is surpassed only by M. Thollon's spectroscope, in which the number of sulphide of carbon prisms and the multiple reflections diminish greatly the intensity of the light.—On the upper limit of the perceptibility of sounds, by M. Pauchon. He used a steam-driven syren, also metallic rods of diminishing length fixed at one end, and rubbed. *Inter alia*, an acoustic cornet slightly removed the limit of perceptibility; exciting the rods with various substances (colophany, alcohol, &c.) also changed the limit-length, sometimes to the extent of double. A sound that had become too high for the ear still acted on a sensitive flame.—On a process for obviating boiler explosions, by M. Trèves. He recommends a thermomanometer, and a methodic feeding according to it; also the introduction of a tube for injection of air.—On some experiments made with dynamoelectric machines, by M. Pollard.—Reply to M. Reynier, by M. Trouvé.—Production of crystalline vanadates by the dry way, by M. Ditte.—Action of sulphur on alkaline phosphates, by MM. Filhol and Senderens.—On a combination of phosphoric acid and silica, by MM. Hautefeuille and Margottet. The formula is PhO_5SiO_2.—On various kinds of borotungstates, by M. Klein.—Application of the phenomena of supersaturation to the theory of hardening of some cements and mastics, by M. Le Chatelier.—On chloride of pyrosulphuryl, by M. Konowaloff.—On the difference of reactional aptitude of halogen bodies in mixed halogen ethers; first part, ethylenic compounds, by M. Henry.—On liquid chlorhydrates of turpentine, by M. Bandrimont.—The structure of the ovary and formation of eggs in the Phasmidæ, by M. Roule.—On the organs of flight in insects, by M. Amans. In both theories of flights he considers (M. Marey's and Mr. Pettigrew's) it is overlooked that the base of the wing is formed of two planes, with obtuse angles, so that in the descending stroke the posterior plane presents its concavity to the column of air struck; the resultant, on the two axillæ, raises the bird.—On the trichromatic origin and formation of some cystoliths, by M. Chareyre.—Physiological researches on Champignons, by MM. Bonnier and Mangin. The ratio of oxygen absorbed to carbonic acid emitted does not vary sensibly with the temperature in a given species. Respiration increases very sensibly with the hygrometric state of the air, diminishes in diffused light, is greatest in the more refrangible rays. Transpiration is greater in diffuse light than in darkness.—Scientific exploration in the Straits of Magellan, on Terra-del-Fuego, and on the coast of Patagonia, with the Brazilian corvette *Parnahyba*, by M. Cruls.—The perception of colours and the perception of differences of brightness, by M. Charpentier. The perception of colour is merely the appreciation of the difference of excitation, by certain rays, of the apparatus of luminous sensibility on the one hand, and of that of visual sensibility, or distinction of forms, on the other.—Experimental researches on the physiological effects of cinchonidine, by MM. Sée and Bochefontaine. Its place (with quinine and cinchonine) is among substances which depress the nervous system after momentary stimulation of the circulation.—On the effects of prolonged stay in an atmosphere charged with vapours of creosote, by M. Poincaré. There was hardening of the brain, sclerosis of liver and kidneys, effacement of the pulmonary cavities, &c.—On the circulation of the fingers and derivative circulation of the extremities, by M. Bourceret. In the last phalanx of the fingers there is a special arrangement for rapid return of the blood; it consists of large, very short capillaries, and is merely a modification of the general type. One cannot speak properly of a derivative circulation.—On the attenuation of the virulence of the bacterium of [illegible] by antiseptic substances, by MM. Chamberland and Roux. This was proved with carbolic acid and bichromate of potash.

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THURSDAY, APRIL 26, 1883

SCIENTIFIC WORTHIES

XXI.—WILLIAM SPOTTISWOODE

WILLIAM SPOTTISWOODE, President of the Royal Society, was born in London, Jan. 11, 1825. He belongs to an ancient Scottish family, many members of which have risen to distinction in Scotland and also in the New World.[1] He was first sent to a private (we believe) school at Laleham under Mr. Buckland, brother of Dean Buckland. Here, we read, "the discipline was of a severity unknown at the present day." Thence he was removed to Eton, where however his stay was short. The poet writes, "the child is father of the man"; but science in those days did not hold the place it does now in the scholastic curriculum, and so the future President, venturing to make some researches into the effects produced by the combination of various detonants, came into collision with the "powers that be";[2] the upshot of this *contretemps* was that the brothers Spottiswoode were transferred to Harrow, then under the rule of the present Bishop of Lincoln. His house tutor was Mr. Harris, of the Park.[3] On entrance he was placed in the upper shell, a high form in those days for a newcomer: here he was a very studious, quiet, and thoughtful boy, not much given to athletic games. He remained at Harrow three years, and in 1842 obtained a Lyon Scholarship.[4] In this same year he entered Balliol College, Oxford, and had the present Bishop of Exeter for his mathematical tutor; subsequently, in 1845, the last year of his residence as an undergraduate, he read with the Rev. Bartholomew Price, of Pembroke College. This gentleman writes: "He showed extraordinary liking for, and great skill in, what I may call the morphology of mathematics, such as the theory of simultaneous equations and the results deducible from the *form* of these equations, a department in which he has since shown great ability. He had, I think, greater taste for these branches in their algebraical and geometrical developments than for any other. His power of work was very great and his industry equally so; he read a great deal outside the usual range." In 1845 he took a first class in mathematics, and he afterwards won the Junior (1846) and Senior (1847) University Mathematical Scholarships. He returned to Oxford for a term or two, and gave a course of lectures in Balliol College on Geometry of Three Dimensions—a favourite subject of his. He was Examiner in the Mathematical Schools in 1857-58.[5] On leaving Oxford, he immediately, we believe, took an active part in the working management of the business of the Queen's printers, about this time resigned to him by his father, Andrew Spottiswoode, brother of the Laird of Spottiswoode. The business has largely developed under his hands.

Other subjects than mathematics have occupied his attention: at an early age he studied languages, as well Oriental as European; of his acquaintance with these ample evidence is furnished by his contributions more particularly referred to below.

In 1856 Mr. Spottiswoode made a journey through Eastern Russia; of this he has published a graphic and, in parts, very lively account in his book entitled "A Tarantasse Journey through Eastern Russia in the Autumn of 1856" (Longmans, 1857). "I neither made the journey, nor do I now write, with any political object, but simply as a traveller to whom every square mile of the earth's surface is interesting, and the more so in proportion as it is less known."[1]

In 1860 the brothers Spottiswoode, accompanied by a sister, went through Croatia and Hungary.[2] In 1861 Mr. Spottiswoode married the eldest daughter of the late William Urquhart Arbuthnot, a distinguished member of the Indian Council. His exceptional qualifications as an organiser have not only served to advance his business in the way we have mentioned above, but these same qualifications, together with the broad and liberal education on which they were based, have combined to raise him to his present high position in science. As Treasurer and President he has been continuously on the Council of the Royal Society for a great many years, and through his exceptional gifts as an administrator he has rendered it invaluable services. He has rendered similar services to the British Association, to the London Mathematical Society, and to the Royal Institution.[3] We have permission to make the following extract from a letter written by a friend of many years standing: "In the councils (of the various societies) he has always been distinguished by his sound judgment and his deep sympathy with their purest and highest aims. There never was a trace of partisanship in his action, or of narrowness in his sympathies. On the contrary, every one engaged in thoroughly scientific work has felt that he had a warm supporter in Spottiswoode, on whose opportune aid he might surely count. The same breadth of sympathy and generosity of sentiment has marked also his relations to those more entirely dependent upon him. The workmen in his large establishment all feel that they have in him a true and trustworthy friend. He has always identified himself with their educational and social well-being."[4] We give here a list of some of the offices Mr. Spottiswoode has held, and of the honours that have been bestowed upon him: Treasurer of the British Association from 1861 to 1874, of the Royal Institution from 1865 to 1873, and of the Royal Society from 1871 to 1878. In 1871 he succeeded Dr. Bence

[1] John Spottiswoode, born 1565, Archbishop of St. Andrews, "had few equals, and was excelled by none"; another John (1616) was "a youth of extraordinary parts"; and Sir Robert Spottiswoode, second son of the Archbishop, was "a man of extraordinary parts, learning, and merit." ("Genealogy of the Spotswood Family in Scotland and Virginia," by C. Campbell, Albany, 1868.)

[2] "The feeling and opinion" at Harrow "were that no blame whatever attached to them."

[3] One who knew Mr. Spottiswoode in his earliest days says: "Our numbers at the school were comparatively very small, but I remember well the great ease with which he did all his school work. I knew him well at Oxford, and he several times lent me his horse—a sturdy, Roman-nosed animal of great courage and strength—for a day's hunting. He rode but little himself, and did not read much in an orderly way." He also gives other particulars of interest, which we forbear to give here.

[4] A son of Bishop Colenso also obtained a scholarship in the same year. The mathematical prizes of the present day were not then founded, so that the name of Spottiswoode does not occur among the prizemen of that time.

[5] He also acted as an Examiner in the Civil Service Commission in its first year of operation, and subsequently for the Society of Arts, and also for the Cowper Street Middle-Class Schools.

[1] The hotel accommodation was of the scantiest (p. 23); the description of the vehicles is plea-anter to read than to realise. The only peculiarly personal statement is that the writer was a non-smoker. There are several illustrations by the author, and a route map of Russia.

[2] For a description, see a paper by Mr. G. A. Spottiswoode in Galton's "Vacation Tourist in 1860."

[3] He has, we believe, also rendered valuable services to the Astronomical and Geographical Societies.

[4] This last statement we have corroborated from other sources. "Spottiswoode's people" have "many institutions for healthful recreation as well as mental improvement, such as library, rowing and cricket clubs, a choral society, and a volunteer corps."

Jones as Honorary Secretary to the Royal Institution. *President* of Section A, 1865 ; of the British Association, 1878; of the London Mathematical Society, 1870 to 1872; of the Royal Society, 1879, which office he still holds. Correspondent of the Institut (Académie des Sciences), March 27, 1876. He is also LL.D. of the Universities of Cambridge, Dublin, and Edinburgh, D.C.L. of Oxford, and F.R.A.S., F.R.G.S., F.R S.E. In addition to these honours he has many other literary and scientific distinctions.

Of Mr. Spottiswoode's willingness to communicate from his stores of knowledge many have had frequent proof. We are breaking no faith, we believe, when we mention that it was his wish to purchase the late Prof. De Morgan's valuable library to present it to the Mathematical Society, of which that distinguished mathematician had been the first President.

Few students of the present day are acquainted with Mr. Spottiswoode's earliest work which appeared in the shape of five quarto pamphlets (136 pp. in all) with the title, "Meditationes Analyticæ" (London, 1847). The author's dedication runs thus: "To those who love to wander on the shore till the day when their eyes shall be opened and they shall see clearly the works of God in the unfathomed ocean of truth, these papers are inscribed;" and in his preface he says, "The following papers have been written at various periods, as the subjects presented themselves to notice from time to time. If leisure had been afforded, an attempt would have been made to draw some of them up into a distinct treatise; but it was thought that even in their present form they might interest some of those who take pleasure in the pursuit of mathematical science. Some of the papers are entirely original." The papers are entitled, "Symmetrical Investigations of Formulæ relative to Plane Triangles," "On some Theorems relative to Sections of Surfaces of the Second Order," "On the Reduction of the General Equation of the Second Order," "On the Partial Differential Equations of certain Classes of Surfaces," "On some Theorems relating to the Curvature of Surfaces," "On certain Formulæ for the Transformation of Coordinates," "On the Principle of Virtual Velocities," "On Infinitesimal Analysis," "Examples of the Application of the Infinitesimal Calculus," "On certain Formulæ made use of in Physical Astronomy," "On the Calculus of Variations," "Problems in the Calculus of Variations," and "Note on Lagrange's Condition for Maxima and Minima of Two Variables"—a fair epitome of his subsequent mathematical labours. The treatment calls for no special comment, except that we may note that "in the form of the equations symmetry has been preserved wherever the circumstances of the case would permit."

At a slightly later date (1851) appeared, of a uniform appearance with the "Meditationes," a much more notable pamphlet (63 and viii. pp.), "Elementary Theorems relating to Determinants," of which a writer remarks, "full of interest for the mathematician, but terrible to the unmathematical vision." A second edition of this, rewritten and much enlarged, was published in *Crelle's Journal* (vol. li. 1856, occupying pp. 209–271, 328–381).[1] This was the earliest elementary treatise on a subject which has since risen to such importance, and contains a good sketch of what had previously been done in the same direction. The friend, some of whose words we have already cited, remarks, "that Spottiswoode should have devoted himself at an early period to its cultivation is to me perfectly natural, for the prevailing character of all his mathematical work is *symmetry* (one might generalise still further indeed and say that it, combined with graceful elegance, is the salient feature of all his activity, mathematical, physical, and literary). Bertrand once said of Serret that he was '*un artiste en formules*,' and in a far more general sense one might say that Spottiswoode is the 'incarnation of symmetry.'" To go back to the criticism just now quoted, Mr. Spottiswoode is indeed a leviathan in symbols, and he takes his pastime amongst them: the "gay determinant" is a familiar form nowadays, and "Hamilton's weird delta turned" (the *Nabla* of Clerk Maxwell) is conspicuous on many a page devoted to physics, but in some of the papers we are about to describe there are not only inverted deltas, but Nablas turned to the right and to the left run riot on the pages.

It is since 1870 that Mr. Spottiswoode has more especially divided his attention between physics and mathematics. "His nearest friends," we are informed, "induced him to take up the less abstract one of these two branches of science in order that the general public might have better opportunities of appreciating his abilities. His work in the new field has been of the same character as in the former one. It aims less perhaps at exhaustive treatment than at a study of subtle and beautiful phenomena."

An early consequence of his new study was the publication in 1874, in the NATURE Series, of his "Polarisation of Light." This contains a popular exposition of the subject, and its pages "constitute a talk" with his workpeople "rather than a treatise" on "this beautiful branch of optics."[1]

Before we give a list of the several papers which, of course, do not admit of quotation and passing over, as still within the recollection of most of our readers, the most admirable address delivered before the British Association at Dublin in 1878[2]—though one finds it hard to pass over the many brilliant passages, of more special interest however to the mathematician, who alone can be supposed to care for any other than the ordinary space of three dimensions—we must trespass to the extent of taking the following passage from the earlier address to Section A in 1865. This address, in the words of Prof. Sylvester, is a combined history of the progress of mathematics and physics, and of it Clerk Maxwell said he had endeavoured to follow Mr. Spottiswoode, "as with far-reaching vision he distinguishes the systems of science into which phenomena, our knowledge of which is still in the nebulous stage, are growing."

"A detailed summary of recent progress in pure mathematics would probably prove either interesting to the mathematician or unintelligible to the general hearer;

[1] "On the request of the editor of this *Journal* to reproduce it he (Mr. Spottiswoode) requested permission to revise the work. The subject had, however, been so extensively developed in the interim, that it proved necessary not merely to revise but entirely to rewrite the work. The result is given in the following pages."

[1] He has also contributed a lecture on the same subject to the "Science Lectures at South Kensington" Series.

[2] See NATURE, vol. xviii. pp. 404-415.

with a view to sparing the patience of both, I shall restrict myself to a few general remarks. In both the great branches of mathematics, viz. geometry and algebra, new schools have arisen within the last few years. In its primary aspect the movement has tended to separate the two; geometry has become more purely geometrical in its conceptions and methods, algebra more independent of geometrical considerations. The geometry of to-day is more like the Greek than was that of fifty years ago; and yet at the same time they have not only many principles really in common, but many methods which, although independent, are strictly analogous. Geometry regards its figures, algebra its forms, not as isolated individuals, but as associated with others (concomitants, as they are called) whose properties characterise those of their primitives. The principles of both may be regarded as the same, but dual in their application. Geometry, again, is dual within itself: points and lines may be so viewed that theorems concerning the one give rise to analogies concerning the other; the principle the same, but dual in its manifestation. In this way we seem to be rising to laws which transcend the distinctions between the two parts of geometry—between geometry and algebra.

"Descending a little further into particulars, in another way again we seem to be gaining some steps—but as yet only a few steps—towards a higher scheme both of geometry and algebra. There are a few certain relations so elementary in their conception, yet so universal in their application, that they seem capable of forming the basis of extensive theories: such, for example, in geometry, is that of Anharmonic Ratio—a particular kind of ratio applicable alike to points and rays, to lines and to angles, on which M. Chasles has founded his new and classical work on Conic Sections. Such, again, in algebra, are those of homogeneity and of symmetry, which prove to be not merely improvements in form, but actually new powers for progress in the hands of the mathematician. The calculus of homogeneous forms has marked a new era in the history of algebra; the theory of equations has been transfigured in its light; mechanics, both ordinary and molecular, have been elucidated by it; and the remote applications of the integral calculus have felt its ever-extending influence. Under these, as it were, new fundamental conceptions, whole theories may be co-ordinated, and of these, again, perhaps some coordination may one day be contemplated. As another instance of this generalisation of principles and of this dual aspect of the principles so generalised within almost the present generation, it has been discovered, or at all events been duly realised, that symbols of operation combine according to definite laws, comprising as a particular case those of ordinary number. This fertile idea has, year by year, been receiving fuller developments, till it has at last assumed the form of a complete calculus."

We, too, must join our apologies with those of the learned speaker for lingering so long upon a favourite subject.

The following is as complete a list of Mr. Spottiswoode's papers as we have been able to make:[1] they are grouped, not according to subjects nor in order of time, but as they occur in the several journals in which they originally appeared:—

Phil. Magazine.—(1) On the Equation $Q=q\,(\omega, x, y, z)$ $=w+ix+jy+kz$ (vol. xxxvi. 1850); this is a theorem of considerable importance in the calculus of quaternions, and indeed essential for the application of that method to geometrical and physical problems. (2) On the Quaternion Expressions of Coplanarity and Homoconicism (*ib.*). (3) On the Geometrical Interpretation of Quaternions (vol. xxxvii. 1850), the working out on other lines of results stated in a previous volume by Prof. Donkin. (31) On a Geometrical Theorem (*ib.*, 1850), viz. if three cones of the second order, having a common vortex, intersect one another two and two, the nine lines of intersection (three being selected from each pair of cones) will lie on a cone of the fourth order. (7) On a Problem in Combinational Analysis (vol. iii. 1852) connected with the 15-girl Problem and a more general form of it, the solution of which turns upon certain determinants. (41) On some Experiments on Successive Polarisation of Light made by Sir C. Wheatstone (vol. xli. 1871); the introduction of instrumental means for converting the plane of polarisation of the ordinary apparatus into successive, or, as it is more commonly called, circular polarisation, and the explanation of the phenomena thence arising, constitutes the main purpose of the communication. See also *Proc. of R. Inst.*, vol. vi. 1872. 1875 (*a*) on a Revolving Polariscope; 1882 (*b*) on a Separator and Shunt for Alternate Currents.

Camb. and Dub. Math. Journal.—(4) On certain Geometrical Theorems (vol. vi. 1851). This is an anonymous article which gives simple algebraical demonstrations of certain of Steiner's Theorems in the *Systematische Entwickelung*, and also of some relations given by M. Chasles in his "Aperçu." (9) On Certain Theorems in the Calculus of Operations (vol. viii. 1853); an extension of theorems by Boole (*Phil. Trans.*, 1844), relating to the operation symbol $D=x\frac{d}{dx}$, and by Carmichael relating to the symbol $\nabla = x_1\frac{d}{dx_1} + x_2\frac{d}{dx_2}$ . . . to the cases (1) in which the order of the Variables by which the Symbols of Differentiation are Multiplied is not the same as that of the Variables with respect to which the Differentiations are to be performed; (2) in which the Variables by which the Symbols of Differentiation are Multiplied are any linear Function of the Given Variables; (10) on Certain Geometrical Theorems (*ib.* 1853); two Elementary Theorems in anharmonics proved by aid of determinants. (11) On the Curvature of Curves in Space (vol. ix. 1854); on this M. Chasles ("Rapports," p. 162) remarks: "M. W. Spottiswoode est parvenu à la même expression dans une Note . . . " *i.e.* to the expression—

$$\frac{1}{\rho} = \operatorname{cosec}\phi\left\{\frac{1}{\rho^2}+\frac{1}{\rho_1{}^2}-\frac{2\cos\phi}{\rho\rho_1}\right\}^{\frac{1}{2}}.$$

Quarterly Journal of Mathematics.—(15) Note on Axes of Equilibrium (vol. i. 1857). The axes (Möbius, "Statik") possess the property of allowing the body to be turned about them, the forces retaining their directions in space without a disturbance of equilibrium. The paper is an application of formulæ given by Rodrigues to a proof of the property. (16) On a Theorem in Statics (*ib.* 1857) is a proof of the following, due to Möbius ("Statik"): "If there be any forces in equilibrium, and a series of pyramids be constructed having for one edge a common line, and for their opposite edges the lines which represent the forces, in both magnitude and direction, respectively, the algebraical sum of the volumes of the pyramids will vanish. It is of this M. Chasles ("Rapport," p. 59) writes: "M. W. Spottiswoode, à qui toutes les ressources des nouvelles théories de l'analyse sont si familières, s'est plu à les appliquer à la démonstration de cette proposition (*i.e.* Möbius's) et d'un autre passage du traité de statique de Möbius, sur les axes de l'équilibre." (23) On Petzval's Asymptotic Method of Solving Differential Equations (vol. v. 1862). Also in a somewhat different form in *Brit. Assoc. Report* (part ii.), 1861. (29)

[1] We trust our readers will pardon our imperfect treatment of these papers: we had formed quite a mass of notes—a "rudis indigestaque moles"—but we have had, through circumstances over which we had no control, an utterly inadequate period in which to prune them and shape them into comely form. The prefixed numbers are those of the "Royal Society's Catalogue" and the notes are in most cases derived from the papers themselves. In our haste we have preferred to insert notes to the less familiar papers; the papers read before the Royal and Mathematical Societies are without doubt those by which Mr. Spottiswoode's rank as a mathematician has been determined, but these are just the ones that are most familiar to students.

On Differential Resolvents (vol. vi. 1863). A subject first brought into notice by Mr. J. Cockle, subsequently discussed by Rev. R. Harley. The functions considered are derived from equations in a factorial form; see also *Manch. Phil. Soc. Memoirs*, ii. 1865. [In the *R. S. Catalogue* these are also numbered (33)]. (37) Note on the Contact of Curves (vol. vii. 1866). "In my former paper" (*Phil. Trans.* 1862, see *infra*) "one set of expressions is unsymmetrical with respect to the variables; the other, although symmetrical, involves certain arbitrary quantities which remain to be eliminated by special methods in the course of the developments"—the object of the note is to establish general expressions which are both symmetrical and free from arbitrary quantities. (38) Note on the Resolution of a Ternary Cubic into Linear Factors (*ib.* 1866) is in effect a note on a paper by Mr. J. J. Walker in the previous volume, entitled "On the Resolution of Composite Quantities into Linear Factors."

Crelle's Journal.—(5) Mémoires sur les points singuliers d'une courbe à double courbure (vol. xlii. 1852). (6) Mémoire sur quelques formules relatives aux surfaces du second ordre (*ib.* 1852). (12) Correspondence between Prof. Donkin and Mr. Spottiswoode (vol. xlvii. 1854); extracts from letters (one from each) on a Method for Determining Two Cyclic Sections of a Surface of the Second Order. (14) The Memoir on Determinants (vol li. 1856). (25) Sur quelques formules générales dans le calcul des opérations (vol. lix. 1861), connected with a *Phil. Trans.* paper (17). In this he shows the method by which he obtained the formulæ in (17). (32) Note sur la transformation de la cubique temaire en sa forme canonique (vol. lxiii. 1864).

Tortolini Annali di Scienze.—(8) Sulla trasformazione delle equazioni differenziali lineari dell' ordine secondo (vol. iii. 1852).

R. Soc. Proc.[1]—(13) Researches on the Theory of Invariants (vol. vii. 1854). "The view of invariants here taken has suggested a series of other functions of which invariants form the last term. These functions I propose to call *variants*. With the exact relation between these functions and covariants I am not at present acquainted." (17) On an Extended Form of the Index Symbol in the Calculus of Operations (vol. x. 1859, *Phil. Trans.* 1860). A more detailed form of (9). (20) On the Calculus of Functions (vol. xi. 1861). (21) On Internal and External Division in the Calculus of Symbols (*ib.*). Connected with a paper by Mr. W. H. L. Russell (*Phil. Trans.* 1861), a generalisation and an extension. (30) On the Equations of Rotation of a Solid Body about a Fixed Point (vol. xiii. 1863). In treating the equations of rotation of a solid body about a fixed point it is usual to employ principal axes of the body as the moving system of coordinates. Cases, however, occur in which it is advisable to employ other systems. The object of the paper is to develop the fundamental formulæ of transformation and integration for any system. [This is also given as (34) in the *R. S. Cat.*]. (35) On the Sextactic Points of a Plane Curve (vol. xiv. 1865; *Phil. Trans.* 1865). (40) On the Contact of Conics with Surfaces (vol. xviii. 1870; *Phil. Trans.* 1870). (43) On the Contact of Surfaces (vol. xx. 1872; *Phil. Trans.* 1872). (45) On the Rings Produced by Crystals when Submitted to Circularly Polarised Light (vol. xx. 1872); 1874 (*a*) On Combinations of Colour by Polarised Light; 1874-5 (*b*) On Stratified Discharges through Rarefied Gases; 1875-6 (*c*) On Multiple Contact of Surfaces; (*d*) An Experiment in Electromagnetic Rotation; 1876-7 (*e*) On Stratified Discharges (ii.); Observations with a Revolving Mirror (iii.); (*f*) On a Rapid Contact Breaker and the Phenomena of the Flow; 1877 (*g*) On Hyperjacobian Surfaces and Curves; (*h*) Stratified Discharges (iv.): Stratified and Unstratified Forms of the Jar-Discharge; (*i*) Photographic Image of the Stratified Discharge; 1878 (*j*) Stratified Discharge (v.); Discharge from a Condenser of Large Capacity; 1879 (*k*) On the Sensitive State of Electrical Discharges through Rarefied Gases [with J. F. Moulton], *Phil. Trans.*; 1879-80 (*l*) On some of the Effects Produced by an Induction Coil with a De Meriten's Magneto Electric Machine; (*m*) On the Sensitive State (ii.) (with J. F. M.), *Phil. Trans.*; 1880-1 (*n*) On the 48 Coordinates of a Cubic Curve in Space (*Phil. Trans.*); 1881 (*o*) On Stratified Discharges (vi.), Shadows of Striæ (with J. F. M.); and (*p*) Multiple Radiations from Negative Terminal; 1881-2 (*q*) Note on Mr. Russell's Paper on Definite Integrals; (*r*) Note on Mr. Russell's Paper on Certain Geometrical Theorems; (*s*) On the Movement of Gas in Vacuum Discharges (with J. F. M.).[1]

R. Asiatic Soc. Journal.—(18) Note on the supposed Discovery of the Principle of the Differential Calculus by an Indian Astronomer (vol. xvii. 1860). While not granting that Bhóskarácharya had discovered the principle, "it must be admitted that the penetration shown by him in his analysis is in the highest degree remarkable, and that the formula which he establishes and his method of establishing it bear more than a mere re-emblance—they bear a strong analogy—to the corresponding process in modern astronomy." (28) On the "Súrya Siddhánta" and the Hindoo Method of calculating Eclipses (vol. xx. 1863). It had been suggested that Mr. Spottiswoode should undertake an edition of the above work. For reasons stated, the attempt was not made; but the object of this paper is the translation into modern mathematical language and formulæ of the rules of the work in question.

R. Geog. Soc. Proc.—(19) On Typical Mountain Ranges: an application of the Calculus of Probability to Physical Geography (vol. iv. 1861; *Journal*, vol. xxxi. 1861).

R. Astron. Soc. Memoirs.—(22) On a Method for determining Longitude by Means of Observations on the Moon's greatest Altitude (vol. xxix. 1861; also in *Geog. Soc. Proc.* vol. v. 1861).

British Assoc. Report.—(24) On the Reduction of the Decadic Binary Quantic to its Canonical Form (1861, part 2); (36) Address to Section A (1865); (*a*) Address to the Association (1878).

Phil. Trans.—(26) On the Contact of Curves (1862); (27) On the Calculus of Symbols (1862); 1874 (*a*) On the Contact of Quadrics with other Surfaces. See also above under *R. Soc. Proc.*

Comptes Rendus.—(39) Note sur l'équilibre des forces dans l'espace (vol. lxvi. 1868); (48) Note sur la représentation algébrique des lignes droites dans l'espace (vol. lxxvi. 1873); (49) Sur les plans tangents triples à une surface (vol. lxxvii. 1873); 1874 (*a*) Sur les surfaces osculatrices; 1875 (*b*) Sur la représentation des figures de géométrie à *n* dimensions par les figures corrélatives de géométrie ordinaire; 1876 (*c*) Sur le contact d'une courbe avec un faisceau de courbes doublement infini.

R. Inst. Proc.—(44) On Optical Phenomena produced by Crystals submitted to Circularly Polarised Light (vol. vii. 1872. See also *Phil. Mag.* vol. xliv. 1872); (46) On the Old and New Laboratories at the Royal Institution (vol. vii. 1873); (47) On Spectra of Polarised Light (*ib.* 1873); 1874 (*a*) On Combinations of Colour by Polarised Light; 1878 (*b*) A Nocturne in Black and Yellow; (*c*) Quartz: an old chapter rewritten; 1880 (*d*) Electricity in Transitu; 1882 (*e*) Matter and Magnetoelectric Action.

Musical Society Proc.—1879 (*a*) Lecture on Beats and Combination Tones.

Royal Society.—Presidential Addresses for the Years 1879, 1880, 1881, 1882.

[1] When there is a paper in the *Phil. Trans.* as well, the reference is also given under this head.

[1] For analyses of the papers on "Sensitive Discharges," &c., consult vol. ii. of "A Physical Treatise on Electricity and Magnetism," by J. E. H. Gordon, 1880 (see pp. 47-50, 71-81, 88-111).

L. Math. Soc. Proc.—1866 (*a*) A Problem in Probabilities connected with Parliamentary Elections; 1868 (*b*) Equilibrium of Forces in Space; 1871 (*c*) Question in the Mathematical Theory of Vibrating Strings; 1872 (*d*) On some recent Generalisations in Algebra (Presidential Address); 1874 (*e*) On the Contact of Quadrics with other Surfaces; 1876 (*f*) On Determinants of Alternate Numbers; (*g*) On Curves having Four-point Contact with a Triply-infinite Pencil of Curves; 1879 (*h*) On the Twenty-one Coordinates of a Conic in Space; (*i*) On Clifford's Graphs; 1881 (*j*) On the Polar Planes of Four Quadrics; 1882 (*k*) On Quartic Curves in Space.

"*A MANUAL OF THE INFUSORIA*"

A Manual of the Infusoria; Including a Description of all known Flagellate, Ciliate, and Tentaculiferous Protozoa. By W. Saville Kent, F.L.S., F.Z.S. (London: David Bogue, 1882.)

THE *Philosophical Transactions of the Royal Society of London* for the year 1677 contain the first published account of the minute organisms to which the term "Infusoria" is now very generally applied. The account is by "Mr. Antony van Leeuwenhoek," who, taking up the line of study so successfully pursued by his compatriot, Swammerdam, was the first to apply the microscope to the investigation of the otherwise invisible fauna and flora which teem in inconceivable abundance in the waters of ponds, rivers, and seas, in the infusions of organic substances prepared by man's agency, and in even the minutest drops of moisture which accumulate on the surfaces of natural objects.

Henry Baker (1742), O. F. Müller (1773), and other names are connected with the history of this field of investigation in the period antecedent to Ehrenberg, who in 1836 gave a new aspect to the subject by his descriptions and figures of a great number of forms and of their intimate organisation. The minute creatures at one time spoken of as "animalculæ," and later as "Infusoria," are now known to comprise many very diverse series of organisms—unicellular plants, variously organised unicellular animals, as well as animals of multicellular structure and high organisation, although of minute size. The improvement of the microscope within the last forty years and the studies of a host of observers, among whom are Dujardin (1841), von Siebold (1845), Stein (1854), Claparède and Lachmann (1858), Max Schultze (1860), and more recently of Haeckel, Engelmann, and Bütschli —have gradually resulted in the recognition of a series of minute animals included amongst the "animalculæ" and "Infusoria" of earlier writers, which are characterised by having their living substance in the form of one single nucleated corpuscle or "cell," whilst nevertheless exhibiting a considerable degree of organisation, possessing a mouth into which solid particles of food are taken, pulsating spaces within the protoplasm of the cell, special organs of locomotion, prehension, and protection. These are the mouth-bearing Protozoa, distinguished as such from the other unicellular animals which have not a special orifice for the ingestion of food and constitute the mouthless Protozoa.

It is to these mouth-bearing Protozoa and a few allied mouthless forms that Mr. Saville Kent restricts (as is not unusual) the old term Infusoria. Among them the most numerous and highly organised are the Ciliata; far less abundant and varied are the Tentaculifera (Acinetæ), whilst the Flagellata have, on account of their excessive minuteness, not been properly understood (and were for the most part altogether unknown) until very recently, some important features in their organisation having been first made known by the author of the book which forms the text of this article.

Mr. Kent's "Manual of the Infusoria" consists of two large volumes and an atlas of fifty-one plates. The first volume contains chapters on the history, the organisation, the affinities, and the classification of the Infusoria. Then the three classes, Flagellata, Ciliata, and Tentaculifera, are taken up one by one and systematically divided into orders and families, genera and species—a diagnosis and usually a figure being given of every species. The systematic treatment of the Ciliata and Tentaculifera occupies the second volume. Altogether Mr. Kent describes 900 species of Infusoria, arranged in 300 genera and 80 families. To go over this ground in any case involves a vast amount of labour and perseverance. To do so in the thorough and conscientious manner which distinguishes Mr. Kent's work requires special capacity. Mr. Kent has spared no pains to make his work a trustworthy source of information on all points relating to the group with which it deals; the most comprehensive as well as the smallest and most obscure of recent publications relating to the organisation or to particular species of Infusoria have their contents duly set forth in the proper place in Mr. Kent's work. So far as a frequent reference to these volumes enables one to come to a conclusion, little if anything of importance, whether published in English, French, German, or Italian, has been overlooked by our author. Even the quite recent observations of Foettinger on the parasitic *Benedenia* found in Cephalopoda, and of Joseph Leidy on the parasitic Ciliata occurring in the Termites, are incorporated, as well as the observations of Cunningham on *Protomyxomyces*, little more than a year old.

This is by no means the only merit of Mr. Kent's work. He might have contented himself with recasting the materials to be found in the three great volumes published by Stein, in Claparède and Lachmann, and in Pritchard's "Infusoria" (a valuable book in its day), and have simply incorporated with these the results scattered through the various English and foreign journals and transactions of the past twenty-five years. Mr. Kent has duly done all this, but he has done more, since he has himself made a very careful and prolonged study of a large number of Infusoria. Accordingly we find throughout the present work original observations brought forward for the first time. These include a number of new species and genera, especially among the Flagellate and Tentaculiferous forms. The beautiful cup-forming monads mounted on branching stalks like a colony of Vorticellæ were first brought prominently into notice by that keen observer, the late Prof. James-Clark of Boston, and Mr. Kent has followed up the study of these beautiful forms in a very thorough manner. On the whole, it may be said that the portion of Mr. Kent's work devoted to the Flagellata will have, for those naturalists who have not very closely followed the periodical literature of the subject, the charm of complete novelty, for very many of these forms were completely unknown or misunderstood

till within the last ten years, and have not found their way into general treatises and text-books before the present occasion.

With Mr. Kent's views as to the affinities of the different classes of Infusoria it is not necessary to agree in order to appreciate the value of his work in general. The view is maintained in the "Manual" that the Sponges are genetically related to the Flagellata, whilst an opinion is quoted to the effect that the Ciliata are related to the Turbellaria. Without discussing the grounds for either of these views, we must simply express our entire disagreement with Mr. Kent, who is not (it seems to us) so happy in these speculations as in the more substantial portion of his work. The woodcut on p. 477, vol. ii., comparing the disposition of the ciliated bands of various Infusoria with that of the similar ciliated bands of various larvæ of higher animals, is exceedingly instructive and useful. It serves to point out the close similarity in form and disposition which such ciliated bands may assume in organisms totally unrelated to one another in the genealogical sense. Mr. Kent, however, takes the view, which we think will not be shared by many zoologists, that there is a deeper significance in the occurrence of these similar modifications of similar structures in forms so widely apart as unicellular Protozoa and multicellular Molluscan, Polyzoan, and Echinoderm larvæ; according to Mr. Kent they indicate "affinity," "phylogenetic connection," and "biogenetic relationship." In every direction Mr. Kent detects possible instances of such affinity, which he sets forth in a tabular form on p. 479; but it is not always quite obvious what Mr. Kent means when he speaks of certain Infusoria as "prototypes of" and as "foreshadowing" higher organisms. Had he confined himself to drawing attention to the remarkable parallelism or homoplasy presented by Infusoria on the one hand, and certain higher organisms on the other, we could have appreciated his capacity for detecting structural coincidences. But it appears to be Mr. Kent's opinion that the Holotrichous Ciliata are the forefathers of the Annelida, which are *also* traced by him to the Peritricha. The latter have (according to Mr. Kent) given rise to the Polyzoa, Mollusca, and Echinoderms; the Hypotricha are the ancestors of the Rotifera and of the Arthropoda! whilst the Tentaculifera are the progenitors of the Cœlenterata and the Choano-flagellata of the Sponges.

Mr. Saville Kent is no doubt entitled to hold and to promulgate an opinion on these matters, but we regret, inasmuch as his opinion is a very singular one, that he should have allowed it to take a prominent position in this "Manual."

Upon the question of spontaneous generation Mr. Kent is in accord with the prevalent doctrine, and gives a clear exposition of the history of the discussion of the subject, and so exhibits the importance of the researches carried on by Messrs. Dallinger and Drysdale upon the reproductive process in certain flagellate Infusoria, and the power of the ultra-minute germs of these Flagellata to resist the destructive influence of high temperatures.

With regard to the normal generation of Infusoria Mr. Kent is not so satisfactory. He distinguishes Fission, Gemmation, Sporular Multiplication, and "Genetic" Reproduction—the latter term being, without explanation, applied to sexual reproduction. Our author clearly has not—amongst his numerous and widespread researches upon the Infusoria—devoted any time to a personal investigation of the phenomena of conjugation and rejuvenescence amongst the Ciliata. The account which he gives of the work of Engelmann and Bütschli is meagre in comparison with the space which he has devoted to speculative digressions, and we find no figures illustrative of the exceedingly important results attained by those authors. In view of the great biological interest of the phenomena of conjugation in unicellular organisms generally, this is a serious omission. It is a mistake to have introduced the bygone error of attributing sexual reproduction to the Infusoria into this work at all, as Mr. Kent has done by the heading of his paragraph. Fission, gemmation, and possibly spore-formation preceded at a certain period in the family history by conjugation, constitute all that is *known* to occur in Infusoria. Mr. Kent makes too much of the isolated cases of spore-formation among Ciliata which stand upon good evidence. Admitting them as cases of spore-formation (that is of multiple fission), it is not possible to use them in support of the exploded view as to the production of embryos in the Ciliata by the breaking up of the nucleus after conjugation. Mr. Kent still clings to this notion of a special and peculiar formation of embryos within the parent ciliate Infusorian after, and as the immediate result of, conjugation, but he does not adduce any new fact in support of it. There is no reason adduced by Mr. Kent for regarding the nucleus of Infusoria as anything more than a cell-nucleus, and one is surprised to find that he should express so strong a disagreement with Bütschli as to the fate of the cast-out fragments of the nuclei of conjugated Ciliata, when he does not detail to us any original observations made by him upon the process in question. The cast-out fragments of the nucleus of the conjugated Ciliate possibly have the same significance (Mr. Kent calls it an "unprofitable destiny," p. 98) as the cast-out præseminal apoblasts or "directive corpuscles" of an ordinary egg-cell.

Undoubtedly the best part of Mr. Kent's book, and one which will prove of constant value to that large body of working naturalists who are scattered throughout English-speaking lands, who delight to follow with care and accuracy, by the aid of the microscope, the forms and life-histories of the minute beings first made known by Mr. Antony van Leeuwenhoek, is that which contains the systematic description of every known species of Infusoria.

Accuracy is one of the first requisites in any attempt at scientific work, and Mr. Kent's descriptions and figures will enable numberless good observers in country places and small towns where there are no libraries containing the big books of Stein and Ehrenberg to *accurately* identify the organisms which they observe. By familiarity with Mr. Kent's book such an observer will be able to tell whether he has observed a new species or a new fact about a known species, and he will rise at once from the position of an isolated spectator of the curiosities of microscopic life to that of a possible contributor to the world's knowledge of animal structure, a fellow-worker with all the naturalists of civilised humanity. It is an excellent thing for the cause of science in England, and an excellent thing for other good causes too, that there

are so many unprofessional naturalists in all classes of the community and in all parts of the kingdom. Our dilettanti naturalists not only pursue their favourite study with a devotion and energy which Englishmen always exhibit in regard to a "hobby," but they assist in all quarters in gaining for science true appreciation and popularity. Not merely so, but from their ranks many honoured leaders have sprung. It is chiefly for the service which he has rendered to this class of students that we consider Mr. Saville Kent is entitled to thanks, as was Andrew Pritchard and his editors in a past generation.

In conclusion we may briefly epitomise the classification of the Infusoria followed by Mr. Kent. He regards the Infusoria as a legion or section of the Protozoa or unicellular animals characterised by having appendages which are *not* pseudopodia, lobose, or radiate (Rhizopoda), but are either flagelliform, cilia, or tentaculiform. The character of possessing a distinct mouth or mouths cannot be strictly applied to the whole group, since some few flagellate forms have not even a localised ingestive area. The classes and orders and families recognised in this legion are as follows :—

CLASS I.—FLAGELLATA.

Order 1. TRYPANOSOMATA (Trypanosoma).

Order 2. RHIZOFLAGELLATA (Mastigamœba).

Order 3. RADIOFLAGELLATA.

Family 1. Actinomonadidæ; 2. Euchitonidæ.

Order 4. PANTOSTOMATA.

Family 1. Monadidæ; 2. Pleuromonadidæ; 3. Cercomonadidæ; 4. Codonœcidæ; 5. Dendromonadidæ; 6. Bikœcidæ; 7. Amphimonadidæ; 8. Spongomonadidæ; 9. Heteromitidæ; 10. Trepomonadidæ; 11. Polytomidæ; 12. Pseudosporidæ; 13. Spumellidæ; 14. Trimastigidæ; 15. Tetramitidæ; 16. Hexamitidæ; 17. Lophomonadidæ; 18. Catallactidæ.

Order 5. CHOANOFLAGELLATA or DISCOSTOMATA.

Section I. Gymnozoida.

Family 1. Codonosigidæ; 2. Salpingœcidæ; 3. Phalansteriidæ.

Section II. Sarcocrypta (The Sponges).

Order 6. EUSTOMATA.

Family 1. Paramonadidæ; 2. Astasiadæ; 3. Euglenidæ; 4. Noctilucidæ; 5. Chrysomonadidæ; 6. Zygoselmidæ; 7. Chilomonadidæ; 8. Anisonemidæ; 9. Sphenomonadidæ.

Order 7. CILIOFLAGELLATA.

Family 1. Peridiniidæ; 2. Heteromastigidæ; 3. Mallomonadidæ; 4. Stephanomonadidæ; 5. Trichonemidæ.

CLASS II.—CILIATA.

Order 1. HOLOTRICHA.

Family 1. Paramœcidæ; 2. Prorodontidæ; 3. Trachelophyllidæ; 4. Colepidæ; 5. Euchelyidæ; 6. Trachelocercidæ; 7. Tracheliidæ; 8. Ichthyophthiriidæ; 9. Ophryoglenidæ; 10. Pleuronemidæ; 11. Lembidæ; 12. Trichonymphidæ; 13. Opalinidæ.

Order 2. HETEROTRICHA.

Family 1. Bursariidæ; 2. Spirostomidæ; 3. Stentoridæ; 4. Tintinnodæ; 5. Trichodinopsidæ; 6. Codonellidæ; 7. Calceolidæ.

Order 3. PERITRICHA.

Family 1. Torquatellidæ; 3. Dictyocystidæ; 3. Actinobolidæ; 4. Halteriidæ; 5. Gyrocoridæ; 6. Urceolariidæ; 7. Ophryoscolecidæ; 8. Vorticellidæ.

Order 4. HYPOTRICHA.

Family 1. Litonotidæ; 2. Chlamydodontidæ; 3. Dysteriidæ; 4. Peritromidæ; 5. Oxytrichidæ; 6. Euplotidæ.

CLASS III.—TENTACULIFERA.

Order 1. SUCTORIA.

Family 1. Rhynchetidæ; 2. Acinetidæ; 3. Dendrocometidæ; 4. Dendrosomidæ.

Order 2. ACTINARIA.

Family 1. Ephelotidæ; 2. Ophryodendridæ.

The only feature in the above classification upon which it occurs to us to offer a remark is the limitation assigned to the class Flagellata. Putting aside the author's speciality as to the inclusion of the Sponges in that group, it seems that he has drawn up a very neat and, on the whole, satisfactory classification of the group. But on the one hand exception may be taken to the inclusion amongst the Flagellata of such forms as Mastigamœba and Euchitonia, whilst, on the other hand, those who follow Stein will ask why such forms as Volvox and Chlamydomonas are excluded. Further we cannot accept as satisfactory the subordinate position assigned to Noctiluca, the proboscis of which is no ordinary flagellum, but of so special a character as to entitle its owner to a distinct order or even a class. The fact is that it is excessively difficult to say what monadiform or flagellate unicellular organisms should be associated with forms such as the Choanoflagellata and Eustomata which undoubtedly are rightly placed in one legion with the Ciliata, and what should be left among lower plants, or again in association with the pseudopodic Rhizopods. A flagellate condition in the early stages of development (a "monad form") is common to a vast number of Protozoa and Thallophyta, and the mere flagellate character is not a sufficient basis for the construction of a natural group. Mr. Kent very properly proposes to separate as plants those flagellate forms which do not ingest solid particles of nutriment; but he is no doubt aware of the difficulty of observation in this matter, and of the statement (probably an erroneous one) by Stein, that certain Volvocineæ actually possess a cell-mouth and gullet.

The true limitations of the natural group of the Flagellate Infusoria will probably be ultimately found in the characters afforded by the series of events constituting the life-history of the various flagellate organisms which at first sight may appear to have a claim to be placed in that group.

E. RAY LANKESTER

OUR BOOK SHELF

The Micrographic Dictionary. By J. W. Griffith, M.D., and A. Henfrey. Fourth Edition, Edited by J. W. Griffith, M D. (London: Van Voorst, 1883.)

THE interval of eight years since the publication of the third edition of the "Micrographic Dictionary" has been marked by substantial progress, not only in the microscope itself, but also in our knowledge of the structure of various classes of organisms included in the subjects specially treated. In the former of these two departments the present edition may be regarded as fairly keeping pace with the advance of science; and the introduction forms a very useful treatise on the structure and use of the microscope and of the various appliances

and reagents which the microscopist should have at his command, as well as of the mode of examination of microscopic objects. In the second department the editor has been again assisted by the Rev. M. J. Berkeley in the cryptogamic articles, and by Prof. Rupert Jones in those on Geology and on Foraminifera, as well as by other specialists. To put new wine into old wine-skins is proverbially an unsatisfactory proceeding; and we do not know that it has been more successful here than elsewhere. We are far from saying that the syndicate who have assisted the editor have not contributed much from their vast stores to bring down the work to the date which it now bears on its title-page; but in some of the articles which we have had occasion to consult for work that we have happened to have in hand, the most recent observations are certainly not alluded to, and the system of classification is not the best or newest. But granting these defects, the work is one which no practical microscopist can afford to be without, and which must always lie on his table for ready reference. The present edition is enriched by five new plates, and some new woodcuts.

A. W. B.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Speke and Grant's Zebra

IN or about 1882 a zebra was presented to the French Government by King Menelik of Shoa (which is in lat. 10° N., south-east of Abyssinia). It differed in certain respects from zebras hitherto supposed to have been described, and being regarded therefore as a new species or variety was named by Mr. Milne Edwards *Equus Grevyi*, after the President of the French Republic.

The species of zebra hitherto known were *E. quagga*, *E. Burchelli*, and *E. zebra*. The new one recently received in Paris apparently approximates to *E. zebra*, but the arrangement of the stripes, which are more numerous and more closely set, especially on the haunches, as well as its geographical distribution, seem to give sufficiently distinctive characters to entitle it to rank as a new species or variety.[1]

In a recent communication to the Zoological Society of London, Col. A. Grant, C.B., F.R.S., has called attention to the fact that the late Capt. Speke and he observed, hunted, and shot zebras during their expedition (chiefly in the lake regions of Equatorial Africa) in 1860-63, which from his description are either identical with the zebra from Shoa, or, if not, are entitled to be considered as a new species or variety.

Col. Grant has described the animal both in his notes written during the expedition and also in a paper to the Geographical Society in 1872, of which extracts are subjoined.

Should further examination and comparison show that the zebra described by Grant in 1861, and again in 1872, is identical with the Paris animal, it would seem that priority of discovery, although hitherto unclaimed, is due in title, as it is in fact, to Speke and Grant. If, on the other hand, the animal described by them should turn out to be distinct from any other form yet described, it would appear to have a claim to be named after these discoverers.[2]

The following extracts from Col. Grant's notes and papers addressed to the Geographical Society—written many years ago—appear to substantiate Speke and Grant's claim to the discovery and description of a new variety of zebra:—

Speke and Grant's Expedition of 1860-63, from Journal of Royal Geographical Society of London, 1872

"*Equus zebra* (?), *Native name* '*Phoonda*.'—This animal was frequent in Ugogo, Unyamezi, and north of Uganda. He differs from the *Equus Burchelli* of Regent's Park Gardens in being larger and differently striped. The stripes of our zebra were black upon white (not yellow) ground, and extended to the hoofs, whereas *Burchelli* has broader stripes, yellow ground, and the stripes on the legs are few. However, a sketch of an old mare shot by me shows the same black muzzle and hog-mane as *Burchelli*, and Mr. Blyth says my sketch is of this last species."[1]

From Notes of Expedition

"Oct. 25, 1860.—Zebra shot through chest, shape superb, scarcely any pile, thickly striped over every inch of it, feet, legs, and all; fine hoofs, immense intestines. Flesh had quite the look of prime beef, ears rounded like deer's; a mare. A second zebra brought in by Ruyter. This was at Zungomero.

"Dec. 18.—Halt, in lat. 6° 22′ S., long. 30° 50′ E., altitude 2500 feet to 3329 feet. Zebra spoor again. . . . Shot another zebra.

"Dec. 21.—Again got zebras! . . .

"Dec. 22.—Do. do. . . .

"Jan. 2, 1861.—Eight to ten zebras.

"Jan. 6.—Zebras came among camp donkeys." . . .

Recent Note by Col. Grant, C.B., F.R.S.

"When we were shooting these zebras in Africa, we thought we were shooting the zebra which is common to Africa; but after our return, on my seeing Burchell's at the Zoological Gardens, I felt convinced we had never seen a Burchell's zebra; I said so to Blyth, who looked at my sketch, but who never saw Speke's specimens, and he seems to have called our zebras *Burchelli*. As soon as I saw Speke's specimens in 1873, and on hearing Prof. Flower describe by drawings the various zebras, I brought forward the matter, and got Speke's specimens up from Speke's brother. My journal notices *the stripes to be an inch apart all over the body, and extended to the hoofs;* but it says nothing of the marks on the haunches, though I believe that in our zebras, as well as in all other species, the haunch stripes are farther than an inch apart."

"The twelve zebras which were shot by the Speke and Grant expedition in 1861-3, were found at the undermentioned places in Africa:—

Places.	Lat.	Long.	Alt. above Sea. Feet.
Zungomero ...	7 27 S.	37 36 E.	516
Jiwe la M'koa	6 0 S.	34 0 E.	4690
Rubuga ...	5 0 S.	33 0 E.	3402
Usui District ...	2 49 S.	32 0 E.	About 4000
Uganda District	0 52 N.	32 30 E.	About 4000

"The zebras pasture in the forest and also in open country which is covered with bushy jungle, or where granite crops up, as this bears the richest grass, whilst hills with running water are always within their access."

However the zebras in question may be named, it seems right that the facts connected with Speke and Grant's discovery should be known.

J. FAYRER

April 17

Leaves and their Environment

I AM taking steps to have some analyses instituted, by a highly qualified authority, of the atmosphere (or water) in the natural environment of certain typical plants, in order, if possible, to produce experimental evidence upon the points impugned by Prof. Thiselton Dyer. The results of such (necessarily very inconclusive) evidence I shall publish in NATURE, if the Editor will grant me space, whether they are favourable or otherwise to my own allegations.

Meanwhile, as Prof. Dyer has himself relied upon purely *a priori* considerations, may I urge (1) that in the papers themselves I did not overlook the other factors of the problem to which he alludes; (2) that in woods, hedgerows, and thickets, the air is generally very still; (3) that the layer of air from time to time in actual contact with the surface of plants must always be in course of being deprived of its carbonic acid; and (4) that

[1] It should be added that *E. Burchelli* is the only zebra known to occur *north* of the equator, and that *E. zebra* has not been seen for many years. (Refer to figure in *Proc. Zool. Soc.* 1882, iv. p. 721, published April 1, 1883.)

[2] It is possible that it may be a local variety of *E. zebra*, hitherto found much further south.

[1] Evidently Blyth was mistaken, as the zebra was thickly striped on the legs, which is not the case with *E. Burchelli*.

wherever plants get free access to the open air above, it was one of my own assertions that they must necessarily obtain carbonic acid in abundance. It seems to me difficult to understand how in a still place, where many plants at once are engaged in deoxidising a compound which only normally forms 0·03 per cent. of the atmosphere, there can always be as much of it left as any of them can possibly want. I do not presume to argue with Prof. Dyer upon the subject; but as far as my own comprehension goes, he has not made this point clear to me.

May I venture also to suggest that perhaps another danger surrounds biology, and especially botany—the danger of becoming too technical and too academic? Now that perfect instruments, immense collections, and a long technical training are necessary in order to do anything in biology by the regular road, does not the science run just a little risk of falling into a groove? And is it not well from this point of view that there should be an outside body of amateurs, who will take occasionally a fresh non professional view of the subject, handling their own problems in their own way, and publishing their own little guesses or glimpses for what they may be worth? No doubt they will often go demonstrably wrong; no doubt the masters of the science will usually find numerous blunders of detail in their work, and may often see reason to disagree with them altogether; and in that case the amateurs ought to receive their corrections with all humility; but is it not a healthy thing after all that the amateurs should do their best, and try to follow out their own lights to their own conclusions? GRANT ALLEN

Forms of Leaves

YOU have recently inserted several letters from Mr. Grant Allen on the forms of leaves, a question in which I have myself been working lately. Mr. Grant Allen's letters open up a number of interesting questions, but for the moment I will only refer to his suggestion with reference to the reason why water plants so often have their leaves cut up into fine filaments. He tells us that this is because the proportion of carbonic acid held in solution by water is very small, and that therefore for this amount there is a great competition among the various aquatic plants.

The question has already been asked on what grounds Mr. Allen makes this statement with reference to the proportionate amount of carbonic acid. Without entering on this point, I would, however, venture to suggest that the reason for this tendency in the leaves of water plants is mechanical rather than che[illegible]ical.

It is, of course, important for all leaves to present a large surface for the purposes of absorption with as little expenditure of material for purposes of support as possible. Now delicate filaments such as those of water plants present a very large area of surface in proportion to their mass. On the other hand, they are unsuited to terrestrial plants, because they are deficient in strength and unable to support themselves in air. Take, for instance, a handful of the submerged leaves of an aquatic ranunculus out of the water, and, as every one knows, the filaments collapse. This seems to me the real reason why this form of leaves is an advantage to water plants. It is perhaps for the same reason that low-growing herbs, which are thus protected from the wind so often have much divided leaves.

April JOHN LUBBOCK

The Föhn

MAY I be allowed the space of a few lines to point out a defect in the account of the Föhn, given by Mr. Scott in his recent work on "Meteorology," and quoted in the review of that work which appeared in NATURE, vol. xxvii. p. 575. This phenomenon has been fully and clearly explained by Dr. J. Hann in a paper entitled "Einführung in die Meteorologie der Alpen," published under the auspices of the *D. und O. Alpenverein*. Mr. Scott's account of the Föhn attributes rightly the dryness and the cooling of the wind at high altitudes to expansion; but he appears to entirely overlook the *heating effect due to condensation of moisture* during the ascent of the wind.

From observations made in Switzerland, where the Föhn is chiefly felt, Hann has established the following rule: the Föhn is as many half degrees C. warmer at any place in its descent, than it is at an equal altitude during its previous ascent on the other side, as the place is hundreds of metres below the mountain ridge. This he explains by the fact that compression during the descent of the Föhn reverses the loss of temperature due to rarefaction during its previous ascent; while the wind brings with it over the mountain ridge the heat gained by the liberation of latent heat in the condensation of moisture. This latter amounts at 15° C. to about half a degree C. for each ascent of 100 metres for saturated air. "Therein," says Hann, "lies the explanation of the heat of the Föhn." A. IRVING

Wellington College, Berks, April 21

The Zodiacal Light (?)

THE same "peculiar appearance in the western sky" as that described by your correspondent, "J. W. B.," was observed here by me on the same evening, April 6. At 7h. 0m. G.M.T., or fifteen minutes after sunset, I noticed a bright, golden-coloured column of light, well defined, about 4° in length and slightly more than 1° in width, and inclined towards the south. "J. W. B." says it "rose vertically from near the horizon" at his station, Bath. Here it was decidedly inclined to an angle of about 15° towards the south. At 7h. 20m. no traces of it were visible. I have not seen any similar appearance since.

W. H. ROBINSON

N.B.—In the observer's book this observation is entered as "Bright zodiacal light (?), seen at 7h. 0m." E. J. STONE

Radcliffe Observatory, Oxford, April 20

REFERRING to the letter of your correspondent, "J. W. B.," Bath, in your last issue (p. 580), allow me to say that this peculiar ray of brilliant light was seen here by myself and many other people at about 6.40 p.m. on Friday, April 6. The sunset was brilliant and cloudless, but from the horizon to about 25° in height immediately above the spot where the sun had disappeared there appeared a ray of light of great beauty and extreme brilliancy; its centre, a delicate rose colour, graduating to the edges into the purest gold. This single ray was perpendicular, and appeared to be little, if at all affected, in its brilliancy by the approaching dusk of evening, but continued to exhibit itself with little-diminished brilliancy for nearly half an hour, finally disappearing with the twilight.

ROBERT DWARRIS GIBNEY

Glan-y-dwr, Crickhowell, South Wales, April 21

WHAT your correspondent, "J. W. B.," saw after sunset was not the zodiacal light, which is easily distinguishable by its great extent of area, lenticular shape, and invisibility during strong twilight, but it may be not incorrectly termed a sun column. I find the following entry in an old journal, of a similar appearance:—"1868, April 17.—Sun column, continuing half an hour after sunset, which was perfectly bright, without clouds." Perhaps some of your readers may be able to explain the cause of it. E. BROWN

Further Barton, Cirencester, April 21

THE phenomenon observed on the evening of Friday, the 6th inst., in Bath, by your correspondent J. W. B. (vol. xxvii. p. 580) was seen at Dolgelly by the writer when on a tour through Wales. On his pointing it out to a companion and some of the townsfolk, all agreed it was quite unique in their experience.

A bright, slender pillar of light, hazy toward the edges, rose majestically from the western horizon, in a cloudless sky, and so continued for about three-quarters of an hour after the sun had set. To one long habituated in meteorological observation it was of a character differing *toto cælo* from the path of sunbeams through a cloud-rift, which is invariably divergent in appearance, as if from a focus. The "pillar" was uniform in width, perfectly vertical, and straight, the centre line alone brilliant. The height was, however, greater than your correspondent indicates.

Having fortunately with me a pocket-compass, with plumb-bob for "dip" measurements, I determined (1) the light-pillar was exactly vertical; (2) the height, which scarcely varied during visibility, was 20°, dying out faintly at that elevation; (3) the azimuth 25° north of west. By terrestrial bearings there was an appearance or a slight movement northward, but smallness of the compass dial (1 inch diameter) precluded any reliable angular determination of azimuthal change.

Further, the evening was very cold, and a continuous easterly wind had during the day obscured the hills, which still showed many unmelted snowdrifts upon their summits and flanks. First observed at 7 p.m., the strange appearance faded out at 7.30 p.m.

From the verticality, linear form, and condition of atmosphere I was led to remark at the time to my companion that the phenomenon appeared more of the nature of parhelia than referable to the zodiacal light. An intensely cold easterly wind encountering ocean-warmed airs to the westward would not improbably lead to the ice-molecule condition of atmosphere now assumed to be associated with the occurrence of parhelia.

It may be added (though of little probable significance) that the time corresponded roughly with the time of high water along that coast.

D. J. ROWAN

Kingstown, April 24

On the Value of the "Neoarctic" as one of the Primary Zoological Regions

PERMIT me to make a few remarks relative to Mr. Wallace's criticisms (NATURE, vol. xxvii. p. 482) of my paper on "The Value of the Neoarctic as one of the Primary Zoological Regions." Briefly stated it is maintained in the early portion of this paper (1) that the Neoarctic[1] and Palæarctic faunas taken individually exhibit, in comparison with the other regional faunas (at least the Neotropical, Ethiopian, and Australian), a marked absence of *positive* distinguishing characters, a deficiency which in the mammalia extends to families, genera, and species, and one which, in the case of the Neoarctic region, also equally (or nearly so) distinguishes the reptilian and amphibian faunas; (2) that this deficiency is principally due to the circumstance that many groups of animals which would otherwise be peculiar to, or very characteristic of, one or other of the regions, are prevented from being such by reason of their being held in common by the two regions; and (3) that the Neoarctic and Palæarctic faunas taken collectively are more clearly defined from any or all of the other faunas than either the Neoarctic or Palæarctic taken individually.

In reference to these points Mr. Wallace, while not denying the facts, remarks: "The best division of the earth into zoological regions is a question not to be settled by looking at it from one point of view alone; and Prof. Heilprin entirely omits two considerations—peculiarity due to the absence of widespread groups and geographical individuality." Numerous families and genera from the classes of mammals and birds are then cited as being entirely wanting in the western hemisphere, and which—in many cases almost sufficient to "characterise the Old World as compared with the New"—"must surely be allowed to have great weight in determining this question." No one can deny that the absence from a given region of certain widespread groups of animals is a factor of very considerable importance in determining the zoological relationship of that region, and one that is not likely to be overlooked by any fair-minded investigator of the subject. But the value of this *negative* character afforded by the absence of certain animal groups as distinguishing a given fauna, is in great measure proportional to the extent of the positive character—that furnished by the presence of peculiar groups—and indeed may be said to be entirely dependent on it. No region can be said to be satisfactorily distinguished from another without its possessing both positive and negative distinguishing characters. Mr. Wallace has in his several publications laid considerable stress upon the negative features of the Neoarctic fauna as separating it from the Palæarctic or from any other, but he has not, it appears to me, sufficiently emphasised the great lack, *when compared to the other faunas*, of the positive element, the consideration of which is the point aimed at in the first portion of my paper, and which has led to the conclusions already stated, that only by uniting the Neoarctic and Palæarctic regions do we produce a collective fauna which is broadly distinguished by both positive and negative characters from that of any other region. If, as Mr. Wallace seems to argue, the absence from North America of the "families of hedgehogs, swine, and dormice, and of the genera *Meles*, *Equus*, *Bos*, *Gazella*, *Mus*, *Cricetus*, *Meriones*, *Dipus*, and *Hystrix*" be sufficient, as far as the mammalian fauna is concerned, to separate that region from the Palæarctic, could not on nearly equally strong grounds a separation be effected in the Palæarctic region itself? Thus, if were to consider the western division of the Palæarctic region, or what corresponds to the continent of Europe of geographers, as constituting an independent region of its own, it would be distinguished from the remainder of what now belongs to the Palæarctic region by negative characters probably fully as important as those indicated by Mr. Wallace as separating the Neoarctic from the Palæarctic region. The European mammalian fauna would be wholly deficient, or nearly so, in the genera *Equus*, *Moschus*, *Camelus*, *Poephagus*, *Gazella*, *Oryx*, *Addox*, *Saiga*, *Ovis*, *Lagomys*, *Tamias*, in several of the larger *Felidæ*, as the tiger and leopard, and in a host of other forms. A similar selection could be made from the class of birds (among the most striking of these the *Phasianidæ* and *Struthionidæ*), but it is scarcely necessary in this place to enter upon an enumeration of characteristic forms. Divisions of this kind, to be characterised principally or largely by negative faunal features, could be effected in all the regions, and in some instances with probably more reason than in the case under discussion.

But the question suggests itself, What amount of characters, whether positive or negative, or both, is sufficient to distinguish one regional fauna from another? Mr. Wallace states: "There runs through Prof. Heilprin's paper a tacit assumption that there should be an equivalence, if not an absolute equality, in the zoological characteristics and peculiarities of all the regions." Is it to be inferred from this quotation that Mr. Wallace recognises no such general equivalence? Is a region holding in its fauna, say, from 15 to 20 per cent. of peculiar or highly characteristic forms to be considered equivalent in value to one where the faunal peculiarity amounts to 60 to 80 per cent? If there be no equivalence of any kind required, why not give to many of the subregions, as now recognised, the full value of region?

Surely, on this method of looking at the question, a province could readily be raised to the rank of a full region. In the matter of geographical individuality little need be said, as the circumstance, whether it be or be not so, that the "temperate and cold parts of the globe are necessarily less marked by highly peculiar groups than the tropical areas, because they have been recently subjected to great extremes of climate," does not affect the present issue, seeing that the peculiarity is greatly increased by uniting the two regions in question; nor does it directly affect the question of the Neoarctic-Palæarctic relationship.

The second part of my paper deals with the examination of the reptilian and amphibian faunas, and the general conclusion arrived at is: "that by the community of its mammalian, batrachian, and reptilian characters, the Neoarctic fauna (excluding therefrom the local faunas of the Sonoran and Lower California subregions, which are Neotropical) is shown to be of a distinctively Old World type, and to be indissolubly linked to the Palæarctic (of which it forms only a lateral extension)." Towards this conclusion, which, it is claimed, is also borne out by the land and freshwater mollusca and the butterflies among insects, I am now happy to add the further testimony of Mr. Wallace (overlooked when preparing my article respecting the *Coleoptera* ("Distribution," "Encycl. Britann." 9th ed. vii. p. 274).

As regards the name "Triarctic," by which I intended to designate the combined Neoarctic and Palæarctic regions, and which may or may not be "somewhat awkward," I beg to state that, at the suggestion of Prof. Alfred Newton (who, as he informs me, has arrived from a study of the bird faunas at conclusions approximately identical with my own), it has been replaced by "Holarctic." In conclusion, I would say that, while the views enunciated in my paper may not meet with general acceptance at the hands of naturalists, it is to be hoped that they will not be rejected because they may "open up questions as regards the remaining regions which it will not be easy to set at rest."

ANGELO HEILPRIN

Academy of Natural Sciences, Philadelphia, April 6

[1] In the paper under consideration I have given what appear to me satisfactory reasons for detaching certain portions of the South-western United States from the Neoarctic (my Triarctic), and uniting them with the Neotropical region.

Mock Moons

A LITTLE before midnight on Monday, the 16th inst., the moon, being nine days old and about 30° above the western horizon, was surrounded by an unusual halo. Its radius was certainly more than the normal 22°. By careful estimation I judged it to be about 30°, the lower edge resting on the horizon. On the right and left limbs of the ring were very distinct bright patches, rather broader than the ring itself, and slightly elongated outwards. The right-hand patch appeared to be in its normal position on a line passing through the moon, parallel with the horizon, but the left-hand patch was distinctly elevated

above this line, and seemed to be unaccountably out of place. As, however, the moon was little past the first quarter, and the terminator nearly a straight line, and only slightly inclined from the vertical, a line drawn perpendicular to it would have passed through the left-hand patch, and I imagine that its position was due to this inequality in shape of the two sides of the visible moon. The atmosphere was hazy, the moon though clearly visible appearing as in a slight fog. No colours were distinguishable at any part of the halo. F. T. MOTT

Birstal Hill, Leicester, April 17

Benevolence in Animals

Two or three years ago Dr. Allen Thomson gave me an instance of benevolence in a cat which is so closely similar to one communicated to you by Mr. Oswald Fitch that for the sake of corroboration I may state it.

The cat belonged to Dr. Thomson, and one day came into the kitchen, pulled the cook by the dress, and otherwise made signs showing a persistent desire to attract attention. Eventually the cat led the cook out of doors and showed her a famishing stranger cat. The cook thereupon gave the stranger some food, and while this was being discussed, Dr. Thomson's cat paraded round and round her companion, purring loudly with a satisfied sense of well-doing. GEORGE J. ROMANES

"Medioscribed Circle"

IN this week's NATURE (p. 595) the use of the *medioscribed* circle is suggested in place of the well-known "nine-point" circle. If a change is desirable, would not "mid-point" circle be equally expressive? R. T.

April 20

AGRICULTURE IN MADRAS[1]

THE Government establishment at Saidápet has now been in existence about twelve years. It consists in part of an experimental farm, and in part of an educational establishment, in which, at the date of the last report, forty-one native students were receiving instruction in the science and practice of agriculture. The whole is under the superintendence of Mr. W. R. Robertson. The object in view is to improve the condition of agriculture in the Presidency. This is indeed urgently needed. With a large and increasing population, the soil is in general wretchedly cultivated, and reduced to a low state of fertility. The farm at Saidápet is the centre of many useful agencies. Here new crops, new breeds of cattle, and improved implements are carefully tried. Here the teaching of European science is reduced to practice, and methods of cultivation suitable to the conditions of Indian agriculture are perfected. While by means of the educational department, by tours in the country, distributions of seed, ploughing competitions with different implements, and various other agencies, the endeavour is made to bring these improved methods into use by the native farmers.

The meteorological records kept at the farm exhibit in a striking manner the difficulties under which Indian agriculture must be pursued. Thus in the season 1880-81 the rainfall in September was 10·9, in October 10·7, and in November 19·6 inches, while during the whole six months from January to June only 2·35 inches were recorded. Long-continued heat and drought are thus followed, on the arrival of the monsoon, by a deluge of rain. It is pleasing to notice that the director of the farm is quite abreast of the latest scientific teaching respecting the best mode of meeting the difficulty in question. It is plain that in the rainy season the land will be washed clear of all soluble plant-food; all nitrates formed in the soil during the hot season will thus be lost, unless they have been already assimilated by a crop. Mr. Robertson recommends that, whenever possible, advantage should be taken of the first commencement of rain in June or July to sow the land with a green leguminous crop (horse-gram). In most years there will be enough rain to maintain such a crop in growth during the summer months. This crop will collect and assimilate a great part of the nitrates in the soil. At the commencement of the wet season the green crop is to be ploughed into the soil, and forms an excellent manure for the principal crop of the year, which is then sown. Mr. Robertson refers apparently to the experiments at Rothamsted when speaking of the quantity of nitrates annually formed in a soil; the amount he mentions (40 lbs. of nitrates[2] per acre) is, however, far below the truth. The quantity of nitrates found in five successive years in the drainage water from uncropped and unmanured land at Rothamsted amounts, indeed, on an average, to nearly 3 cwts. of Indian saltpetre per acre per annum.

In India agriculture depends much for its success and permanence on irrigation, and vast sums have been, and will be, expended on irrigation works. Here again, however, the question of the presence or absence of nitrates is an important factor, which has been almost entirely overlooked, engineering rather than chemical skill having been employed in the direction of the work. It should always be borne in mind that a water containing nitrates supplies not only water but *manure*. The native farmers are generally quite aware of the difference in value of different water-supplies, and reckon the water from the village well as worth far more than that procured from the Government canal. To the engineer it appears a ridiculous waste of power to lift water from a well when a water-supply is available at the level of the land. But the native is right; his well-water is rich in nitrates, and for the farmer's purpose far more valuable than the purer water of surface drainage found in rivers and canals. It should always be borne in mind in plans for irrigation, that the drainage from arable land, and from inhabited districts will always yield the best irrigation water. By restoring to land in time of drought the plant-food lost in time of flood we are pursuing a truly scientific economy.

R. W.

[1] Annual Reports on Government Agricultural Operations in the Madras Presidency, 1880-81 and 1881-82.

ANTHROPOLOGICAL NOTES IN THE SOLOMON ISLANDS

WITHOUT going into the general question as to the position which these islanders hold to the other Pacific races, I will briefly state the results of numerous measurements and observations which I made during my visit to these islands in 1882. As the surveying work of this ship was confined for the most part to the large island of St. Christoval and the adjacent small islands, my remarks will refer more particularly to the natives of this part of the group.

The average height of a man of St. Christoval is about 5 feet 3 inches or 5 feet 4 inches, whilst the span of the arms generally exceeds the length of the body by from 4 to 5 inches. Both men and women are usually of a good physique, robust and well-proportioned; but one may find in the same village community weak, puny, thin-limbed individuals associated with others of a strong muscular frame, with well-rounded limbs and a good carriage.

The colour of the skin varies considerably in shade from a very dark brown, approaching black, to a dark copper hue. The elderly adults are as a rule more dark-skinned than those of younger years, the difference in shade being attributable partly to a longer exposure by reason of their age to the influence of sun and weather, and partly to those structural changes in the skin which accompany advancing years. Not unfrequently, amongst a group of dark-skinned natives a man may be observed whose skin is of a pale sickly hue, and who at the first glance may be thought to afford an example of recent

[2] Possibly in Mr. Robertson's Report "nitrates" is here a misprint for "nitrogen."

intermingling between the Melanesian and Polynesian races. On a closer examination I always found that such men were covered from head to foot with an inveterate form of body ringworm—a scaly skin-eruption, which affects in a greater or less degree quite two-fifths of the natives of this part of the group—and that in all their other physical characters they belonged to the Melanesian type. In its most aggravated and chronic condition this parasitical disease implicates the skin to such a degree that the rapid desiccation and desquamation of the epidermal cells lead to a partial decoloration of the deeper parts of the cuticle, as though the rate of the production of pigment was less rapid than the rate of its removal in the desquamative process.

The hair of the head is generally black, frizzly, and bushy; more particularly amongst the younger adults of both sexes this last character prevails. Amongst middle-aged men I have sometimes observed that the air arranges itself into entangled corkscrew-like spirals,



FIG. 1.—Native of Santa Anna (an island off the east extremity of St. Christoval). The round disk of wood in the lobe of the ear should be quite white, the dark spots being due to the imperfections of the dry plate. The faint linear markings on the cheek due to a form of tattoo are rarely well marked.

the whole head of hair having much the appearance of a mop placed erect on its handle. Now and then, though rarely, the hair shows a tendency to become straight; I met with one such native near Cape Keibeck, on the north coast of St. Christoval; and I am informed that straight-haired varieties do exist among the hill-tribes in the interior of the island. With regard to the amount of hair on the face, limbs, and body, great diversity is observed amongst natives of the same village. Epilation is commonly employed, but there can be no doubt that the development of hair varies quite independently of such a custom. Out of ten men taken promiscuously from any village, perhaps five would have smooth faces, three would possess a small growth of hair on the chin and upper lip, the ninth would wear a beard, a moustache, and whiskers of moderate growth, whilst the tenth would present a shaggy beard and a hairy visage. The surfaces of the body and limbs are as a rule comparatively free from hair; but hairy men are to be met with in most villages; and on one occasion when in the vicinity of Cape Surville—the eastern extremity of St. Christoval—I visited a village where the proportion of hairy-bodied, hairy-visaged men was in excess of the smooth-skinned element.

From my measurements the form of the skull would appear to be mesocephalic: the cephalic indices ranged between 73 and 82—the greater number of the [illegible]cing included between 74 and 77. The facial angle varied in amount between 85° and 90°. The nose is generally straight, coarse, and somewhat short, the nostrils wide, and the bridge depressed in some instances. Not uncommonly the nose is arched or aquiline; out of fifty natives amongst whom I took especial notice of this feature, I found that ten possessed an aquiline nose. The countenances of the younger of both sexes are often prepossessing, and amongst the adults I have frequently met with men of some intellectual expression.

Such are some of the leading physical characters of the natives of this part of the Solomon group. To the inhabitants of the small island of Santa Anna, which lies off the east extremity of St. Christoval, the same description will apply; but we find in the still smaller adjacent island of Santa Catalina a subvariety of the Melanesian type characterised by a lighter colour and probably a greater height, although I made no measurements there. The few natives which I saw belonging to the large island of Malayta, which we did not visit, resembled in appearance those of St. Christoval; and from a few measurements and observations which I made in the Florida subgroup, where the St. Christoval type prevails, it was evident that thus far to the westward the same description of a native of the Solomon Islands was equally applicable. The large neighbouring island of Guadalcanar I had no opportunity of visiting. In the small island of Simbo, further to the west, I found no important difference in the physical characters of the natives except perhaps a rather darker shade of colour. Proceeding westward as far as Treasury Island, our furthest point in that direction, we for the first time came upon a distinct variation in the type of native—a difference which has been a subject for remark even by such usually unobservant people as the masters of trading ships. In their greater height and in the almost black colour of the skin, the natives of Treasury Island are at once distinguished from the prevailing native type to the eastward. Their features are more finely cut, and the form of the skull, as shown by the cephalic indices, is more brachycephalic—the range of seven measurements being 78 to 84, and the mean cephalic index 81. In some individuals the cheekbones were prominent and the foreheads retreating. As a race the Treasury Islanders are said to evince a fiercer disposition than do the natives in the eastern islands of the Solomon group. The natives of the large adjacent island of Bougainville have the reputation of being amongst the most daring and warlike of the inhabitants of this archipelago; and probably the examination of their physical characters will exhibit them as a more pronounced type of the Treasury Islanders.

H. B. GUPPY

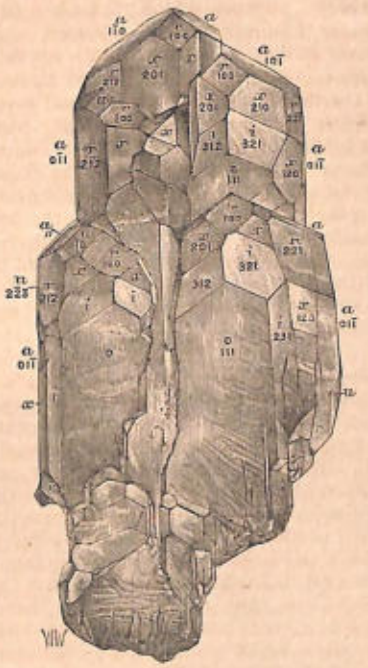
H.M.S. *Lark*, Auckland, N.Z., February 27

ON A FINE SPECIMEN OF APATITE FROM TYROL, LATELY IN THE POSSESSION OF MR. SAMUEL HENSON

THE specimen of apatite represented in the diagram was submitted to my inspection by Mr. Henson last November, and is the most beautiful specimen of this mineral which I have seen. The faces observed were not, however, determined on the specimen itself, but from a plaster cast and a smaller specimen with which Mr. Henson supplied me. From these latter approximate measurements of some of the more prominent angles were obtained by means of a contact-goniometer, which,

on comparison with the table of angles given in Miller's "Mineralogy," rendered the identification of the more conspicuous planes easy. The remaining planes were then easily determined from the relation which connects three planes lying in a zone. The forms present are : $o\{111\}$, $a\{10\bar{1}\}$, $r_1\{100\}$, $r\{22\bar{1}\}$, $x\{210\}$, $i\{321\}$, $u\pi\{4\bar{1}0\}$, $u_1\pi\{2\bar{2}3\}$.

I had no intention of describing the specimen at the time it was shown me, and did not pay enough attention to the physical characters of the faces to be able to recall them. The specimen was for the most part remarkably limpid, with a pale mauve tint in its purest portions. It was in part penetrated by fine delicate needles of epidote,



ao	90°	0′
io	157	05
xo	139	47
xa	130	13
ro	124	20
aa′	120	0
ra	135	39
rr′	131	14

as is shown in the very excellent diagram attached, which gives a very clear and accurate idea of the specimen. A remarkably large crystal from the same locality has recently been added to the mineral collection of the Natural History Museum, Cromwell Road. It is of a much deeper mauve colour than Mr. Henson's specimen; it shows the same general forms, the planes *o* and *a* are bright and even, but the small planes, *r*, *x*, *i*, are somewhat rough. These same characters are also those observed on the faces of such smaller specimens as I have examined.

W. J. L.

THE EVOLUTION OF THE AMERICAN TROTTING-HORSE.[1]

THE American trotting-horse is an example of a new breed of animals in process of formation. As yet it can hardly be called a definite breed in which the special and distinctive character is either fully developed in quality or satisfactorily fixed by heredity. Great progress has, however, been made, many individual animals have attained great speed, and all the better ones have derived their trotting excellence in part, at least, through heredity.

The origin of most breeds is involved in considerable obscurity, as to how much they are due to conscious and how much to unconscious selection, what motives led to this selection, how far the enhancement of the special qualities have been due to physical environment, and how far to education, training, nourishment, or cultivation. The formation of this new breed is so recent, the development of a special quality has been so marked, there is such an abundant literature pertaining to its history, the system of sporting "records" is so carefully planned and comprehensively conducted, and withal has become so extensive, that we have the data for a reasonably accurate determination of the influences at work which led to this new breed being made, the materials of which it is made, and the rate of progress of the special evolution.

It is as an implement of gambling and sport that the trotter has his chief value to the biological student. Sporting events are published or recorded as the mere everyday use of animals is not, and the records of races give numerical data by which to measure the rate of progress. Similar data do not exist for the study of the evolution of any other breed.

Incidental to the preparation of a paper pertaining to this matter for farmers and breeders, I have compiled and collated certain data which have a scientific as well as economic value, the more interesting portion of which I condense for this paper.

The horse has several gaits which he uses naturally, that is, instinctively. And besides those which are natural, he has been taught several artificial ones, some of which have been much used, particularly in the middle ages. But to trot fast was not natural to horses; when urged to speed they never assumed it, and until within a century the gait was neither cultivated nor wanted by any class of horsemen. A breed of fast trotters, had it been miraculously created, would doubtless soon have perished in that it would have had no use, satisfied no fancy, and found no place in either the social or industrial world as it then was.

Before the present century the chief and almost sole uses of the horse were as an implement of war, an instrument of sport and ceremony, an index of rank and wealth, and an article of luxury.

For all these uses, as then pursued, a fast trotter was not suited, nor was he better adapted to the heavy coaches over rough roads, or the slow waggon-trains of armies. The horse best adapted to all these, however much he may have varied as to size, strength, and fleetness, was one whose fast gait was the gallop or run rather than the trot. For leisurely horseback travelling the ambling gait (or *pacing* gait as it came to be called in America) was preferred. With increasing use of horses for draft, certain heavy but slow breeds were developed in the Old World, of which the Dutch, Clydesdale, and Norman breeds are examples.

The causes which led to the cultivation of the trotting gait in this country, and the evolution of a breed with which it should be instinctively the fast gait, were various, and the separate value of each as a factor in the problem would be very differently estimated by different persons studying the subject from different points of view. Now that he is so valuable and plays such a part as a horse of use, it is easy to see why a breed of trotting roadsters should be produced to meet certain important demands of our modern civilisation. But this does not explain how the process actually began.

Reasoning *a priori*, the trotter, as a horse of use, should have originated in western Europe; as a matter of fact, he not only did not begin there, but he was unpopular there until well developed here. Locomotives began to draw armies to the battle-field, the war-horse declined in actual as well as relative importance, the modern, light, steel-spring, one-horse, convenient business waggon as well as the modern buggy came into common use after trotting as a sport was established, and after the gait had been extensively cultivated and bred to. The trotting-horse is specially adapted to various modern uses, but these uses followed his development, rather than led it, although in later days this factor has been an important one in the rate of progress.

[1] By Wm. H. Brewer, from the *American Journal of Science*.

The influences which originally led to the starting of the breed were more social than economical; a similar fact a century earlier marked the founding of that famous running breed, the English thoroughbred. The origin of the trotter, however, was not so simple as that, and several diverse social factors were involved, only the chief of which will here be noticed.

From early colonial times horses have been more generally owned by the masses of the people here than in any country of western Europe. They have had a more general use in agriculture and in business, their ownership or possession has had less social significance, and they have had less importance as instruments of gambling. The colonists who settled north of Delaware Bay, although of various nationalities, were largely those whose religious prejudices and social education was opposed to horse-racing. With the great majority of them it was considered a sort of aristocratic sport, and at best led to unthrifty ways, even if not open to the objection of positive immorality. Consequently but few race-horses were imported into this region in colonial times. The original horse stock of the northern colonies came from several European sources. England, Holland, France, and Spain certainly, and Sweden, Denmark, Germany, Ireland, and Italy probably, contributed to it. The blood from this variety of sources, variously mingled, formed the mongrel stock of the country. This was further modified by local conditions and local breeding assuming different characters in different places, and the hardships of horse life incident to a new country, with strange forage and a rough climate, caused deterioration in size and form. Early writers are unanimous on this point, but many add that what was lost in size and beauty was gained in hardiness and other useful qualities.

After the war of independence there was an improvement in the live stock of the country. English thoroughbred horses were imported both for sporting and to improve the horse stock of the country, and horse-racing rapidly grew in favour as wealth and leisure increased. The export trade in horses to the West Indies increased, particularly from New England. Pacers were most sought for this trade, but sometimes trotters were advertised for.

As horse-racing increased in the last years of the last century the opposition to it revived, and in the earlier years of the present century this became ascendant, and stringent laws forbidding the sport were passed in most of the northern States. The prohibition was sweeping and the penalties severe.

Horse-racing was then a contest between running-horses, and during this repression of racing, trotting as a sport began, at first in a very unostentatious, irregular, and innocent sort of way. Probably no people or class of people have ever bred good horses which they prized and were proud of, who did not find pleasure in seeing them compete in speed or show their fleetness in some way, and during the repression of racing (which meant running), trotting came in as a substitute, poor though it was at first. It had a sort of encouragement from very many thrifty people who were not sportsmen, and was in a measure considered a sort of democratic sport in which even plough-horses could take part. Racing of any kind in those days was a strife between two or more things, as it still is in most countries; no one thought that a single horse could run a race alone, but the instinctive inclination to see a spirited horse in action could be mildly gratified by letting him trot, even if single and alone, and testing by the watch how quickly a given distance could be covered. So "timing" animals came to be practised. We hear of it on the Harlem racecourse in 1806, four years after the laws forbidding horse-racing had been enacted, and again, a little later, near Boston, and it was reputed that certain horses could trot a mile in three minutes. This speed seemed so extraordinary that in 1818 a bet of a thousand dollars was staked (and lost) that no horse could be found that could trot a mile in three minutes. Some authorities date the beginning of trotting as a sport with this event. It is said that in the betting the odds against the successful performance of the feat were great, which shows, strikingly, the enormous progress since made in developing speed at this gait.

In 1821, certain persons on Long Island were allowed by special statute to train, trot, etc., horses on a certain track, under certain restrictions, exempt from the penalties against horse-racing. Other organisations followed, and by 1830 the "training" of trotters was going on at several points, and trotting may be said to have become established as a sport. During this decade the record had been successively lowered to 2.40, 2.34, and 2.32. The times of performance were carefully taken at these "trials of speed," as the statute called them, and "records" became established by more formal sporting codes.

The ostensible object of these associations was the "improvement of the breed of roadsters;" driving single horses to waggons became fashionable, and this led to the improvement of light one-horse waggons for business and pleasure. Those with steel springs were rare luxuries in 1830; by 1843, when the record of mile heats dropped to below 2.30, they were already common. During this thirteen years, the record had been lowered only half a second on mile heats, but three-minute horses were no longer rare.

The fashion of wealthy men driving a single fast trotter for pleasure was for a long time a peculiarly American one, and played an important part in the development of this breed. But, as stated earlier, many influences have contributed: changes in the modes of travel, changes in the methods of war, sentiments regarding horse-racing, the incentives of the course, the general improvement of roads, improvement in carriages, the needs of modern business requiring quick roadsters, these and other influences have all been at work.[1]

The material out of which this new breed is made is a liberal infusion of English thoroughbred blood (usually more than two generations removed), with the mongrel country stock, previously described. There is a voluminous literature relating to special pedigrees, and much speculation as to the comparative merits of the several ingredients of this composite blood.

Regarding the ideal trotter there is as yet a difference of opinion as to what the form should be, and it is too early to decide from actual results. That the gait is now hereditary, that it is the instinctive fast gait with some animals is certain, but whether this is due to inherited habit, inherited training, or to mere adventitious variation and selection, I will not discuss.

The gain in speed is given in the following table, which is the best records at mile heats, omitting the names of the special performers:

Date.	Best Record.	Date.	Best Record.
1818,	3	1865,	2.18¼
1824,	2.40	1866,	2.18
,,	2.34	1867,	2.17¼
1830,	2.32	1871,	2.17
1834,	2.31½	1872,	2.16¾
1843,	2.28	1874,	2.14
1844,	2.26½	1878,	2.13¾
1852,	2.26	1879,	2.12¾
1853,	2.25½	1880,	2.10¾
1856,	2.24½	1881,	2.10¼
1859,	2.19¾		

A sporting paper published in 1873 a list of three hundred and twenty-three horses, with their best records, down to the close of the preceding year. This first list

[1] For more details regarding the history of this development and the factors involved, see the paper already cited, *Rep. Conn. Bd. Agr.* for 1882, p. 215.

of the kind known to me was very imperfect in its details; it was revised for the next year, and since that time many lists, in one form or another, have been published. The figures for the animals with records of 2.25, or better, are reasonably accurate; for the others there is much discrepancy. In the following table the numbers are my own, counting down to 1872, inclusive; the numbers after that date are derived from various lists published since that time in the sporting and breeding periodicals. From the very nature of the case, the table cannot be accurate in the larger numbers, but the numbers do not lose their value for comparison with each other from such faults as to the details of the larger numbers, and, as such, it is undoubtedly the most significant series of numbers ever compiled to show progress in evolution, whether of a breed or species. The number of horses with records of 2.40, or better, is now stated to be over five thousand.

I leave it to mathematicians to plot the curves which immediately suggest themselves, and determine how fast horses will ultimately trot, and when this maximum will be reached.

Table showing the numbers of Horses under the respective Records.

	2.30 or better.	2.27 or better.	2.25 or better.	2.23 or better.	2.21 or better.	2.19 or better.	2.17 or better.	2.15 or better.	2.13 or better.	2.11 or better.
1843	1									
1844	2	1								
1849	7	2								
1852	10	3								
1853	14	5								
1854	16	6								
1855	19	6								
1856	24	7	1							
1857	26	7	2							
1858	30	7	2							
1859	32	9	2	1	1					
1860	40	11	4	2	1					
1861	48	14	4	2	1					
1862	54	17	7	4	1					
1863	59	19	9	4	1					
1864	66	22	12	4	1					
1865	84	29	15	5	2	1				
1866	101	32	17	6	3	1				
1867	124	42	21	9	5	2				
1868	146	52	28	13	6	2				
1869	171	63	34	15	10	4				
1870	194	72	35	16	11	5				
1871	233	99	40	17	12	6	1			
1872	323	—	—	—	—	—	—			
1873	376	—	74	28	15	5	2			
1874	500	—	98	40	16	11	5	1		
1875	—	—	134	61	30	13	5	2		
1876	794	—	165	81	39	16	6	2		
1877	836	—	214	105	51	19	8	2		
1878	1,025	—	270	129	68	24	9	4		
1879	1,142	—	325	164	88	33	11	5	1	
1880	1,210	—	366	192	106	41	14	6	2	1
1881	1,532	—	419	227	126	49	15	7	2	1
1882	1,684	—	495	275	156	60	18	8	2	1

INSTITUTION OF MECHANICAL ENGINEERS

THIS Institution held their usual Spring meeting at the Institution of Civil Engineers, 25, Great George Street, on April 11 and 12, the president, Mr. Percy G. B. Westmacott, in the chair. Three papers were read, and discussed at length; a fourth, by Mr. A. C. Bagot, on "The Application of Electricity to Coal Mines," was postponed for want of time.

The first paper was by Prof. A. G. Greenhill, of Woolwich Arsenal, and dealt with the strength of shafting when exposed both to torsion and end-thrust. He has worked out for this case, by a complete mathematical investigation to be published in the *Proceedings*, the following formula:—

$$\frac{\pi^2}{l^2} = \frac{P}{EI} + \frac{T^2}{4E^2I^2}$$

where P = end-thrust, T = twisting moment, I = moment of inertia of cross-section, E = modulus of elasticity, l = maximum distance between bearings, which will allow a shaft to be stable.

When there is no twisting moment, as in a long column, the second part of the right-hand expression vanishes, and we have the ordinary formula of Euler. If there be no end-thrust, as in ordinary mill shafting, the first part vanishes. The special case where both occur together is that of the screw-shaft of a steamer; but here, it appears, on working the figures out with ordinary dimensions, that the second part is small in comparison with the first, and may be neglected. Hence a screw-shaft may so far be treated as if it were a long column only; and it follows at once that the numerous bearings interposed between the engines and propeller (say, about every 25 feet) are quite unnecessary so far as stiffness is concerned. If retained, as seems desirable, simply to support the weight of the shaft, they might at least be made in some way elastic, so as to enable the shaft to accommodate itself to the sagging and straining of the vessel. It was, in fact, admitted on all hands that screw-shafts never give way from twist or thrust, but always by cross-breaking through strains induced by the unequal movements of the ship; and if so, there seems every reason for taking some steps at least in the direction which Prof. Greenhill indicates.

Another point which the paper touched upon was the question of hollow *versus* solid shafts. Now that shafts can be conveniently cast out of ingot steel, they are frequently made hollow, with the obvious advantage of increasing the stiffness as compared with the weight. Thus, in the case of the screw-shaft of the *City of Rome*, which is 25 inches diameter, with an internal hole of 14 inches diameter, it appears that the moment of inertia is 0·9 of that of an equal solid shaft, while the weight of the latter would be 1·45 that of the former. Again, if a solid shaft were used of the same weight as the hollow shaft, or 20·7 inches diameter, its moment of inertia, and therefore its stiffness, would be barely half that of the latter. Even if a transverse crack, 1 inch deep, were to occur in the hollow shaft (which it might be urged would place it at a serious disadvantage) the loss of stiffness comes out to be 6 per cent., whereas in a solid shaft of equal diameter the corresponding loss would be .5 per cent.; so that even here the advantage on the side of solidity is only 1 per cent.

These figures might seem to be conclusive, yet the solid shaft has its defenders. Mr. Edward Reynolds, of Messrs. Vickers and Co., stated roundly that the history of hollow screw-shafts was a mere history of disaster (which, however, was denied by a subsequent speaker); and he quoted some experiments of his own on shafts one-fourth the size of that in the *City of Rome*, where, tested under a 1-ton weight falling from about 20 feet, the hollow shaft was rapidly destroyed, while the solid shaft remained uninjured. This occurred even when great care was taken to prevent the hollow shaft from getting flattened during the process. His explanation was that the comparatively unstrained fibres towards the centre of the section came in to support and relieve the exterior parts, whenever, by cracks or otherwise, these became unduly loaded. Prof. Kennedy, who followed, seemed to lean to the same view, and quoted the increase of strength observable in the metal between the holes of a drilled plate, as being due, in some unexplained manner, to the influence of the unstrained metal behind the holes. A very satisfactory explanation of this fact was, however, given by Mr. Wrightson at the last meeting of the British

Association. The real question to which Mr. Reynolds's tests point is probably how far theories which rest on the hypothesis that elasticity is perfect can properly be applied to cases where the breaking point has been nearly reached; and this is a question on which more light is very urgently needed, especially with reference to such cases as screw-shafts, where fractures, as a matter of fact, do very commonly occur.

The second paper, by Mr. W. Ford Smith, dealt with twist drills, milling machines, and other methods for the cutting and dressing of metal surfaces, which have been introduced within the last few years; and was almost entirely of a practical character. The third paper, by Mr. John Jameson, was on "Improvements in the Manufacture of Coke," and dealt with a new method, invented by the author, for recovering the gas, gas-tar, and ammoniacal liquor, which are separated from coking coal during the process of carbonisation. As the paper points out, these products are not originally present in the coal. There is, for instance, no ammonia in coal; but there are combinations containing nitrogen and hydrogen, and in almost any process of distillation parts of the evolved nitrogen and hydrogen unite, under very obscure conditions, to form ammonia, which, however, is not stable, but readily decomposes in the presence *e.g.* of oxygen. Every process of distillation, in fact (but some much more than others), favours the formation of gas on the one hand and of condensable hydrocarbons on the other. With regard to the former, its value in the neighbourhood of coke-ovens is not usually high, and it is a question whether it may not best be burnt in the oven itself, to furnish the heat required in any case for the distilling process; but the value of tar and ammonia is great, and would probably not fall very low, even if the production were largely increased. At the same time, as a fuel they are not even equal to the same weight of pure carbon. It will be seen, therefore, that there is ample room for a process which will enable us to separate and utilise these by-products, instead of simply using them as fuel, or, which is far worse, discharging them unburnt to poison the air and destroy vegetation. Mr. Jameson's method of effecting this end is very simple. He takes an ordinary "beehive" coke-oven, makes it tolerably airtight by letting tar soak into the brickwork, and covers the floor with an impervious substance, in which are inserted some large bricks or quarls, pierced with holes. Below these is a chamber connected with a pipe, which leads, through any convenient form of condenser, to a small exhausting fan. The oven is now charged and lighted from the top, to which alone air is admitted. The heat of combustion, penetrating downwards, gradually distils the pitch and gases out of the coal, and the fan being set to work, these products, instead of passing upwards to the fire, are sucked downwards through the holes in the floor, and afterwards separated, the tar being left in one condenser, the ammoniacal liquor in another, and the gas either used at once for steam-raising, &c., or stored in a gas-holder till required.

In the discussion which followed, the advantage of saving the waste products was fully admitted, though some rather startling estimates of the author (who had assumed that 75,000,000*l.* per annum was practically wasted under our present system of coal consumption) were sharply criticised. But by the ironmasters who were present it was strongly laid down that the first duty of a coke-oven was to make good coke—such coke as would give the best results in a blast-furnace; and that to this duty all consideration of by-products must give way. It was further suggested that pitch was a valuable ingredient in coke, and that this pitch was left in it by the present system, but withdrawn on the new one. This idea, however, seems to be founded on a misapprehension. Mr. Jameson and others were able to state positively that the coke made by his process could not be distinguished in quality from the product of the old beehive oven; that the quantity per ton of coal was the same; and that the by-products, though differing very greatly in quantity according to the character of the coal, method of condensation, &c., were almost always sufficient to repay, within a few months or even weeks, the 10*l.* or 15*l.* required to adapt an existing oven to the new arrangement. If these results are confirmed by more extended trials in different localities, the process seems likely, as one speaker phrased it, "to take a pretty prominent position among the great inventions of the present day."

CORONERS' SCIENCE IN CHINA

WHETHER Chinamen are or are not believers in the principle that it is better that nine guilty persons should escape rather than that one innocent person should suffer, they do at all events, by their manner of conducting inquests, leave open a wide door for the escape of murderers. A deeply-rooted repugnance to dissection of the human body and a consequently slight acquaintance with anatomy, coupled with an entire ignorance of the action of poisons, deprive coroners of every means of arriving at decisions except those furnished by outward symptoms and appearances. From early times, however, the importance attaching to human life has been recognised by the custom of holding inquests in cases of sudden death, and various works have been published embodying all the knowledge available on the subject to assist coroners in their duty of investigation. The best-known of these was the *Se yuen luh*, or "Record of the washing away of wrongs," which was given to the world in the thirteenth century, and which, under the same title, subsequently received the *imprimatur* of the officers of the Board of Punishments, who, in the exercise of their legislative function, issued it as a manual for coroners. In this work is expounded the whole system of Chinese medical jurisprudence, of which the following is a slight sketch

One of the first directions given to coroners reminds one of Mrs. Glasse's celebrated dictum, and is to the effect that before issuing a warrant for an inquest they should be quite sure that there really is a corpse. This admonition is no less curious than the reason which makes it necessary. It appears to be not uncommon for unscrupulous swindlers to demand inquests on imaginary corpses for the purpose of extorting money from the wealthy owners of the houses where the bodies are said to be, who, rather than fall into the clutches of the law, generally pay the sum demanded on condition that all proceedings are stayed. But being well assured of the existence of a corpse, the coroner should proceed to the spot well provided with onions, red pepper, salt, white prunes, and vinegar with the lees. If death has just taken place, he should examine the top of the head, back of the ears, throat, and any other vital part where a sharp-pointed instrument may have been inserted. In case of his failing to find any such cause of death, he should interrogate the friends and neighbours, and then proceed to examine any wounds there may be on any other part of the person.

An infallible guide to the date of a wound is found in the colour of the bone affected. If it is a recent one or of a slight nature, the bone will be red, but if old and severe, the bone will be of a dark blue colour. Particular care should, however, be taken to ascertain that these colours are genuine, and not manufactured to agree with the story told by the relatives. A red tint may be given to the bone by painting it with an ointment of genuine safflower, sapanwood, black plums, and alum, with the addition of boiling vinegar. On the other hand, green alum or nutgalls, mixed with vinegar, impart a dark blue or black hue. These counterfeit colours may, however, be distinguished by their want of brightness. Again, not uncommonly a fictitious wound is made after death by

burning the spot with lighted strips of bamboo, but such a wound will be level with the surrounding flesh and be soft to the touch. If willow bark has been used for the same purpose, the flesh will be rotten and black, livid all round, and free from hardness. A lighted paper placed inside a cup and applied to the flesh makes a wound which resembles the result of a blow with the fist; but it will be observed that all round there is a red, scorched mark, that the flesh inside is yellow, and that although it swells, it does not become hard. On the other hand, a genuine wound can be distinguished by the well-defined colours of the surrounding flesh. At the extremity of the wound there should be "a halo-like appearance, like rain seen from a distance, or like fleecy clouds, vague and indistinct."

Murders, it is held, are seldom the result of premeditation, but are in a great majority of cases to be traced to drunken brawls; and further, coroners should remember that the relatives of a wounded man, unless their ties be of the closest, desire his death that they may extort money from his slayer. It becomes their duty, therefore, on hearing of a fray in which any one has been seriously wounded, to see that the injured man be carefully tended and provided for. If death ensues, a careful examination of the corpse should be made, beginning from the head downwards, and in doing so, should it be suspected that tattoo-marks on his cheeks or elsewhere have been obliterated, such parts should be tapped with a slip of bamboo, which will have the effect of making the marks reappear. Attention should be given to see if the ears have been bitten or torn, whether the nostrils have been wounded, and whether the lips are open or closed. The teeth should be counted, the jaws examined, and the limbs carefully scrutinised down to the finger- and toe-nails. If the body bears marks of corporal chastisement, it should be noted, and any scars there may be, both on the inside and outside of the ankle-bones, may be safely set down to torture. When the mark of a wound which is known to have been inflicted cannot be traced, vinegar with the lees should be poured on the spot, and a transparent piece of oilcloth be held between the sun and part to be observed. On a dull day live charcoal must take the place of the sun. If the result be not satisfactory, spread powdered white prunes, with more vinegar and lees, and examine closely. Should this also prove unsatisfactory, then a cake composed of the flesh of white prunes, red pepper, onions, salt, and lees should be made very hot over a fire and applied to the parts, when the wound will appear.

In the same way, when violence is suspected, but no injury is at first sight apparent, it is directed that vinegar with the lees should be poured on the body, over which the clothes of the deceased saturated again with hot vinegar should be laid, and, covering all, mats spread to keep the steam in. The temperature of the vinegar should be regulated by the season of the year, and in very cold weather, when the vinegar, however hot, is insufficient to relax the rigidity, the corpse should be laid in a hole in the ground, in which a roaring fire has been subdued by copious sprinklings of vinegar. The fumes of steam which will then arise may be expected to accomplish the object. A careful examination should then be made, and if the marks of a wound or wounds are observed on the skin, their size, shape, and position are to be carefully noted, and death attributed to the one on the most vulnerable part. One of the most curiously perverted pieces of coroners' science is contained in the assertion that, if death has arisen from a blow on the lower part of the abdomen, the injury is discoverable by the condition of the roots of either the top or bottom teeth in the case of men, and in that of women by the appearance of the gums.

If the services of the coroner should not be called in until the body is in so advanced a stage of decomposition that the condition of the bones is the only test left him, he should choose a bright day, and having steamed them in the fumes of hot vinegar he should examine them under a red oilcloth umbrella. The blood having soaked into the injured parts, these will at once become visible, and will leave clearly-defined red, dark blue, or black marks. A long-shaped, dark-coloured mark so discovered points to a wound inflicted by a weapon, a round one to a blow of the fist, a large one to a butt of the head, and a small one to a kick. The fact of saturation of blood in the bone is evidence that the wound was inflicted before death. Should there be any doubt as to the identification of the bones, it is only necessary for a child or grandchild of the deceased to cut himself and herself with a knife, so that the blood may drip upon the bones, when, if they be really those of the parent, the blood will soak into them, otherwise it will not. In connection with this test it is curious to find stated the old-world belief that the blood of relations, if dripped into a basin, will mix, and not in the case of others. This test would appear to be often appealed to, since coroners—though it is difficult to see what it has to do with coroners—are warned to see that those interested in proving a relationship do not smear the basin with salt or vinegar, under the influence of either of which any bloods will mix.

Observations have shown, so coroners are told, that a man who has been killed with a knife dies with his mouth and eyes open and his hands clenched. The skin and flesh about the wounds will be shrunken, and in case of a limb having been cut off the bone will be protruding. Where decapitation has taken place, the muscles will have shrunk backwards, the skin will have curled over, and the shoulders will be shrugged up. These appearances will be wanting if the wounds have been made after death has taken place. It is necessary to be particular on these points, we are told, as murderers constantly endeavour to mislead coroners by inflicting wounds after death in such a way as to lend a colour to vamped-up stories of suicide. The exact frame of mind in which a man was when committing suicide can be readily discovered by the features of the corpse. If the teeth are firmly set, the eyes slightly open and looking upwards, a fit of violent passion prompted the act; if the eyes are closed, but not tightly, the mouth slightly open, and the teeth not shut, then it was due to an excess of pent-up rage; if fear of punishment has driven him to it, his eyes and mouth will be placidly closed, "for he looks on death merely as a return home and a happy release from the responsibilities of life." The hands also furnish a test when there is a doubt whether the case of a man whose throat has been cut be one of murder or suicide. The hand with which a suicide commits the deed will remain soft for a time, and will curl up a day or two after death, neither of which symptoms will occur when death has been caused by another person.

Strangulation is one of the commonest means by which persons tired of life "shuffle off this mortal coil," and full directions are given as to the points to be observed when holding inquests on such cases. The exact position of the body, the kind of scar on the neck, the existence or absence of the mark of a knot, the expression of the face, and a thousand other matters are detailed at length, and are contrasted with similar appearances in the case of murders. One curious piece of superstition receives the sanction of the Board of Punishment in connection with suicide by hanging. Beneath the spot where the crime was committed, at the depth of three or more feet below the surface of the soil, there will be found a deposit of charcoal, and by this test, should any doubt exist as to the scene of the suicide, the matter may be settled. The directions given in the case of deaths by drowning are voluminous, and, speaking generally, accurate. The habit of generalising from insufficient data, which is so common with Chinamen, occasionally leads them astray here as elsewhere. It has been reserved for them, for example,

to discover the law that bodies take a longer time to float in winter and the beginning of spring than in the summer and end of autumn. That a drowned man floats on his face and a woman on her back is mentioned, and it is left to be implied that in case of bodies having been thrown into the water after death this does not hold good. With the same minuteness every possible circumstance connected with death by fire is gone into at length, the presence of traces of ashes in the mouth and nose being described as "a crucial test of death by burning."

The chapters on poison are, as might be expected in the absence of dissection, the most unsatisfactory in the book. Practically very little light is thrown on the distinguishing symptoms arising from the effects of different poisons. The common test applied to most is that of inserting a silver needle washed with a decoction of *Gleditschia sinensis*, into the mouth of the corpse. If, when after a time this is withdrawn, it should be stained a dark colour, and remain so stained after it has been again washed with the decoction, poison has been the cause of death. Another proof is furnished by the effect which a pellet of rice, after having been some time in the mouth of the corpse, has on poultry who can be induced to swallow it. The commonest poisons are said to be opium, arsenic, and certain noxious essences derived from herbs. But besides these, other things are taken by suicides and given by murderers to cause death. In some of the southern provinces there exists a particular kind of silkworm, known as the Golden Silkworm, which is reared by miscreants to serve either purpose as occasion may require. Quicksilver, which is also used with fatal effect, is either swallowed, or, like the "juice of cursed hebenon" which sent Hamlet's father to his account, is poured into the ear. The torture necessarily consequent on this last method of using it must be so excessive that it may safely be assumed that it finds favour only with murderers. Swallowing gold, on the other hand, seems to be the favourite way of seeking death with wealthy suicides. It has been held by some writers that the expression "swallowing gold" is but a metaphorical phrase meaning "swallowing poison," just as when a notable culprit is ordered to strangle himself he is said to have had "a silken cord" sent to him. But the "Coroners' Manual" puts it beyond question that gold is actually swallowed, and it prescribes the remedies which should be adopted to effect a cure. Gold not being a poison, death is the result either of suffocation or laceration of the intestines. When suffocation is imminent, draughts of strained rice-water, we are told, should be given to wash the gold downwards, and when this object has been attained, the flesh of partridges, among other things, should be eaten by the patient to "soften the gold" and thus prevent its doing injury. Silver is also taken in the same way. But though wealthy Chinamen thus find a pleasure in seeking extinction by means of the precious metals, they have never gone the length of pounding diamonds to get rid of either themselves or their enemies after the manner of Indian potentates.

ROBERT K. DOUGLAS

ZOOLOGY IN JAPAN

A CORRESPONDENT in Tokio sends us the following:—During the late summer and autumn some good work has been done in the ornithological way. Mr. P. L. Jouy, of the Smithsonian Institution, collected extensively in the region of Mount Fujiyama, at Chiu-senji Lake, near the celebrated shrines of Nikko, and on Tateyama Range, between the borders of the provinces of Shinshiu and Hida. A large number of beautifully prepared skins, with a good deal of information regarding the breeding habits of some of the rarer birds, is the result, which will be recorded in the February number of the *Chrysanthemum*, a magazine published at Yokohama, appearing in enlarged form with the commencement of this year. An article contributed by Capt. Blakiston in the January number, follows up those of his for September, October, and November, 1882, on ornithological work in Yezo during the past summer; in which is noticeable the occurrence of *Locustella certhiola* (Pall), and *Phylloscopus borealis* (Blasius) on that island; and the discovery of a new species of *Motacilla* (probably described by Seebohm in the *Ibis* for January, 1883), allied to *M. ocularis* (Swinhoe) and *M. amurensis* (Seebohm), which has hitherto somehow been mixed up with *M. lugens* of the "Fauna Japonica," which latter is now found to be —to quote Capt. Blakiston's words (*Chrysanthemum*, January, 1883, p. 31)—"a species unique in its genus, having in the adult state the same appearance winter and summer, and in which the young pass at once before their first winter into the adult dress."

Messrs. Owston, Snow, and Co.'s otter hunters at the Kuril Islands have also during the past season added some new localities for Japan birds. The specimens are in the hands of Capt. Blakiston, and will be duly mentioned in the following number of the *Chrysanthemum*, as additional notes to the "Birds of Japan," *Trans. As. Soc. Japan*, vol. x. part 1 (noticed in NATURE, vol. xxvi. p. 362).

In the way of *mammalia* late investigation points to the distinctness of Yezo from Japan proper. The Rev. Père Heude, who is now engaged upon a revision of the *Cervidæ* of Eastern Asia, has come to the conclusion that the common deer of Yezo is not *C. sika* of the "Fauna Japonica," but *C. manchuricus-minor*, or an undescribed species. Two parts are already published—very creditably got up at the Mission Press at Sikawei, near Shanghai—cf. "Mémoires concernant l'Histoire Naturelle de l'Empire Chinois," others being promised to follow.

NOTES

THE following is the list of fifteen candidates recommended for election by the Council of the Royal Society:—Surgeon-Major James Edward Tierney Aitchison, M.D., James Crichton Browne, M.D., LL.D., Surgeon-Major George Edward Dobson, M.B., James Matthews Duncan, M.D., Prof. George Francis Fitzgerald, M.A., Walter Flight, D.Sc., Rev. Percival Frost, M.A., David Gill, LL.D., Charles Edward Groves, F.C.S., Howard Grubb, F.R.A.S., John Newport Langley, M.A., Arnold William Reinold, M.A., Roland Trimen, F.L.S., F.Z.S., John Venn, M.A., John James Walker, M.A.

THE loss sustained by mathematical science in the premature death of Henry Stephen Smith is still fresh in the minds of our readers. They will find their best consolation in the fact that his successor in Oxford may possibly be Prof. Sylvester. Such an opportunity of recovering for England the services of one of her two greatest mathematicians is not likely to recur, and will, we doubt not, be eagerly turned to advantage. It has been a humiliating thought to many to whom the highest interest of science is dearer than the prosperity of mere mediocrity that, of the two greatest mathematicians that England has produced in the nineteenth century, one has altogether and another almost been obliged to seek for refuge in a foreign land.

UNIVERSAL regret will be felt at the sad intelligence which has just reached England by telegram, from Madeira, of the untimely death of Mr. William Alexander Forbes, B.A., Fellow of St. John's College, Cambridge, and Prosector to the Zoological Society of London. Mr. Forbes left England in July last, along with Mr. McIntosh and Mr. Ashbury, upon what was expected to be a three or four months' expedition in a steam-yacht up the river Niger. He died of dysentery at Shonga on January 14, aged 28.

THREE months ago Mr. Raphael Meldola, as retiring President of the Essex Field Club, gave an interesting address, which is now printed in a separate form, on "Darwin and Modern Evolution." It gives a clear and well-condensed account of Darwin's life and work. The following extract concerning the first publication of the theory of natural selection at the Linnean Society is of historical interest, and also, we think, of some instructive value:—"Mr. Wallace has narrated to me that one of his correspondents, a well-known entomologist, wrote to say that it was a general remark in natural history circles, with respect to the paper, that it was much to be regretted that the author had not more confined himself to statements of fact!" This shows that the naturalists of the Linnean Society at that time had the same intolerance of anything like speculative brain-spinning which still finds occasional expression. But to-day we have to thank the sagacity of the greatest of naturalists that, while cautious of speculation, he nevertheless courted it as a friend to the highest interests of science, while leaving "the well known entomologists" to shun it as the worst of enemies. The truth is that in biology, as in all other branches of science, unless the only aim of a worker is to accumulate knowledge of details, he is bound to resort to hypotheses as feelers after principles. On the other hand, of course, speculation, like fire, while the most valuable of servants, may also be the most dangerous of masters. The truest scientific judgment, therefore, consists in using speculation as not abusing it; and if in particular cases it is asked how much latitude is thus to be allowed to speculative thinking, the answer must be that this is just the question which in all particular cases it requires the truest scientific judgment to decide. All that can be said, as a matter of general principle, is that quite as much and even more harm may arise from an over-nervousness of deductive method in biology, as may arise from an over-confidence in them; and also that the theory of evolution—at least in our opinion—is now sufficiently well established to admit of being used deductively in no stinted measure, without danger of violating the best methods of scientific procedure.

THE great and deserved success which has attended the Girls' Public Day School Company has now led to the formation of a similar company for establishing schools for boys. A meeting, under the presidency of Lord Aberdare, was held on the 24th inst. at the rooms of the Society of Arts, at which the objects of the company were explained. The basis of the new schools is that of a self-supporting company, independent alike of Government and charitable aid. It is stated that premises will shortly be secured in Kentish Town, where the first school will be opened in a few months.

CAPT. C. E. DUTTON, of the United States Geological Survey, who spent half of last year in Hawaii studying the volcanic phenomena there, and whose researches among the plateaux of Utah have brought to light so many interesting phases of volcanic action in that region, is about to undertake the exploration of a still more extensive volcanic region. He is organising his forces for a summer campaign in the Cascade Range, beginning at the southern end in California, among the volcanic piles of Mount Shasta, and working northwards across Oregon to the remote peaks of Mounts Hood and Rainier, in Washington Territory. In this way a preliminary survey of the region will this year be made, and the information will be gained that will serve as the basis for future more detailed exploration. That vast region contains possibly the most colossal outpouring of volcanic matter anywhere to be seen in the world. Geologists will rejoice that it is now to be systematically examined by one so competent as Capt. Dutton, who has specially trained himself for the task. The American Congress is to be congratulated on the enlightened spirit in which these surveys of the Western Territories are conceived and carried out.

THE Central Swedish Meteorological Observatory, in Stockholm, has issued a request, signed by Baron Nordenskjöld, calling upon those who may witness meteoric phenomena to send minute particulars of the same to him. He requests that the following details may be noted:—Time, duration, direction as well as height above horizon, whether the meteor had a tail, emitted smoke, burst, or simply disappeared from view, whether any sound and any fall of objects were observed. He also requests that a drawing of the phenomena may if possible be forwarded. In conclusion, he says: "There often appears a peculiar dry mist or 'sun-smoke' over extensive tracts of land in Sweden, sometimes accompanied by a remarkable smell extending for hundreds of square miles. The nature of this phenomenon has not yet been ascertained. As I am informed that it was recently noticed in certain parts of Norrland, I beg that any observer of the same will forward all particulars he may possess."

GEOLOGISTS will learn with regret that Mr. Alexander Murray, who has so long and so ably directed the Geological Survey of Newfoundland, feels himself compelled by advancing years and enfeebled health to retire from his duties. For many years he was one of the late Sir William Logan's chief officers in the Geological Survey of Canada, where he long ago gained his geological spurs. His iron constitution and indomitable enthusiasm have carried him through more hardships than have fallen to the lot of almost any living geological explorer, but they have been borne with a quiet courage and good-humoured indifference altogether admirable. May he find now the honourable rest and recognition to which his long devotion to the colony so justly entitles him. He will be succeeded by his present second in command, Mr. James P. Howley, in whose experienced hands the Surveying Department of Newfoundland will be excellently administered.

PROF. TYNDALL will on Thursday next, May 3, at the Royal Institution, give the first of a course of three lectures on "Count Rumford, Originator of the Royal Institution."

ALTHOUGH the circulation of books from Newcastle-upon-Tyne Free Library has not quite kept up this second year, yet the Report, with its account of the handsome new building, is a very satisfactory one. The carrying on of education in various ways, in combination with the Science and Art Department and with the City and Guilds of London Institute, by literary and commercial classes, including even a competition in oratory supported by a "Bequest," is valuable work that ought naturally to fall, as it has done here, into the same hands as control the library. The method of encouraging juvenile readers by permitting the use of the whole library to the more intelligent is good where these readers are sufficiently known to the librarian. There is no doubt that the true reason is given for the large increase in the issue of fiction, viz. that the committee have added to their stock in that class in a proportion twice as great as in any other class. "The love of" fiction "increases as much as the" fiction "itself increases."

THE Mitchell Library at Glasgow has taken the only method of repressing this circulation of fiction, viz. that of not buying the books! In this well-endowed and promising institution, only open five years, yet now containing 45,000 volumes, there are only 374 volumes of fiction; yet so great is the demand for this class of reading that every volume has been issued ninety-eight times, *i.e.* changed twice every week throughout the year! This library, however, supported mainly by the splendid bequest of 70,000*l.* left by Mr. Stephen Mitchell in 1874, has purposely relegated this department and that of branch libraries to the 1*d.* rate, able to produce in Glasgow 11,000*l.* a year, while itself takes the form of a great reference department, already the

largest free library in Scotland, and promising in a second five years to be among the most important collections of books in the kingdom. One of the best functions of a public library in any town is to become the centre to which will gravitate all publications of any local value or interest. For since every subject or author is naturally connected with some locality, if this were well carried out all over the kingdom, information would gradually be as well arranged and as readily accessible as in a cyclopædia. The collections undertaken by the Mitchell Library at Glasgow are (1) the works of Burns and other Scotch poets and verse writers, one object of which will be "to preserve local dialects, local customs, and local memories"; (2) all papers which in any way illustrate the city's growth and life; (3) specimens of early Glasgow printing. The Scotch Covenanters is another subject in which a collection of publications has been commenced. Still nearly one-fifth of the volumes in the Mitchell Library, and more than one-fifth of the volumes in circulation, belong to the department of Arts and Sciences. The attendance of readers has been quite as large as the present premises will accommodate.

THE Council of the Society of British Artists opened their gallery in Suffolk Street on Sunday last to the members of the Sunday Society. A similar privilege has been granted for Sunday next, the 29th inst., when the public will be admitted during the afternoon and evening by free tickets, to be had by all who apply, inclosing a stamped and addressed envelope, to the Honorary Secretary, 8, Park Place Villas, W. The eighth annual meeting of the Sunday Society will be held in the Princes' Hall, Piccadilly, on May 5, under the presidency of Sir Coutts Lindsay.

WE have received the Memorandum of Association of the National Smoke Abatement Institution, signed by the Dukes of Westminster and Northumberland, Lord Mount-Temple, Sir W. F. Pollock, Sir Lyon Playfair, Sir Hussey Vivian, and Mr. Ernest Hart. The objects of this institution are already well known to all our readers.

THE diplomas and scholarships of the Spring Session of the Royal Agricultural College, Cirencester, were conferred on the successful students on the 19th inst. Among those on whom the diplomas were conferred were Messrs. Sen and Hossein, the two Indian scholars first sent to the College by the Government of India. It is worthy of note that one of these gentlemen, Mr. Sen, obtained the highest number of marks ever reached for the diploma.

A LARGE collection of weapons and implements from the Stone Age in Japan has, we are informed, arrived in London. The collector, Herr von Siebold, is an official of the Austrian Embassy in Japan, and has resided for many years in the latter country. The collection embraces, we believe, a large number of flint arrowheads, celts, axes, as well as numerous specimens of pottery taken from shell-heaps in various parts of Japan. The well known *magatama* and *kudatama* ornaments are also well represented. Except a few in the Christy collection in the British Museum, and a small collection given by Herr von Siebold himself to the Copenhagen Museum, the Japanese Stone Age is not, we believe, fairly represented in any archæological museum in Europe.

MORE than 250 years ago the English residents in Japan were perplexing themselves, as they are to-day, on the subject of earthquakes. In the diary of Richard Cocks, just published for the Hakluyt Society by Mr. Maunde Thompson, we find, under date November 7, 1618, the following entry:—"And, as we retorned, about 10 a clock, hapned a greate earthquake, which caused many people to run out of their howses. And about the lyke hower the night following hapned an other, this cuntrey being much subject to them. And that which is comunely markd, they allwais hapen at a hie water (or full sea); so it is thought it chanseth per reason is much wind blowen into hollow caves under grownd at a loe water, and the sea flowing in after, and stoping the passage out, causeth these earthquakes, to fynd passage or vent for the wind shut up."

NEWS from Mr. Stanley dating down to the middle of December has just been received at Brussels. Stanley had reached the African coast, and, after having augmented his party by 223 natives from Zanzibar, under the leadership of the Belgian traveller, M. de Cambier, had started for Vivi, the first station established by the International African Society. At Vivi preparations were being made for the construction of a railway line to the landing-place on the river, but the work proceeded slowly, owing to the total absence of beasts of burden. Up to now seven stations have been established—Vivi, Isanghila, Manyangha, Lutété, Stanley Pool, Ibaka Nkoutou, and Bolobo; the latter is distant about 700 miles from the mouth of the Congo, and is the last one established. Of the four small steamers taken to Africa three had been launched, and the fourth was being transported from Manyangha to Leopoldsville. The seven stations already seem to become centres of civilisation, and exercise a beneficial influence upon the surrounding native tribes. Horned cattle had been introduced at Vivi, and at Leopoldsville agricultural work had begun, cabbage and lettuce thriving exceedingly in that locality. At Bolobo a fertile and well-populated country was reached, which extends far beyond the limits of De Brazza's journey. The progress of the latter was contemplated with equanimity, yet fears were entertained regarding the claims of the Portuguese Government, and also concerning the freedom of way and commerce.

SEVERAL Swedish officers have recently left Europe, being invited to join Mr. Stanley on the Congo.

THE Swedish Academy of Sciences has offered a reward to the vessel which first brings despatches, &c., to the meteorological observing party wintering at Spitzbergen.

THE despatch of the Swedish corvette *Vanadis* on a voyage round the world is contemplated. Several men of science will accompany her, among whom is Dr. Stolpe, the well-known ethnographist.

ON the 13th inst., between 8 and 9 a.m., a remarkable mirage was seen at Ölsta, in the parish of Sala, Sweden. It displayed distinctly a town in Eastern style situated by the sea, with temples and minarets, while to the left a forest of fine cypress trees was seen. In front was a train in motion, while a body of soldiers appeared marching along a road, with their bayonets flashing in the sun. The whole was visible for nearly an hour, when it gradually faded away.

THE French Academy of Sciences, at its meeting on Monday last, selected MM. Bonnet and Resal as candidates for filling the place vacated by the decease of M. Liouville, in the Bureau des Longitudes.

LAST week M. de Lesseps delivered several speeches at the Sorbonne and in other places, showing that the Roudaire Inland Sea will be useful and profitable, and the speaker met with very decided success. On Monday, M. Cosson, his usual antagonist, delivered a long speech, pointing out the danger of the operations, but the French Academy of Sciences took no notice of it, and no commission being appointed the matter dropped.

AT the March meeting of the Russian Physical Society, M. Sreznevsky read a communication on an instrument largely employed but the theory of which is from being established, namely, the hygrometer of Saussure. Its scale, usually traced by comparison with a psychrometer, varies with the month when

the comparison is made, a difference which is probably due to influence of temperature, as already pointed out in 1783 by Saussure. The matter, however, has never yet received thorough investigation. The cause of the elongation of the air in consequence of an increase of moisture remained also unexplained. It might be explained now, however, as it is known that the air contains water in a liquid state in its microscopical cavities. The curvature of the surfaces of these microscopical meniscuses, which depends upon the tension of the vapour that incloses the air, must influence the tension on its surface and therefore change its length. Both these causes can be expressed mathematically, at least for the simplest cases, and if we admit a state of equilibrium we can easily see that the tension of the meniscuses on the surface of the air is a function of the relative moisture, and is proportionate to the logarithm of the moisture. The elongation of the air would thus be a function of the relative moisture of a capillary constant, of the coefficients of elasticity of the air, and of the suspended weight.

DR. ARNOD DODEL PORT has recently published the final part of his incomparable "Atlas der physiologischen Botanik." The six plates which constitute it illustrate: *Cystosira barbata*, I.Ag. (a genus of sea wracks), the archegonium and antheridium of *Marchantia* (one of the Liverworts), *Pinus laricio* (third plate), *Lavatera trimestris*, two plates (a genus of Malvaceæ), and *Datura stramonium*, L. (the common thorn-apple). Together with the plates is published the final part of the descriptive text.

IN the current number of the *Annales de l'Extrême Orient*, M. de Lucy-Fossarien draws attention to the interesting fact in connection with education in Japan, that a large part of its development is due to private assistance. In the past five years forty-two millions of francs have been given voluntarily by private persons for the extension of education. Even this large sum, however, is probably less than the value of the land, houses, &c., given in particular districts for the use of schools.

THE tenth annual Report of the Museum für Völkerkunde, at Leipzig, has just been published, and gives an interesting account of the flourishing condition of this excellent ethnographical institution. The Emperor of Germany again contributes a large sum to the funds of the Museum, and the Crown Prince of Austria has become a member of the institution; the collections have been largely increased, and there are no less than 106 gentlemen at work in various parts of the world extending the connections of and acquiring material for the Museum.

AN earthquake was observed at Tashkend on March 31, at 7 a.m. The shocks were of considerable violence. In the Etna district the volcanic phenomena continue. A violent earthquake occurred at Riposto on the 5th inst., and on the following day oscillations were felt also at Catania, Paternò, and Randazzo. A thick volume of steam emanates from the crater as well as from lateral openings. At Salinella the mud crater had resumed its activity and had caused considerable destruction of property.

DR. PAUL GÜSSFELDT of Berlin, the eminent traveller who started for South America some time ago in order to make geological and other scientific researches in the Cordilleras, reports that he is well satisfied with the results of his journey, and that he had discovered a glacier of the first order in the style of the Aletsch glacier. The glacier is between fifteen and twenty miles in length. Dr. Güssfeldt has measured many summits trigonometrically, made a collection of alpine plants (amongst them a wild potato from above the glacier), and another of geological specimens. On December 31 he intended to leave for the Argentine Republic; thence he proposed to return to Maipu, and then investigate the Aconcagua district.

A NUMBER of unusually bright and large meteors were observed at Prossnitz (Austria) and other places in the neighbourhood on the evening of March 13 last, between 6 and 11 p.m. Some lit up the whole sky and lasted five or six seconds. No trace of any meteoric stone has as yet been discovered.

THE additions to the Zoological Society's Gardens during the past week include a Leopard (*Felis pardus* ♀) from India, presented by Mr. A. P. Marsden; an Ocelot (*Felis pardalis*) from South America, presented by Mr. C. G. Leith; a Ring-tailed Coati (*Nasua rufa*) from South America, presented by Mr. E. Dance; two Porto Rico Pigeons (*Columba corensis*) from the West Indies, a Common Boa (*Boa constrictor*) from Brazil, presented by Mr. C. A. Craven, C.M.Z.S.; an Osprey (*Pandion haliaetus*) from Australia, presented by Dr. Plummer; a White-bellied Sea Eagle (*Haliaetus leucogaster*) from Australia, presented by Mr. E. P. Ramsay, C.M.Z.S.; three Common Rheas (*Rhea americana*) from Monte Video, presented by Mr. John Fair; a Green Turtle (*Chelone viridis*) from the West Indies, presented by Mr. Fleetwood Sandeman; a Leopard (*Felis pardus* ♂) from India, a Small Hill Mynah (*Gracula religiosa*) from Southern India, a Greater Sulphur-crested Cockatoo (*Cacatua galerita*) from Australia, a Gannet (*Sula bassana*), British, deposited; an Iceland Falcon (*Falco islandus*) from Iceland, purchased.

OUR ASTRONOMICAL COLUMN

SCHMIDT'S VARIABLE STAR NEAR SPICA.—On June 6, 1866, Dr. Julius Schmidt remarked to the south and east of Spica a conspicuous star which he estimated 5·4m., and which was wanting in Argelander's *Uranometria*. It was brighter than the neighbouring reddish-yellow star, 68 Virginis. He found its place for 1866·0, R.A. 13h. 27m. 33s., Decl. −12° 31′·5. It is Lalande 25086, estimated 6·7 on May 10, 1795, and Piazzi XIII. 126, called 8m. in the catalogue, but 7 and 6·7 in the *Storia Celeste*. It was not observed by Bessel nor Santini, but occurs in Lamont's Zone 355, 1846, May 22, when it was rated 8m. In Bremicker's Berlin Chart it is 7m., and 6·7 in Heis. But a special point of interest about this object is Schjellerup's inference that it is identical with the 19th star in Virgo in Ptolemy's Catalogue, as indicated in a note at p. 160 of the translation of the Catalogue of Abd-al-Rahman al-Sûfi, which the Persian astronomer says was of the smaller fifth magnitude, nearer the sixth, though Ptolemy calls it "absolutely of the fifth." In Baily's edition of Ptolemy's Catalogue in vol. xiii. of the *Memoirs of the Royal Astronomical Society*, the star in question is No. 515, and there identified with 68 *i* Virginis: it is called ὁ νοτιώτερος τῆς ὀπισθίας πλευρᾶς. Schjellerup, translating from Al-Sûfi, says: "La 19e est la méridionale du côté postérieur du quadrilatère, après *al-simâk*, s'inclinant vers le sud; elle est des moindres de la cinquième grandeur; Ptolémée la dit absolument de cinquième, mais elle est plus près de la sixième. Entre elle et *al-simâk* vers le sud-est, il y a environ une coudée et demie et entre elle et la 17e il y a la même distance. Avec *al-simâk* et la 17e elle forme un triangle isoscèle, cette étoile étant au sommet. La latitude de cette étoile, indiquée dans le livre de Ptolémée, se trouve erronée, parce que, au ciel, elle se fait voir autrement qu'elle ne tombe sur le globe. Car, d'après cela, elle devrait se faire voir au nord d'*al-simâk*, tandis que, en verité, elle se trouve au sud." Al-simâk is Spica, and the 17th star appears to be 76 Virginis. Baily in his Catalogue places the 19th star in longitude 178°, with 3° 0′ south latitude, but in a note he points out that in the edition of Ptolemy, published by Liechtenstein at Venice in 1515, the latitude is 0° 20′ and *north*; with the remark, "The star 68 Virginis agrees with the position given by Ptolemy; but it is difficult to make it accord with the description, as being in the 'latus sequens' of the quadrilateral figure."

Both the variable and 68 Virginis are found in Mr. Stone's Southern Catalogue, the epoch of which is 1880. The auxiliary quantities for the reduction of positions for this year to the assigned epoch of Ptolemy's Catalogue, the first of Antoninus, are, in the usual notation—

$$[A \ldots 168° 47′·3 \ldots A′ \ldots 191° 0′·8 \ldots \theta \ldots 9° 40′·6,$$

whence with the obliquity of the ecliptic = 23° 41′·1, Stone's places for A.D. 138 become—

	Longitude.		Latitude.
Var. Schmidt (Piazzi, xiii. 126) ...	180° 52′	...	−2° 58′
68 Virginis	178° 53′	...	−3° 14′

As we have seen, Ptolemy's 19th star of Virgo is placed in longitude 178° 0′, latitude −3° 0′; but, as is well known, the longitudes of the Almagest are about one degree too small. Hence Schjellerup's identification of the variable with Ptolemy's star is likely to be correct; the object deserves frequent attention.

D'ARREST'S COMET.—With reference to the remarks last week in this column on the first announcement of the observation of D'Arrest's comet in the Dun Echt Circular, Prof. Krueger, Director of the Observatory at Kiel, writes us from that establishment, as the "Centralstelle für astronomische Telegramme," as follows:—"I wish to state with reference to No. 703, p. 589, as I have done in *A. N.* No. 2507 [not yet received], that Dr. Hartwig had not telegraphed any daily motion of the supposed comet D'Arrest on the 4th April. The hypothetical daily motion was added by myself in the cable-telegram to Cambridge, U.S., because I assumed that the American astronomers were not in possession of an ephemeris. Lord Crawford received, as usual, the same telegram as Cambridge, U.S., with the additional note (in order to avoid double-telegrams) that the telegram had been sent to America. European astronomers received only Dr. Hartwig's original communication."

ON THE SENSE OF COLOUR AMONGST SOME OF THE LOWER ANIMALS[1]

AT the meeting of the Linnean Society on Thurday, April 19, Sir John Lubbock read a paper on this subject. Some years ago M. Paul Bert made a series of interesting experiments with the common Daphnia, or water-flea, which is so abundant in our ditches and pools. He exposed them to light of different colours, and he thought himself justified in concluding from his observations that their limits of vision at both ends of the spectrum are the same as our own, being limited by the red at one end, and the violet at the other.

In a previous communication Sir John Lubbock showed, on the contrary, that they are not insensible to the ultra-violet rays, and that at that end of the spectrum their eyes were affected by light which we are unable to perceive. These experiments have recently been repeated by M. Merezkowski, who, however, maintains that, though the Daphnias prefer the yellow rays, which are the brightest of the spectrum, they are, in fact, attracted, not by the colour, but by the brightness; that, while conscious of the intensity of the light, they have no power to distinguish colours. Given an animal which prefers the brightest rays, it may seem difficult to distinguish between a mere preference for light itself rather than for any particular colour. To test this, however, Sir John Lubbock took porcelain troughs about an inch deep, eight inches long, and three broad. In these he put fifty Daphnias, and then, in a darkened chamber, threw upon them an electric spectrum arranged so that on each side of a given line the light was equal, and he found that an immense majority of the Daphnias preferred the green to the red end of the spectrum. Again, to select one out of many experiments, he took four troughs, and covered one-half of the first with a yellow solution, half of the second with a green solution, half of the third with an opaque plate, and he threw over half of the fourth a certain amount of extra light by means of a mirror. He then found that in the first trough a large majority of the Daphnias preferred being under the yellow liquid rather than in the exposed half; that in the second a large majority preferred being under the green liquid rather than in the exposed half; that in the third a large majority preferred the exposed half to that which was shaded; and in the fourth that a large majority preferred the half on which the extra amount of light was thrown.

It is evident, then, that in the first and second troughs the Daphnias did not go under the solution for the sake of the shade, because other Daphnias placed by their side under similar conditions preferred a somewhat brighter light.

It seems clear, therefore, that they were able to distinguish the yellow and green light, and that they preferred it to white light. No such result was given with blue or red solutions. In such cases the Daphnias always preferred the uncovered half of the trough.

It is, of course, impossible absolutely to prove that they perceive colours, but these experiments certainly show that rays of various wave-lengths produce distinct impressions on their eyes; that they prefer rays of light of such wave-lengths as produce upon our eyes the impression of green and yellow. It is, of course, possible that rays of different wave-lengths produce different impressions upon their eyes, but yet that such impressions differ in a manner of which we have no conception. This, however, seems improbable, and on the whole, therefore, it certainly does appear that Daphnias can distinguish not only different degrees of brightnes, but also differences of colour.

[1] By Sir John Lubbock, Bart., M.P.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—Prof. Dewar commenced a short course on Chemical Technology in its relation to Organic Chemistry on April 23.

Mr. Sedgwick is lecturing on the Embryology of Mammals and Birds, and Mr. Caldwell on the Morphology of Gephyrea, Brachiopoda, Polyzoa, Chætognatha, and Larval Forms, practical work accompanying both courses.

Dr. Hans Gadow is lecturing on the Tegumentary and Muscular Systems of the Vertebrata.

Prof. Darwin's lectures on the Theory of the Potential will include an account of Gauss's treatment of those problems generally associated with the name of Green.

The Demonstrator of Mechanism is giving a course of Mechanics applied to the strains in winding, pumping, and blast engines, and in other machines. A practical class is being formed for instruction in Surveying.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, April 12.—"Introductory Note on Communications to be presented on the Physiology of the Carbohydrates in the Animal System." By F. W. Pavy, M.D., F.R.S.

My last communication (*Proc. Roy. Soc.*, vol. xxxii. p. 418) was entitled "A New Line of Research bearing on the Physiology of Sugar in the Animal System."

During the time which has since elapsed, I have been actively engaged continuing my investigations in the direction started, and the results obtained give an entirely new aspect to the whole subject of the physiology of the carbohydrates in the animal system.

Modern research has shown that, besides the well-known carbohydrate principles, such as sugar, &c., there are several dextrins distinguishable by their optical properties and their cupric oxide reducing power.

From the colloidal principle starch, which has no cupric oxide reducing power, principles (dextrins) are producible by the action of ferments possessing gradually-increasing cupric oxide reducing power until maltose is reached, which constitutes the final product, and which possess a little more than half the cupric oxide reducing power of glucose.

This is one foundation point connected with the researches I have been conducting upon the physiology of the carbohydrates in the animal system.

The other foundation point is that the various members of the carbohydrate group are brought into glucose by the agency of sulphuric acid and heat.

Proceeding upon these facts, and taking the cupric oxide reducing power before and after subjection to the converting action of sulphuric acid and heat, I have prosecuted investigations upon the transformation of the carbohydrates within the animal system with the result of acquiring knowledge of an altogether unexpected nature.

Hitherto what has been observed as regards the transformation of carbohydrates by the action of ferments and chemical agents, has been a change attended with increased hydration—for example, the passage of starch into the successive forms of dextrin and maltose and cane-sugar into glucose.

The issue of the researches, however, which I have been conducting recently, is to demonstrate the passage of carbohydrates exactly in the opposite direction by the action of certain ferments existing within the animal system.

Alike in the alimentary canal, the circulatory system, and the

liver, the conditions exist by which this kind of transformation is effected.

From the mucous membrane of the alimentary canal a ferment is obtainable which converts (1) glucose into a body possessing the same kind of cupric oxide reducing power as maltose; (2) cane-sugar into maltose, and not glucose as formerly asserted; and (3) starch either into maltose or a dextrin of low cupric oxide reducing power.

The presence of carbonate of soda modifies the action of a maltose-forming ferment, and leads to starch passing into a dextrin of low cupric oxide reducing power instead of into maltose.

The portal blood contains a ferment which possesses a maltose or a dextrin-producing power, and the contents of the portal system during digestion are charged with a notable amount of maltose sometimes, and at other times a low cupric oxide reducing dextrin.

After the introduction of glucose into the circulatory system, I have observed the presence of maltose.

The liver also contains a ferment capable, under certain conditions, of carrying glucose into maltose, and I have further witnessed, by the same kind of action as the sugars and dextrins are moved from one to the other, the conversion of a carbohydrate into the colloidal material belonging to the animal system (glycogen) which holds the analogous position of starch in the vegetable system.

Evidence has likewise been supplied that by an action of the same nature as that which moves the carbohydrates from one to the other in the carbohydrate group, they are, under certain conditions, carried into a body out of the group, and thence not susceptible of being brought into glucose by the converting action of sulphuric acid; and, on the other hand, under other conditions a substance is brought into the carbohydrate group, and its nature made recognisable by the converting action of sulphuric acid and its cupric oxide reducing power.

The subject as it even now presents itself is a large one, and I propose to deal with it in detail in a series of communications. The first will be devoted to that which refers to the alimentary canal.

Linnean Society, April 5.—Sir John Kirk, vice-president, in the chair.—Messrs. R. M. Barrington, G. E. Comerford-Casey, F. V. Dickins, and E. Cambridge Phillips were elected Fellows of the Society.—Mr. E. M. Holmes exhibited a specimen of birch-tree sap which had been found to exude from a cut branch one inch in diameter, at the rate of 4 oz. per hour during the night and 7 oz. to 8 oz. per hour during the day before the leaf buds had expanded, showing that the rapid rise of the sap was in this case not dependent on transpiration, but probably on endosmose accelerated by the expansion of the wood caused by solar heat. The sap had been collected and analysed by Dr. Attfield, and its contents recorded in the *Pharmaceutical Journal*.—There was exhibited for Mr. R. Morton Middleton a well-marked example of wood showing the extensive ravages of the Isopod, *Limnoria lignorum*. The wood was from the pier piles of West Hartlepool, where the said Crustacean's depredations are very destructive.—The Secretary read a paper on the indiarubber-tree of the Gold Coast, by Capt. Alf. Moloney. In this the author stated that the *Landolphia owariensis* grows extensively in the countries of Akim, Aquapim, and Croboe; and he strongly recommended the natives and traders of Lagos to encourage rubber as an article of trade instead of solely depending as at present on palm oil. He described the habit of the live plant, and the method employed in extracting the rubber therefrom.—Mr. F. W. Phillips in a communication described a new species of freshwater Infusorian allied to the genus *Gerda*. It was proposed provisionally to name the new form *G. caudata*. It was obtained at Hertford, and in company with the rotifer *Æcistes pilula*.—A paper was read on *Hemicarex*, Benth., and its allies, by Mr. C. B. Clarke; in this he gives a revision of the genera and species of *Kobresia*, *Hemicarex*, *Schœnoxiphium*, and *Uncinia*.

Zoological Society, April 3.—St. George Mivart, F.R.S., vice-president, in the chair.—The Secretary read some extracts from a letter he had received from Mr. J. Sarbo in reference to the Gayal. The writer observed that *Bos gaurus* (the Gaur) and not *Bos frontalis* (the Gayal) is the Wild Ox of Assam, and that the *B. frontalis* is not known in a wild state, but only as a semi-domesticated animal owned by various wild tribes from Assam to Arracan.—Mr. Sclater called the attention of the meeting to the skin of a Brown Crow from Australia, which had been sent to him for examination by Mr. Albert A. C. Le Souef, C.M.Z.S., and which he was inclined to regard as a variety in plumage of *Corvus australis*.—Mr. A. G. Butler read a paper containing an account of a collection of Indian Lepidoptera made by Lieut.-Col. Charles Swinhoe, chiefly at Kurrachee, Solun, and Mhow. Thirty-two new species were described, and numerous field-notes by Col. Swinhoe were incorporated in the paper.—Col. J. A. Grant read some notes on the Zebra met with by the Speke and Grant Expedition in the interior of Central Africa in 1860-63, which certainly belonged either to the true Zebra (*Equus zebra*) or to its closely allied northern form, the recently described *Equus grevyi*.

Meteorological Society, April 18.—Mr. J. K. Laughton, M.A., F.R.A.S., president, in the chair.—T. G. Bowick, E. C. Clifton, H. Culley, Dr. W. Doberck, A. N. Pearson, Prof. H. Robinson, and J. E. Worth were elected Fellows of the Society.—The following papers were read:—On cirrus and cirro-cumulus, by the Hon. F. A. Rollo Russell, M.A., F.M.S. The author points out that next to frequent readings of the barometer and a knowledge of the distribution of atmospheric pressure, observation of the character of clouds, especially of cirrus, is of the greatest use in attempting to forecast coming weather. Observation of cirrus can plainly be made use of in a telegraphic system of weather forecasts as easily as observation of the barometer, and the employment of a number of scattered cirrus observers largely increases the probability of this form of cloud being noted. The paper contains a description of twelve different varieties of cirrus, with the weather they signify or at least precede, as observed by the author during the last eighteen years.—Some notes on waterspouts, their occurrence and formation, by George Attwood, F.G.S. This contains an account of several waterspouts observed in the Pacific Ocean, and also one seen in the Atlantic Ocean. The author believes that the waterspouts in the Pacific Ocean were caused by a cloud heavily charged with cooled moisture drifting from the high mountains of Costa Rica coming into contact with air-currents and clouds travelling in a different direction, and of a warmer temperature; by which contact the cloud heavily charged with moisture was given a rotatory motion, causing it to discharge part of its moisture and make it assume a cylindrical figure and fall down by its own gravity.—Records of bright sunshine, by W. W. Rundell, F.M.S. This is a discussion of the sunshine records made in the United Kingdom during the years 1881 and 1882, from which it appears that there is more bright sunshine upon the coast than there is inland.—Note on wind, cloudiness, and halos; also on results from a Redier's barograph, by E. T. Dowson, F.M.S.—On the cold weather of March, 1883, by W. Marriott, F.M.S. The weather of this month will long be remembered for its very cold, dry, and windy character. The winter had been very mild, dull, and wet, and continued so to the beginning of March. A sudden change took place, however, on the 6th. A severe northerly gale set in on that day, accompanied with snow showers and a keen biting wind. This gale was most violent in the North Sea, and caused sad havoc among the fishing fleet on the east coast, no less than 382 men and boys being drowned. The temperature fell considerably, the maximum being below 40° almost all over the country, and in the North of England only a trifle above the freezing point. The same conditions prevailed for the next two or three days, the temperature however falling still lower, and on the 10th the minimum occurred in the central and northern districts. The most remarkable weather of the month took place from the 21st to the 24th. Owing to a brisk fall of the barometer over France an easterly gale was experienced over this country, and as the temperature was low and the air very dry the wind was exceedingly bitter and keen, and its effect upon the human frame was most distressing.

SYDNEY

Linnean Society of New South Wales, February 28.—C. S. Wilkinson, F.G.S., president, in the chair.—The following papers were read:—On the coal flora of Australia, by the Rev. J. E. Tenison-Woods, F.L.S., F.G.S., &c. This was a complete monograph of all the known fossil coal plants, including the new species recently discovered by the author. A diagnosis of each genus and species was given, together with a history of the subject and its literature. The author also added his own views with reference to the classification, in which he regards some of the Newcastle beds as Pervian, some as Trias, and the Ipswich beds (Queensland), the Victorian carbonaceous

(Bellerine, Cape Otway, Apollo Bay, Colac and the Wannon), Tasmanian (Jerusalem), and the Hawkesbury sandstone as Jurassic or Lower Oolite. He expresses a doubt whether the Wianamatta beds can be regarded as a distinct formation, his own opinion being that they are shales distributed at various levels all through the Hawkesbury sandstone. The new species of plants described are: *Phyllotheca concinna, Equisetum rotiferum, Vertebraria tivoliensis, V. towarrensis, Sphenopteris (Aneimoides) flabellifolia, S. (A.) f.* var. *erecta, Trichomanides laxum, T. spinifolium, Thinnfeldia media, T. australis, T. falcata, Alethopteris currani, Taeniopteris carruthersi, Gleichenia* (?) *lineata, Jeanpaulia bidens, Ptilophyllum oligonerum, Brachyphyllum crassum* (which the author thinks may be a variety of *B. manidare*), *Sequoiites australis, Walchia milneana, Cunninghamites australis*. Besides these new species, the following Indian or European fossils are new to Australia:—*Podozamites lanceolatus*, Lindley and Hutton; *Merianopsis major*, Feist; *Angiopteridium ensis*, Oldham. The monograph is meant to be a complete reference for students on the subject of Australian coal fossils, and is illustrated by six plates of heliographs and two of lithographs.—Further contributions to the flora of Queensland, by the Rev. B. Scortechini, F.L.S.—Descriptions of two new fungi, by the Rev. Carl Kalchbrenner. The species described are *Polyporus Pentzkei* and *Paxillus hirtulus*, both from the Daintree River, Queensland.—Notes on the fructification of the Bunya Bunya in Sydney, by the Hon. James Norton, M.L.C.—Descriptions of some new fishes from Port Jackson, by E. P. Ramsay, F.L.S.—The President read some notes on the Tuena Gold Reefs, by M. F. Rate, mining engineer.

BERLIN

Physical Society, March 16.—Dr. Frölich exhibited a torsion galvanometer prepared in Messrs. Siemens and Halske's establishment for measuring electricity mechanically, in which the deflection of the magnetic needle is indicated by the corresponding torsion of a spring whose constant expansion power is known. The torsion galvanometer was at first constructed for measuring the current of the large dynamoelectric machine fitted up in Ocker for copper electroplating, and which at least resistance possesses a power of 800 amperes. Here it was impossible to employ either a dynamometer, owing to the irregularity of the mercurial contact, or a tangent compass, which has to be directly inserted in the main circuit. Hence measurement could be effected only by lateral closing, and as Dr. Frölich fully explained, the determination of the potential at any required number of points in the circuit, as rendered possible by the new apparatus, gives the data for ascertaining the electromotor strength, the resistance, and the power of the current. He described in great detail the construction and adjustment of the new appliance, in which, after insertion of determined resistances in the lateral circuit, the number of volts can be read off, and from these the amperes and ohms determined in the simplest manner. The torsion galvanometer is prepared in two forms, vertical with a magnet suspended to a cocoa fibre, and horizontal with a magnet resting on an edge. The latter form is intended especially for cases in which the apparatus undergoes no delicate manipulation.—Prof. Neesen briefly mentioned modifications which he has introduced both in the heat regulator used by him and in his ice calorimeter, illustrating them with diagrams. He has found them work well in practice.

PARIS

Academy of Sciences, April 16.—M. Blanchard in the chair.—M. Jordan read a note on the works of the late Prof. H. Smith, and M. Bertrand added some remarks on the award of the mathematical prize.—Two new methods for determination of the right ascension of polar stars, and of the inclination of the axis of a meridian above the equator, by M. Loewy.—Memoir on the temperature at the surface of the ground and of the earth to 36m. depth, as also of the temperature of two pieces of ground, one bare, the other covered with turf, during 1882, by MM. Becquerel. This confirms previous results.—Graphic demonstration of a theorem of Euler concerning the partition of numbers, by Prof. Sylvester.—On the project of the interior African Sea, by M. de Lesseps. After a visit to the region, he affirms (with several associates) the urgency and feasibility of the scheme.—M. Wolf was elected Member in the Section of Astronomy in place of the late M. Liouville.—On the evolution of malignant pustule in man and its treatment with iodised injections, by M. Richet. So long as general infection has not commenced, by bacteria or their spores entering the blood, active local treatment with tincture of iodine is efficacious.—Experiments on caustic anæsthesia, and observation of a case of ulcerated tumour of the breast operated with the aid of this method, by M. Guérin. A space was cauterised round the tumour with Vienna caustic and incised throughout; then the tumour was detached.—Mechanical action produced by magnets and by terrestrial magnetism (second memoir), by M. Le Cordier.—Calculus of a double integral, by M. Callandreau.—Observations of the Swift-Brooks comet at Lyons Observatory, by M. Gonessiat.—Law of periods (continued), by M. de Jonquières.—On the groups of transformations of linear differential equations, by M. Picard.—On functions with lacunar spaces, by M. Poincaré.—On a generalisation of the theorem of Fermat, by M. Picquet.—On the heat of combination of glycolates and the law of thermal constants of substitution, by M. Tommasi.—On the liquefaction of oxygen and nitrogen, and on the solidification of sulphide of carbon and alcohol, by MM. Wroblewski and Olszewski. By making ethylene boil in vacuo, they obtained temperatures as low as −136° C. Liquid oxygen was obtained easily; it is colourless and transparent like carbonic acid; is very mobile and forms a very distinct meniscus. Sulphide of carbon freezes about −116° C. Alcohol solidifies (after being viscous about −129°) about −130°·5, forming a white body. Liquid nitrogen (colourless, and with visible meniscus) was obtained later.—Researches on phosphates, by MM. Hautefeuille and Margottet.—On artificial Hausmannite, by M. Gorgeu.—On the chloride of pyrosulphuryl, by M. Konovaloff.—On the difference of reactional aptitude, &c. (continued), by M. Henry.—Researches on the essence of Angelica of roots (*Angelica officinalis*), by M. Maudin.—Some effects of climate on the rapidity of growth of plants, by M. Capus. His measurements of various trees and shrubs in the botanical garden of Samarcand show the remarkable rapidity of growth there in April, May, and June.—Orientation of leaves with reference to light, by M. Mer. Certain parts of leaves (the border generally) receive the luminous impression, while other parts (petioles, motor-enlargements) perform the movements necessary to place the former in a favourable position.—Contribution to the experimental study of the elongation of nerves, by M. Minor. He supports the view that this stretching is a purely local operation, a sort of incomplete section of a nerve.—Experimental studies on the physiological action of iodoform, by M. Rummo.—New experimental researches on the physiological action of veratrine, by MM. Pecholier and Redier.—The synthesis of the heavens and the earth, by M. [illegible]. He deduces all from ether, first forming hydrogen. Universal gravitation is the direct effect of impulsions of ether.—A *frontal electric photophore*, for medical use, was described by MM. Helot and Trouvé. It is an incandescent lamp, supplied from a bichromate battery, and fitted with a reflector and convergent lens. It is attached to the forehead.

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THURSDAY, MAY 10, 1883

EDUCATION IN THE UNITED STATES

United States Report of the Commissioner of Education for the Year 1880. (Washington: Government Printing Office, 1882.)

ANOTHER valuable survey of education in the United States has been published, relating to the year 1880; a survey made by the Bureau whose duty and purpose, it is laid down, shall be "to collect statistics and facts showing the condition and progress of education in the several States and Territories, and to diffuse such information respecting the organisation and management of schools and school systems and methods of teaching, as shall aid the people of the United States in the establishment and maintenance of efficient school systems, and otherwise promote the cause of education throughout the country." The Bureau has no authority, it tells us, and seeks none, to interfere with school organisation, but aims to report institutions precisely as they are; and the variety of experiments tried in the States, which in kindred and spirit of government are so close to our own, must make this publication a repertory of experiences of the utmost value to the English educationalist.

The following is the immense provision made for education in the United States:—For public or common schools, every sixteenth section of public land in the older States, and every sixteenth and sixtieth in the newer ones: calculated to equal nearly 68 million acres; for seminaries or universities, two townships, or 46,080 acres in each State, and in some instances a greater quantity. An additional grant in 1862 of 30,000 acres for each senator which each State was entitled to send to Congress was awarded for the establishment and support of agricultural and mechanical colleges, amounting to 9,600,000 acres. Total, 78 million acres!

Yet, with this immense provision, the Old World difficulty is making itself felt strongly now in America as population increases, which was not foreseen when each State laid it down that education should be provided for every child, viz. that a considerable proportion of that population now will not avail themselves of this education. In very few States is the increase in scholars nearly in proportion to the increase in population, and our Report gives serious confirmation to the alarming statistics lately brought forward by the Rev. Joseph Cook in his Boston lectures. Private effort to attract children to school by providing them also with clothing is said now to be "occupying a very important place." Like other signs of "Progress and Poverty" which Mr. Henry George urges so warmly, there is now enough truancy and absenteeism from school to become a serious hindrance to education. In some New England cities truant officers are appointed, but in other cases the popularity of education without class-feeling allows the important business to be left in the hands of the police.

Cities (under which definition are enumerated 244 municipalities of above 7500 inhabitants) contain one-tenth of the teachers and one-sixth of the school population, and expend more than one-fourth of the money. "While the municipal systems of the United States are more defective, more assailed, and doubtless requiring greater efforts to reform them than any other part of the civil machinery, the city school affairs are in the main well systematised." The Boards of Education are variously constituted in different cities. In some cases the members of the Board are elected directly by the people; in some they are appointed by the Mayor; and in the District of Columbia by the Commissioners.

The powers of School Boards in the United States are in some instances restricted to the care and management of the public schools, while in others they extend to the charge of school funds. In nearly all the cities referred to above, superintendents are appointed—with few exceptions men of superior ability and specially adapted to the work of school supervision, who combine a great deal that is done in England partly by the Boards and partly by the Government Inspector at his occasional visits. They bring to bear more special knowledge of the subject than the former, and give far more time to each school than the latter can.

This Report contains a review of education through the decade, and perhaps the most striking thing is the absence of uniformity in the circumstances and changes in the different States. Thus, in Maine and New Hampshire during the last ten years, and in Indiana during the last year, population has decreased, but the attendance at schools has nevertheless increased. In Rhode Island, New York, and Iowa the reverse has taken place; the population has increased, but school attendance has decreased. In Arkansas a change of system made in the middle of the decade has resulted in a reduction of everything; the reduced number of scholars attending, however, having largely increased again the last year. In New Jersey and Pennsylvania there is an improvement every way; while in Massachusetts the attendance equals the school population. In Virginia the increase in every particular has been great. The Maryland schools only suffer from a decrease in the income for public school purposes.

The Report is very satisfactory as to the difficult matter of educating the coloured race. In 1870, out of 2,500,000 above fifteen years of age less than 150,000 had attended school. At the time of this present Report (1880) there is a total attending school of more than 800,000—over 15,000 of whom are, moreover, attending the higher grade schools. Those of them who are attending normal institutes for coloured teachers manifest great interest in the opportunities for improvement thus afforded. There is still, however, great deficiency of such trained teachers, and the poverty of the country is so great that the schools in rural districts are held in their churches, and the duty of assistance to them is urged by the Commissioner upon the national Government that has made them free. Considerable help has been given to the work among them by the Peabody fund, but the religious denominations of all the States have done most—in fact, five-sixths—of the work. Of 44 normal schools, 29 are under their auspices; of 36 institutions for secondary instruction, 31; 13 of the 15 universities or colleges; and all the schools of theology. But in all the States with mixed population now, except Delaware, Kentucky, and Maryland, school funds are devoted to school population without regard to colour.

In our crowded island we need not refer again to the

other special difficulty of the United States. The scattered population leads inevitably to small schools: in Maine, 1200 out of 4000 had average attendances ranging from two to twelve; this leads, of course, to low pay; and this to low attainments on the part of the teachers, of whom not more than 4 per cent. have had normal training. A great drawback to teachers also is the uncertainty of their tenure of office. In some States the School Committee have no power to hire teachers for more than a year, and engagements are seldom made for a longer time. In others, men are employed for winter and women for summer terms, thus causing an uncertainty in the profession, which must be highly mischievous. It is a feature in American education, that in both elementary and secondary schools more than half the teachers are women. In this respect the United States differs from every other nation; and a fear is expressed lest it may involve the sacrifice of some of the conditions essential to the development of strong self-reliant characters. As the Transatlantic ladies are supposed not to be wanting in these themselves, let us hope that it may not have such an effect; but that it may be said of this arrangement that—

"Emollit mores, nec sinit esse feros."

The Commissioner in his Report says that, "carefully considering the position of woman in the work of education, what she has done, and may do, as a teacher, what her nature and experience may fit her to do better than man, as an officer, inspector, or superintendent, he has favoured the opening of appropriate offices to her in connection with institutions and systems of instruction." He "regrets to say that women have shown more indifference to this opportunity than he expected." There are 227 women's colleges in which every advantage is offered that men have, but they are not popular. Still he points out that since women were elected in 1873 to the Boston School Board, and subsequently admitted to that and other Boards, the employment of them on sub-committees, for which they were best adapted, has been the introduction of a new force; in other words, it is in the line of progress.

The Report urges the desirability of well-trained teachers, more particularly in the case of scientific knowledge. "Such knowledge finds its application in all arts and industries, and in all measures for the preservation of health and life, and it offers the only means of dissipating the fears and superstitions, and correcting the foolish practices arising from ignorance of the phenomena and laws of nature." It points out also that the general Government is doing more in behalf of scientific work and publications than all the other agencies put together.

Partially, no doubt, the result of a feeling making its way among educationists, but partially also a sign of the moderate level of education reached, is the small number (448) in Ohio who learn Greek. A curious mark of changed relations is to be found in the fact, that still fewer (418) learn French; while nearly 100 times the number (40,813) learn German; against nearly 650,000 who learn spelling.

Where the ordinary primary education is good in America, evening schools of elementary grades are less sought after than those of advanced grade, except in cities where there is a large foreign population. In communities, distinguished alike for intelligence and business enterprise, evening high schools are especially appreciated, the most promising artisans and clerks looking to them for the means of continuing their studies.

The peculiarly American institution of summer schools is being turned to admirable use by teachers occupied with regular school duties during the rest of the year, who go with scientific expeditions and to stations maintained by the universities, and profit by the facilities for study and investigation thus offered them in combination with fresh air and change of scene.

There has been scarcely any increase since 1875 in the number of universities or colleges as they are indiscriminately called, but the new States are many of them overprovided with these higher-branch schools, while deficient in the elementary schools at present more necessary. The disproportion between colleges and preparatory schools in certain States may be judged by the report that while Tennessee has twenty-one colleges, Massachusetts with a larger population reports seven. The former State has two preparatory schools; the latter twenty-three. Under such circumstances it is not strange that some of these universities or colleges should be doing the work of the lower grade schools, as thirteen are reported as doing only.

In 1871 Arkansas established an industrial university which soon after possessed classical, agricultural, engineering, commercial, and normal courses, and a preparatory department. In various other States similar centres of education in practical subjects were opened, and the variety of subjects and arrangements for teaching them, which are to be found in so many independent centres, will be found very instructive to all who are inquiring about technical education, especially agricultural. Several of these courses are such as have been approved of after varied experiment; an advantage which they have over the schools of science not endowed by the national grant, where the will of the founder has had a contrary effect.

Michigan University has inaugurated an excellent work in providing that a faculty will visit once every year any public high school in Michigan on request of its School Board, and report its condition. "If the faculty shall be satisfied that the school is taught by competent instructors and is furnishing a good preparation for any one or more of the regular courses of the University, then the graduates from such preparatory course or courses will be admitted to the freshmen class of the University without examination, and permitted to enter on such undergraduate course as the approved preparatory work contemplated." This is a method of making the same labour serve the double purpose of inciting the school to efforts, and also of matriculating the University students. The matriculation examinations at so many of these universities were naturally of most various standards, and some approach towards a uniform standard has been made between ten principal colleges in New England.

In the Illinois State University a peculiar government has been tried called "The Students' Government," by which every official was selected or appointed by the president whom they had elected, and all the forms of a Republican Government are gone through; forming an excellent practice to the students and probably raising a good *esprit de corps*.

The comparison of the state of medical instruction at

the present with what it was ten years ago, although showing great improvement, still draws a discreditable picture of what so important a profession is allowed to remain in America; and quite a romantic tale is told of the means by which men getting a living by selling false degrees were brought to justice. The number of the universities and other bodies which claim the right to bestow degrees makes the tracking down such forgeries very difficult.

The business of nursing the sick is rising to its proper position as that of an intelligent assistance to the profession of medicine. Our Report wisely recounts the good results to be gained by student-nurses, though chiefly moral qualities are inculcated.

In the schools of science, the number of students which increased so largely in 1878, but fell off in 1879, has begun to increase again; the number of institutions as well as teachers having increased steadily all the time. Our Report says:—"The multiplication and growth of schools of science has been a marked feature in the recent history of education in America. Either the stimulus given to them by the national aid, or the sentiment which compelled Congress to give help to higher education, has carried forward and deepened the interest in industrial, scientific, and technical instruction. Students are now more frequently choosing lines of study which lead to a life of business activity or to prominent positions in industrial pursuits. Colleges that a few years since held strictly to a rigid classical course are feeling the new impulse and are striving to add to their efficiency by making provision for special instruction preparatory to definite occupations. Men of wealth are endowing schools of science and technology more richly than other institutions; for they believe that the practical education which has now come to the front will do more than anything else to promote the industry and prosperity of individuals, and to utilise the resources of the nation."

The requirements for admission to the scientific department of colleges and schools of science are not so great as to classical collegiate courses.

It is rather curious that the study of Latin is allowed to be dropped in a law school of Harvard; but the following remarks made upon the value of law schools, as compared with that of articling pupils to lawyers, may well be applied not to them only but to all technical instruction:—"In schools systematic training is received. Less opportunity is afforded for desultory and spasmodic reading. Regular habits of study are required. Examinations to be passed give steadiness and thoroughness to the work. Companions make emulation. The desire for the respect of the professors is a further stimulus to faithfulness, and they are ready to aid in the understanding of intricate questions. Underlying principles are given an attention which corresponds to their relative importance."

Forestry is taught in some of the higher institutions, with plantations of trees arranged in their natural orders; and its value is pointed out, both as a branch of knowledge to the students, and also as adding to the knowledge of the range of possible and profitable cultivation of many species.

A system of teaching the deaf and dumb to read from the lips of others instead of the old finger reading is described as wonderfully successful and fast gaining ground.

Not a small advantage will science gain if the system of making full inquiries into the antecedents of every case of the above, as also of blindness, is patiently and thoroughly carried out. Some generalisations have already been made with regard to the latter. In the case of 100 feeble-minded scholars their weakness is traced to consumption in their stock. An inquiry into colour-blindness in the Boston schools leads to the recommendation that a systematic process of giving instruction in colour, its names, and shades, should be introduced into primary schools.

The importance of reform schools is steadily and strongly upheld. The needs of their inmates are wisely consulted by an education more moral than intellectual being instilled into them, and by a knowledge also of some method of gaining a living when dismissed being carefully given to them. The better feelings are drawn out and encouraged by a system of rewards for all good conduct, instead of only punishments for bad. Two curious observations are recorded: one is, that working among flowers has a softening tendency upon such characters; and the other, that prisoners are, in general, singularly short of mathematical ability.

The increase in the number of free libraries since the previous year's Report alone nearly equals the entire number of them in England, making a total nearly reaching 3000. Though many of these are very small and to be compared with school libraries here, yet they average all through 4000 volumes in each. A large increase also is noticeable in Kindergarten schools, in schools for nurses, in deaf and dumb, orphan, and reform schools.

The Bureau is indebted to private enterprise for a competition on the subject of schoolhouse plans organised during the year by the "Plumber and Sanitary Engineer." It has drawn forth from the committee of award a sketch of the qualifications they believed to be necessary for a public school building in a large and densely populated city. They lay down ten primary requisites which every plan ought to contain; and the Commissioner hopes that an impulse has been given by their report, which will not be lost or wasted.

Education, we are told, has become in every section of the country a matter of more active public interest than usual. City and country papers have given a place in their columns to the subject, besides periodicals discussing them. It is rather curious to us in aristocratic England to find not selfishness and stupidity only but demagoguism also charged with creating discouragements!

W. O.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

The Microphone

IT is probable that the writer of the note at p. 588 has not had an opportunity of seeing the paper of mine to which he refers, and an abstract of which is given at p. 376 of the present volume of NATURE.

The adhesion between metallic contacts consequent upon the passage of a current has been carefully investigated by Mr. Stroh, who observed it in the case of all of a great number of metals with which he experimented. My first observations on the subject (one of which is mentioned in the paper) were made with the refractory metal platinum, and not with bismuth, as the writer of the note seems to infer; and though Mr. Stroh's explanation—that the adhesion is due to fusion—is quoted, I express no opinion of my own on the matter. Whatever may be the cause, it seems evident enough that such adhesion must necessarily be detrimental to the perfect action of a microphone, though I am not aware that attention has been previously directed to this point.

It is not correct to attribute to me the opinion, as stated in the note, "that the heat generated by the current plays an important part, for in carbon this reduces the resistance, whilst in metals it increases it." On the contrary I give reasons for believing that at least a moderate degree of heat increases the resistance of loose carbon contacts. Increased *current*, however, is accompanied by diminished resistance, and while I am not prepared to say that heat plays no part whatever in the matter, it appears to me probable that the effect is mainly owing to some other incident of the stronger current, *e.g.* greater difference of potential.

My experiments on metals were not, as the writer supposes, entirely confined to bismuth. More than a hundred observations were recorded of the resistance of platinum and copper contacts under different conditions, and some of these are referred to in the paper. Owing, however, to the low specific resistance of these metals, the methods which I had applied with success in the case of carbon were found to be unsuitable, and the results obtained, though not on the whole inconsistent with those yielded by bismuth, were unsatisfactory and inconclusive. Bismuth was chosen for the bulk of the experiments, principally on account of its bad conductivity, which renders changes in the resistance of the contact easier of observation; but since it was my object to contrast the behaviour of metals with that of carbon (which is infusible), its ready fusibility is another advantage. If I had desired to make a good metallic microphone, I should probably have thought with the writer of the note that bismuth was "the very metal which ought to have been avoided." But for experiments conducted with the object of ascertaining the causes of the generally recognised fact that metals, as a class, are inferior in microphonic efficiency to carbon, it is evident that the metal which gives the poorest microphonic effects is *the very one which ought to be selected*, on account of the probability that with such a metal these causes would be most conspicuous.

As a matter of strict scientific exactness I agree with the writer that "no conclusion of any value as to metals in general can be drawn from experiments on bismuth alone." But since the physical properties with respect to which bismuth differs from carbon, and which have any probable connection with microphonic action, seem to be common in various degrees to all metallic bodies, I venture to predict with tolerable confidence, that if the experiments described in the paper are repeated with suitable apparatus, it will be found that all the conclusions arrived at with regard to bismuth (as summarised in the abstract before referred to) are also true to a greater or less extent for any other ordinary metal.

SHELFORD BIDWELL

Wandsworth, April 22

[The necessary brevity of the note to which Mr. Bidwell refers precluded lengthy quotations. At the same time it was only natural to draw attention to the weak point in Mr. Bidwell's argument, namely, that the behaviour of the metals generally could not with any certainty be argued from observations made, as Mr. Bidwell admits, on the very metal which for practical ends ought to be avoided. It is greatly to be wished that Mr. Bidwell will so far further improve the capabilities of his apparatus as not only to be able to get conclusive results with other metals, but also so as to enable him to say why his apparatus gave results that were unsatisfactory and inconclusive with good conducting metals such as platinum and copper.]

The Soaring of Birds

FOR more than twenty years I have watched with admiration the soaring of the black vulture of Jamaica (*Vultur aura*). When once well up in the air it rarely moves its wings, except to change the direction of its flight. It can soar wherever there is even a light wind.

I entirely concur with Mr. Hubert Airy in the main point of his general conclusion, as given in vol. xxvii. p. 592. "Variations in the strength and direction of current" can, as he says, be so "utilised" by birds as to enable them to soar. But a high wind is not necessary; and a downward current, even when approaching the perpendicular, may, if of sufficient velocity, be utilised.

Whenever there is a wind there will be ascending and descending currents in some places. This will be so even in a level plain which presents no considerable obstacles, such as trees or buildings, to the stream of air. The plain will be bounded by hills of varying height, and it will vary in breadth. A stream of water would merely flow more rapidly through the narrower channels; but a stream of air, being highly elastic, will also rise and fall, and it will have its eddies in planes more or less inclined to the horizon, and will often acquire a rolling motion. Assuming the existence of ascending and descending currents, the soaring is a very simple matter. *The bird contrives to remain much longer in the upward currents than in the downward.* It will glide along the ascending side of a wave of air and cut across the descending side. It will make many spiral turns in an ascending current of sufficient amplitude. I have often seen the vulture ascend thus for more than 2000 feet, keeping near a steep mountain side. If the bird encounters a descending current, of which it is instantly aware through the diminished pressure on its wings, it will either wheel to the right or left to get out of it, or, altering the pitch of its wings, will descend swiftly so as to acquire the necessary impetus for a rapid escape, or will do both.

It can also avail itself of inequalities in the velocity of horizontal currents flowing parallel to one another at the same elevation. The bird, let us suppose, encounters a strong horizontal current, as warm as it is rapid, issuing from a mountain valley or a cutting through a forest. Instantly throwing its wings into a plane nearly vertical, it receives on them the force of the current, and in a few seconds acquires its velocity. Pitching its wings also for a downward flight it shoots quickly through the current, having acquired a speed more than sufficient for the recovery of its original elevation. If the current be very strong and very narrow, it need not be horizontal, but may approach the perpendicular. The bird will not remain in it long enough to be carried far down, while it acquires an impetus much more than compensating for the slight loss of elevation. It must be remembered that when the bird is gliding at a high rate of speed, the resistance of the air, through its inertia, to any movement except in the plane of the wings, almost equals that of a solid body, and a change of direction causes a very slight loss of momentum.

What rapidity of currents is necessary for soaring must depend in great measure on the structure of the bird. The vulture is, I believe, comparatively heavy, but I think that, having once acquired speed by a short and steep descent, it can glide through still air (or at right angles through air having a uniform horizontal notion) at the rate of twenty miles an hour, descending not more than one in twenty. If, therefore, the bird could be always in an upward current of only one mile an hour, it could maintain itself in the air. A gentle breeze of ten miles an hour, with one mile an hour of ascent—and a much steeper ascent than this must be frequent enough where there are hills—would suffice to sustain the bird; and as an average of ten miles an hour implies local or occasional gusts of greater velocity, of which the bird knows how to avail itself, it could ascend in such a current, and so be able to work to windward. If besides hills of moderate inclination, there are also trees, walls, houses, the air will often be driven upwards, vertically or nearly so, with as great or even greater speed than that of its average horizontal movement; and of this upward movement the birds avail themselves most skilfully. I have frequently seen the vultures working their way thus against a high wind. Their movements are very irregular. Sometimes, to avoid a violent gust, they will drop almost perpendicularly to within a yard or two of the ground, and shooting abruptly sideways with the high velocity gained by the drop, will get into an upward current in which, if ample enough, they will wheel, or else will cross and recross it, till they have gained a sufficient elevation, and then, taking advantage of a lull, will glide to windward.

With a breeze of only five miles an hour, there will be in many places upward currents of high inclination caused by the usual irregularities of surface. Keeping sometimes in these and sometimes in currents more slightly ascending, for, say, two-

thirds of its time, and utilising also, as I have above explained, the more rapid of the descending currents, the bird can more than sustain itself. It can at will glide to windward at the rate of fifteen miles an hour against the breeze, losing of elevation only one in twenty. R. COURTENAY

L'Ermitage, Hyères (Var), France, April 28

Flight of Crows

I CAN corroborate the observation of Mr. Murphy as to the oblique flight of crows. When I have seen them so flying there has always been a cross current, and they have merely kept their heads a little to the wind.

Cambridge THOS. MCKENNY HUGHES

Sheet Lightning

Du choc des opinions jaillit la vérité. I still adhere to your assertion that sheet lightning is not, at least in most cases, the mere reflection of a common but distant storm. On the highlands of Ethiopia, in the years 1842 to 1848 I was diligently engaged in investigating the electrical phenomena so frequent in that region. The details of my observations were printed in 1858 by the French Institute, and I have published again my results in my "Observations relatives à la Physique du Globe" (Paris, 1873). The following cases may be of interest:—

Near the zenith eight successive flashes of lightning were seen 21 seconds before their thunder, which lasted exactly 12 seconds. Another day it lasted 24·4s. thirty successive times, and, as previously, without any rain. My greatest observed interval was 11″·2s., corresponding to a distance of 38,500 metres, &c.

I have seen more than once straight or zigzag lightning unaccompanied by thunder. One afternoon it went to and fro *twice* between two horizontal cloud banks, and ended in sheet lightning which illuminated, not the lower dark bank, but only the under surface of the upper cloud. I have observed frequently thunder without lightning and lightning without thunder.

When in Adwa I recorded silent sheet lightning towards Gondar, 240 kilometres distant, where a violent storm was raging at the same time. Before leaping to a hasty conclusion, let us hear a case bearing pointedly to the opposite opinion: in 1845, at Saga (latitude 8° 11′), a semi-transparent fog which had mantled over the valley, and could not be more than 3500 metres distant, gave out a flash of sheet lightning without thunder.

Although my numerous observations have given me a strong bias in favour of your opinion, I do not wish to impose it on reluctant philosophers, but suggest the following system to clear up the question:—Let two observers, A and B, 40 or 50 miles asunder, mention instances of lightning seen in each other's true bearing. If they can also secure the help of a third observer located on or near the straight line from A to B, and who can watch in two opposite directions, many important results may be obtained. ANTOINE D'ABBADIE

Paris, May 5

The American Trotting-Horse

MR. BREWER'S memoir on the evolution of the breed of the American trotting-horse (NATURE, vol. xxvii. p. 609), and the statistical tables that accompany it, are full of interest, but I only propose now to concern myself with the latter, which may be easily and usefully discussed by employing a statistical method that I have long advocated. In explanation I will begin by extracting the final terms of four of the lines of his table, as follows:—

Year.	2·27 or better.	2·25 or better.	2·23 or better.	2·21 or better.	2·19 or better.	2·17 or better.	2·15 or better.	2·13 or better.	2·11 or better.
1871	99	40	17	12	6	1			
1874		98	40	16	11	5	1		
1877			105	51	19	8	2		
1880				106	41	14	6	2	1

The meaning of these entries are, that in the year 1871 there were 99 horses that could trot a mile in 2 minutes 27 seconds, or less; that in the same year there were 40 that could trot it in 2 minutes 25 seconds, or less; and so on. Their significance is that the rate per mile of the hundred fastest American trotting-horses has become 2 seconds faster in each successive period of 3 years, beginning with 1871, and ending with 1880; also that the *relative* speed of the hundred fastest horses in each year is closely the same, though their absolute speed differs.

We may read the table in another way. If the number of horses that can run a mile in 2 minutes 27 seconds or less is 99, we may infer without risk of sensible error that the 99th horse in the order of running accomplishes a mile in *that time exactly*, because the 100th horse certainly takes a longer time, and it is statistically incredible that the rate of the 99th and of the 100th horses should differ by more than a barely perceptible interval. For the same reason we may infer that the 40th horse in that same year runs a mile in 2 minutes 25 seconds, and so on. We can now draw curves, and by graphical interpolation find with the greatest facility the mile rate of the horse in *any* order of running in any year that we please to select. I have selected the 100th, 50th, 20th, and 10th horse respectively for each year beginning with 1874, when we are informed that the returns first begin to be accurate, and have thrown the results into the following simple table. The curves obviously required a little smoothing here and there, and in three or four places the readings have been thereby modified by one or two tenths of a second. Otherwise they are given directly from the rough plottings.

Number of Seconds and Tenths of Seconds in Excess of Two Minutes that are required for Running One Mile by the Horses whose Order in the Rate of Running in each Year is given at the Top of the Columns

Year.	100th.	50th.	20th.	10th.
1874	25·1	23·4	20·5	18·8
1875	24·1	22·5	19·9	18·2
1876	23·5	21·6	19·5	17·7
1877	22·9	21·0	19·0	17·4
1878	22·1	20·2	18·5	17·0
1879	21·3	19·6	18·0	16·6
1880	20·8	19·3	17·6	16·0
1881	20·4	18·8	17·2	15·7
1882	19·9	18·4	17·0	15·4
Anticipated 1890	16·8	15·5	14·4	13·4

Mem.—The first horse runs the mile in about 5 or 6 seconds less than the tenth horse.

It will be found on plotting the figures in the vertical columns into curves, that they run with much regularity and differ little from straight lines. The general conclusion to be derived from them is that the improvement of the running shows as yet little tendency to slacken, though no doubt if the number of horses bred for trotting ceased to increase yearly at the same large rate as hitherto, it might do so. Supposing, however, the conditions to be maintained, I should anticipate that in 1890 there will be about 15 horses that will run a mile in 2 minutes 15 seconds or less, and that the fastest horse of that year will run a mile in about 2 minutes 8 seconds.

FRANCIS GALTON

The Shapes of Leaves

MR. GRANT ALLEN'S papers in NATURE will evidently serve to direct attention to a most interesting subject which hitherto appears to have been much neglected. Every contribution of observed facts may tend to throw further light upon it, and I therefore venture to remark that one cause of the frequently filiform character of the leaves of water-plants appears to be the elongating action exercised upon the cells by the pressure of a rapid current of water, since it is obvious that growth must take place in the direction of the least resistance. With a radiate-veined leaf the tendency must be towards lateral pressure, which would compress and elongate, and so give a linear form to the leaf-cells. I have been much interested to observe that on the seashore, in places where Fuci are exposed to this action by the ebbing tide, as when growing on the edge of a large boulder or hanging over its sides, the fronds and even the receptacles become unusually elongated.

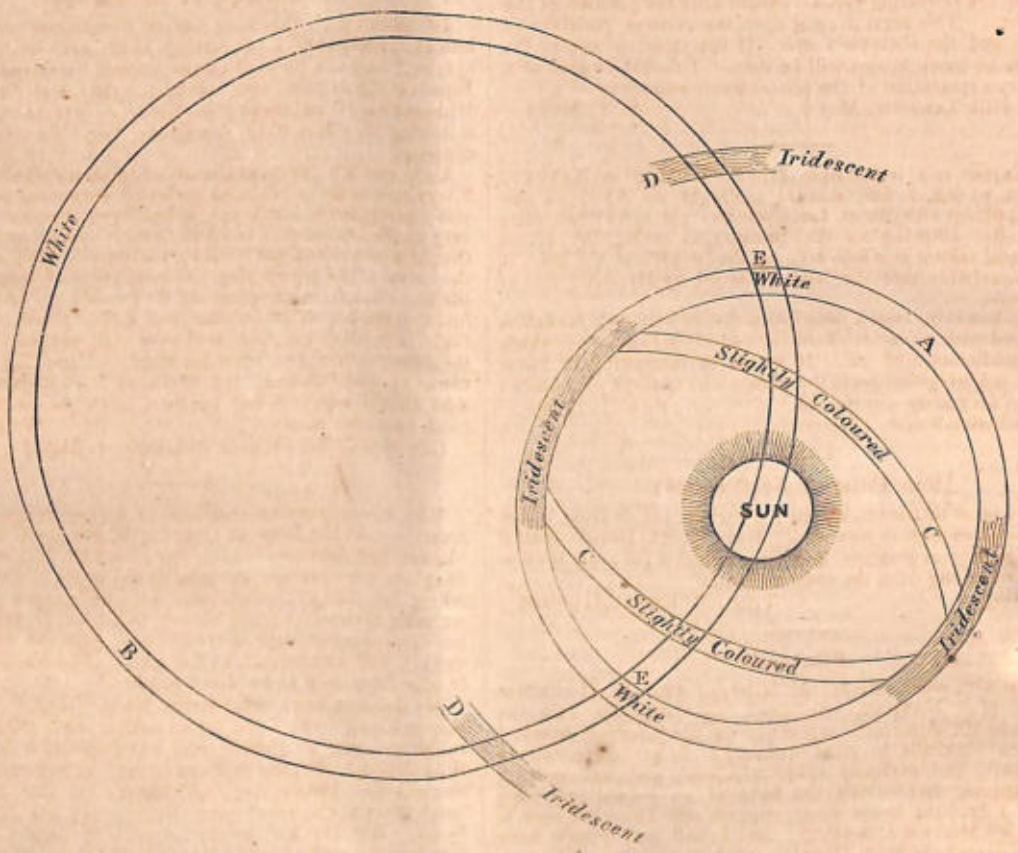
On the other hand, where a freshwater stream mingles with the salt water in pools left by the tide, and the endosmotic action of the water set up by its reduced density is greater, the algæ become broader if flat, or of more inflated character if tubular. This is well seen in *Dumontia filiformis*, *Enteromorpha intestinalis*, and *Chondrus crispus*. The influence exerted by the character of the surrounding medium and pressure may also be observed in that interesting genus of freshwater plants, Callitriche.

E. M. HOLMES

Solar Halo

I BEG to forward herewith diagram and remarks of solar halo as observed here to-day, thinking it may be of interest, being an unusual phenomenon. The cause depends upon many circumstances necessary for such observations, chiefly a calm reflecting surface of water in front, behind, and around the observer, making their appearance local as well as unfrequent.

The halo marked A, caused by the sun's rays passing through the thin cirri, was reflected from the surface of the water on the English Channel side of the island, producing the large white halo B, and passing over the sun's centre, the non-concentric arcs C being most probably reflected from the harbour side, the bright iridescent arcs at D on the large white halo B being the reflected crossings of the two halos at E. I have fixed the points of the zenith and due south horizon as approximately as possible; the observer facing due south, the iridiscent arc D was nearly



Solar Halo as observed at Portland, Dorset, April 28, 1883.

vertical, and about the same distance from the sun's centre to the estimated centre of large halo B, viz. 12°; the diameter of halo B was a little over double that of A.

Latitude of Verne ... ... 50° 32′ 86″ N.
Longitude of Verne ... ... 2° 23′ 40″ W.

Altitude of highest point of Verne 495 feet above sea-level (Ord. B.M.).

Time (local): first observed at 12.20 p.m.; brightest aspect 12.30 p.m.; duration about three-quarters of an hour. Cloud, thin cirri, with cumulo-stratus low in northern horizon. Amount, 9. Wind, S.

E. CARDWELL,
Late Supervisor Meteorological Department, Bombay
Verne Citadel, Portland, Dorset, April 28

I SEND inclosed a diagram of a system of solar halos observed here on Saturday last. If one may credit the oldest inhabitant, the phenomenon is very rare in these latitudes; in fact the ancient mariners frequenting the New Key End declare they never saw the like in all their wanderings.

My attention was first called to it at 12.25 p.m., when it presented the appearance I have depicted; but I am told that earlier in the day a white halo was seen *south* of the sun. The smaller circle had the sun for its centre, round which the sky was of a leaden cast as far as the edge; the fringed portions represent brilliantly coloured partial halos, or coronæ. The larger circle was, as near as I could guess, 40° diam. Its circumference cut the centre of the smaller circle, was brilliantly white, perfectly defined as seen from here, and narrow. I am told that, seen from high ground some four miles from here, it presented the appearance of two horns. The phenomenon lasted after I observed it about an hour, during which time a peculiar haze was drifting over the sky, which, when viewed carefully, seemed to have a hair-like structure, especially when seen passing the bright edge of the larger halo. I may add that the surface wind was southerly, the drift of the haze S.S.E.: a line joining the centres of the circles would point E.N.E. at about one o'clock.

THO. B. GROVES

Weymouth, April 30

Mock Moons

THE explanation of the phenomenon observed on the 16th ult., which is given by "Sm." of Rugby, appears to me to be scarcely satisfactory. According to the usual explanation of halos, parhelia, and paraselenæ, which attributes them to refrac-

tion by prismatic ice-crystals at high elevations, the parhelia always appear on the horizontal parheliacal ring which passes through the centre of the sun, and generally at the intersection of this ring with the vertical halo. The two parhelia must therefore always lie in a line parallel to the horizon, and at the same elevation as the sun itself. The same laws regulate the appearance of the paraselenæ or mock moons. It therefore surprised me to learn that the left-hand mock moon appeared at a greater distance from the horizon than the right-hand one. It seemed to me to be "unaccountably out of place." That the circle should have subtended an angle of 50°, as stated by "Sm.," is in itself unusual. The normal diameter is understood to be from 44° to 47°. Did "Sm." actually measure it? To my judgment it was considerably more than this, but of course mere estimates are not trustworthy. I do not see how a "change of level of the refracting cloud" should alter the position of the mock moons. This must depend upon the relative positions of the moon and the observer's eye. If the cloud is not in the right place no mock moons will be seen. I should be glad of a satisfactory explanation of the phenomenon recorded.

Birstal Hill, Leicester, May 7 F. T. MOTT

REFERRING to a letter from Mr. F. T. Mott in NATURE, vol. xxvii. p. 606, I find that at midnight on April 16 the moon's apparent altitude at Leicester was not more than 26°; so that after allowing for the difficulty of seeing the actual horizon, and taking also into account the breadth of the halo, it seems improbable that the halo observed by Mr. Mott was of unusual size.

I have, however, seen a description somewhere of a *parhelion* measured with a sextant about the end of last century—which had a semidiameter of 26°. It would be interesting to know whether such irregularities in the dimensions of these phenomena have been accurately ascertained. R. C. JOHNSON

19, Catherine Street, Liverpool

Sun Pillar of April 6, 1883

IT may be of interest to record the various points from which the above phenomenon was seen. I was at St. David's with a party of geological students, and we watched it for some time as we were returning from the coast at sunset.

Cambridge THOS. MCKENNY HUGHES

Fibreballs

I READ with much interest the letters of Prof. G. H. Darwin and "J. H.," NATURE, vol. xxvii. pp. 507, 580. On the coast of South Australia, especially on the Coorong beach, I have seen fibreballs in great quantity; some larger than a cricket ball, and perfectly spherical, hard, and well-matted; others tapering and having the form of an exceedingly long ellipse. I brought home many specimens. These are now in the Wragge Museum at Stafford; and I shall be happy to have some forwarded for Prof. Darwin's inspection.

Fort William CLEMENT L. WRAGGE

Helix pomatia

ONLY a few more lines to say, in consequence of the communication of Mr. Stokoe in your last number (p. 6), that he will find the mollusca in their geological relations treated in the introduction to my work on "British Conchology," vol. i. p. cix. The distribution of *H. pomatia* in this country and on the Continent is noticed in pp. 177 and 178 of that volume, and in the supplement to the fifth volume. J. GWYN JEFFREYS

1, The Terrace, Kensington, May 4

I HAVE found this freely in the hedge-bottoms of Hertfordshire lanes, where the soil was a dark alluvial mould, certainly not cretaceous. I suspect that even in its known localities it is very local. HENRY CECIL

Bregner, Bournemouth, May 5

IN two of the localities mentioned for this snail—Dorking, Surrey; and Woodford, Northamptonshire—there seems some reason to suspect it to be a modern introduction. From 1849 to 1852 I lived within two miles of Woodford, and often found the shells in a small wood known as Woodford Shrubbery. It was commonly said in the neighbourhood at that time that the snails were brought from abroad by the gentleman—I think General Arbuthnot—who had formed the Shrubbery some thirty years before that date.

I also found, many years ago, shells of the same species about the foot of Box Hill, near Dorking, and was told by a former resident in that neighbourhood that the snails were brought from Italy by Mr. Hope, of Deepdene, who was well known in the early part of this century as a writer on the mediæval architecture of Italy. I give the statements for what they may be worth.

Loughton J. C.

Intelligence in Animals

IN addition to the long list of "emotions which resemble human intelligence as occurring in animals below the human species," as given by your correspondent on the authority of Dr. Romanes (NATURE, vol. xxvii. p. 580), and the instance of "benevolence" subsequently cited, I venture to submit the following as illustrating something very like the emotion of contempt.

Until recently our domesticated animals included two cats—one a very fine tabby (a trimmed male) of somewhat morose nature, and a pretty little black cat, a half-bred Persian (a female) of very gentle character. On a noticeable occasion the tabby cat caught a mouse and ate it all up with much relish in a corner of the room. The proceeding was watched with much interest by the black cat from her place on the hearthrug. After the tabby had finished his repast he also took up his place on the hearthrug. The black cat then went over and smelled the spot where the dainty morsel had been devoured. Upon this the tabby cat came up and "boxed" the black cat's ears once or twice, as who should say, "What business have you with my affairs? catch your own mice!" W. R. HUGHES

Handsworth Wood, near Birmingham, May 5

MAY I contribute another case of higher thought in the lower animals. At the farm of Granton Mains, near Edinburgh, an old cat had become blind; her daughter had kittens. The daughter was observed bringing in a sparrow to the boiler-house, where her blind mother and her half-grown kittens were warming themselves: the kittens came up to get the sparrow, but their mother kept them off and gave the sparrow to her mother, and watched whilst she ate it. She was frequently seen to give other food to her blind mother.

My children have a fox terrier bitch, "Dot." Dot loves to kill anything from a cat to a mouse, and sometimes a wild rabbit gets into the garden, and it is a red-letter day for Dot and the children. But the children have also tame rabbits; of course any one who knows dogs will understand that it is simple to teach them not to touch pets—for instance, the cat of their own house. But Dot had a curious case to decide. The children had found a nest of wild rabbits, and two of the tame rabbits (black and white) had made a hole in a bank and there had young ones. This nest was respected by Dot. The children took the young wild rabbits (gray) and fostered them on the tame ones by slipping them into the nest. A few days after this, Dot must have discovered these gray young ones with the black and white. Had she found them anywhere else, one snap, and they were dead; but this was the line she took: she was found at the front door under the porch with one of the young gray rabbits, quite fifty yards from the nest; it was quite unhurt, although it died afterwards, I believe from cold and exposure at the time. Are we to suppose that Dot wished to ask the question, "May I kill this gray one?" DUNCAN STEWART

Knockrioch, May 2

THE SOLAR ECLIPSE OF 1883

THIS eclipse, as our readers have already been made aware, took place on Sunday last, and we may hope, although we shall not know for more than a month, that the weather was favourable. We shall not hear whether the French arrived in time, but we do know that the English observers met the American party, consisting of Prof. Holden, Dr. Hastings, Mr. Rockwell, Mr. Preston,

	SIDEROSTAT.					EQUATORIAL.				PHOTOHELIOGRAPHS.	
Time.	Hilger.	Rowland grating. 1st order.	Rowland grating. 2nd order.	Prismatic camera.	Slit Spectroscope. 2 prism.	7 prism. F.	Grating. F. Red 1st order.	Grating. F. Blue 2nd order.	¼ in. slide. Dense prism F.	Large photoheliograph.	Corona camera.
Before Totality											
Minutes.											
10						expose	expose	expose			
9											
8									expose		
7									expose		
6	ref. spectrum 30 secs.								expose		
5									expose		
4									expose		
3							expose	expose	expose		
2						expose			expose		
Seconds.											
60									expose		
40		expose	expose				run ¼ in.				
20		expose	expose								
2	expose & start clock										
1											
Totality		expose	expose	expose col. plate	expose	expose	expose	expose	expose	expose	expose 1 sec.
280											expose 20 sec.
230				shut							
220				expose gel. plate							expose
210									expose	expose	
200		expose	expose								
120				shut							
110				expose col. plate							
100											
90											shut
70											expose 3 sec.
50										expose	
40											expose 10 sec.
20											expose 2 sec.
Just before end		expose	expose	shut	shut	expose	expose	expose	expose	shut	
After Totality											
Seconds											
1							run ¼ in.				
4		expose	expose								
10		expose	expose								
Minutes											
1		shut	shut				expose	expose	expose		
2	shut										
3									expose		
5						expose	expose	expose	expose		
7									expose		
9									expose		
10						shut	shut	shut	shut		
	refs. 25				2 sec	10 sec.	10 sec.	10 sec.	1 sec.	run	run

Lieut. Brown, and Mr. Upton, the first mentioned astronomer being in charge, at Panama. They expected to arrive at Callao on the 20th March last, and to leave either in the *Hartford* or the *Pensacola* within the next few days. That would give them ample time to reach the Caroline Islands, and make the arrangements necessary for the observation. It was the intention of Prof. Holden to take the combined English and American party on to Flint Island if he found that Dr. Jannsen had already established his party on Caroline. This, of course, was a very proper decision, as it would double the chances of favourable weather. We give the time-table for observation supplied to the English observers, which they were instructed to carry out down to its most minute detail, if all the instruments were landed and set up without damage.

It will be seen that the English attack was to be entirely photographic; no eye observations were to be made. And if all has gone well, something between fifty and sixty photographs may be hoped for. The table perhaps requires a little explanation, which we will now proceed to give. It followed from work undertaken with that special object in Egypt last year, that eclipse observations can now definitely begin ten minutes before totality, and end about ten minutes after. With an eclipse therefore of about five minutes' duration, as in the present case, the work ranges over a period of five-and-twenty minutes, and if the plates are as sensitive as they can now very well be, it is quite easy to see that a very large number of photographs may be taken. The greatest interest of course attaches to the spectroscopic photographs to be taken by means of a siderostat and the equatorials. As in former eclipses the plates exposed in the photoheliographs will secure the appearance of the corona surrounding the image of the dark moon; but on the present occasion an attempt was to be made to take these photographs on a much larger scale than usual, a scale of four inches to the moon's diameter. Coming to the spectra themselves, we find

four spectroscopes fed by the light reflected by a siderostat of 30 cent. diameter, these four spectroscopes being bracketed together very much like a Gatling gun, and pointed to the siderostat, which has a very excellent clock attached to it. The first spectroscope, called the "Hilger," in the programme, is an integrating one, and will integrate for us the light of the whole region round the sun during the entire period of totality on a plate which is allowed to fall very slowly by means of a clockwork arrangement. If any change, therefore, takes place in the spectrum of these regions during this period, it will be recorded on this moving plate in historical sequence, so that, the beginning and end of exposure being known, the time at which any definite change takes place can be determined. The Rowland grating coming next on the list, which was generously given to Mr. Norman Lockyer by Professor Rowland, is one of ten feet focus, and has a large surface with 14,000 lines to the inch, forming of course a most excellent and simple prismatic camera, the first and second order spectra both being utilised. The prismatic camera and slit spectroscope of two prisms were two instruments arranged by Capt. Abney for the eclipse last year. They are on the model of the instruments designed for the eclipse of 1875 in Siam, but have the advantage of possessing plates which are sensitive to the whole of the spectrum. The work to be done by the equatorial is of a very similar nature to that to be attempted with the siderostat, except that it was intended by varying the time of exposure from long to very short periods to make certain of something. All the cameras, except the "Hilger," in which the plate moves by clockwork, are fitted with long plates, of which only small strips are exposed at a time, and the exposure is managed, not by changing the plate as in the ordinary method, but by turning a screw. The word "expose" in the table therefore shows the precise moment, at which, if the instructions are carried out, a new strip of plate will be exposed to the action of the light before, during, and after totality, and it will be seen that the exposures are varied both before and after totality, so as to get the greatest possible difference in time during which each part of the plate receives its impression. From a letter received by Mr. Norman Lockyer from Messrs. Lawrance and Woods, we know that the American astronomers intended giving them all possible facilities for carrying out the combined Royal Society and Solar Physics Committee's programme; and that the attention of the English observers will be concentrated on the siderostat and equatorials, as two officers of the American ship have been told off to work the photo-heliographs and look after the eclipse clock, which is so arranged that it keeps all the observers together by indicating to each one of them the exact number of seconds still left for his work, with the additional advantage that each number of seconds announced by the officer in charge is a distinct order to do a certain thing, as in the case of the various exposures indicated in the list.

LECTURES TO WORKING MEN

THE three courses of Lectures to Working Men given at the Museum of Practical Geology, Jermyn Street, during the present session, by the staff of the Normal School of Science and the Royal School of Mines, came to an end last Monday, and, as on former occasions, it gave rise to regret that more cannot be done in this direction, both with regard to the number of courses given, and the number of persons accommodated in each case. The theatre at Jermyn Street restricts the audience to something over 600, while of late years the applications for tickets have never been less than 2,000. The tickets for each course—for which sixpence is charged as a registration fee—are given only to *bonâ fide* working men, who must bring with them a paper on which is stated their name and occupation. Some of the lectures of the last course—that given by Mr. Norman Lockyer, on "The Earth and its Movements"—were listened to by the Japanese Minister, and an official connected with the Education Department of Japan. At their request a list was drawn up showing the trades of the audience. This list, in the case of 500 who attended the last course, we are permitted to give, and we think our readers will find it an interesting one. Seeing that there is this anxiety on the part of working men to learn, and that less than one in three of those so desirous of learning can have an opportunity of doing so, we trust that in future years the Lecture Theatre at South Kensington will be utilised in this direction, as well as that at Jermyn Street. There is little doubt, of course, that a Liberal Government, represented by the Treasury officials, naturally anxious in all ways to protect the public purse against all claims, whether good or bad, might object to this being done at the public cost, but seeing that the lectures are given as a labour of love by the various professors such an objection would scarcely be urged, and we confess too that we should not only like to see the theatre at South Kensington utilised in this way, but the theatres at University College, King's College, and other institutions that might be named. We do not believe that the professors at these institutions are less anxious for the progress of knowledge among the working classes than those who are connected with the Government School, and this being so, we may hope to see at some future time a united effort to supply what is at present a great want, and a gap in our educational programme.

Trades of 500 of the audience at the last course of Lectures to Working Men, April and May, 1883:—

1 Bag Maker.
6 Bakers.
2 Basket Makers.
28 Boot and Shoe Makers.
1 Brewer.
2 Brush Makers.
1 Billiard Table Maker.
6 Builders (Foremen).
1 Butler.
3 Brass Finishers.
2 Bricklayers.
7 Bookbinders.
6 Cabinet Makers.
52 Carpenters and Joiners.
8 Coach Painters.
9 Compositors.
8 Carvers.
1 Cigar Maker.
4 Chemists and Druggists.
13 Clerks.
3 Curriers.
6 Dentists.
4 Designers.
1 Die Sinker.
1 Draper.
2 Draughtsmen.
25 Engineers.
2 Engravers.
1 Envelope Maker.
1 Fishing Rod Maker.
5 Gasfitters.
1 Gardener.
7 Gilders.
1 Greengrocer.
2 Grainers.
3 Glasscutters.
4 Gun Makers.
7 Hatters.
1 Hairdresser.
1 Hinge Maker.
1 Hammerman.
1 Hemp Dresser.
23 House Painters.
6 Iron Founders.
23 Instrument Makers.
37 Jewellers.
1 Lamp Maker.
1 Lead Glazier.
3 Lithographers.
1 Locksmith.
4 Stonemasons.
1 Mattress Maker.
2 Milkmen.
6 Opticians.
9 Pianoforte Makers.
3 Perfumers.
6 Photographers.
1 Picture Frame Maker.
16 Plumbers.
2 Pocket Book Makers.
2 Polishers.
10 Porters and Messengers.
6 Portmanteau Makers.
6 Plasterers.
1 Quarryman.
6 Salesmen.
5 Saddle & Harness Makers.
1 Saw Maker.
1 Soda Water Bottler.
7 Shop Assistants.
4 Stationers.
1 Smith.
1 Stoker.
2 Storekeepers.
17 Tailors.
4 Teachers.
1 Traveller.
6 Tinmen.
5 Turners.
1 Twine Spinner.
2 Umbrella Makers.
6 Upholsterers.
8 Watch Makers.
1 Warehouseman.
3 Wheelwrights.
7 Zinc Workers.

CIRRIFORM CLOUDS

IN a "Note on a Proposed Scheme for the Observation of the Upper Clouds" the Rev. W. Clement Ley has written an abstract of part of a large work on clouds, which he is now preparing for publication. This note has been circulated with a view of obtaining suggestions on the scheme of classification, observation, and telegraphy, which the writer has submitted to his colleagues of the Committee on Cirrus observations, nominated by the International Meteorological Committee in 1882.

The author follows the primary outlines of cloud-classification proposed by Luke Howard, dividing the objects of observation into cirriforms, cumuliforms, stratiforms, and composites; while in the subdivision of these primary types he has been induced by reasons, the cogency of which he hopes to demonstrate, to deviate very considerably from Howard's classification. The true cirriforms, to the discussion of which the note is restricted, are divided by Mr. Ley as follows: cirrus, cirro-filum, cirro-velum (with its variety mammatum), cirro-nebula, and cirro-granum. The author has been, "after many years devoted to the consideration of the subject, reluctantly compelled to give up the employment of the two terms 'cirro-stratus' and 'cirro-cumulus.' Their use has led to endless confusion. In point of structure the clouds usually called cirro-cumuli belong essentially to the higher stratiforms, consisting of nubecules separate, or partly coalescing, occupying a layer of atmosphere of very small vertical thickness, but of very great horizontal extent, and they are not formed in nature by those processes which are productive of clouds either of the cirrus or of the cumulus type. They are not, in fact, either in appearance or in mode of physical formation, either compounds of cirrus with cumulus or hybrids between cirrus and cumulus. Therefore in practice the use of the word cirro-cumulus has led to a large number of clouds of no great elevation being classified among the cirriforms, a result which was of little consequence when the laws regulating the upper currents of the atmosphere had received no examination, but which must be absolutely fatal to a scheme based upon those laws, according to which new and most valuable results will be attained. The name cirro-stratus is almost equally objectionable, and for similar reasons."

Six pages of this note are devoted to instructions on a system of observing and reporting by telegraph the structure and movements of the upper clouds; and the author shows that, if this system be adopted on an extensive scale, results of great practical importance may be anticipated. The indispensable pre-requisite is a clear and scientific classification of clouds according to physical structure.

SCIENTIFIC PROGRESS IN CHINA AND JAPAN

VARIOUS steps in the progress of China and Japan in the adoption of Western science and educational methods have from time to time been noticed in these columns. To the popular mind the names of the two countries are synonymous with rigid unreasoning conservatism and with rapid change respectively. The grave, dignified Chinese, who maintains his own dress and habits even when isolated amongst strangers, and whose motto appears to be, *Stare super vias antiquas*, is popularly believed to be animated by a sullen, obstinate hostility towards any introduction from the West, however plain its value may be; while his gayer and more mercurial neighbour, the Japanese, is regarded as the true child of the old age of the West, following assiduously in its parent's footsteps, and pursuing obediently the path marked out by European experience. There is considerable misconception in this, as indeed there is at all times in the English popular mind with regard to strange peoples. Broadly speaking, it is no doubt correct to say that Japan has adopted Western inventions and scientific appliances with avidity; that she has shown a desire for change which is abnormal, and a disposition to destroy her charts and sail away into unsurveyed seas, while China remains pretty much where she always was. Superior, now, with some exceptions, what she was twenty, [illegible] hundred, perhaps two thousand years ago, while a new Japan has been created in fifteen years. All this, we say, is true, but it is not the whole truth. China also has had her changes; not indeed so marked or rapid, not so much in the nature of a *volte-face* on all her past as those of her neighbour. The radical difference between the two countries in this respect we take to be this: that while Japan loves change for the sake of change, China dislikes it, and will only adopt it when it is clearly demonstrated to her that change is absolutely necessary. To the Japanese change appears to be a delightful excitement, to the Chinese a distasteful necessity; to the former whatever is must be wrong, to the latter whatever is is right. As a consequence of this difference between the two peoples, when China once makes a step forward it is generally after much deliberation, and is never retraced. Japan is constantly undertaking new schemes with little care or thought for the morrow, but with the applause of injudicious foreign friends. In a short time she discovers that she has underrated the expense or exaggerated the results, and her projects are straightway abandoned as rapidly and thoughtlessly as they were commenced. Swift suggested as a suitable subject for a philosophical writer a history of human projects which were never carried out; the historian of modern Japan finds these at every turn. Where, for example, are the results of the great surveys, trigonometrical and others, which were commenced in Yezo and the main island about ten years ago? A large, expensive, but highly competent foreign staff was engaged, and worked for a few years; but suddenly the whole survey department was swept away, and the valuable instruments are, or were recently, lying rusting in a warehouse in Tokio. The same story may be told of scores of other scientific or educational undertakings in Japan. An able and careful writer, Col. H. S. Palmer, R.E., who has recently, with a friendly and sympathetic eye, examined the whole field of recent Japanese progress, in the *British Quarterly Review*, is forced to acknowledge this. "Once having recognised," says this officer, "that progress is essential to welfare, and having resolved, first amongst the nations of the East, to throw off past traditions and mould their civilisation after that of Western countries, it was not in the nature of the lively and impulsive Japanese to advance along the path of reform with the calmness and circumspection that might have been possible to a people of less active temperament. Without doubt many foreign institutions were at first adopted rather too hastily, and the passing difficulties which now beset Japan are to some extent the inevitable result." It would be blindness to deny that the net result of the Japanese efforts is progress of a very remarkable kind, but it is a progress which in many respects lacks the firm and abiding characteristics of Chinese movements.

The proverb, *Chi va piano va sano*, which was recommended ten years ago to Japanese attention by an eminent English official, and apparently disregarded by them, has been adopted by their continental neighbours. To the blandishments of pushing diplomatists or acute promoters, the Chinese are deaf. However we may felicitate ourselves on our inventions, scientific appliances, "the railway and the steamship and the thoughts that shake mankind," our progress, the newspapers, the penny post, and what not, China will not adopt them simply because *we* have found their value and are proud of them. But if, within the range of her own experience, she finds the advantage of these things, she will employ them with a

rapidity and decision surpassing those of the Japanese. A conspicuous instance of this will be found in her recent action with respect to telegraphs. For years the Chinese steadily refused to have anything to do with them; the small land line which connected the foreign community of Shanghai with the outer world, was maintained against the violent protests of the local authorities, and the cable companies experienced some difficulty in getting permission to land their cables. But during the winter of 1879-80, when war with Russia was threatening, the value of telegraphs was demonstrated to the Peking Government. The Peiho at Tientsin was closed by ice against steamers, and news could only be carried to the capital by overland couriers from Shanghai. Before a year elapsed a land line of telegraph was being constructed between this port and Tientsin; in a few months the line was in working order, and the Chinese metropolis is now in telegraphic communication with every capital in Europe.

This conservatism, respect for antiquity, conceit, prejudice, call it what we will, has something in it that extorts our respect. Let us imagine a dignified and cultivated Chinese official conversing with a pushing Manchester or Birmingham manufacturer, who descants on the benefits of our modern inventions. He would probably commune with himself in this wise, whatever reply Oriental politeness would dictate to his interviewer: "China has got on very well for some tens of centuries without the curious things of which this foreigner speaks; she has produced in that time statesmen, poets, philosophers, soldiers; her people appear to have had their share of affliction, but not more than those of Europe; why should we now turn around at the bidding of a handful of strangers who know little of us or our country, and make violent changes in our life and habits? A railway in a province will throw thousands of coolies and boatmen out of employment, and bring on them misery and starvation. This foreigner says that railways and telegraphs have been found beneficial in his country; good, let his countrymen have them if they please, but let us rest as we are for the present. Moreover, past events have not given us such faith in Europeans that we should take all they say for wisdom and justice." A day will undoubtedly come when China also will have her great mechanical and scientific enterprises; but what we contend for here is that nothing we can say or do will bring that time an hour nearer. European public opinion is to China a dead letter; she refuses to plead before that tribunal. Each step of her advance along our path must be the result of her own reflection and experience; and our wisest policy would be to leave her to herself to advance on it as she deems best. SINENSIS

PROF. LINDSTRÖM ON OPERCULATE CORALS[1]

THE extinct stony corals, the calicles of which are provided with calcareous opercula, have ever been a puzzle to naturalists, since they are almost entirely without parallel amongst existing Anthozoa. The genera and species are not numerous, and are all of Palæozoic age. By far the finest and best preserved specimens of the most important forms are found in the Silurian strata in the Island of Gothland in the Baltic, and are collected for the National Museum at Stockholm, where they come under the care of Prof. Lindström, the author of the present memoir, so justly distinguished for his palæontological researches generally, and especially for those on corals. In this memoir he gives a *résumé* of all the forms of operculate corals as yet known, embodying an immense amount of important new information derived from his own prolonged investigations on a series of most remarkable specimens which I had the advantage of seeing and having explained to me by him in the summer of last year. The whole paper forms a most valuable contribution to our knowledge of these especially interesting and peculiar corals.



FIG. 1.—*Goniophyllum pyramidale* (*mutatio secunda*), viewed from above, with the opercular valves *in situ* of the natural size.

The first operculate coral described was *Goniophyllum pyramidale*, which Bromell in 1729 placed amongst the corals. The best known, and the one concerning which there has been the greatest difference of opinion, is *Calceola sandalina*, which was first figured in 1749 from the collection of Rosinus of Hamburg, by Brüchmann, who pointed out the resemblance of the coral to the front of a woman's slipper.

Brüchmann referred Calceola to the corals just as Bromell had Goniophyllum, but this was mainly because neither he or other early authors following him were acquainted with the opercula belonging to the specimens. Linné placed Calceola with the Mollusca as *Anomia sandalina*. Later it was referred to the Brachiopoda, a position in which a large number of eminent modern authorities retain it. If Calceola stood alone, the gravest doubts might certainly be entertained as to its having any relations to the corals; but now that a series of clearly allied forms such as Goniophyllum and Rhizophyllum, also bearing opercula, have had their structure so fully and satisfactorily worked out as has been done by Prof. Lindström, it is hardly possible not to follow him in placing the whole amongst the Anthozoa. The curious arrangement of the septa in Calceola closely resembles that in Goniophyllum as regards the septa both in the calicles and on the opercula. It is almost impossible to doubt the Anthozoan nature of Goniophyllum, whilst both it and Rhizophyllum, which has like Calceola an operculum of a single piece only, demonstrate their close relation to numerous recognised Palæozoic corals by exhibiting intracalicynal gemmation, and developing, like many other corals, abundance of roots.

The author divides the *Anthozoa operculata* into two families—

I. Calceolidæ (or Heterotœchidæ), distinguished by having the septa on the inner face of the operculum not alike and a median septum the largest.

II. Araeopomatidæ (or Homotœchidæ), with the septa on the operculum all alike and no defined median septum.

The Calceolidæ include all those forms in which the operculum, whether composed of one or four valves, has this valve or valves marked inside with a stout prominent median septum.

The family falls into two groups—the one in which the operculum consists of a single valve containing three genera, namely, the well-known Calceola, distinguished by not multiplying by budding, being thus never compound, by having no root-tubes, and not showing vesicular structure internally; and two others—Rhizophyllum and Platyphyllum—in both of which calicynal gemmation occurs and the internal structure is vesicular, somewhat as in Cystiphyllum.

In Rhizophyllum, a genus founded by Lindström, the

[1] "Om de Palæozoiska Formationernas Operkelbärande Koraller." Af G. Lindström. Bihang till K. Svenska, Vet. Akad. Handlingar, B. 7, No. 4.

corallum is very much like that of Calceola in shape, but more elongated. It may be simple or form large masses by two modes of budding—either calicynal budding, or budding from the abundant rootlets. In the specimen of



FIG. 2.—*Rhizophyllum elongatum.* View of the interior of the calicle to show the six young calicles developing as buds within it.

Rhizophyllum here figured, six young corals are seen in the act of developing as buds in the interior of the calicle.

In consequence of budding taking place from the "rootlets," the author advances the new suggestion that these bodies, the nature of which has always been doubtful, are to be regarded as stolons. The operculum of Rhizophyllum, by which the mouth of the calicle is completely closed, is, as in Calceola, of a semicircular shape, with a prominent ridge-like median septum on its inner face. In *Rhizophyllum attenuatum*, a compound form from Louisville, in Kentucky, first described by Mr. V. W. Lyon under the genus Calceola, the stolon tubes partly serve to connect together adjacent calicles, partly become themselves developed into fully-formed calicles. Specimens of *Rhizophyllum Gothlandicum* are found in abundance in the Island of Gothland with their opercula detached. The separate opercula are also common; specimens with the opercula *in situ* are comparatively rare. A remarkably perfect one is shown in the accompanying figure.



FIG. 3.—*Rhizophyllum elongatum.* View of the calicle with the operculum, a single valve only *in situ*.

The second group of the Calceolidæ contains only a single genus—Goniophyllum—in which the mouth of the calicle is rectangular in form, and four opercular valves are present which, with their bases resting on the four sides of the mouth of the calicle, slope inwards to meet one another, and form a four-sided pyramidal roof over



FIG. 4.—*Goniophyllum pyramidale* (*mutatio prima*). View of the mouth of the calicle, with the opercular valves *in situ*, the dorsal right and left valves being entire, the ventral incomplete.

the calicle. From Gothland Prof. Lindström has been able to obtain two or three specimens with all the four opercular valves *in situ*. So that doubts as to the connection of the valves with the coral calicle are now inadmissible. On careful study of these specimens and numerous others with a fewer number of valves *in situ*, he finds that the four valves always present well-marked differences and form, and septal striation, so that it is possible to distinguish with certainty an anterior and posterior, or ventral and dorsal, and a right and left valve, and pick these out from any collection of well-preserved loose valves. The anterior and posterior valves are trapezoid in form, the lateral triangular. A most remarkable discovery he has made is that these corals must have shed their valves periodically, and replaced them, and that when shed the valves frequently happened to become attached to the wall of the calicle, and fused with it. In the accompanying figure of *Goniophyllum pyramidale*, seen from behind, a well-formed operculum is seen near the base firmly attached to the wall of the calicle near its base. Such



FIG. 5.—*Goniophyllum pyramidale*, viewed from the ventral side to show the shed opercular valve (*a*) coalesced with the wall of the calicle near its base.

specimens are not uncommon in Goniophyllum, but in another genus, a new one, Araeopoma, belonging to the second family of operculate coral, devoid of a median septum, and which has four triangular valves like Goniophyllum, very many specimens bear numerous valves fused to their outer walls, the valves being of increasing size from below upwards, in accordance with the growth and expansion of the coral and the mouth of its calicle. In one abnormal specimen of *Goniophyllum pyramidale* there are five opercular valves present, a minute extra triangular valve being interpolated at one of the corners.

Prof. Lindström, whose important researches on the development of the septa in so-called Rugose corals are familiar, has been able to trace the development of the corallum in *Goniophyllum pyramidale*. He finds that in the youngest stage of the calicle there is no septum present at all, then that one septum is formed on what may be termed the dorsal side of the calicle, and since in this genus and several others it remains conspicuously prominent, it may be termed the primary septum. Two further septa, the light and left median, are next formed, and last of all the ventral septum, long after the others. He points out that a similar process of development is followed in most Rugose corals, and that it is therefore erroneous to treat of four septa as primary in these forms.

In Rhytidophyllum shaped somewhat like Calceola, but belonging to the Araeopomatidæ, by reason of the absence of a defined median septum on the operculum there is only a single opercular valve. A further genus of the group is possibly represented by a single broken operculum, of which further specimens have not yet been found.

In connection with the operculate corals, Prof. Lindström describes certain coral forms in which remarkable exothecal structures are present, which may be considered as more or less homologous with opercular valves. In *Pholidophyllum tubulatum*, a compound coral, first described as *Tubiporites tubulatus* by Schlotheim in 1813,

and which has lately been made a subject of research by G. von Koch, the costæ are in solitary specimens clad each with a longitudinal rib composed of a double row of rhomboidal calcareous scales placed close together at an angle to one another as shown in the figure. These rows of scales form an almost complete outer covering to the corallum. The scales seem to be wanting in colonial specimens. They are extremely conspicuous and definite in some of the best preserved solitary specimens, and their regularity of disposition is such that it is impossible to believe that they do not definitely belong to the coral. They are shown enlarged in the figure. In vertical sections of the coral they are seen to be attached at their bases to the wall of the calicle. In *Syringophyllum organum* similar scales occur covering the exterior of the corallum, but these have a remarkable definite form somewhat like that of the bowl of a teaspoon with the handle cut off short.



FIG. 6.—*Pholidophyllum tubulatum*. A calicle viewed from the side to show the rib-like prominences formed upon the costæ by the double rows of calcareous scales.

As an illustration of the possibility of Anthozoa bearing opercula and scales, but not as implying any direct relationship between the Operculata and the Alcyonaria, Prof. Lindström points to the structure of *Primnoa lepadifera*, and figures one of the polyps viewed from above, showing how eight of the valve-like calcareous scales present in this species close in over the summit of the polyp, forming a conical operculum over it somewhat as in Goniophyllum, whilst the remaining scales form a representative of the calicle. He shows that, as in Goniophyllum, the opercular valves differ in form and size according to their position when *in situ*.

In his concluding paragraph he states that he considers the Calceolidæ to be probably nearly allied to such forms as Cmphyma and Chonophyllum, whilst the Aræopomatidæ, on account of their vesicular internal structure, approach more nearly to Cystiphyllum. He does not consider the presence or absence of an operculum in corals to be necessarily of an important classificatory value, and cites the case of the presence and absence of opercula in Gastropoda as a parallel one of minor systematic importance. In this it is rather difficult to agree with him. Of course the opercula in Gastropods and corals are alike only in name, and as they occur so rarely in the latter the suspicion naturally arises that the two groups of the operculate corals may be more closely allied than he suspects. However, no man knows them better than he, or has a better right to an opinion on the subject, and his conclusion, guarded as it is, must be treated with all respect.

A very interesting result attained and given in tabular form on p. 91 is that in successive Upper Silurian strata a series of three modifications of form of *Goniophyllum pyramidale*, starting from *Goniophyllum pyramidale, primigena*, the form occurring in the lowest beds can be traced succeeding one another in time. Similarly *Rhizophyllum Gothlandicum, forma primaria* passes by a modification, *R. G. mutalia*, into *R. Gervillei*, of specific rank in the Devonian formations. Other operculate corals show similar modifications in progress of geological time. Prof. Lindström's important memoir cannot here be followed farther. It is illustrated by nine lithographic plates most beautifully executed in Stockholm, from which the engravings here given are copied, and are some of the most excellent ever published of corals.

H. N. MOSELEY

BARON NORDENSKJÖLD'S EXPEDITION TO GREENLAND

[THE following Programme, drawn up by Baron Nordenskjöld for his expedition to Greenland, has been kindly placed at our disposal by Mr. Oscar Dickson, who, with his well-known enlightened liberality, provides all the expenses. The Programme has not hitherto been made public, even in Sweden, as Baron Nordenskjöld did not wish to be interrupted in his preparations with correspondence on his plans and theories. The expedition leaves Sweden in the *Sofia* on the 20th instant, and will call at Thurso, where Baron Nordenskjöld will join the vessel.]

NINE centuries have elapsed since the Norwegian, Erik Röde, discovered Greenland, and founded Scandinavian Colonies; from these, Norwegian navigators some ten years after sailed south to "Vinland," the fecundus, *i.e.* to the shores of the present Canada and the United States, thereby acquiring the honour for the Norse race of being the real discoverers of the New World. It is not known whether these voyages led to any fixed settlements being established in America, but we know, on the other hand, from a number of Icelandic sources, that the colonies in Greenland became very flourishing. There were upwards of three hundred farms, "Gaarde," of which about two hundred, embracing twelve parishes, were situated in the "Österbygd," and about one hundred, embracing three or four parishes, were situated in the "Vesterbygd." During four centuries the country formed a bishopric, from which funds towards the Holy Wars were even contributed. Unfortunately, the connection between the colonies and the mother country ceased after a couple of centuries, while Greenland's ancient Norse population was extirpated, either through plagues or by "Skrællings," *i.e.*, the Eskimo who descended from the North. Another explanation of their disappearance is that they lost their nationality, and were absorbed into the Eskimo population, during their contact with the more numerous tribes of the Polar regions, whose mode of living was more suited to the climate of the country and the resources at their disposal. However that may be, there remains the fact that one of the most distinct and enterprising peoples in the world have been annihilated, or, perhaps, absorbed in one distinguished as among the lowest, both physically and intellectually. The old country, belonging to the Norwegian Crown, was even so far forgotten, that it was only Columbus's discovery in the south which recalled the attention of the Norsemen to the fact that they had once colonised a part of this world, which was being parcelled out among the southern nations by "Bulls" from Rome, as if it had just been discovered.

By the aid of traditions and old journals of navigation several attempts were made to reach the old, forgotten colonies from Iceland, but these were frustrated by the enormous masses of drift ice, which then seemed to inclose the shores of Greenland's east coast, more than formerly. Eventually, John Davis, in his attempts to find the north-west passage, discovered that the west coast was easily reached, and that the seas around it offered a fine hunting-ground for the profitable whale-fisheries. This, with the reported discovery of gold in the country resulted in the despatch of several Danish hunting and commercial expeditions, which did not, however, meet with much success, until the Norwegian,

Hans Egede, by his zeal for bringing the blessings of the Gospel to the descendants of the old Norse colonists, caused trading and missionary stations to be established on the west coast. These were subsequently considerably augmented and extended, and henceforth held by the Danish crown, under "The Commercial Association of Greenland."

Greenland was thus inhabited by Norsemen from 983 until the 15th century, and its west coast has, during the last hundred and sixty years, been a place of sojourn for many able Danish administrators and missionaries. Besides this, nearly all expeditions which have been bound for the American Polar sea have stayed here more or less, while the west coast has on many occasions been the object of carefully prepared expeditions of research. This part of Greenland is, therefore, scientifically and ethnographically one of the best-known of the Arctic lands. But in spite of this we encounter here several of those *lacunæ* in the knowledge of the globe which it is of great importance that we should fill up, and some of these it is now my intention to deal with.

The east coast of Greenland was visited in 1822 by the Englishman, Scoresby, jun., and in 1823 by Sabine and Clavering, in 1829-30 by the Dane, W. A. Graah, as well as by the second German expedition under Koldewey in 1868-69, and also by some whalers. It is, however, in its greatest extent wholly unknown, a circumstance, which must be detrimental to the proper understanding of the history of the first Norse colonisation of Greenland, and of early voyages of conquest and discovery therefrom to the shores of America. Thus, until the east coast of Greenland is fully explored, one must continue to doubt the very forced explanation of the site of these colonies which is now predominant in the world of science. And it is, on the other hand, not worthy of the geographical discoveries of the 19th century, that a coast-line extending south to the latitude of Stockholm should be so utterly a *terra incognita*.

The interior of Greenland is even less known than the east coast, and here we encounter a purely scientific problem, whose great importance is apparent from the circumstance that the unestablished theory,—that the interior of the island is one continuous mass of ice,—forms one of the corner stones in glacial science, which again is closely connected with several of the fundamental principles of modern geology. If we except a trip on the inland ice of Greenland in 1751, in lat. 62° 31′ by the Danish merchant Lars Dalager, who penetrated nearly thirteen kilometres across a comparatively even plateau, and the unsuccessful attempt by Whymper in 1867, in lat. 69° 30′, where no progress was possible, in consequence of the difficult nature of the ground, only two serious attempts have been made to explore the interior of Greenland. The first of these was made by Dr. Berggren and myself between July 19 and 26, 1870, in lat. 68° 30′. Favoured by the most magnificent weather, we were able to penetrate nearly fifty kilometres across a country at the outset very difficult and rent by bottomless abysses, but which gradually improved in condition the further we advanced. We had on starting the company of two Eskimo, but they left us after two days' journey. As those who claimed to know the coast glaciers of Greenland had advised me not to waste time and labour on such a hopeless undertaking as that of penetrating over the inland ice, my outfit was very incomplete; we were in want of ropes, tent, suitable sledges, and on the Eskimo leaving us we could not even carry the utensils necessary for cooking. I could not therefore on that occasion get very far, but I certainly came to the conclusion, that I should have been able, with a couple of smart sailors or Arctic hunters and a suitable outfit, easily to have extended my wanderings to 200 or perhaps 300 kilometres. I may also mention here that in the month of June, 1873, I effected with Capt. Palander and nine sailors a journey of more than 190 kilometres over the inland ice of Spitzbergen, which journey was of special interest to me from the circumstance that I here learnt to know the character of inland ice before thaw sets in, as well as the difficulties which are at such a time attendant on journeys on the glaciers of the Polar regions. This experience, I believe, will be of great use to me during the journey now in course of preparation, as I shall have to cross portions of the inland ice which, on account of the altitude, will still be covered with snow at the time of my visit.

The second journey of research on the inland ice of Greenland was made between July 14 and August 4, 1878, in lat. 62° 40′, by the Danes, J. D. Jensen and A. Kornerup. This expedition was carefully equipped, but the country being much fissured, and the weather unfavourable, it did not reach much further inland than the Swedish of 1870.

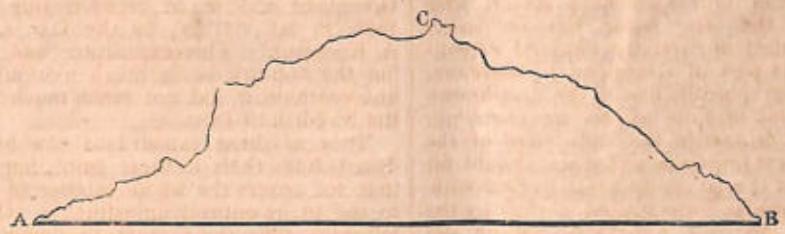
None of these expeditions saw any limit to the ice desert from their farthest point, but to infer from this that ice covers the whole interior of Greenland appears to me to be entirely unjustifiable. On the contrary, the following reflections seem to demonstrate that it is a *physical impossibility* that the whole of the interior of this extensive continent can be covered with ice, under the climatic conditions which exists on the globe south of the 80th degree of latitude.

The ice masses of the glaciers are commonly termed "permanent," a denomination which was once taken so literally that certain *savants* asserted that the ice in course of time was transformed into mineral crystals, such as those which are so frequently found in the clefts of the Alps. We know now that this term is entirely erroneous. The glacier which seems century after century to fill the same valley is not only in constant, although imperceptible, motion, by the ice masses which slowly advance from higher to lower elevations, but is also subjected to a change in its form by the circumstance that the lower stratum melts away through contact with the mountain on which it rests; while the surface on one side wastes away by thawing in the warm season and evaporation in the cold, and on the other is added to by falling snow, which latter after a time changes from snow dust to granular snow, then to crystals of ice, and eventually to a compact homogeneous mass of ice. And, if the advancing glacier is "fed" by enormous ice-fields, or what I may term "ice-lakes," situated so high that snow always falls there copiously, it can penetrate far below the border of the perpetual snow, yea, even to parts where the snow-fall is far from sufficient to make up for the loss of melting and evaporation. It is therefore clear, that glaciers, or other constant ice-masses, cannot form in places where they cannot be "fed" by descending ice, or where the snow-fall is less than the quantity which appears and disappears yearly; a circumstance which, among others, explains why no glaciers exist in the vicinity of the north polar coasts of the new or old world.

With regard to Greenland it is not difficult to demonstrate that the above-described conditions for the formation of glaciers do not exist there, if the country does not rise gradually both from the eastern and the western shore to the centre, and thus be like a loaf of bread in shape, and with sides slowly and symetrically terminating in the ocean. Such a land-formation is, however, not found in any part of the orography of the known world, and one may therefore safely conclude that neither is it to be found in Greenland. In fact, the geological nature of Greenland, very similar to that of Scandinavia, seems to indicate a similar orographical formation, viz., a formation formed of mountainous ridges alternating with deep valleys and plains; while one may even assume that the culminating line of the land in Greenland runs, as in England and Sweden, and in both American continents, along the west coast.

The winds, therefore, which should produce snow in the interior, must, if coming from the Atlantic, have in the first instance, crossed the broad ice-belt generally encircling the east coast of Greenland, and then the mountains on the coast, some of which we know are very high, and, if coming from Davis Sound, the mountain ridge itself. In both cases the wind would assume the character of the "Föhn" wind, *i.e.*, it must, after passing the mountain-chains on the coasts, be *dry and comparatively warm*. The law of the "Föhn" is, as is generally known, dependent on the circumstances explained below.

If *ACB* indicates a mountain, and a wind so dry that no deposit of moisture takes place on the top passes from *AC* to *B*, the air will certainly be chilled in passing *C*, in consequence of the lower barometric pressure and consequent expansion of the air; but the same cause which produces the lowering of temperature when the wind ascends has also the effect of liberating its heat, and the air will become warmer as it descends from *C* to *B*. The compression and rise of temperature are in the latter case precisely analogous to the expansion and fall of temperature in the former, and the dry air, in passing



the mountain, has, therefore, on reaching *B*, not suffered any change whatever in temperature or quantity of moisture. Quite different will, however, be the result if the air ascending at *A* is saturated with moisture, as, for instance, air passing a great expanse of water. In that case the air will expand and become colder, just as it ascends from the water surface to the mountain top, but, at the same time, part of the moisture will be condensed on the top, whereby the latent heat of the hydrogen will be set free and a rise of temperature take place, and this will, to a certain extent, minimise the fall of temperature caused by the expansion of the air. The air will retain the heat thus set free, even after it has reached, in a dry state, the point *B*, and the air, originally moist, has, when it has passed the mountain, attained a higher degree of heat, but less moisture than at the moment of ascending. *It is in fact dry and warm.*

These causes are not only the reason of the dry warm "Föhn" winds in Switzerland, and the very remarkable circumstance that it is under winds from the snow-covered mountains that the snow disappears in Swedish Lappland, but they play also an important part in the climatic conditions of the whole globe. They are, for example, the cause of the difference in climate and flora of the two sides of the Andes, of the east and west coasts of Tierra del Fuega, and the eastern and western parts of Australia. They are the chief cause of the deserts which cover the interiors of Asia, Australia, the northern portion of Africa, and certain parts of America, while in Sweden they produce the constant western winds, and the consequent prolonged drought which invariably occurs in spring time in the central part of the country. The same laws of the temperature and moisture of the air must also prevail in Greenland. Here too the ocean winds must be moist, and this moisture is usually deposited in the form of snow on the mountains along the coast, whereas all those reaching the interior, whether from east, west, north, or south, must—if the orographical construction of the country is not entirely different from that of others on the globe—be dry and comparatively warm. And in consequence of this circumstance, the snow-falls in the interior of Greenland cannot be sufficient for maintaining a "perpetual" inland ice.

It cannot, however, be asserted that the country should here form a deserted, treeless tundra; one encounters in Siberia forests with giant trees under climatic conditions far more severe than those we may assume are to be found in the interior of Greenland. That the country should prove true to its name has besides been asserted by the celebrated botanist Hooker, from his studies of the flora of Greenland, and even the natives on the west coast themselves have a suspicion that such is the case, from the large herds of reindeer which from time to time are seen to migrate across the inland ice to the west coast.

It is most probable that the interior, if free from ice, is like a North European high plateau, with a flora far more copious than that of the coast.

But this I maintain, that whether the interior of Greenland is richly covered with forests, as the land round the frigid pole of Siberia, or is a treeless, ice-free tundra, or even a desert of perpetual ice, the solution of the problem of its real nature is so important, and of such consequence to science, that there could hardly, at the present moment, be conceived an object more worthy of an Arctic expedition than to ascertain the true conditions of the interior of this particular country.

Besides the object of penetrating to the interior of Greenland, the expedition will have several others in view, of which I may mention the principal:—

To Fix the Limit of the Drift-ice between Iceland and Cape Farewell, and to take Soundings and Dredgings in the Adjacent Seas

This part of the Atlantic has hardly ever been subjected to any other kind of examination than that made on laying the first cables. A knowledge of the same is, however, of great importance, both for completing the missing link in the hydrographic chain of the Ocean dividing Europe and America, and for the discovery of the causes of the change in the ice-conditions in the seas of the east coast which seem to have taken place since Greenland was first discovered. Without much waste of time the expedition may be occupied with these researches during the voyage from Iceland to the Greenland promontory, which will take place in a season when fine weather may be presumed to reign here, while they may, although with less probability of being favoured by this condition, be effected during the return journey.

The Collection of Fresh Specimens of the Flora of the Ice and Snow

Professor Wittrock is at present engaged in publishing a very interesting and important work on the microscopic flora, with several varieties, whose home is in the snow and ice-fields of the Alps and the Polar regions, for which materials have been brought home from the latter by the Swedish Arctic expeditions. My expedition will, during the coming visit to Greenland, be in a position to gather further materials for researches, which have already revealed to us the startling fact, that even snow and ice can form the bed for a flora regular in development, and of many varieties.

New Systematic Researches of the Strata which in Greenland contain Fossil Plants

From the previous Swedish Arctic expeditions, and through the publications in the *Journal* of the Swedish Academy of Sciences of articles by Prof. Oswald Heer, of Zurich, we know that students of science have received during the last twenty years from the sandstones and slates of North Europe an additional abundant material for determining the pre-historic climatic conditions of the earth; as well as a knowledge of the certainly variable, but even in the last geological era copious, flora which during these ages existed in the present ice-covered regions of the pole. It is well known, that valuable materials for the study of this phenomenon has been collected in Greenland by English, Danish, and Swedish explorers; but hitherto what has been brought home has been gathered under very difficult conditions, and generally by men who have not been students of the science in question, viz., palæontology. I hope to attach to my coming expedition one of the most celebrated students of this science, and if this be the case, the expedition will undoubtedly bring home novel and circumstantial details relating to this important chapter of the history of the earth. This task is so much easier to accomplish as the richest fields for the discovery of such fossil objects are situated just where it is my intention to invade the inland ice. During the period I shall be absent wandering on the ice, the other members will be occupied in this pursuit.

The Collection of New Data Connected with the Fall of Cosmic Dust

It is clearly demonstrated by the discovery of cobalt containing iron particles in fresh snow in Europe, and the carbon-dust, also containing cobalt-iron, which is found on the ice-fields north of Spitzbergen, as well as by the appearance of metallic iron in "krykonit," a remarkable dust which I brought home from the Greenland ice, that the fall of a small quantity of cosmic dust always takes place, regularly or periodically, most probably in every part of the globe. But that there is a greater variation in the nature of this fall than is generally assumed, is clearly indicated by the discoveries which were made in the *Vega* expedition, viz., of tiny yellow crystals in the snow on an ice-plateau near the Taimur peninsula. This certainly necessitates fresh researches into this phenomenon, in order to settle questions of great moment to geology and cosmology. It is very difficult to investigate this feature of cosmology, in consequence of the small quantities of dust which falls, and when it takes place in closely populated districts, covered by dwellings and factories, and where the ground is perhaps only clad in snow during a short period of the season. The Polar countries, on the other hand, are particularly suited for researches of this kind, both in consequence of the purity of the air and the absence of terrestrial dust, and by the ease with which the dark dust-particles are noticed on pure snow. The coming expedition will, while steaming along the ice-belts between Iceland and Greenland, and during my wanderings on the ice, be able to direct a great deal of attention to these captivating problems.

Should the conditions of the ice in Baffin's Bay be favourable, and the vessel, when in the vicinity of Disco Island, have sufficient coals for a journey northwards, or should it be possible to obtain a sufficient supply from the beds found in these parts, it would be highly desirable that the members remaining on board whilst I am away on the inland ice make an excursion along the west coast as far as Cape York.

There are, according to statements made to the Arctic explorers Ross and Sabine, lying here on a mountain, Savilik, *i.e.*, the iron mountain, lat. 76° 10′, a couple of large, round, solitary iron blocks, from which the natives obtain the little supply of iron which they require for hunting implements and domestic utensils. The metallic constituents of these blocks are, according to an analysis made of one of these utensils brought home, iron and a small percentage of nickel, and, to judge from the descriptions by the Eskimo, these blocks are of the same nature as those which I discovered in 1870 at Ovifak, on Disco Island. It is very strange that at remarkable descriptions by Ross and Sabine should not have been the object of investigation by the many Arctic explorers who have passed this spot. An opportunity is now at hand to ascertain the much-disputed nature and origin of these iron blocks, while a few days' sojourn on this part of the coast, which is so little known, ought to be of considerable scientific value, especially as the geological features here are similar to those on Disco Island,—strata bearing fossil plants.

This expedition will, as has been the case with previous Swedish Arctic expeditions, be accompanied by a specially selected scientific staff, whose members will individually contribute to make the expedition worthy of earlier achievements, and who will lose no opportunity of adding to our knowledge of the Polar regions. They will attempt to solve some of the many problems which await investigation in the far north. It is, however, impossible to detail at length the researches which may be undertaken, as these must depend on the nature of the special studies to which the members have devoted their time.

There is, however, one more object of research to be mentioned, which should not be lost sight of by any expeditions to Greenland, viz., to attempt to solve the question: Where were the former Norse colonies, Eriksfjord, Brattelid, Garda Cathedral, Herjolfsnæs, and others situated? This question has already been answered by the most eminent students of the early history of Greenland, who maintain that the ancient Österbygd lies west of the southern part of the island, between Cape Farewell and lat. 61°, and the Vesterbygd, a little north of the west coast. But it appears to me that, if the Icelandic Sagas are examined carefully, and without any preconceived opinion, it will become apparent that former investigators of this problem have been led astray, and that the true Eriksfjord, with its cathedral and numerous parishes, has never been found, but must be sought for on the inaccessible east coast north of Cape Farewell.

With the experience I have of the conditions of the ice in other parts of the Polar seas, I believe that this coast may be reached, without much difficulty, by sailing in the autumn from the south in the ice-free channel, which I have every reason to believe forms along this coast. This journey will, however, not be begun until the commencement of September, and this circumstance will exactly fall in with the plan in view, viz., on returning from the inland ice to attempt this voyage of discovery.

With the premises I have here detailed before me, I now have to suggest the following plan for the journey of the expedition:—

The expedition should leave Sweden near the end of May in a suitable, but not too large, steamer built of Swedish iron and constructed with water-tight compartments. And although the journey is intended for the summer months only, it should be provisioned for one year, and be provided with the necessary winter outfit, with the scientific instruments desirable, and accoutrements which would be of service for the journey on the inland ice. The steamer should be navigated by a man accustomed to sailing in the Arctic seas, whilst there ought also to be on board as ice-master a skipper who has hunted in the Arctic. The scientific staff should, besides the commander, consist of four persons, the doctor included.

From Sweden the steamer should steam to a port in Scotland, where more coals are to be taken on board, and the journey continued to Reikiavik in Iceland. There a

stay will be made for a few days, when the edge of the ice in the west will be made for, which will be followed southwards, but no attempt should then be made to penetrate the pack. Only in case there should be found an open lead, which is not expected, this will be entered. The probability of this is, however, very small. After having passed Cape Farewell, the vessel will call at Ivigtuk, where again it will be coaled from the depots which have been laid up here for the use of the steamer. The course will then be shaped for the west coast of Greenland, probably calling at Egedesminde, to the Auleitsivik Fjord, from the bottom of which the ice journey will be commenced.

This latter will occupy, it is estimated, thirty to forty days, and should be finished thus by the middle of August next. During this time the vessel will steam through the Waigatt to Omenak, where the many deposits of fossil plants will be visited. If the ice and coals should permit, the vessel will steam northwards, if possible as far as Cape York, whereby an excellent opportunity will be offered for geological, mineralogical, botanical, and zoological studies. In the middle of August the vessel will again be due in the Auleitsivik Fjord, and taking the members of the ice-expedition on board, will steam south to Ivigtuk, where a few days' stay will be made for coaling, etc. From here the vessel will steam round Cape Farewell, along the east coast in the open channel, which I expect to find there at that season; and my intention is then, with due reference to the old geographical descriptions of the Icelandic Sagas, carefully to search the fjords which may be accessible.

At the end of September the return journey will be commenced outside the belt of pack-ice to Reikiavik and home.

The distances the vessel will cover are, in round figures, these :—

			Nautical Miles.
Gothenburg ...	to	Thurso	500
Thurso	„	Reikiavik	700
Reikiavik	„	Ivigtuk	870
Ivigtuk	„	Auleitsivik Fjord	540
Auleitsivik Fjord	„	Omenak	330
Omenak	„	Cape York	400

A. E. Nordenskiöld

NOTES

The Fifty-third Annual Meeting of the British Association will commence on Wednesday, September 19, 1883, at Southport. The President Elect is Arthur Cayley, LL.D., F.R.S., Sadlerian Professor of Mathematics in the University of Cambridge. The Vice-Presidents Elect are the Right Hon. the Earl of Derby, the Right Hon. the Earl of Crawford and Balcarres, the Right Hon. the Earl of Lathom, Prof. J. G. Greenwood, LL.D., Prof. H. E. Roscoe, LL.D., F.R.S. The General Treasurer is Prof. A. W. Williamson, Ph.D., F.R.S., University College, London. The General Secretaries are Capt. Douglas Galton, C.B., F.R.S., and A. G. Vernon Harcourt, F.R.S. The Secretary is Prof. T. G. Bonney, F.R.S. The Local Secretaries are J. H. Ellis, Dr. Vernon, T. W. Willis; and the Local Treasurer the Mayor of Southport. The Sections are the following:—A.—Mathematical and Physical Science.—President: Prof. Henrici, F.R.S. Vice-Presidents: Prof. Balfour Stewart, M.A., LL.D., F.R.S., F.R.A.S.; Prof. Stokes, M.A., D.C.L., LL.D., Sec.R.S. Secretaries: W. M. Hicks, M.A.; Prof. O. J. Lodge, D.Sc.; D. McAlister, M.A., M.B., D.Sc. (Recorder); Prof. Rowe, M.A., B.Sc. B.—Chemical Science.—President: J. H. Gladstone, Ph.D., F.R.S. Vice-Presidents: Hugo Müller, Ph.D., F.R.S., For. Sec. C.S.; Prof. T. E. Thorpe, Ph.D., F.R.S., F.C.S. Secretaries: Prof. P. Phillips Bedson, D.Sc., F.C.S. (Recorder); H. B. Dixon, M.A., F.C.S.; H. Foster Morley, M.A., B.Sc., F.C.S. C.—Geology.—President: Prof. W. C. Williamson, F.R.S. Vice-Presidents: Prof. W. Boyd Dawkins, M.A., F.R.S., F.S.A., F.G.S.; J. W. Hulke, F.R.S., Pres.G.S. Secretaries: R. Betley, F.G.S.; C. E. de Rance, F.G.S.; W. Topley, F.G.S. (Recorder); W. Whitaker, B.A., F.G.S. D.—Biology.—President: Prof. E. Ray Lankester, F.R.S. Vice-Presidents: Prof. Gamgee, M.D., F.R.S.; W. Pengelly, F.R.S., F.G.S.; Prof. Schäfer, F.R.S.; W. T. Thiselton Dyer, M.A., B.Sc., F.R.S., F.L.S. Secretaries: G. J. Haslam, M.D.; W. Heape; Prof. A. M. Marshall, M.A., M.D., D.Sc.; Howard Saunders, F.L.S., F.Z.S. (Recorder); G. A. Woods. Department of Anthropology: W. Pengelly, F.R.S., F.G.S. (Vice-President), will preside. Secretaries: G. W. Bloxam, M.A., F.L.S. (Recorder); Walter Hurst. E.—Geography.—President: Lieut.-Col. H. H. Godwin-Austen, F.R.S., F.G.S., F.R.G.S. Vice-Presidents: Sir Rawson W. Rawson, K.C.M.G., C.B., F.R.G.S.; The Rev. Canon Tristram, M.A., LL.D., F.R.S., F.L.S. Secretaries: John Coles, F.R.A.S., F.R.G.S.; E. G. Ravenstein, F.R.G.S.; E. C. Rye, F.Z.S. (Recorder). F.—Economic Science and Statistics.—President: R. H. Inglis Palgrave, F.R.S., F.S.S. Vice-Presidents: Prof. R. Adamson, M.A., LL.D.,; J. Heywood, F.R.S., F.G.S., F.S.A., F.S.S. Secretaries: Prof. H. S. Foxwell, M.A., F.S.S.; J. N. Keynes, M.A., B.Sc.; Constantine Molloy (Recorder). G.—Mechanical Science.—President: James Brunlees, F.R.S.E., F.G.S., Pres. I.C.E. Vice-Presidents: W. H. Barlow, F.R.S., M.I.C.E.; Prof. Osborne Reynolds, M.A., F.R.S. Secretaries: A. T. Atchison, M.A., C.E.; Edward Rigg, M.A.; H. T. Wood, B.A. (Recorder). The first general meeting will be held on Wednesday, September 19, at 8 p.m., when Sir C. W. Siemens, D.C.L., LL.D., F.R.S., F.C.S., M.I.C.E., will resign the Chair, and Prof. Cayley, M.A., LL.D., F.R.S., V.P.R.A.S., President Elect, will assume the Presidency, and deliver an address. On Thursday evening, September 20, at 8 p.m., there will be a soirée; on Friday evening, September 21, at 8.30 p.m., a Discourse on Recent Researches on the Distance of the Sun, by Prof. R. S. Ball, LL.D., F.R.S., Astronomer Royal for Ireland; on Monday evening, September 24, at 8.30 p.m., a Discourse on Galvani and Animal Electricity, by Prof. J. G. McKendrick, M.D., LL.D., F.R.S.E., Professor of Physiology in the University of Glasgow, and in the Royal Institution of Great Britain; on Tuesday evening, September 25, at 8 p.m., a soirée; on Wednesday, September 26, the concluding general meeting will be held at 2.30 p.m.

The number of members of the British Association who have announced their intention of being present at the proposed meeting at Montreal in 1884, continues to increase, and now exceeds 410. It is stated that Lord Rayleigh has accepted the presidency for the Canadian meeting.

Prof. Huxley has been elected a Foreign Member of the U.S. National Academy.

From a list of seven names proposed by the Incorporated Societies throughout the colony, the following gentlemen have been elected honorary members of the New Zealand Institute:—Sir William Thomson, F.R.S., Professor of Natural Philosophy, University of Glasgow; Dr. W. B. Carpenter, F.R.S., C.B., of London; and Mr. R. L. J. Ellery, F.R.S., Government Astronomer at Melbourne.

Prof. Tyndall has resigned his position as Scientific Adviser to the Board of Trade and the Lighthouse Boards.

We are glad to see the ample recognition of music during the present week by the conferring of knighthood on Messrs. Sullivan, Grove, and Macfarren in connection with the New College of Music. Let us hope that the science of music as well as the art will receive due cultivation in the new institution; with Sir George Grove's well-known wide sympathies, however, this may be taken for granted.

THE first eclipse news comes from Lima, under date May 7; the sky was overcast on the 6th, preventing any observation of the eclipse. It is to be hoped the conditions were more favourable on the other side of the Pacific.

THE proposal to open museums and galleries on Sundays was, we regret to say, lost in favour of Lord Shaftesbury's compromise to keep them open in the evenings on two or three nights a week, which does not in the least meet the case of the working classes.

M. RICHI, Professor in the Paris School of Medicine, was nominated Member of the Academy of Sciences on Monday, to fill the place vacated by the death of M. Sedillot. Another election will soon take place in the same section of medicine and surgery.

MR. A. H. KEANE proposes, as soon as he obtains a sufficient number of subscribers, to begin the publication of "A Classification of the Races of Mankind," copious materials for which have now been collected. It will form two large octavo volumes of about six hundred pages each. The publication will probably extend over two years, and at least five hundred names will be required to justify the undertaking, although subscribers to the first need not be committed to purchase the second part also. Subscriptions will be received by Mr. Edward Stanford, 55, Charing Cross, or by Mr. Keane, at University College, London. With regard to the scope and contents of the work, it may be briefly stated that its aim will be to place in the hands of the ethnological student a comprehensive treatise on the races of mankind harmonising with the present state of anthropological inquiry. In the general introduction such broad questions will be dealt with as the evolution of man, the antiquity and specific unity of the species, the present varieties of mankind, the physical and moral criteria of race, the fundamental human types, their evolution and dispersion, the peopling of the continents, the origin of articulate speech, the morphological orders and families of speech, the problem of specific linguistic diversity within the same ethnical group. The great physical divisions of the human family will then be dealt with *seriatim*, and here the same arrangement will be adhered to as that observed in Mr. Keane's ethnological appendices to the Stanford Geographical Series. Each of the main sections of mankind will thus be treated in three separate parts. In the first the salient physical and moral characteristics of the type will be discussed. The second will be occupied with the several main branches of each, and here the proper work of classification will be carried out in detail. Lastly, the third part will consist of an alphabetical index, comprising, as far as Mr. Keane has been able to collect them, all the known races, tribes, and languages of each main division, briefly described, and with copious references to authorities. The price of the work will be 16*s.* per volume to subscribers.

THE Dutch Polar ship *Willem Barents* started again on Saturday from Amsterdam, under the command of Capt. Dalen, for the Arctic regions, to try to discover the Dutch expedition in the *Varna*. Mr. Leigh Smith was present, as well as Mr. Clements Markham, to wish the *Willem Barents* "God speed." Sir Allen Young went over to express his best wishes to the crew of the Dutch ship. Mr. Leigh Smith presented to Capt. Dalen two magnificent silver cups, bearing as an inscription the following words, taken from his journal: "August 3d, 1882; Matotchkin Skarr, Nova Zembla, 10.0 a.m. A sail! A sail! The *Willem Barents*."

THE Danish expedition to Greenland left Copenhagen on Wednesday last week. Its purpose is to explore the east coast of Greenland; and it will probably be away for two years.

THE Sanitary Institute of Great Britain held its annual meeting at 9, Conduit Street, on Monday, Prof. de Chaumont, M.D., F.R.S., in the chair. A report was presented by the Council on the progress of the Institute and on the work at the Congress and Exhibition held by the Institute at Newcastle last autumn. The Chairman gave an address, and the officers for the ensuing year were elected, the President being the Duke of Northumberland, and the trustees Sir John Lubbock, Bart., Mr. Thomas Salt, M.P., and Dr. B. W. Richardson.

THE Prince of Wales opened the Indian Institute at Oxford last week.

TWO divisions of the pupils of Sainte Barbe left Paris on May 4, one for London, and the other for Germany, where they are to stay for three months under the care of their professors, for the purpose of obtaining practical knowledge of the English and German languages.

REAR-ADMIRAL MOUCHEZ is leaving Paris within a few days. for Algiers, where he will establish an observatory at Coubar. This site is deemed excellent for observations.

WE have received the announcement of an aëronautical exhibition, to be held from June 5 to 15 at the Trocadéro, Paris.

THE Whit Monday excursion of the Geologists' Association will be to Hunstanton; and on May 26 to Ealing and Perivale.

THE well-known explorer of the fauna and depths of Lake Baikal, Dr. Godlevsky, who is now in Kamchatka, writes to the East Siberian Geographical Society, from Petropavlovsk, that last summer he transported on board of a steamer of the company Hutchinson, Kool, and Co., fifteen reindeer, of which eleven were males and four females, to Behring's Island. The reindeer were brought to Petropavlovsk from the west coast of Kamchatka in a flock of 150, and the journey took no less than two and a half months, as it was made across the mountains in order to avoid the mosquitos of the valleys. On board the steamer the reindeer were fed with stems and leaves of birch, as also with fresh grass kept in casks with water. Afterwards they also ate hay, and even bread, contrary to the affirmations of the Lamuts, who said that reindeer never eat grass that has been gathered by man. The steamer-journey was made in two days, and the reindeer arrived in good health.

WHEN leaving Yakutsk for the mouth of the Lena, M. Yurgens distributed a number of thermometers to different persons. The *Izvestia* of the East Siberian branch of the Russian Geographical Society now publish the meteorological observations made at Markha, in the province of Yakutsk, district of Viluisk, during the four months of August to November last, by an exile, M. Pavloff. The rapidity with which the cold weather sets in in those latitudes is very interesting. In August the thermometer rose at 1 p.m. as high as 31° Cels. (on August 1), and reached 14° to 20° during the second half of the month. The first frosts were experienced (at 7 a.m.) in September, and already in the first days of October the thermometer sunk (at 7 a.m.) as low as −11° to −20°; and as low as −30° to −35°, and even −39°, during the second half of October. In November it never rose above −32°, and usually stood at seven o'clock in the morning between −39° to −50°; even at 1 p.m. it did not rise, during the whole of the month, above −31°, and usually stood between −33° to −42°, occasionally sinking to −48° and −50° Cels. The underground thermometer, placed at 1·5 feet below the surface, decreased with a remarkable regularity, from 6°·8, on August 1, to 0° on September 12 to 17, to −2° on October 17, to −6° on November 1, and to −17° on November 30.

A CORRESPONDENT points out that at the conclusion of the address by Prof. Du Bois Reymond on "Darwin and Copernicus"

(NATURE, vol. xxvii. p. 558), it is stated that "Charles Darwin lies buried in Westminster Abbey among his peers, Newton and Faraday," whereas Faraday lies buried in the unconsecrated ground at Highgate, and not in Westminster Abbey.

ACCORDING to information received from the United States, some wonderful telephonic feats are being performed by the new Hopkins arrangement. It seems that two operators—one at New York and the other at Chicago, 1100 miles apart—had no difficulty, not only in talking to each other, but in listening to music, and fulfilling other tests. Even the noise produced by changing the diaphragm at New York was distinctly heard at Chicago.

THE Institute of Agriculture at South Kensington completed the work of its first session on Monday, the 7th inst. The Earl of Aberdeen, Chairman of the Council, presided, and distributed Certificates of Merit to 103 students. Notice was given of largely increased opportunities for study during the next session, which commences on October 1.

THE National Eisteddfod of Wales is not without an appreciation for science; in its list of prizes three are offered in natural history, one of 10*l.* 10*s.*, open to all comers, being for the best model of a skeleton of Plesiosaurus. Length of model not to be less than 4 feet, each bone to be separately modelled and removable except those of the cranium and phalanges. Competitors may obtain information and aid on application to Prof. Sollas, University College, Bristol.

THE statue of Leibnitz, intended for Leipzig, has recently been cast by Herr Lenz of Nuremberg, after the model of Prof. Hähnel, and the casting was a complete success. Herr von Miller of Munich has received an order for a colossal statue of Columbus, destined for the city of Cincinnati.

WE hear of a curious incident occurring on Siemens' electric railway at Portrush. Owing to the fact that as yet only part of the line is furnished with electric conductors, a steam-engine is still used as well as the electric locomotive. A few days ago the steam-engine while drawing its load along the line came to a stand through the accident of bursting a boiler-tube. News of the disaster having been sent to the terminus, the stationary dynamo-electric machine which supplies the current was set into action and the electric locomotive despatched to the rescue. It returned an hour later bringing the disabled steam-engine behind it.

M. FOUSSEREAU has lately measured the electric resistance of glass by charging a condenser from a known battery through a given thickness of the glass, and observing the time required to raise the potential of the condenser to a given degree. Bohemian glass was found to be from 5 to 20 times as good a conductor as ordinary glass, whilst flint glass was from 1000 to 1500 times as good an isolator. M. Foussereau found also that annealing the glass increased its resistance in some cases elevenfold.

AT a special general meeting of the Entomological Society of London, held on May 2, Prof. J. O. Westwood, M.A., F.L.S., was elected titular life-president of the Society by acclamation, the occasion being the fiftieth anniversary of the meeting at which steps were taken to establish the Society.

OUR readers will remember the accounts given last autumn of the electric launch which was successfully tried upon the Thames last autumn. This little craft has been running at intervals all the winter and is still in good trim. We learn that the Electrical Power Storage Company have three other electric boats on hand; one of them for the British Government.

A STRONG earthquake shock, with an undulating motion, was felt on the morning of the 8th, at Biancavilla, Sicily.

REPEATED shocks of earthquake were observed in various places in the Spanish province of Valencia on April 14 and 16. Some were of 2 to 3 seconds' duration.

LITERARY piracy would appear to be one of the institutions of the West to which young Japan has taken rather kindly. According to the *Japan Gazette*, the practice of pirating patents, stamps, and labels, in order to palm off spurious imitations for the genuine article, has been carried on for years, and the evil is extending in every direction. Recently a native company called the "Tokio Bookselling Company" was established in the capital, and its chief business appears to be the pirating of English and American schoolbooks. Todhunter's well known Elementary Algebra, Euclid, and other mathematical books have already been reproduced, as well as several American books, Mill's "Liberty," and other volumes. These are published as much like the originals in size, covers, &c., as possible. The Company affixes its imprint to the titlepages, but offers no explanation as to the publication in Japan, and indeed they have no hesitation in reprinting (probably through ignorance) the foreign publishers' notices, such as "Entered according to the Act of Congress in the year, &c., &c." An examination of the reprint of Todhunter's Algebra shows letters upside down, wrong fount letters, letters misplaced, and words improperly spelt, testifying to the slovenly way in which the books have been printed. There is said to be scarcely a page in the book which does not contain one or more errors in orthography, and the mathematical formulæ, which always require such care at the printers' hands, must be in a bad state when ordinary words are so neglected. The direct damage to foreign authors, patentees, and manufacturers by these petty thefts cannot be very great; the real injury is to the Japanese people themselves. Among well-known English labels, that of Bass and Co. has for many years been the subject of innumerable depredations. The striking red diamond on white ground lends itself easily to imitation, while to people who cannot read English letters at all, any strange marks below the diamond are sufficient to represent the names of the eminent brewers. Large quantities of some decoction brewed in Tokio are thus passed off in the interior as Burton ale. A patent law is wanted in Japan, not so much for the protection of foreign inventors (the Japanese Government is far too advanced to think of such a minor consideration as this) as for the protection of the pockets and stomachs of the Japanese themselves.

WE are pleased to observe that the *Chrysanthemum* magazine of Yokohama has commenced its third year with a more ambitious flight. The size is considerably enlarged, as is also the number of subjects treated and the staff of writers. The theological discussions appear to be wholly eliminated, to the increase of the general interest of the journal. The first two numbers of the new issue are now before us, and exhibit no lack of mental power. The literary treatment of the varied subjects discussed is in most cases excellent. The most powerful recruit appears to be Capt. Brinkley, R.A., who writes a serial tale with Japanese characters and scenes, as well as a serial history of Japanese keramics, which deserves more attention in this country than it is likely to get. Higher education in Japan is elaborately treated by Dr. Groth, while Capt. Blakiston, probably the best authority on the ornithology of Japan, writes on that subject. Mr. A. H. Cole is responsible for a popular paper on the Darwinian theory, and Prof. Summers describes some ancient caves near Osaka. These are but a few of the papers in the first numbers of the new magazine, the editor of which appears determined at least to deserve success. We may, however, draw his attention to one defect, surely a very grave one in a scholarly publication such as this, viz. the notices of current literature, which so far have been quite unequal to the other departments.

THE additions to the Zoological Society's Gardens during the past week include a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mrs. Florence A. Hill; a Common Rhea (*Rhea americana*) from Uruguay, presented by Mr. F. R. S. Balfour; a Common Kestrel (*Tinnunculus alaudarius*), British, presented by Mr. A. Lidbury; a Wood Owl (*Syrnium aluco*), British, presented by Mrs. W. Duncan; two Horned Lizards (*Phrynosoma cornutum*) from Texas, presented by Mr. John G. Witte; two Marbled Newts (*Triton marmorata*) from France, presented by Mr. G. H. King; two Viverrine Cats (*Felis viverrina*), an Indian Otter (*Lutra nair*), an Indian Darter (*Plotus melanogaster*), a Hamilton's Terrapin (*Clemmys hamiltoni*), three Indian Gazelles (*Gazella bennetti*) from India, deposited; two Natterjack Toads (*Bufo calamita*), four Marbled Newts (*Triton marmorata*), four Short-nosed Sea Horses (*Hippocampus antiquorum*), from France, purchased; a Black Wolf (*Canis niger*) from India, received in exchange; an Eland (*Oreas canna* ♀), born in the Gardens.

OUR ASTRONOMICAL COLUMN

D'ARREST'S COMET.—Although M. Leveau's elements of this comet for the approaching return to perihelion were communicated to the Academy of Sciences of Paris on January 22, they were unaccompanied by predicted places, and it would appear that the ephemeris has had only a very limited circulation, being confined, if we are rightly informed, to those observers who are in possession of the larger instruments. Hence comparatively few persons may have become acquainted with the circumstances under which this return of the comet to perihelion takes place, and it may not be without interest if we briefly examine the conditions as compared with those of former appearances.

Assuming as usual the intensity of the comet's light (I) to be represented by the reciprocal of the product of the squares of the distances from the earth and sun, we find the following values:—

		I.
1851.	Last observation at Berlin, Oct. 6	0·590
1858.	Last observation at the Cape, Jan. 18 ...	0·151
1870.	Last observation at Athens, Dec. 20 ...	0.154
1877.	Last observation at Athens, Sept. 10 ...	0·127

The greatest distance from the earth at any of these dates was 1·93 on January 18, 1858.

M. Leveau's elements for the approaching return apply to 1883, June 12·0 M.T. at Paris; neglecting, of course, the small effect of perturbation in the interval, the perihelion passage is found to take place 1884, January 13·5765 M.T. at Greenwich. The coordinate constants for apparent equinox of 1883, May 1, are:—

$$x = r.[9{\cdot}99502]\sin(v + 50^\circ\ 13'{\cdot}7).$$
$$y = r.[9{\cdot}99308]\sin(v + 321^\circ\ 48'{\cdot}0).$$
$$z = r.[9{\cdot}36631]\sin(v + 280^\circ\ 38'{\cdot}1).$$

Hence we have the following approximate positions and distances of the comet, with the corresponding values of the theoretical intensity of light, taking dates near the time of new moon:—

G.M.T.	R.A. (12h.) h. m.	Decl. ° ′	Distance from Earth.	Distance from Sun.	I.
June 6 ...	13 9·6 ...	+12 48 ...	2·037 ...	2·615 ...	0·035
July 4 ...	13 13·7 ...	+10 12 ...	2·185 ...	2·415 ...	0·036
Aug. 2 ...	13 37·0 ...	+ 5 42 ...	2·321 ...	2·204 ...	0·038
Nov. 29 ...	17 58·6 ...	−16 55 ...	2·281 ...	1·427 ...	0·094
Dec. 29 ...	19 45·7 ...	−18 8 ...	2·244 ...	1·338 ...	0·111

Whence it will be seen that even when most favourably circumstanced, towards the end of the year, the intensity of light will be less than the lowest value at which the comet has hitherto been observed, viz. 0·127. On November 29 the comet sets about 2h. 8m. after the sun. It was missed at the return in 1864, and the chances of observation at its present visit are by no means encouraging.

Mr. Common informs us that he has made a thorough search for the comet with his large reflector, but without success up to May 7. He remarks that the number of faint nebulæ about its track is surprising.

The orbit of this comet almost intersects that of the lost comet of De Vico, 1844; in heliocentric longitude 339°·37′, with the elements of 1851, the distance between the orbits was only 0·0055 or 507,000 miles.

TEMPEL'S COMET, 1873 II.—The corrected elements of this body by M. Schulhof, from observations at its last appearance in 1878, indicate that, neglecting perturbations, it may be again in perihelion about November 20. The positions calculated on this assumption show that the comet will be very unfavourably placed for observation, and it may escape detection at this return.

RULES AND REGULATIONS FOR THE PREVENTION OF FIRE RISKS ARISING FROM ELECTRIC LIGHTING[1]

THESE rules and regulations are drawn up for the reduction to a minimum, in the case of electric lighting, of those risks of fire which are inherent in every system of artificial illumination, and also for the guidance and instruction of those who have, or who contemplate having, electric lighting apparatus installed on their premises.

The difficulties that beset the electrical engineer are chiefly internal and invisible, and they can only be effectually guarded against by "testing," or probing with electric currents. They depend chiefly on leakage, undue resistance in the conductor, and bad joints, which lead to waste of energy and the dangerous production of heat. These defects can only be detected by measuring, by means of special apparatus, the currents that are either ordinarily or for the purpose of testing, passed through the circuit. Should wires become perceptibly warmed by the ordinary current, it is an indication that they are too small for the work they have to do, and that they should be replaced by larger wires. Bare or exposed conductors should always be within visual inspection and as far out of reach as possible, since the accidental falling on to, or the thoughtless placing of other conducting bodies upon such conductors, would lead to "short circuiting," and the consequent sudden generation of heat due to an increased current in conductors not adapted to carry it with safety.

The necessity cannot be too strongly urged for guarding against the presence of moisture and the use of "earth" as part of the circuit. Moisture leads to loss of current and to the destruction of the conductor by electrolytic corrosion, and the injudicious use of "earth" as a part of the circuit tends to magnify every other source of difficulty and danger.

The chief dangers of every new application of electricity arise from ignorance and inexperience on the part of those who supply and fit up the requisite plant.

The greatest element of safety is therefore the employment of skilled and experienced electricians to supervise the work.

I. THE DYNAMO MACHINE

1. The dynamo machine should be fixed in a dry place.
2. It should not be exposed to dust or flyings.
3. It should be kept perfectly clean and its bearings well oiled.
4. The insulation of its coils and conductors should be practically perfect.
5. All conductors in the dynamo room should be firmly supported, well insulated, conveniently arranged for inspection, and marked or numbered.

II. THE WIRES

6. Every switch or commutator used for turning the current on or off should be constructed so that when it is moved and left it cannot permit of a permanent arc or of heating.
7. Every part of the circuit should be so determined, that the gauge of wire to be used is properly proportioned to the currents it will have to carry, and all junctions with a smaller conductor should be fitted with a suitable safety fuse or protector, so that

[1] Recommended by the Council of the Society of Telegraph Engineers and of Electricians in accordance with the Report of the Committee appointed by them on May 11, 1882, to consider the subject. Members of the Committee:—Prof. W. G. Adams, F.R.S., Sir Charles T. Bright, T. Russell Crampton, R. E. Crompton, W. Crookes, F.R.S., Warren De La Rue, D.C.L., F.R.S., Prof. G. C. Foster, F.R.S., Edward Graves, J. E. H. Gordon, Dr. J. Hopkinson, F.R.S., Prof. D. E. Hughes, F.R.S., W. H. Preece, F.R.S., Alexander Siemens, C. E. Spagnoletti, James N. Shoolbred, Augustus Stroh, Sir William Thomson, F.R.S., Lieut.-Col. C. E. Webber, R.E.

no portion of the conductor should ever be allowed to attain a temperature exceeding 150° F.

8. Under ordinary circumstances complete metallic circuits should be used; the employment of gas or water pipes as conductors for the purpose of completing the circuit should not in any case be allowed.

9. Bare wires passing over the tops of houses should never be less than seven feet clear of any part of the roof, and all wires crossing thoroughfares should invariably be high enough to allow fire-escapes to pass under them.

10. It is most essential that joints should be electrically and mechanically perfect and united by solder.

11. The position of wires when underground should be clearly indicated, and they should be laid down so as to be easily inspected and repaired.

12. All wires used for indoor purposes should be efficiently insulated, either by being covered throughout with some insulating medium, or, if bare, by resting on insulated supports.

13. When these wires pass through roofs, floors, walls, or partitions, or where they cross or are liable to touch metallic masses, like iron girders or pipes, they should be thoroughly protected by suitable additional covering; and where they are liable to abrasion from any cause, or to the depredations of rats or mice, they should be efficiently incased in some hard material.

14. Where indoor wires are put out of sight, as beneath flooring, they should be thoroughly protected from mechanical injury, and their position should be indicated.

N.B.—The value of frequently testing the apparatus and circuits cannot be too strongly urged. The escape of electricity cannot be detected by the sense of smell, as can gas, but it can be detected by apparatus far more certain and delicate. Leakage not only means waste, but in the presence of moisture it means destruction of the conductor and its insulating covering, by electric action.

III. Lamps

15. Arc lamps should always be guarded by proper lanterns to prevent danger from falling incandescent pieces of carbon, and from ascending sparks. Their globes should be protected with wire netting.

16. The lanterns, and all parts which are to be handled, should be insulated from the circuit.

IV. Danger to Person

17. Where bare wire out of doors rests on insulating supports, it should be coated with insulating material, such as india-rubber tape or tube, for at least two feet on each side of the support.

18. To secure persons from danger inside buildings, it is essential so to arrange and protect the conductors and fittings that no one can be exposed to the shocks of alternating currents of a mean electromotive force exceeding 100 volts, or to continuous currents of 200 volts.

19. If the difference of potential within any house exceeds 200 volts, the house should be provided with a "switch," so arranged that the supply of electricity can be at once cut off.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

Cambridge.—Dr. Humphry has formally resigned the Professorship of Anatomy, after having taught anatomy in the University for thirty-six years, at first as assistant to Prof. Clark, and since 1866 as Professor. The Electoral Board for the Professorship consists of Professors Huxley, Allen, Thompson, Flower, Paget, Newton, and Liveing, Dr. Michael Foster, and Mr. J. W. Clark.

The Honorary Degree of LL.D. will be conferred in June upon Count Menabrea, formerly Italian Ambassador to England, Prof. Pasteur, Prof. Michaelis (Strasburg), Sir A. Grant, Bart., Principal of Edinburgh University, Sir John Lubbock, Bart., Sir J. A. G. Ouseley, Bart., Professor of Music at Oxford, Sir Richard Temple, Bart., Lieut.-Gen. Walker, Surveyor-General of India, Mr. Matthew Arnold, Prof. W. W. Goodwin (Harvard, U.S.), Mr. Reginald S. Poole (British Museum), Prof. H. E. Roscoe (Owens College), and Mr. G. F. Watts, R.A.

The Graces creating a Professorship of Surgery without stipend, and authorising the immediate appointment of a Professor of Physiology, are to be voted on to-day (Thursday).

The City and Guilds of London Institute has decided to make a grant of 300*l.* a year for five years, for the purpose of supporting a Chair of Mechanical Engineering in connection with Firth College, Sheffield.

SOCIETIES AND ACADEMIES

London

Royal Society, March 15.—"Atmospheric Absorption in the Infra-Red of the Solar Spectrum." By Capt. Abney, R.E., F.R.S., and Lieut.-Col. Festing, R.E.

Any investigations on the subject of atmospheric absorption are of such importance in the study of meteorology, that we have deemed it advisable to present a preliminary notice of certain results obtained by us, without waiting to present a more detailed account which will be communicated at a future date. From 1874, when one of us commenced photographing the spectrum in the above region, till more than a year ago, the extremely various manners in which the absorption took place caused considerable perplexity as to their origin, and it was only after we had completed our paper on the absorption of certain liquids [1] that a clue to the phenomena was apparently found. Since that time we have carefully watched the spectrum in relation to atmospheric moisture, and we think that more than a year's observations in London, when taken in connection with a month's work at an altitude of 8500 feet on the Riffel, justify the conclusions we now lay before the Society.

A study of the map of the infra-red region of the solar spectrum,[2] and more especially a new and much more complete one, which is being prepared for presentation to the Royal Society by one of us, shows that the spectrum in this part is traversed by absorption lines of various intensities. Besides these linear absorptions, photographs taken on days of different atmospheric condition show banded absorptions superposed over them. These latter are step by step absorptions increasing in intensity as they approach the limit of the spectrum at the least refrangible end. In the annexed diagram [3] Fig. 4 shows the general appearance of this region up to λ 10,000 on a fairly dry day: the banded absorption is small, taking place principally between λ 9420 and λ 9800: a trace of absorption is also visible between λ 8330 and λ 9420. On a cold day, with a north-easterly wind blowing, and also at a high altitude on a dry day, these absorptions nearly if not quite disappear. If we examine photographs taken when the air is nearly saturated with moisture (in some form or another) we have a spectrum like Fig. 1. Except with very prolonged exposure no trace of a spectrum below λ 8330 can be photographed. Fig. 2 shows the absorption-bands, where there is a difference of about 3° between the wet and dry bulbs, the latter standing at about 50°. It will be noticed that the spectrum extends to the limit of about λ 9420, when total absorption steps in and blocks out the rest of the spectrum. Fig. 3 shows the spectrum where the difference between the wet and the dry bulbs is about 6°. Figs. 5 and 6 show the absorption of thicknesses of 1 foot and 3 inches of water respectively, where the source of light gives a continuous spectrum. With ½-inch of water all absorption-bands except that commencing at λ 9420 are absent. It will be seen that there is an accurate coincidence between these "water-bands" and the absorption-bands seen in the solar spectrum, and hence we cannot but assume that there is a connection one with the other. In fact, on a dry day it is only necessary to place varying thicknesses of water before the slit of the spectroscope and to photograph the solar spectrum through them, in order to reproduce the phenomena observed on days in which there is more or less moisture present in the atmosphere. It is quite easy to deduce the moisture present in atmosphere at certain temperatures by a study of the photographs. There does appear a difference, however, in the intensity of the banded absorptions in hot weather and in cold about up to 50°. In the former they are less marked when the degree of saturation and the length of atmosphere traversed are the same as in the latter.

The accepted view, we believe, of absorption of vapours is that they give linear absorptions in certain thicknesses, and as the thickness increases or the density becomes greater, the lines

[1] "The Influence of the Atomic Groupings of the Molecules of Organic Bodies on their Absorption in the Infra-Red Region of the Spectrum." *Phil. Trans.*, Part III., 1881.

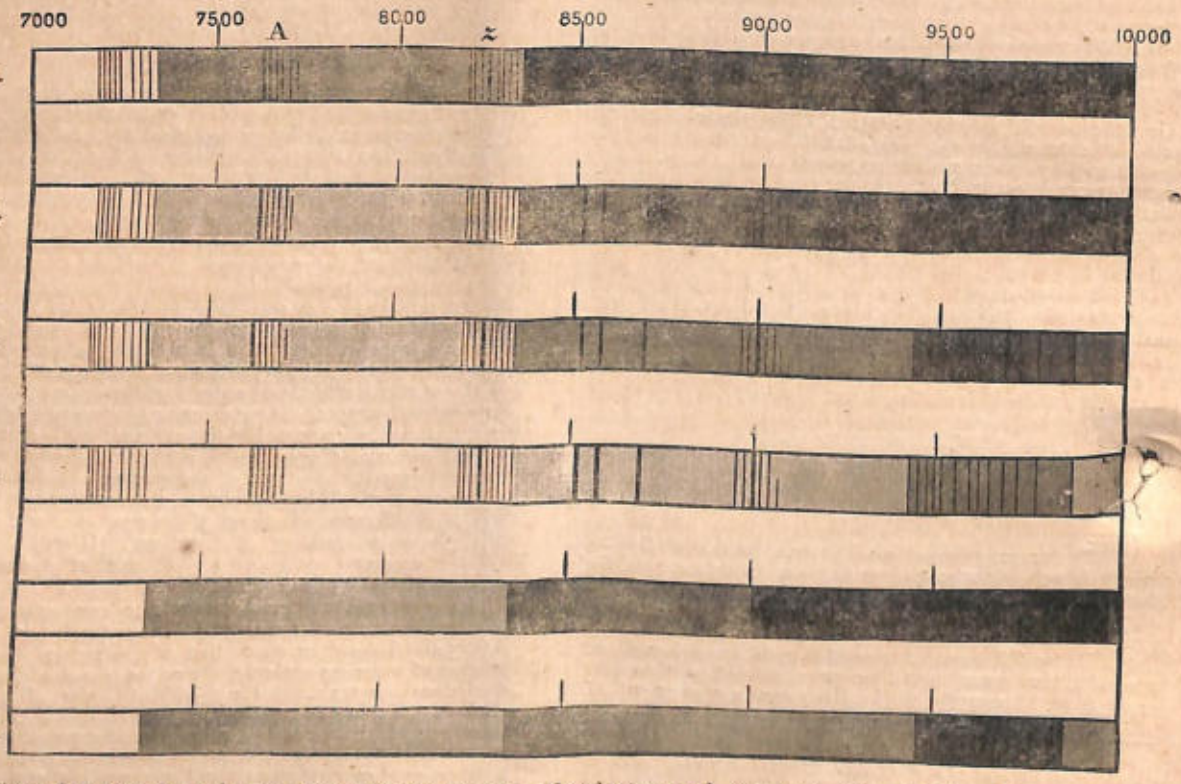
[2] *Phil. Trans.*, 1880.

[3] The black lines given in the diagram are merely lines of reference, and do not represent the aqueous absorption under consideration.

blacken, new lines appear, and gradually total absorption sets in in the region where the lines are most numerous and close. It is in the range of possibility that the presence of a small quantity of vapour might show itself as a haze over some region of the spectrum; if, however, the quantity was gradually increased, the haze would give place to lines, and the phenomena just described would be repeated. Suppose several localities of absorption to exist, the absorptive power of the vapour increasing the further down in the infra-red the locality was situated, it might happen that, whilst one locality showed only a haze of absorption, one further down might show total absorption, and some locality between these two should show linear absorption.

In the case of the absorptions in the solar spectrum we find a very different state of things existing. A comparison of the photographs taken in London on days of different dryness, and with those taken at the Riffel, shows that the linear absorptions are not increased in number or intensity; except so far that the blackness of the lines is increased by the blackness of the banded absorptions, and the same blackness can be induced by placing a certain thickness of water before the slit of the spectroscope: another point is that the Fraunhofer lines in certain regions (say λ 9420 to λ 9800) are so irregularly distributed as to preclude the idea that they all belong to the absorption of aqueous vapour, yet all are equally darkened by the band, and they do not spread out as the blackness of the band increases. This is against the view of the bands being formed by aqueous vapour, as we know it.

The question then arises as to what these "water-bands" can be due—if not due to vapour. This we consider an open question, and one which should be discussed. All we can state is that the absorptions shown are similar to those of water (liquid) and they do not seem to point to the watery stuff existing as vapour,[1] if we take the visible spectrum as a guide. An intense blue sky at sea-level is often indicative of moisture in the atmosphere, and it also seems to be indicative of finely suspended matter of some kind. If this be the case, can this suspended matter be suspended water stuff? for if it be not, there is no reason why the sky should be bluer on a moist day than on a dry day. We would remark that the deep blue sky at sea-level is of a different colour to the black-blue of high altitudes where, if they exist, the fine suspended particles would be largely diminished in number, and the coarser particles which cause white haze would also be fewer. The great difference of



the intensities of the light from the blue sky in England and at 10,000 feet was determined by one of us and communicated to the British Association at Southampton, and the enormous disparity between the two has some bearing on the question we have been discussing.

In the above paper we have described the absorption due to "water stuff" in the atmosphere to λ 9800, as it is only to that wave-length to which the normal spectrum has been as yet published. We wish however to add that there are bands in the solar spectrum commencing at[1] λ 9800, λ 12,200, and λ 15,200, and giving step by step absorptions from one wave-length to the next, as in the diagram, which also corresponds with *cold* water bands. The absorption in the locality from 12,200 downwards is usually total, and it is only on dry cold days or at high altitudes that rays of sufficient amplitude can penetrate to cause photographic impression to be made.

April 5.—"Observations on the Colouring-matters of the so-called Bile of Invertebrates, on those of the Bile of Vertebrates, and on some unusual Urine Pigments, &c.," by Charles A. MacMunn, B.A., M.D. Communicated by Dr. M. Foster, Sec.R.S.

In this paper the result of a systematic examination of the bile and various extracts of the so-called liver of Mollusca and Arthropoda, and of the pyloric or radial cœca and other appendages of the digestive system of Echinodermata is described. The universal distribution of one colouring-matter, which by appropriate experiments is shown to be a chlorophyll pigment, is proved. It occurs in the above organs, and can be detected in the bile of specimens of *Helix* after a six months' fast; for this colouring-matter, since it is found in the appendages of the enteron, the name enterochlorophyll is proposed. The slight differences observable in different cases are shown to be due to the probable greater or less amount of the usual chlorophyll constituents, blue chlorophyll, yellow chlorophyll, and chlorofucine, and the presence of xanthophyll, lutein, or tetronerythrin. Enterochlorophyll is shown to be much more abundant in the liver of Mollusca and in Echinodermata than in Crustacea, as the livers

[1] These wave-lengths are taken from the map from the *Phil. Trans.*, 1880, and are only to be considered approximate

[1] Unless it be held that the water itself holds vapour in solution.

of the last generally contain more lutein, or sometimes tetronerythrin may be present.

The presence of reduced hæmatin is also demonstrated in the bile of the crayfish and in several pulmonate Mollusca, and its respiratory and other uses discussed.

The conclusions which these observations and others led to are summed up as follows :—

(1.) The existence of enterochlorophyll in the so-called liver, or other appendages of the enteron in Invertebrates is definitely established.

(2.) This pigment occurs in greatest abundance in Mollusca, it occurs less frequently in Arthropoda, and its presence in Vermes is not proved.

(3.) The pyloric cœca of starfishes contain it in great abundance, also the intestinal appendages of *Echinus*, which fact shows that the former function like the so-called liver of other Invertebrates.

(4.) The bile of the crayfish and that of most pulmonate Mollusca contains hæmochromogen; in the latter it is generally accompanied by enterochlorophyll, and appears to be concerned more in aërial than aquatic respiration.

(5.) The so-called liver of Invertebrates is a pigment-producing and storing organ, as well as being concerned in the preparation of a digestive ferment.

(6.) The presence of hæmochromogen in the bile of Invertebrates is apparently determined by their mode of living, and it does not appear to be distributed according to purely morphological considerations.

The remainder of the paper deals with vertebrate bile pigments, and contains some observations on abnormal urinary colouring matters, mainly with regard to their spectroscopy. The various bile pigments of Städeler are first dealt with, and some remarks on the bile spectra of animals follow; here it is shown that urobilin can be extracted from the liver of *Salamandra maculata* by means of alcohol, that it is absent from reptilian bile during hibernation, and that the liver of some fishes may contain tetronerythrin which can be extracted from them by suitable solvents. The latter fact suggests an analogous function to that of the liver of Invertebrata.

The results of the examination of a green hydrocele liquid are detailed, which showed beyond doubt that biliverdin was present, and since in that case its origin could be traced to blood pigment, the origin of biliverdin from blood pigment is demonstrated.

The identity of stercobilin and hydrobilirubin got by the action of nascent hydrogen on bilirubin is proved, and a difference between them and febrile urobilin shown to exist.

The statement that the absorption-bands of sheep bile are the same as those which occur in Gmelin's reaction is shown to be erroneous, and a brief description of the method of isolating the colouring-matter of sheep bile and the wave-lengths of its different solutions given. Chlorophyll is shown to be absent.

Under the head of urinary pigments it is shown that the feeble bands described by me in a former paper in the spectrum of febrile urobilin are not due to impurities, but are as much part of the spectrum as the band at F. Urohæmatin and its difference from hæmatoporphyrin and its pathological significance are discussed. A simple method for the detection of indican in urine, some remarks on uroerythrin, and on a peculiar red colouring-matter in pale urine, somewhat like urrhodin, follow. The deductions from this part of the paper cannot be very well given in the form of conclusions, and are therefore scattered throughout the paper.

A drawing of the microscopic structure of the liver of Limax, showing the enterochlorophyll within the liver cells, and maps of the most important absorption spectra described, accompany the paper. All readings are reduced to wave-lengths.

Physical Society, April 28.—Prof. Clifton in the chair.—A paper on colour-sensation, by Mr. H. R. Troop, was read by Mr. Walter Baily, secretary. The author showed that more than three colour-sensations were consistent with the theories of Maxwell and Helmholtz, and explained that four primary separate colour-sensations, in couples, served the theory as well as three. The author gave reasons for the existence of a fifth sensation—that of white. Mr. Stanley mentioned that his father was colour-blind to green, and saw it as a brown. He considered partial colour-blindness very common. Mr. Lewis Wright stated that he found in optical experiments a partial colour-blindness from time to time, and between one of his eyes and the other. He recommended the study of this partial and variable blindness to colours. Prof. Clifton stated that he had found similar variations amongst his students, and considered that one in three was unfit for delicate optical experiments.—Sir John Conroy exhibited a new photometer, which is a modification of Ritchie's, the white screens not meeting at an angle, but almost meeting, and one projecting a little beyond the other, so that the eye could see the outer side of one and a little of the inner side of the other, both visible surfaces being illuminated by the lights. The screens were inclosed in a blackened box.—Mr. Walter Browne then read a paper on the causes and consequences of glacier motion. After reviewing the various theories of glacier motion and criticising each, the author gave reasons for preferring that of Mr. Moseley, namely, expansion by heat. He showed that the regelation theory now commonly accepted appears inadequate, inasmuch as it does not explain the state of flow at low temperatures. Mr. Stanley pointed out that Forbes in his work on Norway gives expansion as a cause of glacier motion. Prof. Perry referred to the experiment of Mr. Bottomley (in which a wire, weighted at the ends, cuts its way through a block of ice) in support of the regelation theory; and Prof. Guthrie described an experiment he had made of the same kind, using a copper wire and a silk cord of the same thickness, equally weighted, on the same block of ice. The wire cut through, but the silk did not. Prof. Ayrton explained this on the assumption that the wire conveyed heat from the air, and enabled the weighted wire to lower the temperature of the ice to the melting point, whereas the silk could not do so without more pressure, that is weight. Mr. W. Coffin referred to the ice-houses of Lake Superior, where he has seen heavy iron implements lying on blocks of ice at a low temperature, without sinking in. Sinking took place when the sun shone on the ice. Prof. G. Forbes said that below forty feet in rock variations of temperature were imperceptible, and probably it was the same with ice. Prof. Macleod, Mr. G. R. Griffiths, Mr. W. Baily, and the President also took part in the discussion.—Prof. Fuller then took the chair, and Prof. Clifton exhibited a new spectrometer of his design. In the spectrometer it is important that the axis round which the prism turns and the axis round which the telescope turns should not be inclined, and in the new instrument these axes are coincident. A single cone, turned very true, has the telescope piece, the circle, and prism plate fixed upon it.

Institution of Civil Engineers, April 24.—Mr. Brunlees, president, in the chair.—The paper read was "Resistance on Railway Curves as an Element of Danger," by Mr. John Mackenzie, Assoc. M. Inst. C.E.

BERLIN

Physiological Society, March 30.—Prof. Du Bois Reymond in the chair.—Instead of the condensed milk, which, owing to its large percentage of sugar, has not kept its place as a food for children, a preparation of milk has lately been introduced into the market from Switzerland, which is protected against fermentation and decomposition by previous cooking. Dr. A. Baginski has chosen the relation of this new infants' food to the digestive ferments as the subject of a comparative investigation, which is not as yet concluded, but which has elicited some physiologically interesting facts about the action and occurrence of these ferments. The rennet ferment is well known to act upon milk both when it is sour and when its reaction is neutral or alkaline, but the rapidity of the curdling when acted upon by the ferment is different for different temperatures. At the temperature of the room the milk curdled only after twenty or thirty minutes; at a temperature of 20° to 25° C. the curdling was already completed in five minutes; at from 30° to 35° C. the curdling lasted about one minute, and it took place at still higher temperatures up to 55° C. in still shorter time. On the other hand, at 60° C. and at higher temperatures the action of the rennet was delayed, and soon ceased altogether. In previously boiled milk rennet also failed to bring about curdling. The rennet ferment was found not alone in the stomach, but also in the small intestines and in several plants, *e.g.* in *Carica papaya*, in artichokes, and in figs. In other plants it was sought for in vain. Decomposition ferments had various actions upon rennet; sometimes they destroyed its action, at other times they did not; the former was particularly the case when the fluid was strongly alkaline. Pepsin had no disturbing influence upon the activity of the rennet, although trypsin had to a marked degree. Dr. Baginski made similar observations upon the effect of decomposition ferment, pepsin, and trypsin upon each other.—Prof.

Brieger reported upon the results of his attempts at a more accurate study and determination of the violent poisons which develop out of animal substances, and which not unfrequently bring about sudden death after eating stale cheese, stale sausage, or stale fish. Prof. Selmi made the first step towards the knowledge of these poisons by preparing powerfully acting bodies out of corpses and decomposing animal matter. These he described under the name of "ptomäin." Prof. Brieger has now investigated their occurrence in analogous substances by means of the reactions that Prof. Selmi gave for these ptomäins, and he has found, among other things, that neurin when exposed to the air very soon develops such a ptomäin, which, like the rest, kills frogs and rabbits with the symptoms of coma. If the neurin remains a long time exposed to the oxidising influence of the air, the violent poison disappears. Further, a poisonous body belonging to the same group was found in a number of artificial peptones, but not in all, and here also this substance was soon destroyed by further alterations taking place in the peptone. This fact could be quite universally established, viz. that a further progressing decomposition in dead bodies destroyed all poisonous substances. Prof. Brieger next approached the task of isolating these easily decomposable and violently poisonous bodies. He succeeded, by working on large masses of dead animal bodies with a series of chemical processes, in preserving the poison in beautiful, large, needle-shaped crystals. He has not as yet determined whether these consist of the poisonous body pure, or whether they also contain other bodies. Therefore the chemical composition of the ptomäins has not yet been certainly determined.

PARIS

Academy of Sciences, April 30.—M. Blanchard in the chair.—The following papers were read:—On the reduction of the barometer and the pendulum to the sea-level, by M. Faye. While Poisson's correction for attraction of a continental mass may be suppressed, the attraction of a hill or mountain on which observations are made must not be neglected. At a station in mid-ocean, such as a volcanic or coral island, the attraction of the island must be considered. Correction for masses like the Alps or Himalayas is much more difficult.—On the pyro-electricity of quartz, by MM. Friedel and Curie. They dissent from Herr Hankel's views as to the distribution of electric tensions in crystals.—On a quaternary base derived from oxyquinoleine, by M. Wurtz.—Prolonged anæsthesia obtained by protoxide of nitrogen at normal pressure, by M. Bert. The new method he has tried on animals is to cause anæsthesia first with the pure protoxide, then give a mixture of the protoxide and oxygen (the blood then recovering the oxygen necessary to it); then the pure protoxide may be given again. Thus both asphyxia and return to sensibility are obviated. A dog was kept insensible half an hour. A mask and two caoutchouc bags are all that is necessary.—On the project of the African interior sea, by M. de Lesseps. He replies to M. Cosson.—On a theorem of partitions of complex numbers contained in a theorem of Jacobi, by Prof. Sylvester.—*Résumé* of meteorological observations made during the year 1882, at four points of Haut Rhin and the Vosges, by M. Hirn. *Inter alia*, the greatest actinometric differences do not always correspond to the most limpid skies. The slight mist or dust, which stops part of the luminous rays, does not absorb the calorific rays.—A new general formula for the development of the perturbative function, by M. Baillaud.—Observations of solar spots and faculæ at the Royal Observatory of the Roman College during the fourth quarter of 1882, by M. Tacchini. After the secondary minimum in August, the spots increased to a considerable maximum (relatively) in November, then decreased suddenly to a minimum in December. During 1882 there appears a greater activity than in 1881. The mean of faculæ is slightly greater in 1881 than in 1882.—Observations of solar protuberances, faculæ, and spots at the Royal Observatory of the Roman College, during the third and fourth quarters of 1882, by the same. The number of protuberances *per diem* was nearly the same as in the first half-year, but the height and extension were somewhat greater. The minimum was in September and October. Spots, faculæ, and protuberances were more numerous near the equator than in the previous half-year.—Observation of the transit of Venus at St. Thomas in the Antilles by the Brazilian Commission, by M. de Teffé. The third and fourth contacts were observed.—On the use of a birefringent glass in certain observations of spectrum analysis, by M. Cruls. By giving the crystal an alternative motion of rotation the extraordinary spectrum is displaced, and the eye is thus helped to see peculiarities better than if the spectrum were at rest. Again, the two spectra of a faint star may be so juxtaposed that the bright parts of the one correspond to the dark channellings of the other, so that vision is aided.—Determination of a particular class of surfaces, &c. (continued), by M. Darboux.—On continuous periodic fractions whose numerators differ from unity, by M. de Jonquières.—On the generalisation of the theorem of Fermat, by M. Lucas.—On the same, by M. Pallet.—On the groups of linear equations, by M. Poincaré.—On some double integrals, by M. Goursat.—On the Eulerian functions, by M. Bourguet.—On the cycle of motors with explosive gases, by M. Witz.—On the transmission of sound by gases, by M. Neyreneuf. With a sensitive flame arrangement he proves that carbonic oxide has about the same transmitting power as air; carbonic acid much greater. He finds Hauksbee's law inexact.—On the analogy that exists between the allotropic states of phosphorus and of arsenic, by M. Engel.—Research on the metallic derivatives of amides; means of distinguishing a monoamide from a diamide, by M. Gal.—On a process of hardening soft calcareous stones by means of fluosilicates with a base of insoluble oxides, by M. Kessler.—On a means of foreseeing liberations of firedamp, by M. de Chancourtois. On the supposition that movements of the earth's crust often cause such liberation, he proposes the use of delicate seismographic apparatus.—New histological researches on the termination of the biliary conduits in the lobules of the liver, by M. Kanellis.—On the structure of the nervous system of Hirudineæ, by M. Saint Loup.—On the incubation of eggs of a hen affected by cholera of fowls, by M. Barthélemy. The eggs contained germs of the microbes; these were only developed with aërial respiration when the allantoid yielded to the blood the oxygen necessary. (The development of the embryo stopped between the eighth and the tenth day.)—Comparison between the bacilli of tuberculosis and of leprosy (continued), by M. Babes.—M. Common presented a negative photograph of the nebula in Orion (taken January 30). An equatorial reflector of three feet aperture was used, and the dry plate was exposed 39 minutes.

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THURSDAY, MAY 31, 1883

HUMAN FACULTY AND ITS DEVELOPMENT

Enquiries into Human Faculty and its Development. By Francis Galton, F.R.S. (London : Macmillan and Co., 1883.)

AMONG all his anthropological brethren Mr. Francis Galton has no competitor in regard to the variety and versatility of his researches. So various and versatile, indeed, have these researches been, that, with the exception of "Hereditary Genius" and "English Men of Science, their Nature and Nurture," we have become accustomed to regard them as disconnected pieces of work, which from time to time were thrown off like sparks from the flame of an active mind. But in the present volume he has collected in one series most of the investigations which he has separately published during the last ten years, and this collection when read in the light of a considerable amount of additional matter, clearly shows that the sundry investigations which were separately published were not separately conceived, but have throughout been united by the bond of a common object. This object, as the title of the book indicates, is that of inquiry into Human Faculty and its Development. And it is evident, when this fundamental note is supplied, that it serves to join not only the researches contained in the present volume, but also those of its above-named predecessors, into one harmony or design.

But although there is one harmony pervading this work, the changes of theme are so numerous that we shall not be able to touch upon them all, and must therefore restrict ourselves to considering the more important.

The book begins with an essay on "Variety of Human Nature," as to features, bodily qualities, energy, sensitivity, special senses, &c. In the course of this chapter the leading results of the author's well-known investigations on composite portraiture are brought in, the audibility of high notes in different individuals, as well as in different species of animals, &c. Next there follows a chapter on "Anthropomorphic Registers," which is mainly directed to showing the desirability of keeping family records of the anthropometry of children until they are old enough to continue the records for themselves. To facilitate this process—which he deems to be one of much practical importance in view of all that is now known touching the potency of hereditary influences—Mr. Galton urges that anthropometric laboratories should be established where all the needful periodic portraiture and other observations on the life-history of children should be made and preserved on the payment of small fees by the parents. Without such systematic observation any one may pass through life without knowing that he presents so strongly marked a peculiarity as that of colour-blindness; while the benefit to the race, a few generations hence, of a large mass of statistics of such consecutive anthropometry of numerous families would probably be of the utmost value. Indeed this suggestion as to anthropometric laboratories may be taken as the foundation of Mr. Galton's proposed science of "eugenics," to a tracing of the main principles of which his work on "Human Faculty" is chiefly concerned.

After a chapter on "Statistical Methods," we come to a consideration of "Character." So far as sex is concerned, "one notable peculiarity in the character of the woman is that she is capricious and coy, and has less straightforwardness than the man . . . and there can be little doubt as to the origin of the peculiarity. . . . The willy-nilly disposition of the female in matters of love is as apparent in the butterfly as in the man, and must have been continuously favoured from the earliest stages of animal evolution down to the present time. It is the factor in the great theory of sexual selection that corresponds to the insistence and directness of the male. Coyness and caprice have in consequence become a heritage of the sex, together with a cohort of allied weaknesses and petty deceits, that men have come to think venial and even amiable in women, but which they would not tolerate among themselves."

The type of character which leads to criminality is next discussed, and is shown by statistics to be strongly inherited. After a few pages on the allied topic of insanity, Mr. Galton passes on to consider the gregarious and slavish instincts, where he shows from first-hand observations on wild or but partly domesticated animals the immense utility of these instincts. We ourselves inherit from our savage ancestry instincts of the same kind, and thus it is that the less intellectually developed among us are so prone to submit ourselves, like sheep, to the guidance of a leader, and even to the tyranny of a despot.

Passing on to intellectual differences, a long and interesting account is given of mental imagery, the main points of which are already known to the readers of NATURE. It is remarkable that men of science, and of hard thinking generally, are for the most part totally deficient in this faculty. The discussion of mental imagery naturally leads to the resemblance which Mr. Galton has previously pointed out between his composite photographs and general ideas; each alike are "generic images," and in many matters of detail the analogy, or, as we should prefer to call it, the illustration, holds good.

Next we come to a chapter on Psychometric Experiments, which is devoted to an account of interesting experiments on the association of ideas. The influence of early association and sentiment is shown by these experiments, and by considerations drawn from them, to be much greater than is generally supposed.

One of the most interesting chapters in the book is that which next follows on the History of Twins. It will be remembered that the main fact elicited by this inquiry is that nature counts for much more than nurture; for it is shown that "instances exist of an apparently thorough similarity of nature, in which such difference of external circumstances as may be consistent with the ordinary conditions of the same social rank and country do not create dissimilarity. . . . The twins who closely resembled each other in childhood and early youth, and were reared under not very dissimilar conditions, either grow unlike through the development of natural characteristics which had lain dormant at first, or else they continue their lives, keeping time like two watches, hardly to be thrown out of accord except by some physical jar. . . . The effect of illness, as shown by these replies, is great, and well deserves further consideration. It appears that the constitution of youth is not so elastic as we are apt to think; but that an attack, say of scarlet fever, leaves a permanent

mark, easily to be measured by the present method of comparison."

The essay which follows on the "Domestication of Animals" is not so interesting, because not so original, as the rest of the book; all its points are obvious to any one who has thought about the subject at all.

A consideration of the Possibilities of Theocratic Intervention next leads the way to a reappearance of the author's paper on the Objective Efficacy of Prayer. Here the logic is unexceptionable as far as it goes, but it is not such as to leave no loophole of escape for orthodox belief. The argument is that if prayer is of any avail in an objective sense, it ought to admit of being shown by the statistical method to be so. But, as the present writer pointed out nine years ago when considering this essay, the statistical method applied to such a case is of doubtful validity. To show this we may quote one paragraph from our previous criticism:—

"What, then, is the whole state of the case? To illustrate it most fairly, we shall take the strongest of the examples supplied by Mr. Galton, viz. that of the Clergy. As Mr. Galton truly observes, in no other class are we so likely to obtain men of Prayer. Suppose, then, for the sake of calculation, that one-half of the clergy are sufficiently prayerful to admit of their petitions influencing the course of physical phenomena. Next, let us suppose that one-half of their successful petitions for physical benefits are offered on behalf of individuals other than themselves: this is equivalent to reducing the number of the prayerful clergy to one-fourth. Here we ought to add that in whatever degree this section of successful prayers may influence the prayerless classes of the community, in that degree is the comparison still further vitiated. Neglecting this point, however, let us lastly suppose that one-half of the petitions for physical benefits offered on the petitioner's own behoof are answered by physical benefits of some other kind; . . . this is equivalent to reducing the original number to one-eighth. Now I do not think any of these suppositions are extravagant. Let us see the result of applying them to Mr. Galton's tables. According to these tables, the clergy as a class live, on an average, two years longer than men of any of the other classes quoted, notwithstanding we are repeatedly told that, as a class, they are the most poorly constitutioned of all. Now, neglecting the last-mentioned point, and also the fact that all clergymen do not pray for long lives; still, even on the above data, an average of two additional years over all the clergy allows, when concentrated into one-eighth of their number, an average of sixteen additional years of life to every pious divine. Of course this illustration is not adduced in order to prove that prayer has in this case been observably effectual. The greater length of life enjoyed by the clergy may be conceded due to the cause assigned by Mr. Galton—viz. the repose of a country life—or to any other cause, without in any way affecting the present argument. All we are engaged in showing is that the statistical method is not a trustworthy instrument wherewith to gauge the physical efficacy of prayer; and the above illustration has been adduced to show that even if the petitions of the pious clergy for lengthened days were somewhat more effectual than those of Hezekiah, statistics would still be so far unable to take cognisance of the fact that the observable average increase of two years over the entire body of the clergy might reasonably be attributed to other causes. Yet length of days is perhaps the most conspicuous, and therefore the most easily tabulated, of all physical benefits for which it is possible to pray."[1]

After some well considered remarks on Enthusiasm, or "to what degree the strong subjective views of the pious are trustworthy," the book begins to draw towards its final object, which is virtually that of marking out the lines of what may appropriately be called a new religion. We have of late had so many manufactures of this kind that the market is somewhat glutted, and therefore it is very doubtful how far this new supply will meet with an appropriate demand; but we can safely recommend Mr. Galton's wares to all who deal in such commodities as the best which have hitherto been turned out. They are the best because the materials of their composition are honesty and common sense, without admixture with folly or metaphor. He says: "We may not unreasonably profess faith in a common and mysterious whole, and of the laborious advance, under many restrictions, of that infinitely small part of it which falls under our observation, but which is in itself enormously large, and behind which lies the awful mystery of all existence." Having, then, this faith in the seen, and observing that, whatever the far-off divine event may be to which the whole creation moves, the whole creation is certainly moving in an upward course of evolution, Mr. Galton submits that man has now reached a level of intelligence which should enable him, not merely to know these things, but to do them. He ought to "awake to a fuller knowledge of his relatively great position," and begin to regard it as his high prerogative to cooperate with the unknown Worker in promoting the great work. He may infer the course that evolution is bound to pursue, and might therefore "devote his modicum of power, intelligence, and kindly feeling to render its future progress less slow and painful. Man has already furthered evolution very considerably, half unconsciously and for his own personal advantages; but he has not yet risen to the conviction that it is his religious duty to do so deliberately and systematically."

Several directions in which such assistance might be yielded are pointed out in the concluding pages of the book, especially in the way of "eugenics"; and there can be no question that, if the idea of promoting evolution could become generally, or even largely, invested with a feeling of obligation, the prospects of the race would be greatly brightened. The most important field of human activity under such circumstances would obviously be that of improving the race by selection, and Mr. Galton throws out several well considered suggestions as to the way in which this might be done without violating so precious a product of evolution as the moral sense, or seriously interfering in any other particular with the ordinary usages of civilised life.

We have said enough to show that in respect of its matter "Human Faculty" is an unusually interesting work; but we should not do it justice were we to conclude this brief notice without alluding also to its manner or style. There is a strand of humour woven through the serious texture of the whole, which, together with the ingenious cast of thought and the ingenuous cast of feeling, affords a most pleasing and instructive study, unconsciously presented, of the nature and nurture of an English man of science.

GEORGE J. ROMANES

[1] Burney Prize Essay on "Christian Prayer and General Laws," pp. 265-6 (Macmillan and Co., 1873), where other and more important considerations against this application of the statistical method are given. [I may observe that this essay was written on a thesis which was set by the Vice-Chancellor of Cambridge, and I still think that, upon its given basis of Christian belief, all the more important of its arguments hold, both as regards prayer and miracles.—G. J. R.]

THE GEOLOGICAL HISTORY OF BRITAIN

Contributions to the Physical History of the British Isles; with a Dissertation on the Origin of Western Europe and of the Atlantic Ocean. Illustrated by 27 Coloured Maps. By Edward Hull, F.R.S., &c. (London: Stanford, 1882.)

If Geology may be correctly described as a history of the earth, then a geologist is in the first place and essentially a historian. His function is to trace back the gradual growth of the world, organic as well as inorganic, and to show through what successive stages the present conditions of geography and of life have been reached. His materials, like those of the historian of human progress, become fewer and less reliable in proportion to their antiquity. More and more as he pilots his way into the records of the remoter past is he driven to piece together their evidence with conjecture, until at last evidence of every kind fails him, and he is reduced to mere speculation. There is undoubtedly a strong temptation to minds of a particular order to indulge in wide excursions into the unknown realms of primeval cosmogony. The fewer the facts that may serve as guide-posts the greater the scope for the fancy. So long as the picture does not appear to outrage our established conceptions of physical law its enthusiastic limner considers himself within the safe limits of fact or, at least, of legitimate inference. He does not stop to consider whether his restoration may not in itself be flagrantly improbable, or whether enough may not be already known on the subject to show that it is quite untenable. In this way much harm has been done to the progress of sound geology.

The attempt to restore former aspects of the globe, or at least of different areas of its surface, may be made with fair measure of success up to a certain point. As the geologist goes beyond that point he leans more and more on conjecture. It is very desirable, for his own sake as well as for that of the subject, that the actual data on which he proceeds should be definitely stated. His readers ought to know exactly where ascertained fact ends and restoration begins. Yet he may be so convinced of the truth of his restoration that, until challenged to set down in definite form the amount of evidence actually at his command, he may honestly have come to regard some of his deductions as well-established truths. He cannot, however, be too careful to draw a clear and sharp line between what he knows and what he infers, when it is his object to write geological history.

One of the most attractive branches of this history is that which deals with the gradual growth of a country or continent. Many interesting and important memoirs on this subject have appeared, more especially in England, where it has long been a favourite study. Sketch-maps have been published indicating in a somewhat vague way what the authors believe to have been the probable distribution of sea and land at former geological periods. Among those who by their original researches have contributed materials towards the restoration of ancient geographical conditions in Britain, Prof. Hull, the Director of the Irish Geological Survey, deserves honourable mention. His papers upon the changes that occurred during Carboniferous, Permian, and Triassic times, and upon the south-eastward attenuation of the Jurassic series in this country are well known to geologists. He has now, however, attempted a much more ambitious task than any one has yet ventured upon in this department of science. He has published a series of maps representing what he conceives to have been the successive geographical phases through which the region of the British Islands has passed from the earliest geological times. Without discussing the question whether the information at the disposal of geologists is yet sufficiently ample and precise to warrant an attempt of this kind, one may at least demand that every care should have been taken to show precisely what is actually known fact and what is inference. But Mr. Hull gives us scanty guidance in this respect. There is not one of his restorations that does not prompt the question on what grounds its details have been put together. The position of former areas of sea is usually sufficiently definable, but it is by no means so easy to say what was land, and still more difficult to assign even the most conjectural outlines to the shores. The author doubtless thinks his geographical boundaries vague enough; we are inclined to regard them as a good deal more definite than the actual evidence in many cases warrants. To take as an illustration his map of Britain during the Upper Silurian and what he terms the "Devono-Silurian" periods; we should like to know on what grounds he makes Wales, the Lake Country, the north-west of Ireland, and much of the Highlands of Scotland elevated land at that time. The evidence, so far as we are aware, is rather in favour of these areas having been under the Upper Silurian sea; at least we know of no proof that they formed high lands, even after the plication and metamorphism he refers to. Nor is there any information as to why the author marks the area from the mouth of the Humber to the middle of Norfolk as part of his continental land. He mixes up in a curiously unintelligible way his "Devono-Silurian" and Lower and Middle Devonian formations, some of the estuaries or lacustrine areas being placed with the older group of strata, others with the younger, in accordance with certain theoretical ideas which he has already published.

According to Prof. Hull's maps, most of the high grounds of Britain have been elevated dry land since the Lower Silurian period. No one, however, who has seriously studied how the land is continuously denuded, can believe this representation to be even approximately true. Our mountains must have been many times, and probably for long intervals, under water. Even if no large amount of sedimentary material were laid down upon them, their submergence would at least protect them from the degradation which would otherwise have worn them down. How does Prof. Hull know that Ireland, which was almost if not entirely under water during the Carboniferous period, did not remain more or less in the same condition through several succeeding ages? The presence of Permian and Triassic deposits in the north-east of the island shows that considerable denudation of the Carboniferous rocks had taken place there before these red strata were laid down. But surely it is rather a large inference from these slender data that all the rest of the country was land, with high grounds where we see them still. How can he tell that Ireland was not entirely submerged beneath the Jurassic sea? Had it not been

for the protecting sheets of basalt in Antrim, probably no fragment of Lias or Oolite would now have been left in the island. Prof. Hull submerges his country a little more in the Cretaceous period, but still keeps the high grounds as islands. Can he produce any evidence that they were so? Has he sounded the Cretaceous Ocean about which he is so precise? The denudation of Ireland has been unquestionably enormous, but had the country been above water as long as the Director of its Geological Survey imagines, we fear that every geological formation would have been worn off its surface down to the very platform of its fundamental or Laurentian gneiss. In fact the continued survival of the country above water could only have been maintained by repeated uplifts that in some measure at least compensated for its superficial degradation.

The chapters accompanying the maps furnish the reader with some of the information he requires to be able to estimate the extent of the data on which the restorations have been constructed. But they do not give him nearly enough of it. Some of their statements moreover will provoke criticism not less than the maps themselves. The author asserts, for instance, as if it were an established fact, that what he regards as the "essentially oceanic" conditions under which the Chalk was formed prevail from Ireland to the shores of the Caspian, and from Belgium to North Africa. We can hardly suppose him to be ignorant of the fact that the Chalk is but a local development of calcareous matter confined to the western part of the European area. Yet the author not only spreads the Chalk across most of Europe and into Africa and Asia, but proceeds to infer from this asserted extension that "according to all the laws of terrestrial mechanics" the site of much of the North Atlantic must have been then dry land. In other words, he first infers a wide deep ocean, and then creates a continent to keep it company.

One of the chapters, with the sounding title of "The Genesis of the North Atlantic Ocean," will be read with amazement by those who have watched the progress of recent research on this question. The author begins it by the following oracular announcement: "I date the genesis of the North Atlantic Ocean, properly so called, from the close of the Carboniferous period; and, consequently, from the same period, that of the British Isles and Western Europe." One is disposed at once to ask what may be his "exquisite reason" for this extraordinary statement, and he frankly volunteers it. It appears to be somewhat as follows:—The Carboniferous rocks of Western Europe were much disturbed at the close of the Carboniferous period, being thrown into east-and-west ridges. Similar movements took place over the eastern States of North America, the direction of the ridges being there more nearly north and south. It may be concluded, therefore, that the formation of the basin of the Atlantic Ocean formed part of these terrestrial movements!

In his Preface the author tells us how he had long entertained the idea of preparing such a series of maps as he has now published, and how he was deterred by the cost of publication. At last, in what we venture to think was an evil hour for his reputation, the Royal Dublin Society generously agreed to bear the expense. The maps were therefore prepared and published in the Society's *Transactions*, and a fresh impression has been printed off from the plates for the volume just issued. Fortune would have been kinder to one whose long services entitled him to gentle treatment at her hands had she induced him still to keep his restorations in the privacy of his own portfolio, at least for some years to come, or, if they must be published, had she insisted on greater accuracy in the statement of what is known and greater precision in the expression of what is conjectured.

OUR BOOK SHELF

Die Verwandlungen der Tiere. Von Dr. Otto Taschenberg, Privat-dozent in Halle. Pp. 268, with 88 Illustrations. Small 8vo. (Leipzig: G. Freytag, 1882.)

THIS forms the seventh volume of the series known as "Das Wissen der Gegenwart," the object of which is to give, in an attractive and popular form an outline of the "science of the day." Metamorphosis and development are always interesting subjects, and we are of opinion that Dr. Taschenberg has contrived to place them before his readers in a specially clear manner by choosing a few types in each class of the animal kingdom upon which to dilate, leaving the blanks to be filled in by more advanced students than those for whose instruction this elementary treatise is intended. The author goes in this manner through the entire animal kingdom, and so far as we can see he is well posted up in most of the latest discoveries and theories bearing upon his subject; we miss, however, any allusion to the disputed position of *Limulus*, although the metamorphoses of that remarkable animal are not entirely overlooked. A work such as this is naturally to a large extent a compilation, and in all such works the good or bad influence exercised depends upon the acumen of the author in his choice of subjects and authorities. In the present instance our author seems usually to have consulted the best and most modern authorities. The numerous illustrations are mostly very good; some of them are superlatively so. In these, as in the text, various works have been laid under contribution; and probably to no work is the author under greater obligation than the text-book on embryology by the lamented F. M. Balfour, but due acknowledgment is always made.

The concluding chapter is devoted to a sketch of the "evolution of species," in which, in a few pages, the author has contrived to give succinct historical information, winding up with a definition of "protoplasm," in connection with which a German translation from well-known English lines is given, and perhaps the definition was so modelled as to fit the lines. We reproduce them, just to show what latitude may be allowable in translation:—

"Der grosse Cäsar tot und Lehm geworden
Verstopft ein Loch wohl vor dem rauhen Norden,
O dass die Erde, der die Welt gebebt,
Vor Wind und Wetter eine Wand verklebt."

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Natural Selection and Natural Theology

I AM very glad to find from Prof. Asa Gray's last communication (NATURE, vol. xxviii. p. 78) that the result of our "amicable discussion" has been that of coming to an agreement on all

points save one, which, as he truly observes, is "seemingly capable of settlement by scientific inquiry." This point simply is as to whether variation in plants and animals is promiscuous (not "lawless") or is restricted to beneficial lines.

Now with reference to this point, I observed in my first letter (NATURE, vol. xxvii. p. 362) that if variation is promiscuous it is only the favourable variations that are able to survive, and hence the sole ground of entertaining natural selection as an agency in the process of evolution; but that, on the other hand, if it could be shown that variations always take place exclusively in the directions required for a development of the adaptations, so as to leave no room for the operation of natural selection, then the evidence of design as deduced from the theory of evolution would become comparable with that evidence as deduced from the theory of special creation. But I also pointed out that "the burden of proof lies with the natural theologian to show that there *has* been some such intelligent guidance of the variations, not with the evolutionist to show cause why there *may not* have been such guidance." And now I understand Prof. Gray accepts this as a correct statement of the case, observing in his last letter that, if variation is promiscuous, "then no doubt the theory of natural selection may be 'the substitute of the theory of special design,' so as to efface that evidence of underlying intelligence which innumerable and otherwise inexplicable adaptations of means to ends in nature was thought to furnish. If it is not so, then the substitute utterly fails."

It is most satisfactory to me that the issue has thus been clearly reduced to a simple matter of scientific observation, and I may add that I am much interested to find that a naturalist of such high standing as Prof. Gray still holds to the view that, "so far as observation extends, it does not warrant the supposition of omnifarious and aimless variation." Of course, if I had not believed in "aimless variation" as of universal occurrence in organic nature, I should never have supposed that the theory of evolution by natural selection could in any way touch the theory of special design; but finding that my fundamental belief in this matter is still open to question by so esteemed an authority as Prof. Gray, and observing that we are here upon the ground of a purely scientific question, I should like to say a few words in justification of this belief.

No one has attended to the subject of variation with a tenth part of the care that was bestowed upon it by Mr. Darwin, and no one has been gifted with a better judgment in matters of this kind. I shall therefore restrict myself to giving a brief outline of his matured opinion upon the subject.

Everywhere he speaks of variation as promiscuous or aimless, but never as "lawless," and only under a carefully guarded meaning as accidental. That is to say, he has no doubt that every variation is due to causes, though not of a teleological kind. Of these causes he regards changes of environment as highly important; but nevertheless he is inclined to lay less weight on these "than on a tendency to vary due to causes of which we are quite ignorant."[1] But with reference to variations not taking place exclusively in beneficial lines he says: "As man has domesticated so many animals and plants belonging to widely different classes, and as he certainly did not choose with prophetic instinct those species which would vary most, we may infer that all natural species, if exposed to analogous conditions, would, on an average, vary to the same degree. . . . We have abundant evidence of the constant occurrence under nature of slight individual differences of the most diversified kinds; and we are thus led to conclude that species have generally originated by the natural selection of extremely slight differences; . . . although each modification must have its own exciting cause, and though each is subjected to law, yet we can so rarely trace the precise relation between cause and effect, that we are tempted to speak of variations as if they arose spontaneously. We may even call them accidental, but this must be only in the sense in which we say that a fragment of rock dropped from a height owes its shape to accident. . . . If an architect were to rear a noble and commodious edifice without the use of cut stone, by selecting from the fragments at the base of a precipice wedge-formed stones for his arches, elongated stones for his lintels, and flat stones for his roof, we should admire his skill and regard him as the paramount power. Now the fragments of stone, though indispensable to the architect, bear to the edifice the same relation which the fluctuating variations of organic beings bear to the varied and admirable structures ultimately acquired by their modified descendants. . . . The shape of the fragments at the base of our precipice may be called accidental, but this is not strictly correct; for the shape of each depends on a long sequence of events, all obeying natural laws; . . . but in regard to the use to which the fragments may be put, their shape may be strictly said to be accidental. . . . Can it be reasonably maintained that the Creator intentionally ordered, if we use the word in any ordinary sense, that certain fragments of rock should assume certain shapes, so that the builder might erect his edifice? If the various laws which have determined the shape of each fragment were not predetermined for the builder's sake, can it be maintained with any greater probability that He specially ordained for the sake of the breeder each of the innumerable variations in our domestic animals and plants;—many of these variations being of no service to man, and not beneficial, far more often injurious, to the creatures themselves? Did He ordain that the crop and tail-feathers of the pigeon should vary in order that the fancier might make his grotesque pouter and fantail breeds? Did He cause the frame and mental qualities of the dog to vary in order that a breed might be formed of indomitable ferocity, with jaws fitted to pin down the bull for man's brutal sport? But if we give up the principle in one case,—if we do not admit that the variations of the primeval dog were intentionally guided in order that the greyhound, for instance, that perfect image of symmetry and vigour, might be formed,—no shadow of reason can be assigned for the belief that variations, alike in nature and the result of the same general laws, which have been the groundwork through natural selection of the formation of the most perfectly adapted animals in the world, man included, were intentionally and specially designed. However much we may wish it, we can hardly follow Prof. Asa Gray in his belief 'that variation has been led along certain beneficial lines,' like a stream 'along definite and useful lines of irrigation.'"[2]

I could give a number of other quotations to the same general effect from the writings of Mr. Darwin, but I think these are enough to show, as I have said, that if there is any evidence of variations being determined in special and beneficial lines, it now lies with the teleologist to adduce such evidence. If this could be done it would be a matter of immense importance, both from a scientific and a speculative point of view, seeing that on the scientific side it would be subversive of the whole theory of natural selection, and on the speculative side would therefore leave us where we were before the publication of the "Origin of Species." But at present the whole weight of such scientific evidence as we have appears to me unquestionably opposed to Prof. Gray's statement that, "so far as observation extends, it does not warrant the supposition of omnifarious and aimless variation."

GEORGE J. ROMANES

[1] "Origin of Species," 6th edition, p. 107.

[2] "Variations of Animals and Plants under Domestication." Secon edition, vol. ii. pp. 401-2, 410, 416, 426-8.

Carson Footprints

IN NATURE (vol. xxvii. p. 578) which I have just seen, the Duke of Argyll calls your attention to the so-called human footprints uncovered in the prison yard at Carson, Nevada. I have carefully examined these tracks, and read a paper on the subject before the California Academy of Science, August 27, 1882. Unfortunately the *Proceedings* of the Academy have not yet been published, though copies of the several papers on this subject have been printed and privately distributed. Perhaps a brief account of these tracks will be interesting to your readers.

The nearly horizontal strata in which they occur consist of beds of sandstone with thin layers of fine shale. The track layer, which is one of these latter, has been uncovered over an area of nearly two acres, and forms the floor of the prison yard, while the stone removed has been used to build the prison. In the course of the excavation a number of fossils have been found, among which the most important are the jaws and teeth of an elephant, probably *E. Americanus*, and two species of horse, *Equus Pacificus* and *occidentalis*; some freshwater shells, all of recent species, have also been found. The age of the deposit seems to be that of the "Equus beds" of American geologists, which by some are put in the uppermost Pliocene, and by others in the lowest Quaternary. It is probably a transition between the two.

The whole surface of the shale exposed in the prison yard is literally covered with tracks of many kinds, but the mud was so soft when the tracks were made that the nature of many of them can only be guessed. Some were probably those of a horse; some probably of a wolf; some certainly of a deer;

many were those of long-legged wading birds. But the most interesting are those of the Mammoth and the problematical so-called human tracks. About the Mammoth tracks there can be no doubt. Some of these were uncovered by blasting in my presence; round basin-shaped impressions, 5 inches deep and 22 inches across, and occurring in regular alternating series, the hind-foot tracking almost perfectly with the fore-foot. The nature of the so-called human tracks, however, is far more doubtful. These occur in several regular alternating series of 15-20. In size they are 18–20 inches long, and 8 inches wide. In shape they are many of them far more curved than the human track, especially in soft mud. The stride is 2½ to 3 feet, and even more. The outward turn of the track is in many cases greater than in human tracks, especially in soft mud. But the most remarkable thing about them on the human theory is the straddle, *i.e.* the distance between the right and left series. This I found to be 18 and even 19 inches, which was fully as great as that of the mammoth tracks. This is probably the greatest objection to the human theory. On the other hand, the great objection to the quadrupedal theory is the apparent singleness of the tracks, and the absence of claw-marks. But it must be remembered that the tracks are deep, and the outlines somewhat obscure, and also that the mammoth tracks, on account of tracking of hind with fore-foot, are in most cases, though not always, single.

After careful examination for several days, the conclusion I came to was that the tracks were probably made by a large plantigrade quadruped, most likely a gigantic ground-sloth, such as the Mylodon, which is found in the Quaternary, or the Morothenium, which is found in the upper Pliocene of Nevada. The apparent singleness, the singular shape, and the large outward turn of the tracks I attribute to the imperfect tracking of hind and fore-foot on the same side, while the absence of claw-marks was the result of the clogging of the feet with mud.

This view seems to me most probable,[1] but many who have seen the tracks think them human, and I freely admit that there is abundant room for honest difference of opinion. On any theory the tracks are well worthy of scientific attention.

Berkeley, California, May 12 JOSEPH LE CONTE

Cloudiness of Aquarium

CAN you tell me the reason why the water in my fresh water aquarium will not remain clear, but becomes cloudy throughout in a few days after filling.

The aquarium in question holds about twelve gallons of water. It stands in a window facing north. I have in the water two or three water-plants, among them a water-aloe. At the bottom are small gravel stones, which have been thoroughly washed before using. Floating on the surface for the benefit of a few newts is a piece of virgin cork, on which is placed some carpet moss. I had a dozen minnows and four newts to begin with, but nine of the minnows and two of the newts have died, manifestly from the fouling of the water.

The framework of the aquarium is iron, with a slate floor. The glass sides are fixed with red lead. There is a copper tube for overflow purposes, which was inserted when a fountain was used in the centre. This has now been removed and the water is stagnant.

It is now some years since I have kept an aquarium, and I cannot divine the reason for the above-mentioned cloudiness of the water. I shall be much obliged if you or some of your correspondents will help me.

May 9 X.

So far as I can judge from "X's" description, the cloudiness of the water in his aquarium is due to the abnormal development of some unicellular algal (Palmellaceæ) or to the prolific spore-production within it of one of the filamentous forms (Confervaceæ). This may be obviated by screening the back of the tank from the access of light. Possibly "X" may find on examination that the cistern whence he obtains his supply has been left uncovered, and that the intruding algal has established itself and entered upon the reproductive process in that position. In that case he should either isolate the water he requires in a dark place for a week or so, when the spores will die, or obtain his supply from a purer source. An investigation with a high power of the microscope of the turbid water complained of will speedily determine whether the explanation here suggested is the correct one. By way of illustration, I may mention that the water of the ornamental pond in the centre of the Horticultural Gardens, supplied clear and bright shortly before the opening of the Fisheries Exhibition, had assumed within a few days and still retains the colour and consistency of green-pea soup through the rapid development, under the action of light, of a unicellular cryptogam in the manner above described. W. SAVILLE KENT

[1] Views similar to my own have recently been expressed by Prof. Marsh and by G. K. Gilbert.

Singing, Speaking, and Stammering

REFERRING to the letters in NATURE (vol. xxvii. p. 580) on my classification of vowel sounds, allow me to explain:—

The classification given in the "Principles of Elocution" (4th ed., 1878) was retained from the earlier editions of that work, because of the difficulty, or impossibility, of exhibiting the complete vowel system of visible speech without V.S. symbols. For the purposes of the book on Elocution, the latter were not required; but a *note* (on p. 36) immediately preceding the "General Vowel Scheme" explains the basis of the complete classification developed in visible speech.

As you have given an abstract of my classification, quoted by Dr. Stone from "Principles of Elocution," I shall be glad if you will show your readers the following abstract of the visible speech classification:—

Classification of Vowels in Visible Speech

Nine Lingual positions yield 9 Primary vowels ... ...
Each Primary vowel yields a "Wide" variety by faucal expansion = 9 Wide vowels ... ... } = 18 Lingual vowels.
Each Lingual vowel yields a "Round" variety by labial contraction ... ... } = 18 Labio-lingual vowels.
} = 36 Normal vowels.

Each Normal vowel yields a possible variety by *higher*, *lower*, *broader*, or *narrower* formation = 36 + 144 = a total of 180 vowels.

The mutual relations of the different sounds may be exhibited in this way:—

LINGUAL.

	Primary.			Wide.		
	Back.	Mixed.	Front.	Back.	Mixed.	Front.
High	7	4	1	7	4	1
Mid	8	5	2	8	5	2
Low	9	6	3	9	6	3

LABIO-LINGUAL.

	Primary.			Wide.		
	Back.	Mixed.	Front.	Back.	Mixed.	Front.
High	7	4	1	7	4	1
Mid	8	5	2	8	5	2
Low	9	6	3	9	6	3

In this arrangement, each No. 1, No. 2, No. 3, &c., in the four sets is formed from one and the same lingual position. These relations are plainly exhibited in the symbols of visible speech. They cannot be shown by ordinary letters, but the use of *numbers*, as above, may make the arrangement clear to those who are not acquainted with visible speech.

Washington, D.C., May 12 ALEX. MELVILLE BELL

On the Cold in March, and Absence of Sunspots

I WAS travelling when Dr. Woeikof's letter appeared in NATURE (vol. xxviii. p. 53), and could not sooner reply to his criticisms on my communication (vol. xxvii. p. 551), "Unprecedented Cold in the Riviera—Absence of Sunspots." Let me first remark that I do not go so far as to "ascribe (as Dr. Woeikof says that I do) the great cold of March, 1883, at the

Riviera, to the absence of sunspots." My observations prove only the *coincidence* of a sudden and unprecedented visitation of cold, with an absence of sunspots (the more remarkable as occurring during a *maximum* sunspot period); and the further *coincidence* of a progressive rise in temperature with the return of the sunspots; but I add, "These observations are too few and too imperfect to warrant any decided conclusions; but they add to those already made in evidence of the connection between the absence of sunspots and the diminution of terrestrial heat; and I trust they may be followed by further and more exact investigations, to determine the influence of our great luminary on the weather and climate of the world."

It does not appear to me that Dr. Woeikof has succeeded in establishing a parallel between Cannes and Suchum-Kale on the Black Sea; which, however sheltered locally, must, far more than Cannes, be liable to chilling influences in the cold winds from the lofty mountains and vast elevated steppes to the north, extending even to the Arctic regions. Therefore the fall of 31° below average in March, 1874, might not be extraordinary, even in a *year* with a considerable number of sunspots. It is not stated that the spots continued in this particular month.

The case of Cannes may be thus stated: With a climate usually so mild in winter that frost and snow are of rare occurrence; and this winter, with slight frost only three times before February, and none at all in that month, the average minimum being 44°,—on March 7 minimum fell to 36°, with a heavy fall of snow; and on the 8th, 10th, 11th, and 12th, the minimum fell further to 27°·7, 27°, 24°·1, 25°·7. The sunspots, which had been observed by my friend, Mr. Campbell, of Islay, to be large and active until February 26, suddenly disappeared, and on February 28 and March 3 I found no spots; on the 10th and 11th only one or two small spots. On the 12th they began to appear in numbers, with a large oval facula. From that day they continued to increase, and the temperature gradually rose to the ordinary average.

I will not occupy space with further arguments, but I will merely state some more facts with regard to the extraordinary intensity and universality of this invasion of cold, and my further observations of the sunspots. At my villa at Cannes, which is favourably placed in position and shelter, the register did not fall so low as in other parts. At Dr. Frank's villa, Grand Bois, more open to the north (thermometer in louvred box, a metre above ground), the minima were: March 7, 27°; 9, 25°·2; 10, 21°; 11, 21°; 12, 20°; 13, 25°. At Villa Beaulieu, more sheltered (therm. also in louvred box), minima were: March 7, 29°; 9, 27°; 10, 25°; 11, 26°; 12, 28°. Dr. de Valcourt's minima are somewhat higher; but he adds this note: "La période de froid du 7 au 14 Mars, 1883, a été très remarquable; elle est unique, depuis que les observations regulières ont été recueillis à Cannes." Where instrumental records are wanting, we refer to the report of the "oldest inhabitants," and learn that there has not been a cold so severe or destructive to oranges and olives since the year 1820.

Extraordinary and intense as was this invasion of cold, it might have been supposed due to local or regional causes only, had it been confined to Cannes and its neighbourhood. In my former paper I stated that I was not informed how far the cold had extended to other countries and latitudes. We still need further exact information on this point, but what has already reached us goes far to prove that the cold was universal, and not limited to a region. In England, Mr. Thomas Plant writes to the *Times* from Moseley, Birmingham:—"After one of the mildest winters registered in the Midland Counties, the month of March, which is generally expected to be the beginning of spring, has been colder this year than any corresponding month for 38 years." "When we consider the power of the sun in March, as compared with December, January, and February, then we can realise some idea of the prolonged and most abnormal cold of the month now ended." By private information I learn that at the same time, in Stockholm, Centigrade's thermometer fell 13°, and at St. Petersburg 18°, below freezing. Unusually intense cold in March is also reported from Canada. In the south we hear of snow and frost in South Italy, Sicily, Algeria, Egypt, and even Nubia. Later still there have been reports of snow on the mountains of Madeira and California, where it had never been seen before.

Since March 19, the date of my former letter, I have been able to make sketches of the sun's position on 49 days.[1] Of these observations the following summary may suffice. In number the spots varied from 3 to 18; the larger showing, more or less, holes or clefts of central umbra, with fringe of penumbra. Faculæ, or clouds of whiteness, were often seen around the larger spots. The spots varied in number and form from day to day; and although the same large spots and even groups could be traced for several successive days, they never retained the same aspect during the whole period of the sun's semi-rotation. On April 17 the spots were at their maximum; in number 18, in three groups. During this period, from March 19 to April 19—thirty-one days—the mean minimum temperature was 46°·2, mean maximum 57°·9.

From April 20 to May 7 there was considerable diminution of the spots; numbers not exceeding 8; and on May 7 there was only one large spot, with surrounding facula. The mean temperature of these seventeen days was—minimum 49°·8, maximum 60°.

From May 8 to 16 spots were few, from 2 to 8; but two of them were very large, with umbra and penumbra and sometimes adjoining faculæ. The mean temperature of these nine days was—minimum 52°·7, maximum 63°·8.

Here my observations terminate, as I left Cannes on the 16th, and have no means of observing in London, even if the atmosphere permitted. But I conclude by strongly commending the attentive study of the sun not only to astronomers and physicists, but also to practical meteorologists, as an interesting and not difficult addition to their work of observation, and one likely to supply information concerning the most important factor in the problems of weather and climate.

C. J. B. WILLIAMS

47, Upper Brook Street, May 25

[1] I use only a modest achromatic of 3½ inches focus, and 2¼ inches aperture, which, projecting the solar image on a white card, exhibits the spots with umbra and penumbra, and the faculæ, sufficiently for this purpose. Of course a more powerful instrument would show a great deal more, both in number and in construction of the spots. For instance, on April 17, when I made out 18 spots, Mr. Campbell's solar image exhibited 104, with marvellous variety in the larger spots, and in the dome-like expansion of the adjoining faculæ. But these details, so deeply interesting in heliography, are not wanted for meteorological purposes.

The Soaring of Birds

MY thanks are due to Mr. R. Courtenay for the notice he has taken (NATURE, vol. xxviii. p. 28) of my letter on the Soaring of Birds (vol. xxvii. p. 592). It is a great satisfaction to me to find my general conclusion supported by his observations. As to the possibility of a soaring bird utilising a downward current of air, I stand corrected. There is no difficulty in agreeing with Mr. Courtenay that the bird, finding itself in a downward current "will descend swiftly so as to acquire the necessary impetus for a rapid escape;"—that is to say, it will seek to make the best of a bad bargain. But it is not so easy to see that the bird, in a current approaching the perpendicular, will "acquire an impetus much more than compensating for the slight loss of elevation;"—that is, will actually make a profit out of a seemingly adverse condition.

This paradox, however, becomes more acceptable by the aid of an illustration:—A marble held lightly just within the rim of a hemispherical bowl, if let drop, will barely reach the opposite rim, but, if struck sharply downward, will run up the opposite side and leap up above the opposite rim. In like manner a bird, struck by a downward current as by a hammer-stroke, may speedily acquire a downward velocity greater than that due (under gravity) to the height through which it has descended; and may therefore rise, if it can escape from the downward current into a horizontal (or *a fortiori* into an upward) current, to a greater height than if it had fallen from the same starting-point through still or horizontally-moving air.

I am very much obliged to Mr. Courtenay for pointing out this interesting result. It gives completeness to the theorem, which now stands thus: that any alternations in the strength or direction of air-currents can be so utilised by birds as to enable them to soar.

HUBERT AIRY

Woodbridge, May 25

The Zodiacal Light

THE phenomenon to which your correspondents allude, under the head of zodiacal light, was seen by me in the month of April, 1852. At the time I wrote a letter to the *Times*, in which I suggested it might be caused by the reflection of the sunlight at the surface of two masses of air of different densities, however irregular the bounding surface might be, in the same manner as the line of light seen reflected between the observer

and the sun across the sea. One of your correspondents has suggested a more probable origin, viz. particles of ice in the air. From other correspondents it seems that the sun column is not always vertical, which might be the result of the general flame of the reflecting surface not being parallel with the earth's surface.

In the June number of the *Philosophical Magazine* there was a notice of a sun column as seen at Orkney by the Rev. C. Clouston, who at that period made meteorological observations for that publication. He says that in the month of April of that year the drought was unprecedented, the atmospheric pressure great, and the temperature high. I believe two of these characteristics belonged to the recent month of April if not the third, the high temperature. He says it was seen six times, and once or twice before he noted the date, and also before sunrise.

Saltburn, May 21 E. R. TURNER

Sheet Lightning

MAY not this be an auroral phenomenon, at times, at least, and hence the differences of opinion as to its nature? Reading Wilkes's "Narrative of the U.S. Expedition," I find the following:—"On the 7th February (1840) the weather had become less boisterous, and having reached latitude 49° S., longitude 155°·23 E., the aurora Australis again appeared. It was first seen in the north, and gradually spread its coruscations over the whole heavens; the rays and beams of light radiating from nearly all points of the horizon to the zenith, when their distinctive outlines were lost in a bright glow of light, which was *encircled by successive flashes, resembling those of heat lightning on a summer's night*. These formed a luminous arc in the southern sky, about 20° in altitude, from the upper part of which rays were continually flashing towards the zenith. Light showers of rain finally shut it out from view." FRED. PRATT

Clapton Park, May 25

Pocky Clouds

FOR twenty years I was constantly observing the forms and appearances of the clouds, as clues to the weather and its changes. I observed this form on a very great number of occasions, and from experience always came to the conclusion, "no rain today," and I can only remember two occasions on which the conclusion was not justified. I saw it again a few days ago, with the same result of good weather.

I always termed it the "bubble" cloud till I saw Dr. Clouston's work. It seems to me to be a body of vapour the upper surface of which is being acted upon by an upper current of very dry and rarefied air, causing a great and rapid evaporation, and thence a gradual and unequal cooling and shrinkage of the under surface in the detached globules from which it takes its name. I have seen a very simple illustration while passing through the laundry, and observing a neglected trough of soapsuds cooling down and nucleating in the exact form presented by the pocky cloud, and with the same gradations of tint.

This kind of cloud is generally observable at periods most probable for storms and electric condensations, the which, acting at a distance, would influence outlying areas of upper atmosphere and cause this form of cloud condensation in the way explained. In my observations I have generally found the cloud revert to uniform sheet stratus rather than to disappear in cloudlets in the upper air. FRED. PRATT

Clapton Park, May 25

Clerk Maxwell's "Devil on Two Sticks"

IN the very interesting life of Clerk Maxwell which has lately appeared there are frequent references to a philosophical toy, from which he seemed to derive endless amusement. He calls it the "devil on two sticks." Can you give your readers any account of it? The editors take it for granted that the apparatus is well known, but I cannot find any one here who can tell me what it is. DENNY LANE

72, South Mall, Cork

The Centres of a Triangle

CONTINUING my suggestion in your number of May 3 (p. 7), I propose not only to call the circle circumscribing a triangle the *circumcircle*, but also to call its centre the *circumcentre*, and in the same way to speak of the *incentre*, the three *excentres* (namely, the *a*-excentre, the *b*-excentre, and the *c*-excentre), and the *midcentre*.

The line joining the circumcentre to the orthocentre, on which the masscentre and the midcentre lie, may be appropriately called the *central line* of the triangle.

Similar abbreviations would apply to the radii of these circles; they might be spoken of as the *circumradius*, the *inradius*, the a-*exradius*, the b-*exradius*, the c *exradius*, and the *midradius*.

May 25 W. H. H. H.

THE ROYAL GEOGRAPHICAL SOCIETY

THE annual meeting of the Royal Geographical Society on Monday was of rather more than usual scientific interest. Sir Joseph Hooker was presented with the Royal Medal which the Society has awarded him, Mr. Colborne Baber being the recipient of the Patron's Medal; while among the speakers at the dinner, besides Sir Joseph Hooker, were Mr. Spottiswoode and Prof. Huxley. From the address of the President, Lord Aberdare, it is evident that geographical research, and especially exploration, has been as active as ever during the past year, yet, as the speakers we have named pointed out, the discovery of new countries must have a limit, and in time must come to an end. Still there will be plenty of work for geographers to do in the wider acceptation of the term geography, implied in the presentation of the Royal Medal to so distinguished a botanist as Sir Joseph Hooker. In the words of Mr. Spottiswoode, and as we have frequently pointed out in these pages, geography in its modern acceptation includes "an accurate delineation of the earth's surface, and an exact account of its inhabitants and of their habits, of the animal and vegetable life, and its distribution over the face of the globe." In this direction the Society has a long and brilliant career before it. But as Prof. Huxley humorously pointed out in replying for the "other societies," these societies "were all growing a little dull. He did not say this in the way of reproach. The progress made in research and accuracy in methods of procedure involved that consequence. So long as there were large regions of knowledge which the methods of modern science had not penetrated, so long was it possible to go to meetings of societies, and to hold brilliant discussions. Looking at the means which now existed for the diffusion of information, he had been led to think that in many cases where the field of knowledge had been extensively explored the utility of societies was constantly diminishing, and that sooner or later it would be necessary to devise other means of effecting the results now attained by meetings of societies. But there was one thing which would not be reached at any period of time by any other organisation than that of societies, and that was the stimulus which was given by their meetings to investigators; and the reward they found for their toils and sacrifices in such a welcome as had been given that night to his long-tried friend Sir J. Hooker."

The prosperity of the society continues to be maintained. Mr. Clements Markham read the annual report, which showed that during the year the number of Fellows elected was 163, besides three honorary corresponding members, and the total number of Fellows on the list (exclusive of honorary members) was 3392. The total net income for the financial year ending December 31, 1882 (exclusive of balance in hand and 1005*l.* sale of Exchequer Bills) was 7937*l.*, of which 5652*l.* consisted of entrance fees and subscriptions. The net expenditure during the past year was 8779*l.*, including 1135*l.* spent on expeditions. The sale of 1000*l.* of Exchequer Bills was rendered necessary to meet the Society's contribution to the Eira Relief Expedition, but this sum had since been generously presented to the Society by Mr. Leigh Smith. The investments and assets of the Society on December 31, 1882, exclusive of the map collection and library, amounted to 39,831*l.*

THE TRUE ORBIT OF THE AURORAL METEOROID OF NOVEMBER 17, 1882

AFTER many fruitless efforts to conciliate the apparently widely diverging data, given by the numerous observations of this most interesting phenomenon; and after having been many times on the same point as Mr. H. D. Taylor (vol. xxvii. p. 434), who has given the first approximate calculations of this orbit, namely, "to give up the reconciling of such contradictory evidence," I have devoted my Easter holidays to new research on the true orbit. Besides the encouraging letters received from some of the English observers, I found still another motive in the observation of Mr. Julius Dupire at Laon (France, $\beta = 49^\circ 34'$), who had the kindness to give me ample information, for which I offer him my sincere thanks, and in the communication of the following citation, kindly given me by Prof. Ch. Montigny, of Brussels, taken from the *Bulletin de l'Observatoire de Bruxelles*, November 18, 1882: "À 6h. 23m. un énorme rayon d'un blanc vif s'éléva à l'horizon E.N.E.; il traversa le ciel en passant le zénith et alla s'éteindre à l'horizon O.S.O." A similar phenomenon has been observed by Dr. F. Terby at Louvain. The great attraction of the Laon observation consisted in the fact that the meteor's apparent path was there seen at the north side of the zenith, this being in harmony with the Brussels zenith observation, and promising a good determination of the sought orbit.

In the first place I took the following apparent orbits from the numerous given observations. They can or must be taken as great circles, and must, in this case, fulfil the condition of intersecting one another in two opposite points of the sphere. In fact their intersections are contained within a small space and gave me an approximate position to one of these two points, $\alpha = 70^\circ 30'$, $\delta = +14^\circ 30'$.

These five apparent orbits, the only ones given completely, are the following:—

No.	Place of observation.	Pages of NATURE, vol. xxvii.	Data of observation.	Deduced horizontal direction.	Local time of max. elevation.	Observer.
1	York, $\beta = 53^\circ 58'$.	87, 140, 434	The centre was 6° or 7° below the moon's centre (given not directly after observation); 30° elevation in meridian (probably a mistake or a printer's error, being in contradiction with the other data).	E. 20° N.–W. 20° S. (nearly); deduced by the observer.	6h. 4 or 5m.	H. D. Taylor.
2	Clifton (Bristol), $\beta = 51^\circ 28'$.	85	8° from Saturn, to the right, in a line inclined 45° to the horizon.	E. 20° N.–W. 20° S.	6h. 4m.	A. M. Worthington.
3	Old Windsor, $\beta = 51^\circ 30'$.	87	First seen a little S. of Aldebaran; moves across the moon's disk.	E. 20° N.–W. 20° S.	6h. 6m.	John L. Dobson.
4	Utrecht, $\beta = 52^\circ 5'$.	296	Aldebaran and two points in the equator at 110° and 290° R.A.	E. 20° N.–W. 20° S.	6h. 24m.	Prof. J. A. C. Oudemans.
5	Zonnemaire (near Zierickzee), $\beta = 51^\circ 42'$.	296	Aldebaran and β Pegasi (α Pegasi on p. 296 was a printer's error).	E. 20° N.–W. 20° S.	6h. 21m.	P. Zeeman.

Tracing these five apparent orbits on a celestial globe they gave the intersection point above mentioned. It is clear that this point, joined with the eye of the observer, gives the direction of the true path. This point lying further, the globe being placed on the different latitudes and hours, not far from the point E. 20° N. of the eastern horizon (at Utrecht 7° above the horizon), it is evident that the lines of intersection, formed by the plane of the mean horizon with the planes of the apparent orbits must be nearly parallel to this direction. That these lines of intersection cannot be true parallels follows from the observations of the four students at Cooper's Hill (p. 97), from that of Mr. Joseph Clark at Street, communicated by Mr. J. E. Clark, at York; and from that of Mr. A. S. P. at Cambridge (p. 87), who saw the phenomenon disappear in the S.W., S.W. and S.S.W. Further the *Revue Mensuelle* of M. C. Flammarion (2me Année, p. 72), containing a short report of Mr. Dupire's observation, mentioned above, gives also an observation made at Ploërmel ($\beta = 47^\circ 55'$, λ 2° 23' W. Greenwich), where the phenomenon disappeared in the west.

Now I have drawn a stereographic map on a large scale, and brought the intersection of the vertical plane through Brussels, with the bearing E. 20° N. It is clear that the true orbit must lie in the vertical plane. Further I have constructed the angles formed by the planes of the apparent orbits with the respective horizons, correcting, if necessary, for the curvature of the earth, and after much trouble found the following path, being a straight line having the properties given in the table on p. 106, that enables us at the same time to compare the results of my construction with the data of the different observations given in order from E. to W.

I hope that the observers will be content with the degree of harmony between their observations and my results. I believe that a small change in the direction of the orbit's plane will give still more harmony between calculation and observation, but the orbit found satisfies the chief observed facts, and gives the greatest divergence, where the observations have the smallest sharpness. I believe I have proved by this research that there existed, with the aurora of November 17, 1882, cosmic dust, passing through the upper strata of our atmosphere with great velocity, and giving, according to the most interesting observation of Mr. Rand Capron (p. 84), "the usual green line" of the aurora spectrum. Thus nature itself has been so kind as to give an experiment that till now, and perhaps for ever, is beyond human

Places of observation.	Height of path in kilos.; meteoroid at max. alt. above horizon.	Max. altitude of the apparent path: Deduced from the found path.	Max. altitude of the apparent path: Deduced from observation.	Comparison of the position with regard to the moon. Constructed.	Comparison of the position with regard to the moon. Observed.	Horizontal bearings of the apparent path. Constructed.	Horizontal bearings of the apparent path. Observed.	Observers.	Remarks.
I. Utrecht.	159·5	° 54	° 54	—	—	E. 22° N.	E. 20° N.	Prof. J. A. C. Oudemans.	The inclination on the horizon, from observation 6° to 7°, from construction 7°.
II. Zonnemaire (near Zierickzee).	150·6	56	62	—	—	E. 22° N.	E. 20° N.	P. Zeeman.	The altitude 62° uncertain, according to the observer's letter.
III. Brussels.	149·1	90	90	—	—	E. 20° N.	E. 22° 30′ N.	(?)	From the *Bulletin* of the Observatory. The horizontal bearing was given E.N.E.
IV. Laon.	140·9	51½	60 to 65	—	—	W. 17° S.	W.	Jul. Dupire.	The maximum altitude seems to have been determined by estimation.
V. Ipswich.	135·1	—	—	The centre 1° above the moon's centre.	Exactly across the moon.	E. 21° N.	E. 10° N. (?)	Hub. Airy.	The eastern horizon cloudy, the width of the meteoroid 5′.
VI. York.	134·3	—	—	The centre 10° below the moon's centre when nearest.	The centre 6° to 7° below the moon's centre when nearest.	E. 37° N.	E. 20° N. (p. 434) given by the observer.	H. D. Taylor.	The angle below the moon's centre is given from memory (p. 140), and was difficult to estimate. I find the horizontal bearing of the apparent path, given by the observer, E. 25° N. The small inclination to horizon makes it very uncertain.
VII. Cambridge.	133·5	—	—	—	—	W. 51° S.	S.S.W., or W. 67° S.	A. S. P.	It disappeared below the horizon in the S.S.W. (p. 87).
VIII. Greenwich.	132·1	—	—	The centre 5° above the moon's centre.	A little above the moon (p. 83).	About E.N.E., W.S.W.	E.N.E., W.	W. H. M. Christie.	P. 83.
IX. Lincoln's Inn Fields (London).	132	—	—	Across the moon's disk.	Across the face of the moon.	—	—	Edw. Pollock.	P. 141.
X. Cooper's Hill (near Windsor).	130·6	—	—	Nearly above the moon, the centre about 2° above the moon's centre.	Just below the moon.	S. 58° W.	S. 45° W.	Messrs. Sykes, Wildeblood, Thornhill, and Wackrill.	P. 99. Communicated by Mr. Herb. McLeod. The meteoroid being seen from Windsor and from Lincoln's Inn Fields, to the north of Cooper's Hill *before* the moon, it could not *possibly* be *below* the moon, when seen from Cooper's Hill. There *must* be a mistake in the communication or in the observation. The being *before* the moon is naturally a *fact*, where a mistake is *impossible*.
XI. Old Windsor.	130·5	33	30½	Across the face of the moon.	Across the face of the moon.	—	—	John L. Dobson.	P. 87. The apparent path not being very sharply given, the difference in the observed and constructed max. altitude is very small.
XII. Ramsbury (near Hungerford).	127·9	—	—	Across the moon. The centre 2° below the moon's centre.	It seemed to pass directly over the moon.	—	—	Alfred Batson.	P. 100. The given position of the moon (p. 141) seems to be geocentric. I find an altitude of the moon of about 27° at Greenwich.
XIII. Clifton (Bristol).	126·2	28½	25	The centre 6° beneath the moon's lower cusp (measured vertically). The upper boundary of the meteor 4° beneath the lower cusp.	Passing close under the moon, the centre 1½° beneath the moon's lower cusp.	E. 32° N.	E. 20° N.	A. M. Worthington.	The apparent path having but little inclination on the horizon, and its eastern extremity being not without some doubt, the intersection point is very uncertain.
XIV. Street.	126·2	—	—	—	—	S.W. by W.	And went right across the heavens to the S.W.	Joseph Clark.	P. 84. Communicated by Mr. J. Edm. Clark at York.
XV. Clevedon.	126·2	—	—	—	—	W. 32° S.	W. 20° S. (?).	Stephen Saxby and another careful observer (p. 86).	Pp. 86 and 100. The western horizon covered by trees. The altitude in the meridian, given by two observers, is 22°. The true path gives 22½°.
XVI. Ploërmel (Bretagne).	123·9	—	—	—	—	W. by S.	W.	—	"Elle s'avance rapidement vers l'Ouest." Given by the *Revue Mensuelle* of Mr. C. Flammarion.

power, for our means are not sufficient to throw projectiles with several thousand metres velocity; and it is very remarkable that this experiment comes at the same time as the interesting experiment of Prof. Lemström, showing that electric currents are able to give a development of light in our atmosphere, possessing the same number of undulations in a second as the auroral light. Now our meteoroid being a part of an aurora, it gives a stronger proof of the origin of that phenomenon than Prof. Lemström's experiment, the greatest attraction of which is that we are able to repeat it arbitrarily and with our own means. Further, I have always maintained that electricity, excited easily by friction, must be one of the causes of the auroral light ("Théorie Cosmique de l'Aurore Polaire," *Journal des Spectroscopistes Italiens*, 1878, vol. vii. chap. ii.), and it seems to me very plausible that cosmic matter, approaching the earth, induces electric currents through the air. Therefore I think that the results of Prof. Lemström are in full harmony with the idea of a cosmic origin of auroræ.

The orbit found does not reach the surface of the earth, being at its nearest approach still 123·9 kilometres (1 mile = 1609·3 metres; 1 German geogr. mile = 7420·4 metres) or 16·7 geogr. miles from that surface. The length of the orbit from the Utrecht perpendicular line to the Utrecht horizon is 1,483,070 metres, and this line being run over in 60 seconds,[1] the mean relative velocity was 24,673 metres, 15·3 miles, or more than 3 German geogr. miles.

The dimensions of the "cosmic cloud" (length 40°, width 5°, as seen from Ipswich) are: length = 182,594, width = 21921 metres. By these dimensions, probably too great from irradiation, it must show at Utrecht an apparent length of 50°; but the extremities were tapered and therefore the length strongly influenced by the transparency of the air. It is therefore not strange that the apparent length at Utrecht was during some few seconds 90 degrees.

To conclude, I will remark that the proved existence of a cosmic cloud, preserving its pretty sharp sides during so long a path as that from Sweden to the Atlantic Ocean, notwithstanding its velocity of 247 kilometres, proves its particles to be nearly spherical. Otherwise these particles should necessarily have diverged sideways from the orbit and spread into space. In connection with the fact observed by Mr. P. Zeeman (p. 297), that auroral clouds gave interference-phenomena, when coming before the moon's disk, and these latter phenomena requiring (Dagnin, "Traité de Phys.," iv. p. 446) the presence of nearly equal particles of dusty matter, Mr. Zeeman's observation proves the same property in the particles of the auroral cloud. Being nearly equal, but not perfectly, the tangential atmospheric resistance must throw the smallest particles backward, and this explains the oblong shape of the cloud.

In presenting my results and reasonings to the readers of this journal, I hope that they will remember that this paper has been written in a shorter time than the author had wished. H. J. H. GRONEMAN

Groningen (Netherlands), April 7

THE AURORA BOREALIS[2]

II.

THE Aurora Borealis at Sodankylä.—Although the Lapland aurora borealis often appeared with considerable intensity, it did not boast many varieties. It began generally with a faint arc in the northern sky, which soon developed into a sharp arc, with streamers and a kind of luminous "drapery" spreading from east to west. The colour of this luminous drapery was not very changeable, so that the spectroscope only returned the usual yellow-green line. Generally it was of a yellow-whitish colour, with a slight shade of green.

There was, however, an observation made of far greater interest, viz. that the spectroscopic "reaction,"[1] *i.e.* λ = 5569, on several occasions was returned from every quarter of the horizontal plane, even from the zenith, *without any aurora being visible.* As this reaction was obtained while the ground was still bare, there can be no question of its being a reflection, but that this place was at the moment within the sphere of an auroral discharge, but of such a weak character that it did not appear in the form of aurora borealis. This observation was therefore precisely similar to the one made in 1871 in Lapland, described above.

These observations were chiefly made by my assistant, Herr Biese, who made another remarkable discovery. Nearly due south-east from the Observatory, he received on several occasions a spectral reaction from a narrow belt of the sky, *although no aurora was visible.* This observation, which was very difficult to effect, as the eye had to be kept entirely away from all light for fully five minutes before the reaction could be traced, I had myself several opportunities of corroborating. In this direction were situated some mountains 300 metres high, about 30 kilometres distant, and in my opinion the reaction was due to the above-described phosphorescent flames, which were seen around the mountain-tops in Lapland and Spitzbergen. All observations were of course made after *every trace* of daylight had disappeared.

The Phosphorescent Luminosity.—On several occasions the attention of travellers in the Arctic regions has been attracted to a peculiar soft light or "shine" during the night. But, as the change from day to night is very gradual in the Polar regions, as compared with that of southern climes, a certain amount of exertion of the mind is required in order to take cognisance and retain the features of this phenomenon. As, however, attention has been once drawn to the same, it will always be observed. Already in October I noticed it at Sodankylä, and directed the attention of my assistants to it. I give subjoined some extracts from my diary concerning this phenomenon:—

December 9, 1882.—The Polar night shows sometimes a peculiar phosphorescent "shine" or diffused luminosity, which possesses several phases, but the general character of which is a luminosity of a yellow-white colour, which renders the night as light as the moon with a thick hazy air. I take here the appearance and disappearance of the light on two nights when its intensity was greatest.

On December 6 I was on a journey between Crajärvi and Sodankylä. The phenomenon became then apparent at 7 o'clock p.m. When daylight had completely disappeared, there seemed to remain a faint light in which the outlines of objects around could only with difficulty be discerned. At 7.40 this increased, however, so rapidly that in a few moments every object around stood out clearly in a yellow-white hazy phosphorescent luminosity of quickly-shifting intensity. I had unfortunately no photometer by me by which I could determine the same. It lasted in this form until about 10 o'clock.

December 8, at 5 p.m., I walked from the Observatory to the church near it, in order to observe from its steeple some fire-signals from Oratunturi. On the way, I noticed that a yellow-white luminosity of shifting intensity filled the entire horizon, while twenty minutes after it had increased greatly in intensity, and was now strongest in the north, whence it gradually faded to the south, where it had least intensity. Near the horizon it was difficult to discern the stars. Higher up it was, however, easier, and from 60° to the zenith the sky was clear, of a mauve colour. It was exceedingly interesting to compare the light with the Milky Way. The yellow-white light

[1] This number is stated also by the sharp determination given by the Astronomer Royal, Prof. Christie.

[2] Continued from p. 63.

[1] [By this term Prof. Lemström of course refers to the *characteristic line in the spectrum* of the aurora. The term might be justified by analogy with the "reactions" characteristic of the presence of the various chemical elements.—ED.

contrasted sharply with that of the latter, particularly where the Milky Way stood out of the same. In the yellow-white light it was difficult to make out the Milky Way. This phenomenon lasted far into the night. Later in the evening, between seven and nine, there appeared an aurora of great intensity, of which I shall speak below. This luminosity gave no reaction in the spectroscope at our disposal, but no doubt it would have been obtained had this been less absorbing. Thus, for instance, the larger Wrede's spectroscope (four prisms) did not give the reaction of the auroral phenomenon at Oratunturi, whereas the smaller, as stated above, really showed the line.

There is not the least reason for assuming that this luminosity is of any but an auroral nature, and the result of these observations is *that the whole of northern Lapland is during most winter nights illuminated by a phosphorescent luminosity*, whose intensity varies greatly according to period and place, *but which is undoubtedly of an auroral nature.*

On the same day, viz. December 8, the expedition was enabled to make the first measurement in the magnetic meridian of the elevation of the auroral arc. The wire, which was laid out north and south for the study of the terrestrial current, was used as a t lephone line, and the observations thus made by signals. Two theodolites with the necessary instruments were employed, viz. one at Sodankylä and the other about 4·5 kilometres distant to the north, near the mouth of Kälujoki. The observations at the observato y were made by Herr Biese, and at the northern end of the telephone line by Herr Petrelius. The auroral arc appeared in the north and shone with a quiet, subdued light, while a streamer now and then shot forth into the sky. Six measurements were made with the following result:—At the northern station the line of sight formed, with the under rim of the arc and the horizontal plane, an angle of 9°, and at the southern station one of 12°, *i.e.* an angle 3° larger at the southern than at the northern station! Even assuming that both observers saw *the same* arc, the result is absurd, as however great the distance between the two might be, the difference of the angle would be very small indeed, and, if a difference at all, the angle of the *northern* station should have been the greatest. As, however, the reverse was the case, I have come to the conclusion *that the two observers did not see the same aurora.* A corroboration of this opinion is that on one occasion Herr Biese telephoned, "Turn the instrument to where the *red* column is," while at the northern station no such colour could be traced. This was proved still further during the return journey from Kultula to Sodankylä by the following circumstance. At Köngäs, 60 kilometres north of Sodankylä, on January 3, 1883, at 4 p.m., the whole horizon was flooded with a yellow-white luminosity of great intensity. At the same time an auroral arc formed in the south about 25° over the horizon, and a similar one was at the same moment observed at the same elevation in the north from Sodankylä. The departure from Köngäs took place just after 4 p.m., and during the journey this arc gradually disappeared, while the luminosity and the arc seen at Sodankylä were seen all through the evening. Here there was an opportunity of measuring the elevation of the auroral arc, but as I was convinced that the two phenomena were not the same, I did not attempt it.

It was clear that we were within an auroral discharge which extended considerably east and west, but the main strike of which was north and south. It is very probable that the electric current which caused this light some thousand metres above the surface of the earth also produced the above-described intense luminosity in a layer some 20 metres in depth, running parallel with the earth. It was this layer which was projected from both points into the sky in the shape of an arc. But it is clear that the auroral "drapery" did not penetrate far into the horizontal plane, but as it is generally produced in the centre of a weak discharge of great penetration its appearance from various places in the line north and south would be very variable according as the layer lends its light to the drapery.

The measurements and results described above exactly correspond with those of Mr. Fritz in Greenland (*Bulletin de la Commission polaire Internationale; Mittheil. der Internationalen Polarcommission*, Heft 3), where he obtained an auroral drapery of 650 feet, 1700 feet distant from the observer, and another one of 170 feet, 350 feet distant.

Without further discussing this question here I must state that I consider that *all* measurements of the height of the aurora, calculated on those with a long base north and south, are always erroneous, as the two observers *never* see the same aurora. And even those calculations which are based on the measurments of the height and length of an arc from *one* point, and the hypothesis that the arc extends around the magnetic pole, must be considered very unreliable, as no satisfactory answer can be given as to what results would have been obtained a little further north or south. This is also the case with auroræ with long bases east and west, as only on a shorter distance is it possible to say if it is the same phenomenon which is seen.

That the height of the aurora borealis is very variable I fully admit, but in my opinion it has been greatly overestimated.

Researches with the Terrestrial Current.—During my expedition to Lapland in 1871, we examined the terrestrial current in two places, viz. Kittilä, lat. 67° 40′, and Enare Vicarage, lat. 68° 55′, with wires 1½ kilometres long—east and west, north and south—of copper 0·4 mm. in diameter, and finishing in platina disks 10 cm. by 5 cm., buried in the earth at a depth of 0·7 to 0·9 metre. The deflexion was measured by a galvanometer with astatic needles with telescope and scale (Weber's magnetometer, of Edlund's improved construction). The remarkable result obtained here was that the galvanometer at Kittilä, with the current east and west, gave a deflexion equal to 60 to 100 parts of the meter scale, whereas the current at Enare only gave *a fraction of one part of the meter.* With the current north and south, the difference was not so great, although even here the deflexions were smaller at Enare. It was unfortunately impossible to ascertain if this remarkable phenomenon was due to *latitude* or *season*, the researches at Kittilä being made in October, and those at Enare in the latter half of November, while on the former occasion the ground was not frozen, which it was on the latter.

The Finnish expedition this year to Sodankylä has also examined the terrestrial current, viz. during certain periods of the phenomenon every five minutes, at other times once every hour, with a wire 5 kilometres long, terminating in small platina disks in the earth. During my visit to Kuttala—Decem er 22 to January 4—I also tested the terrestrial current, but with a wire only 1 kilometre in length, running east and west. Here, too, no deflexion was shown, while in Sodankylä the current was just as strong as ever. At Kuttala the galvanometer was certainly not so sensitive as at Sodankylä, still, the experiments of 1871 are even in this respect not without importance.

I have, therefore, from these researches drawn the inference that, while the condition of the ground is of some influence, *the terrestrial current ceases at a certain latitude.* In 1871 already I maintained that the terrestrial current was caused chiefly by the electricity which descends from the atmosphere in the belt around the Pole, in which the aurora borealis attains its maximum, and my recent researches at Sodankylä have greatly confirmed this theory.

I now intend to discuss the conclusions I have come to from the above detailed researches.

Although the general belief as to the nature of the aurora borealis certainly is, that it is of electric origin,

other theories have been advanced, as for instance by Grönemann, *Astr. Nachr.*, 1874-75, and the reason of this is, I believe, that hitherto no direct proof had been obtained demonstrating its true nature.

But the experiments at Luosmavaara in 1871, *and at Oratunturi and Pietärintunturi in* 1882, *clearly and undeniably prove that the aurora borealis is an electric phenomenon.*

The science of the physical conditions of the globe has hitherto, particularly as regards the electric and magnetic ones, simply advanced by observing the effects of these great forces of nature, without however any successful attempt having ever been made to influence or call them forth either directly or indirectly. My experiments now, however, prove that *aurora borealis may be produced in nature* by a simple contrivance assisting the electric current flowing from the atmosphere to the earth. And although the efforts of man must always be limited in comparison with the grand products of nature, the conclusions which may be drawn from the same are not the less instructive.

In a question wherein the theoretical deductions, supported only by a few indirect proofs, have but slowly advanced, *absolute certainty* has now been obtained, and this result should induce future students of the aurora borealis not to devote attention to the "light" phenomenon itself, but to the investigation of those wonderful forces of nature the existence of which it so "lucidly" demonstrates. We have, of course, much to learn from the light also, but far more, I believe, from the electric forces which create it.

It is, however, far from my intention to insist that the apparatus invented by me is the best or that the method followed may not be improved on; still it has certainly one advantage, viz. that of being effective. It is, of course, evident that the drawbacks under which the experiments suffered—as, for instance, weak wires and defective insulators—must be remedied, and it appears to me that the theory which is the basis of M. Mascart's insulator would be particularly suited to the apparatus. The galvanometer should also be altered so as to consist of a *great* number of well insulated coils, in order better to regulate the deflexions, and the experiments should be made in a warm room. As the electrometrical method hitherto used gives only the electric tension at a certain point, it would, it appears to me, form a good meter for measuring the electric state of the surrounding atmosphere. The galvanometer deflexions depend certainly on the electric potential, as well as on the variable conducting power of the air; but it can, as will be seen from my experiments, be measured and even divided by using a constant galvanic element. The electric condition thus measured will give us *an idea* of the strength of the electric current, which in a certain place descends to the earth, and of the electric changes which take place in the atmosphere.

From the experiments with the terrestrial current described above it seems very probable that the current is closely related to the electricity in the auroral belt. The terrestrial current is, as is generally known, related to the magnetic variations, which is most conclusively shown by Mr. Airy's curves (*Phil. Trans.*, vol. cxxxviii. p. 465). In Sodankylä disturbances of the terrestrial current were always followed by a magnetic one. The exact result has of course not yet been calculated, but a glance at the figures returned is sufficient to show this. Mr. Airy's researches have caused these questions: (1) Are the variations in the terrestrial currents more numerous than the corresponding magnetic ones? (2) Do the terrestrial variations occur about half an hour from the corresponding magnetic disturbances?

We have from the experience gained attempted to explain these peculiarities, viz. by the hypothesis that the earth forms, so to say, the core in a flexible bobbin, represented by the terrestrial current circulating around her. In the first place, many of the changes to which the terrestrial current is subject could not affect the magnetic moment of the core, *i.e.* the earth; and, in the second place, the current acts directly on the instruments whereby the magnetic variations are measured; and in these circumstances we must find the explanation of the first-named peculiarity. With regard to the very remarkable difference in time of about *half an hour*, this is the exact time elapsing before the variations of the terrestrial current can affect the magnetic moment of the earth. It is, by the bye, only necessary to compare the duration of induction currents produced in bobbins with different iron cores, to observe that *half an hour* might well pass before the current became perceptible, *if the earth constituted the core.* In Polar regions the electric current descending from the atmosphere to the earth may also contribute to the variations which are measured by our instruments.

In accordance with this theory, therefore, the *electricity* which descends into the auroral belt is the *primary* cause of the greatest part of the terrestrial current, and, through this, of the many variations of the magnetic elements. There are also others, as the diurnal changes in the temperature on the earth's surface, but *the chief* cause is, in my opinion, the electric current from the atmosphere.

In my belief, therefore, the possibility of explaining the peculiarities of this phenomenon lies in a thorough and complete knowledge of the current from the atmosphere.

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(*To be continued.*)

THE FLORA OF ANCIENT EGYPT[1]

THE discovery made by Emil Brugsch Bey on July 6, 1881, in the vault of a king of the twentieth dynasty is of the greatest importance to botany in consequence of the large number of species of plants contained in the offerings and funeral repasts and in the wreaths which adorned the illustrious dead. Among them are several which were not known to belong to ancient Egypt. I have begun the study of the remains of these plants taken from the breasts of the most celebrated kings of Egypt and of such inestimable value to science. Deputed by Mr. Maspero to arrange these relics for the Egyptological Museum of Boulak, I have classified them according to the high personages for whom they were intended. On the eight cardboards which I have the honour to send you in the name of Mr. Maspero, you have a part of the funeral wreaths belonging to Ramses II., Amenhotep I., and Aahmes I.

The wreaths of Ramses II. were renewed towards the end of the twentieth dynasty (1100 or 1200 B.C.), or at the time of the twenty-first dynasty (1000 B.C.). The king of that period, according to records inscribed on the coffins and translated by Mr. Maspero, caused a new coffin to be made for the great Ramses, the one in which he had first been placed having been accidentally destroyed. In this new coffin were several yards of wreaths, which Mr. Maspero handed to me. I have examined them all and ascertained their composition.

The wreaths of Ramses II. are formed of the leaves of *Mimusops Schimperi*, Hochst., either folded or torn in

[1] "Memoir on the Discovery at Deir-el-Bahari in Relation to the Ancient Flora of Egypt," by G. Schweinfurth. [This article, written in French, was communicated to Sir Joseph D. Hooker by Dr. Schweinfurth, together with a set of the wreaths, flowers, &c., described therein. These objects were exhibited at the annual *soirée* of the Royal Society on the 25th ult., and are now on view in No. 3 Museum, Royal Gardens, Kew. With regard to the orthography of the names of the Egyptian kings, that employed by some of the leading Egyptologists of this country has been adopted in this translation. Thus Amenhotep has been substituted for what looks like Amenhotpan in Dr. Schweinfurth's manuscript.—W. B. HEMSLEY.]

See "La Trouvaille de Deir-el-Bahari." 20 photogr. par M. E. Brugsch. Texte par G. Maspero. (Le Caire: chez F. Maurès et Cie., 1881.)

two and stitched together, and serving as clasps for the sepals and petals of *Nymphæa cærulea*, Savi, and *Nymphæa Lotus*, Hook., the whole strung on strips of the leaves of the date palm. Besides the wreaths, there were in the coffin at the side of the body, and fastened between the bands encircling the mummy, whole flowers of *Nymphæa cærulea* on stalks eighteen or twenty inches long. The water-lilies thus scattered separately on the mummy were all of the blue-flowered species. An examination of these entire flowers and the sepals and petals in the wreaths, whether of the white or of the blue-flowered species, leaves no doubt whatever respecting their identity with the living plants so common in ditches at the present day, especially in Lower Egypt, where they blossom from July to November.

The *Nymphæa cærulea*, Savi, which figures on all the ancient monuments of Egypt and among the offerings painted on the walls of the temples is often recognisable from the blue colour of its petals. In the temple of Ramses II. at Abydos the colour is remarkably well preserved, and be-ides there is always a leaf associated with each cluster of flowers, clearly demonstrating by its entire (not toothed) margin that the species represented is *N. cærulea* and not *N. Lotus*. The latter, whose sepals and petals occur abundantly in the wreaths taken from the coffins of Ramses II. and Amenhotep I., has not been found by me on the ancient monuments, though Unger records an instance at Beni Hassan where the white flower could be recognised. With regard to the question to which of the species the old name Lotus properly belongs, I have been able to ascertain the following facts. No design on the ancient monuments is referable to *Nelumbium*; neither the fruits nor the leaves, so easily characterised, are recognisable. Further, no remains of *Nelumbium* have been found either in the coffins or among the offerings and funeral repasts deposited in the vaults of the Pharaohs. The Lotus was not referred to *Nelumbium* until a very much later epoch. This plant has not been found among the wild plants of any part of Africa. It is eminently Asiatic, and was perhaps not introduced into Egypt before the Persian invasion. At the time of Ramadus it was probably cultivated every-

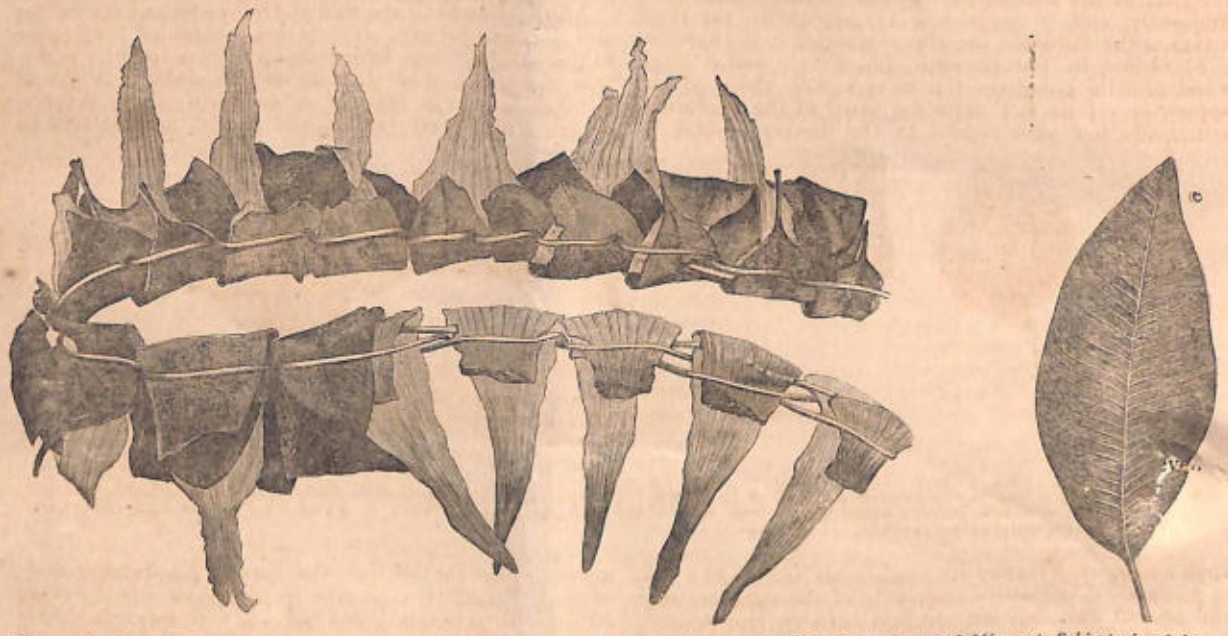


FIG. 1.—Portion of a Funeral Wreath from the tomb of Ramses II. (1000 to 1200 B.C.), composed of the folded leaves of *Mimusops Schimperi* and the petals of *Nymphæa cærulea*, Savi, stitched together with strips of the leaves of the Date Palm. A separate leaf of *Mimusops Schimperi*.

where in Egypt, for we often find it in the mosaics, sculptures, &c., of that period, associated with papyrus and animals characteristic of the Nile, and easily recognised by its fruit.

The most ancient writer who treats of the Egyptian Lotus in such a way as to leave no doubt that he meant the *Nelumbium*, and not a species of *Nymphæa*, is Herodotus (lib. ii. cap. 92); after him Theophrastus ("Hist. Plant." lib. iv.), and then Strabo, while Pliny (lib. xiii.) clearly alludes to a *Nymphæa* in a comparison of the fruit with the capsule of a poppy.

The *Mimusops* was evidently a sacred tree to the ancient Egyptians. The fruits, or the stones of the fruits, which had been eaten, are often found in the funeral repasts in the vaults; and the leaves not only occur in the wreaths of the ancient empire but likewise in those of later times, even down to the Græco-Roman epoch, as specimens in the Leyden Museum testify.

The fruit of *Mimusops* found in Egyptian tombs[1] exactly resembles—except that the stones are a little thicker —that of *M. Kummel*, Bruce, a species spread throughout Abyssinia and the region of the Upper Nile; yet no species of the genus is found wild in Egypt. The leaves forming the wreaths in question should belong to the same species as the fruits found in the tombs. Nevertheless, in comparing them with numerous specimens of *Mimusops Kummel*, I did not meet with the perfect identity one would have expected from the resemblance of the fruits. In Central Africa, and especially in Abyssinia, an allied species, *M. Schimperi*, exists, the leaves of which are much more like those of the wreaths. A longer, and especially a slenderer, weaker petiole, and a more acute, less abruptly acuminate blade characterise these leaves. With regard to the fruit of *M. Schimperi*, I have not had an opportunity of studying it. Moreover the two species under consideration are not sufficiently established as distinct species. But an anatomical character came to my aid. Dr. Westermaier of Berlin has ascertained that the leaves of *Mimusops Schimperi* and of *M. Elengi*, L., have a double layer of epidermal cells, a character they possess in common with the leaves from the ancient tombs; whereas in the leaves of *M. Kummel* there is only a single epidermal layer of cells.

[1] The ancient fruits, however, have usually a thicker stone, the three angles of which appear to be more prominent than in that of *M. Kummel*, Bruce.

Should this distinctive character be constant in the two African species, there is a double reason for naming the ancient *Mimusops M. Schimperi*. The fruit of *M. Elengi* is very distinct from that found in the tombs. I think it very likely that this species, of which we so often find the fruits and leaves in the tombs of the ancient Egyptians, may be the *Persea* of the old authors, which modern botanists have erroneously referred to *Balanites* and *Diospyros mespiliformis*.[1] The latter has not hitherto been found in the ancient tombs; neither does it occur depicted on the monuments. Diodorus (i. p. 34) has transmitted to us a valuable tradition concerning the *Persea*. He states that it was introduced into Egypt with the first colonists coming from Ethiopia, which clearly implies that the ancient authors regarded it as having been introduced from the regions of the Upper Nile and not as belonging to the indigenous flora. *Balanites*, however, grows wild in the valleys of the Eastern Thebaid and on the borders of the Red Sea, and in Nubia this shrub is of general dispersion. True its fruit has been found in the funeral repasts in the tombs, yet that of the *Mimusops* has been found much more frequently, and, in support of my hypothesis, the thick leaves of the *Balanites* are always wanting in the wreaths.

According to Theophrastus, the *Persea* had a black wood, and he compares the flowers with those of the apple-tree. I do not know the wood of the *Mimusops* sufficiently, but with regard to the flowers it must be admitted that no ancient authors ever made a more unmistakable comparison, while the flowers of the *Balanites* have nothing in common with those of the apple. Pliny (lib. xiii. p. 9) does not speak of the *Persea*, but of the *Persica*, and the only surprising thing in it is that he treats it as indigenous in Egypt. He mentions, too, the peculiarity of the Egyptian variety of the peach-tree, which consists in its persistent foliage. Even now in the middle of winter we see the peach-trees in blossom while still carrying their leaves. The same author (lib. xv. p. 13) expressly points out the difference between the *Persica* and the *Persea*. On Egyptian monuments we often see a tree diagrammatically represented, though the distichous, elliptical, acute leaves are evident. This tree, sacred to Hathor or Isis, and often drawn with these divinities, probably represent the *Mimusops* in question. The fruit of *Mimusops Kummel*, of Central Africa, resembles in appearance as well as in taste that of the wild rose; and it may be that under cultivation a still more palatable fruit could be obtained. Indeed, the fruit of specimens of this species collected in Abyssinia appears to be much more pulpy.

All the wreaths of the find at Deir-el-Bahari are of one and the same pattern. The leaves are folded lengthwise in the middle,[1] then folded again in the contrary direction over a string or strip about ½ in. wide, of a leaf of the date-palm. In the fold of each leaf, single flowers, or parts of flowers (sepals and petals), are inserted in

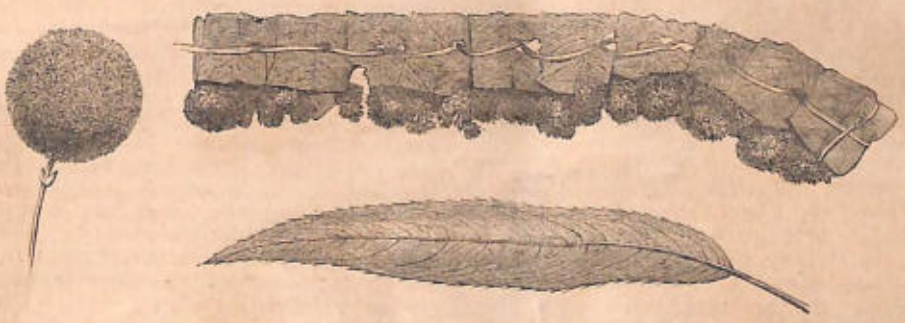


FIG. 2.—Portion of a Funeral Wreath from the tomb of Amenhotep I. (1300 to 1700 B.C.), composed of the folded leaves of *Salix safsaf* and the flower-heads of *Acacia Nilotica* strung together with strips of the leaves of the Date Palm. A separate leaf of the *Salix* (the teeth represented too sharp) and a flower-head of the *Acacia*.

such a manner that they are fixed in the leaf as in a pair of pincers. Then with a finer strip of the date-leaf than the central one, they are stitched through and securely fastened together in long rows side by side, and all pointing in the same direction. These wreaths are arranged in semicircles on the breast of the mummy, so that their disposition is like one sees in the necklaces of the present day. Their thinness rendered them suitable for using in large numbers, and sometimes they occur in several layers one above the other, filling up the limited space between the mummy and the lid of the coffin.

It is probable that it is to this kind of wreath that Pliny alludes (lib. xxi. p. 2) as the "so-called Egyptian wreaths," of which Plutarch and Athenius praised the beauty. Unfortunately these wreaths, which, with ordinary care, might have been removed entire from the mummy when the coffin was first opened, were broken and reduced to powder in several places. The specimens I send you attached to cardboard are the most perfect that I could procure after those selected for the Museum of Boulak. On placing them in boiling or cold water, according to the species, the leaves, &c., recover their original flexibility, especially in *Nymphæa carulea*; and with proper precaution one succeeds in spreading them out and drying them again effectually. The fragility of these objects is only due to the extreme state of dryness they have reached during the thirty to thirty-five centuries they have lain in the tombs. It is at the same time the principal factor in their wonderful preservation.

The wreaths of the other kings of this vault I have at present only partially examined. From their general appearance, however, as well as from the flowers and leaves of which they are composed, which also indicate a different season[2] of the year, one would be justified in attributing them to a different period from that during which the wreaths of Ramses II. were renewed. If they really date from the time when the bodies of the kings of the eighteenth dynasty were first deposited in the vault, we have here to do with specimens four or five centuries older than the wreaths of Ramses II. In any case these objects are at least contemporaneous with the time commonly assigned to the Trojan war, if not several centuries more ancient.

The wreaths of Amenhotep I. (who was found during

[1] Kunth took the stones of *Mimusops* found by Passalacqua to be this plant.

[It may be mentioned that Kunth published his determinations of the relics found by Passalacqua in the *Annales des Sciences Naturelles*, viii. (1826) p. 418. Unfortunately it is not known to what period they belonged. Among them were seeds of a palm, *Areca* (?) *Passalacquæ*, Kunth, which was subsequently identified by Unger with *Hyphæne Argun*, Mart., a palm which inhabits some of the valleys of the Nubian desert in the bend of the Nile between Korosko and Abou Hammed.—W. B. H.]

[1] Or when they were too large they were torn in two.

[2] The records to which I have alluded indicate the day and the month; and these flowers will one day serve to fix the season with which the month of that epoch coincides. The Carthamus could only be had from the end of March to the middle of May; the Water-lilies from July to November while the young leaves of Salix indicate the spring. The Acacia and Sesbania flower at all seasons.

the twentieth dynasty still intact in his coffin, and who, according to Brugsch, preceded Ramses II. by three centuries) are more varied. Among them are some composed, like those of Ramses II., of the leaves of *Mimusops* and the sepals and petals of the two species of *Nymphæa;* while others are formed of the leaves of *Salix safsaf*, Forsk., which serve as clasps for the little balls of flowers of *Acacia Nilotica*, Del., portions of the heads of flowers of *Carthamus tinctorius*, L., or the separate petals of *Alcea ficifolia*, Cav.

Nobody could recognise either the *Salix* or the *Alcea* among the hundred Egyptian species of plants enumerated by Pliny, or in the writings of other ancient authors; whereas the *Acacia* and the *Carthamus* occur under the names of *Acanthos* and *Cnicus*. Concerning the former, Pliny (lib. xiii. p. 19) mentions the employment of its wood in boat-building, the use of its gum, of its pods in tanning; he speaks of the spines, even, which are found on the leaves; in short he indicates the distinctive feature of the species, adding that the flowers are effective in wreaths. Several of the old authors treat of this tree. With regard to the *Cnicus* or *Knekos* (Pliny, xxi. p. 53) it is only recognisable by the indication that it is spiny, that its large white seeds yield an oil, and that there are in Egypt both wild and cultivated species, which is true. The flowers of *Carthamus* found in the wreaths of Amenhotep I. have retained their red colour, and resemble those of the species cultivated everywhere in Egypt at the present day. The colour, as in recent herbarium specimens, has changed from cadmium red to a brownish red or orange. In water the colouring matter is rapidly excreted, and we behold these flowers of some thirty to thirty-five centuries ago intensely colouring the liquid in the phial containing them.[1] All four of the plants which I have just mentioned have now for the first time been actually found in an ancient Egyptian tomb. The leaves of *Salix safsaf*, which form the greater part of the wreaths of Amenhotep I. and Aahmes I., do not differ in the least from those of the present day, and the species is common in Egypt. They are young—that is to say small and pale—thus indicating an early season of the year. In this respect they are in contradiction with the blue and white petals of *Nymphæa* found in the same coffin, though not, it should be stated, in the same wreaths as the *Salix*, but in the wreaths with leaves of *Mimusops*. The latter very closely resemble those found on the mummy of Ramses II. Perhaps at the time of the removal of the kings of the eighteenth and nineteenth dynasties from one vault to another, and finally to the place of concealment at Deir-el-Bahari, when a new coffin was made for Ramses II.—perhaps, I say, they renewed a part of the wreaths of the other kings, or having ascertained the condition of the mummies (whether under the twentieth or under the twenty-first dynasty), they added some new wreaths to the original ones. This would explain the presence in the same coffin of flowers belonging to different seasons of the year.

Salix safsaf, which occurs in a wild state on the banks of the Nile in Nubia, is in Egypt proper only a riverine fugitive, like many other plants, whose real home is in the south. Away from the river it only exists on sufferance, chiefly near wells and canals. To my mind it is an example of the wild flora which agriculture has caused to disappear. *Alcea ficifolia*, Cav., is now found in Egypt only in the ancient Arabian gardens of Cairo and other towns—that is to say, in gardens dating before the introduction of European horticulture by Barillet in 1869, where it grows almost wild as a weed. I have found it in a wild state in Syria and the Lebanon. Boissier, in his "Flora Orientalis," has not clearly defined it, and gives one or two other forms (*A. lavateræfolia*) as distinct species, which they are not. The petals of the *Alcea* contained in the wreaths of Amenhotep I. leave no doubt that they belong to the species named. Their shape, the distribution of the veins, and especially the hairy callosity on the inner surface of the claw, as well as the size even, confirm the identity of the species. Moreover one perceives in the petals of the ancient wreaths traces of a purplish tint corresponding to the crimson of the living plant. The ancients probably esteemed this plant alike for its beauty and its medicinal properties.

I have examined a head of flowers of *Acacia Nilotica* coming from one of the wreaths, and I found that the flowers agreed in the minutest details with fresh ones, with the characters of which I am sufficiently familiar. The proportions of the peduncle, the position of the annular bract, the shape of the bracteoles, the calyx, the petals, and stamens of each flower do not exhibit the slightest differences. This tree, which is planted or tolerated by man all over Egypt, is nowhere completely wild except on the White Nile between 11° and 12° N. lat., where it constitutes large riverine forests.

The wreaths which were found in the coffin of Aahmes I., the great founder of the eighteenth dynasty (1700 B.C., according to Brugsch), are the most varied, and astonish the eyes with the bright colours they have retained. They are partly composed of leaves of the Egyptian willow (*Salix safsaf*), containing separate flowers of *Delphinium orientale*, Gay, of *Sesbania Ægyptiaca*, Pers., petals of *Alcea ficifolia*, or flower-heads of *Acacia Nilotica;* and partly of the leaves of *Mimusops*, serving as clasps for the petals of the two species of *Nymphæa*, like the wreaths of Ramses II. and Amenhotep I. The *Delphinium* and the *Sesbania* had not hitherto been authenticated from ancient Egypt. The colours of their flowers are admirably preserved, the deep violet of the former being especially striking, but the specimens I have communicated to you in a phial of alcohol have lost their colour, just as fresh flowers of our time would. *Delphinium orientale* is now spread over a very wide area of the Mediterranean region. The two nearest localities to Egypt where it has been found are Algeria and Northern Syria, near Raldoun. It is not impossible that it still occurs in some parts of Egypt, while it is equally possible that it was cultivated by the ancient Egyptians as an ornamental plant. In the event of our being able to prove that some of the wreaths of Aahmes I. and Amenhotep I. were removed at the time of the twentieth dynasty, together with those of Ramses II., we should be justified in the assumption that this plant and *Alcea ficifolia* were introduced through the conquest of Syria. A minute analysis of the flowers, and comparison with those from various localities, leaves no doubt that they are of the species mentioned; and if I had had access to a larger number of flowers of the plant of the present period, I am certain that I should have been able to have exactly matched the ancient ones. The differences that I was able to detect between the ancient flowers and recent ones from Algeria, the Caucasus, Phrygia, and Lycia, kindly supplied by Mr. E. Boissier, may be set forth in a few words. In the first place there are two narrow linear bracteoles exceeding the peduncle in length, and reflexed; then the ovary is less pubescent, and the sepals are narrower and less acute. With regard to the bract, the thickened peduncle, the shape, number, and disposition of the stamens, the stigma, and especially the single petals, I have seen recent flowers in which these organs are absolutely identical. It will be seen that the characters in which they differ are only of individual value. Further, the species in question, commonly cultivated at the present time, comprises a considerable range of forms. Thus there are varieties in which the single petal is merely three-lobed, whilst in others the intermediate lobe is again divided. Both conditions occur in the ancient flowers. These flowers are so well

[1] Unger ("Botanische Streifzüge," p. 113) mentions that a chemist named Thomson had proved that the red dye in the mummy bandages was derived from *Carthamus*.

preserved that under the influence of boiling water the spur of the posterior sepal is easily separated from that of the petal projecting into it. That is to say, the latter may be extracted without injury. The numerous details of the petal, its intricate venation, the coloured glands on the margins, the claw with two lateral folds—all correspond to recent specimens. The colour of the ancient flowers is rather a deep bluish violet than a reddish violet, as in the plant of our time.

I have also carefully analysed the flowers of *Sesbania Ægyptiaca*, from the wreaths of Aahmes I. They belong to the typical form of the shrub, which still springs up on the borders of cultivated fields and on roadsides in Egypt, though it is not really spontaneous below the Soudan. The flowers are so perfectly preserved that the minutest detail did not escape my scrutiny. Submitted to the action of boiling water they scarcely differed from flowers taken from my herbarium. One circumstance shows how hurriedly these funeral wreaths were made. The flower torn from its pedicel and pinched with the finger nails always retains only a part of the calyx cut through the middle.

In the find at Deir-el-Bahari other objects besides the wreaths were found for the first time. Thus in the coffin of the priest Nibsoni, of the twentieth dynasty, the leaves of *Citrullus vulgaris* were scattered between the body of the mummy and the sides of the coffin; and flowers of *Nymphæa cærulea* were found fixed beneath the outer bandages of the same mummy. The Egyptian Museum of Berlin already possessed seeds of this *Citrullus* in the collection of Passalacqua, though the epoch to which the collection belongs is unknown. *Citrullus vulgaris* is found wild in the greater part of Central Africa,[1] and its fruit is smaller than that of the cultivated race, and less palatable, though otherwise like it. Among the broken remains in question I found one whole leaf, which enabled me to fully study its specific characters. Placed in cold water it recovered its original flexibility, so that it could be spread out flat and dried again. The chlorophyll was perfectly preserved, and what was curious, it was absorbed by the water to such a degree, that the glass of water in which the leaf and portions of leaves were placed became of an intense green colour. The problem to solve was whether the leaves were those of the water-melon or those of the colocynth, a species spread over the whole desert region, and only differing from the former, which has long hairs on the young fruit, by the complete nudity and spongy nature of its bitter fruit with a hard rind, and by the seeds. The leaves of the water-melon often very closely resemble those of the colocynth, especially in the variety called *Gjurma* (*Gyurma*) in Egypt, which bears fruit no larger than that of the colocynth, though it is always sweet. Nevertheless the large leaves of elongated outline and having less numerous lobes, are rare in the colocynth, and only in places well watered by rains. There is an association of characters in the leaves from the mummy of Nibsoni, that enable one to refer them to varieties of the cultivated water-melon, rather than to the wild colocynth. I have compared them with a long series of specimens of the water-melon from all parts of the Nilotic region, and with a no less numerous series of specimens of the colocynth; and I have come to the conclusion that they may be regarded as belonging to the former species. The uses of the two species would render them equally admissible in a coffin of ancient Egypt. As a funeral offering an alimentary plant might serve as well as a medicinal one. Still the fact that there are seeds of the water-melon in the Berlin Museum from an ancient tomb supports my first supposition. The leaves found on Nibsoni are about a palm long, and of a pinnatisect form, with obtuse lobes. If these leaves were distinctly hairy there would be no doubt of their belonging to the water-melon. Yet, as already mentioned, there is a variety widely spread in Egypt which has not the long and numerous hairs attached to the tubercles with which the leaves are covered, but merely short bristles, which is also the case in the colocynth.

This variety of water melon, which I have named *colocynthoides*, is the *Gyurma* of the Egyptians, and is cultivated in dry neglected ground in Upper Egypt. It is probably the primitive condition of the species before it had reached its present state of perfection. The leaves of the *Gyurma* are sometimes hairy, as in the water-melon, sometimes only provided with short deciduous bristles, as in the colocynth. The leaves from the coffin of Nibsoni exhibit only the latter condition. It may be that they have lost a great part of these deciduous hairs during the long period that has elapsed. I found one character, however, that the *Gyurma* has in common with those in question. There are on the petiole, and especially on the under surface of the leaf in the middle, among the round tubercles with which it is beset, other tubercles or callosities of an elongated linear form and arranged in rows corresponding to the secondary veins. On these leaves, as well as on those of the *Gyurma*, these elongated tubercles are much more prominent than they are in the colocynth. Moreover the numerous specimens that I have compared of the last have all of them leaves more densely furnished with the round tubercles than is the case with those of the water-melon, of the *Gyurma*, and the ancient leaves.

The secret vault of Deir-el-Bahari, besides the coffins of so many illustrious kings, also contained numerous funeral offerings deposited there by the later kings of the twenty-first dynasty who used this collective tomb, so well concealed by the topographical conditions. Among these offerings I was able to recognise dates, raisins, and pomegranates. There was also a basket filled with a lichen (*Parmelia furfuracea*, Ach.) which at the present day is sold in the bazaar of drugs in every town of Egypt. It is now called "Chèba" (Sheba), and is used to leaven and flavour the Arabian bread. Medicinally, also, it is in great request. The presence of a lichen of solely Greek origin, mixed with the species named, and which also occurs in the modern drug, excludes all doubt as to its being a commercial product. *Ramalina Græca*, Muell., Arg., which was mixed with the *Parmelia*, has only been found in the islands of the Greek Archipelago, and the Arab merchants regard that country as the source of their drug. As there is no locality in Egypt where *Parmelia furfuracea* could grow, the only explanation of its presence in the offerings of the twenty-first dynasty (1000 B.C.) is that it was derived from Abyssinia or Greece. In the latter case the find at Deir-el-Bahari would prove the existence of commercial intercourse with Greece at about the time of the Trojan war. Among the *Parmelia* (which was perhaps the *Sphagnos* of Pliny) were fragments of *Usnea plicata*,[1] Hoffmg, and the straw of a grass (*Gymnanthelis longigera*, Anders.) of Nubia, which at the present day is used by the natives as a remedy against affections of the chest and stomach. On searching through the copious remains of this plant I succeeded in finding a few well-preserved flower-spikes, which I carefully examined and determined beyond doubt to belong to the species mentioned. In Arabic it is called "*mâhareb*." The odour even of this grass was preserved to a certain extent in the mixture of the offering. The fragrant secretion is of the same nature as that of the allied section *Schœnanthus* of *Andropogon* of India. Besides the lichens and the grass, this offering contained the hairy buds of some *Composita*, probably an *Artemisia*, with pinnatisect leaves; tendrils of some *Cucurbitacea*; seeds of the coriander; and numerous berries and seeds of the eastern Juniper (*Juniperus Phœnecia*). Inasmuch as we have here to do with plants coming from opposite regions of Africa and from Europe or Asia, it was

[1] I have gathered it in that state in the islands of the White Nile.

[1] Dr. J. Mueller of Geneva undertook the naming of the lichens.

not an easy matter to pronounce an opinion on the *Cucurbitacea* and the *Compositæ* mentioned. The coriander is a plant of early cultivation in Egypt, being mentioned by Pliny as one of the best products of the country. The berries and seeds of the juniper (the latter free in consequence of the decomposition of the former) could only have been derived from Syria or the Greek Islands. I carefully compared them with the allied species, including the Abyssinian *Juniperus excelsa* (which has larger berries and much thicker seeds, to the number of six), and there can be no doubt that they belong to *J. Phœnicea*, L. Kunth had previously determined this species in the collection of Passalacqua.

Among the fragments of the offerings and repasts found scattered on the floor of the vault of Deir-el-Bahari when it was first inspected by Brugsch Bey (some of the objects had already been disturbed by Arab robbers) was a tuber of *Cyperus esculentus*, L., some specimens of which from ancient Egypt are also preserved in the Berlin Museum. It is common in a wild state, and generally cultivated in the country.

In bringing this enumeration to a close I have only to mention the finding of a bundle of the grass called *Halfa* by the Egyptians (not the *Halfa* of Tripoli and Algeria), *Septochloa bipinnata*, Hochst., syn. *Eragrostis cynosuroides*, Retz. This bundle probably formed part of an offering representing the productions of the black and fertile soil of the valley of the Nile, of which this grass was a good sample.

ON THE CHEMICAL CHARACTERS OF THE VENOM OF SERPENTS

DRS. WEIR MITCHELL and E. T. Reichart, of Philadelphia, are now engaged in an inquiry into the chemical composition and characters of snake poison, which promises to yield important results and to supply information long wanted on an aspect of the subject which has made little progress since Prince Louis Lucien Bonaparte published his discovery of an active principle in viper venom, which he considered to be the sole cause of its toxic properties, and to which he gave the name of Echidnine or Viperine. He described the mode of separation of this principle in a paper read before the "Unione degli Scienziate Italiani" at Lucca in the year 1843.

The investigations of Drs. W. Mitchell and Reichart relate chiefly to crotaline snake poison, but include a partial analysis of some dried cobra (colubrine) poison sent to them by Mr. V. Richards from India.

Difference in the mode of action of the colubrine and viperine virus was pointed out by me many years ago in India, when I observed that viperine poison destroys the coagulability of the blood in animals, causes hæmorrhage, and has peculiar effects on the nervous system differing from the cobra's (colubrine) venom, which does not destroy the coagulability of the blood, nor cause so much hæmorrhage.

Dr. Wall of the Bengal Medical Service has added much to our information on the subject, and has defined the different modes of action of the venom of the principal Indian poisonous snakes.

The Philadelphia observers came to the conclusion that the venom of the crotaline snakes with which they have chiefly operated can be subjected to the action of the boiling temperature of water without completely losing its poisonous power. The toxicity of the venom, however, of the *Crotalus adamanteus* seems to be destroyed by a temperature below 176° F. Mitchell some years ago showed that the venom of *Crotalus durissus* is not destroyed by boiling, and they remark on the curious fact that the venom of *C. adamanteus* should thus differ from the venom of other snakes.

The symptoms caused by the venom of the different snakes with which they have operated do not, they say, differ radically save in degree, but there are certain symptoms which they think make it probable that further investigation will enable them to point out certain differences by which it will be possible to discriminate one form of poisoning from the other. This is in accordance with what has already been done by observers in India, and notably by Dr. Wall.

The investigations of Drs. Weir Mitchell and Reichart so far, lead them to conclude that the poison of the cobra is the most active, next the copperhead, then the moccasin, and lastly the rattlesnake; but their researches on this head are not yet complete.

They are unable to confirm the statement of Gautier of Paris that an alkaloid resembling a ptomaine exists in cobra poison; or that of Prof. Wolcott Gibbs, that the poison of crotalus yields an alkaloid; but they have satisfied themselves that the venom contains three distinct proteid bodies, two of which are soluble in distilled water, one which is not soluble. These bodies have certain properties and reactions, which are detailed in their monograph on the subject.

Hitherto observers have regarded the venom of different snakes as each representing a single poison, but it appears from these researches that, of the three proteids before mentioned, one is analogous to peptone and is a putrefacient poison, another is allied to globulin, and is a most fatal poison, probably attacking the respiratory centres and destroying the power of the blood to clot, while the third resembles albumen, and is probably innocuous. The separation of the two poisons necessitates a long and elaborate series of researches, the results of which will be subsequently reported.

They have also ascertained that the poison of the Rattlesnake (*Crotalus adamanteus*), Copperhead (*Trigonocephalus contortrix*), and Moccasin (*Toxicophis piscivorus*), are destroyed by bromine, iodine, hydrobromic acid (33 per cent.), sodium hydrate, and potassium permanganate. It is to be hoped that these important and valuable researches will be continued until the true chemical nature of these poisons be completely made known.

J. FAYRER

NOTES

AT a meeting of the subscribers to the Balfour Memorial Fund, held at Cambridge on the 26th inst., it was stated that 8309*l.* had been promised, all except 100*l.* of which had been paid. Of this 8078*l.* had been invested, yielding an annual income of 284*l.* 10*s.*, which it was hoped further subscriptions would raise to 300*l.* Among the regulations agreed to were the following:—The income of the fund shall be applied (1) to endow a Studentship the holder of which shall devote himself to original research in biology, especially animal morphology; (2) to further by occasional grants of money, original research in the same subject. The Student shall not necessarily be a member of the University, and during his tenure of the Studentship shall devote himself to original biological inquiry, and shall not systematically follow any business or profession or engage in any educational or other work which in the opinion of those charged with the administration of the fund would interfere with his original inquiries. The place and nature of the studies of the Student shall be subject to the approval of the managers provided that the Student shall be bound to pursue his studies within the University during at least three terms during his tenure of the Studentship, unless the managers shall, with the approval of the Board, dispense with this requirement for special reasons. The managers shall take such steps as they may think necessary to satisfy themselves as to the diligence and progress of the Student, and may require from him any reports or other information on the subject of his studies which they may think desirable. The Studentship

shall be tenable for three years, but it may be continued over a second term of three years (but no longer) to the same person if the managers and Board decide that it would be clearly in the interests of biological research. The balance of the income of the fund, after providing for the Studentship and for any necessary expenses connected with the election, shall be devoted to the furtherance of original research in biology, especially animal morphology. Grants may be made for this purpose either to the holder of the Balfour Studentship or to any other person engaged in research.

THE subscription list for the memorial bust of Prof. Henry Smith, to be placed in the University Museum, will be closed at the end of the present term. It would be convenient if subscribers would, as soon as possible, pay their subscriptions into the Old Bank, or send cheques to any of the following gentlemen:—Mr. W. Little, Queen Anne's Mansions, S.W.; Mr. R. L. Nettleship, Balliol College, Oxford; or Mr. E. Chapman, Frewen Hall, Oxford.

DR. JULIUS VON HAAST has been created a Companion of the Order of St. Michael and St. George.

TWO statues which have been erected in front of the Berlin University to the Brothers Alexander and Wilhelm von Humboldt were unveiled on Monday with great ceremony. The Emperor and some of the members of the Imperial family witnessed the proceedings from the Royal Palace, which immediately faces the University, and the Emperor afterwards went on foot to inspect the statues.

DR. GABRIEL GUSTAV VALENTIN, one of the most eminent professors of the University of Berne, and a distinguished physician, died at that city on May 24. Dr. Valentin was born at Breslau in 1810, graduated in 1832, and began practice in his native town in the following year. In 1835 he published a handbook of the history of evolution ("Entwicklungsgeschichte"), and in 1836 was appointed Professor of Physiology in the University of Berne, a position which he held until 1881, when ill health compelled him to resign. He stood very high in his profession, and was the author of many scientific works, two of which were written in Latin, "De phænomeno generali et fundamentali motus vibratorii continui" and "De functionibus nervorum cerebralium et nervi sympathici libri quatuor." He wrote also a "Text-Book of Physiology," a book entitled "Groundwork of Human Physiology," a "Repertory of Anatomy and Physiology," an "Examination of the Effects of Polarised Light on the Life of Plants," an elaborate work on the "Adaptation of the Spectroscope to Physiological and Medicinal Purposes," and several others which attest his vast knowledge and untiring industry.

THE following remarks by our American contemporary, *Science*, on the subject of the Canadian meeting of the British Association in 1884, are deserving of attention:—"It is to be observed that in the present year the meeting of the American Association, at Minneapolis, is early (August 17); while that of the British Association, at Southport, which is, besides, in the immediate vicinity of Liverpool, is unusually late (September 19). This will allow members of the American Association to attend both meetings, and it is stated that the retiring President of the American Association, and possibly others of its members, may avail themselves of this privilege. This may possibly permit arrangements to be made which might substantially unite the meetings of the two Associations in 1884, and so prepare for an international meeting in the future. If the meeting of the American Association for 1884 can be fixed for some north-eastern city, sufficiently near to Montreal, and can be timed so as to occur a week before or after that of the British Association, there can be no doubt that a great number of the members of the latter body would take advantage of the opportunity to enjoy the companionship of their American *confrères*, while, on the other hand, many of these would gladly spend a few days at the meeting of the British Association. In this way it would seem that a greater benefit to science might result than even from an international meeting. There would be time for the complete transaction of the business of both Associations. Neither would suffer, either pecuniarily or in the value of its proceedings; and there would be the best possible opportunity for interchange of ideas between the scientific men of the United States, Great Britain, and Canada. Nor is it unlikely that some scientific workers from the continent of Europe and elsewhere may be attracted by a combination so unusual. It may thus be hoped that the proposed meeting of the British Association in Canada may not only be one of the most successful that this mother of Associations has held, but may inaugurate an epoch of renewed activity and progress in the widely-spread scientific work of the two great Associations of the English-speaking race."

THE New Parkes Museum of Hygiene at 74*a*, Margaret Street, Regent Street, was opened on Saturday under favourable and distinguished auspices. The Duke of Albany presided and formally opened the Museum, and gave besides a sensible and thoughtful speech. "Hygiene," His Royal Highness said, "as we now understand it, is a branch of knowledge of modern growth. It is one of the natural results of the great advance of science which this century has witnessed, and might, I fancy, not inaptly be defined as the application of scientific principles to the varying conditions under which we are called upon to live. Thanks to the labours of many eminent men, we have now advanced some way towards an accurate knowledge of the conditions which are necessary for health; and most of these conditions have long been familiar to the few. One object of the Parkes Museum will be to make them familiar to the many. We have learned, and are daily learning, that many of the luxuries and conveniences of modern life may become sources of danger to us if they be ignorantly used. London would be almost uninhabitable were it not for its wonderful system of sewers; but while enjoying the blessing of effective sewerage, we have had to encounter the difficulty of keeping the air of the sewers out of our dwellings. We all appreciate the brilliant light which is given by a gas lamp; but its wholesome use, we are now beginning to find, involves questions of ventilation which scarcely troubled those who were content with the comparative dimness of a candle. Again, the open coal fire has long been regarded as one of the chief luxuries of the Briton, but the collected smoke of the fires of 4,000,000 of people has become a nuisance too grievous to be borne, and one for which a remedy must be sought. It is notorious that many of our public and private buildings in this country have been constructed without due attention, or, indeed, any attention, to those details which alone make a dwelling wholesome. The experiences of my own family in this matter have indeed been singularly hard. We hope that this museum will tend to hasten the end of this state of things, and that henceforward 'healthiness' will be considered as an essential condition of true architectural beauty. For the healthiness of our dwellings we have to depend, not only upon the master mind which furnishes the plan, but even to a greater extent upon the intelligent hands of those who are called upon to carry out the details. Unless the work of these latter be done with intelligence and faithful honesty, the schemes of the wisest architect avail us little. The instruction which has been and will be given here to the artisans who carry out the sanitary details of our houses must be productive of good results. At least, let us hope that some of the specimens of defective workmanship to be found upon our shelves will impress upon them that death, disease, and sorrow may be the results of ignorance or carelessness on their part." Among the other speakers

were Sir Charles Dilke, Prof. Tyndall, and the Archbishop of York. It is to be hoped that the public, and especially those on whose skill and honesty our sanitary arrangements are dependent, will take ample advantage of the opportunities offered by the new museum.

THE seventh Congress of Russian Naturalists and Physicians will be held this year at Odessa, from August 30 to Sept. 9.

THE district of Pergamos in Asia Minor is now so infested with sparrows that application has been made to the Turkish Government for aid against them. It will be remembered that this district is subject to occasional invasions of rodents.

THE Marine Excursion Committee of the Birmingham Natural History and Microscopical Society announce that, in response to a wish expressed by many members, they have arranged a second excursion to Oban and the West Highlands of Scotland, similar to that which proved so successful in the year 1881. The party will leave on Friday, June 29 next, to reach Oban about 5 p.m. on Saturday. The screw steam yacht *Aerolite*, of about sixty tons, has been hired of Messrs. Ross and Marshall of Greenock for a week, commencing Monday, July 2; facilities will thus be afforded for dredging excursions not only in the district previously worked, but also in distant localities. Arrangements are being made for excursions to several places of interest in the neighbourhood of Oban.

THE sixth annual meeting and *conversazione* of the Midland Union of Natural History Societies will be held at Tamworth on June 12 next. Excursions have been arranged for that day and the 13th. The Darwin Gold Medal for 1882 will be presented to Prof. A. M. Marshall and W. P. Marshall, for their paper on the Pennatulida.

THE additions to the Zoological Society's Gardens during the past week include a Malbrouck Monkey (*Cercopithecus cynosurus*) from West Africa, presented by Mr. C. D. Gordon; two Grisons (*Galictis vittata*) from South America, presented by Mr. Percy Kenyon Slaney; two Sloth Bears (*Melursus labiatus*) from India, presented by Mr. F. A. Curteis; a Surucucu or Bushmaster (*Lachesis mutus*) from Pernambuco, presented by Mr. J. Y. Barkley; a Common Chameleon (*Chamæleon vulgaris*) from North Africa, presented by Mr. Henry W. Weguelin; a Chimpanzee (*Anthropopithecus troglodytes* ♂) from West Africa, two Welsh Sheep (*Ovis aries*) from Wales, a Goffin's Cockatoo (*Cacatua goffini*) from Queensland, five Margined Tortoises (*Testudo marginatus*), thirteen European Pond Tortoises (*Emys europæa*), South European, deposited; a Common Seal (*Phoca vitulina*) from British Seas, a Grey-headed Porphyrio (*Porphyrio poliocephalus*), a Conical Worm Snake (*Gongylophis conicus*) from India, purchased; a Hybrid Tapir, ♀ (bred between *Tapirus roulini* ♂ and *Tapirus americanus* ♀), born in the Gardens.

OUR ASTRONOMICAL COLUMN

THE MINOR PLANET, ANDROMACHE.—Among the small planets mentioned in the last volume of the *Berliner Astronomisches Jahrbuch* as having been observed at one opposition only, though several oppositions have taken place since their discovery, is No. 175, detected by the late Prof. Watson of Ann Arbor, U.S., on October 1, 1877, and named *Andromache*. The orbit has a considerable eccentricity, and the planet recedes to a greater distance from the sun at aphelion than is the case with any other member of this now numerous group so far calculated; indeed at this point of its orbit it is distant from the sun 4·723 (the earth's mean distance being taken as unity), and only 0·594 from the orbit of Jupiter. There should be no great difficulty in recovering this planet during the month of June or July. According to the most accurate elements calculated by Prof. Watson it will be in perihelion about July 25, and in opposition a fortnight earlier, its computed intensity of light being equal to that of a star of fully the ninth magnitude. Its considerable south declination will give an advantage to a search at one of the observatories of southern Europe. To facilitate its reobservation we subjoin positions deduced from the orbit last published :—

At Greenwich Midnight

	R.A. h. m. s.	Decl. ° ′	Log. Distance from Earth.	Log. Distance from Sun.
June 4	19 40 3	−27 23	0·1547	0·3635
12	19 39 0	27 44	0·1368	0·3622
20	19 36 0	28 7	0·1222	0·3610
28	19 31 5	28 29	0·1117	0·3600
July 6	19 25 9	28 48	0·1059	0·3594
14	19 19 9	−29 1	0·1055	0·3590

The planet will probably be situated at some distance in R.A. from these positions, which are only intended as an approximate indication of its places. The last reference to a search for it which we find in the circulars of the *Berliner Jahrbuch*, occurs in No. 118 (Correspondenz), 1881, March 3, where we read, "*Andromache* innerhalb −6m. 30s. bis −3m. 55s., und −2m. 20s. bis +4m. 15s. vergeblich gesucht." A special rough chart of stars in the vicinity to the tenth magnitude inclusive would be readily formed with the stars in the Bonn and Washington Zones as reference points.

THE GREAT COMET OF 1882.—M. W. Fabritius of Kieff has calculated the following elliptical elements of this comet from two normal positions for September 9 and October 6, and an observation at Königsberg on March 3 in the present year :—

Perihelion passage, 1882, September 17·2753 M.T. at Berlin.

Longitude of perihelion	276 28 40·1	M. Eq. 1882·0
,, ascending node	345 58 4·1	
Inclination	38 0 44·7	
Log. $(1-e)$	5·938209	
Log. semi-axis major	1·943548	
Log. perihelion distance	7·881757	

Motion—retrograde.

The corresponding period of revolution is a little less than 823 years, and as M. Fabritius attaches some weight to his result, he thinks the comet must have appeared about the middle of the eleventh century.

We shall doubtless have in due course a thorough discussion of all reliable observations; those made since September 30, when the disintegration of the nucleus commenced, will need special treatment.

THE OBLIQUITY OF THE ECLIPTIC.—In NATURE, vol. xxvii. p. 618, we quoted 23° 41′·1 as the value of the obliquity of the ecliptic at the assigned epoch of Ptolemy's catalogue. With reference to this statement Mr. W. J. Cockburn Muir, of Melrose, N.B., has made a discovery, on which he writes us as follows :—"In NATURE of April 26, at p. 618, I read that the 'obliquity of the Ecliptic' is 23° 41′·1, and I wondered much what had suddenly happened in the Kosmos. So I took means to ascertain from the Royal Observatory of Greenwich how the record stands, and I am comforted to find that, by the determinations in 1882, the earth's axis still remains at home—23° 27′ 16″·8." Our correspondent may be referred to any elementary treatise on astronomy.

GEOGRAPHICAL NOTES

MR. OSCAR DICKSON'S Greenland Expedition, under the command of Baron Nordenskjöld, sailed from Gothenburg in the *Sofia*, 180 tons, 65 horse-power, drawing 10 feet, and of 11 knots speed, navigated by Capt. Nilsson and a crew of 13 men. With Baron Nordenskjöld are Dr. Nathorst, geologist; Dr. Berlin, doctor and botanist; Dr. Forsstrand, zoologist; Dr. Hamberg, hydrographer; Herr Kolthoff, zoologist; Herr Kjellström, typographer and photographer; two Laplanders, two Norwegian icemasters, and one harpooner. There is on board a complete scientific equipment and 14 months' provisions for subsistence on the inland ice. Eight or nine picked men accompany Baron Nordenskjöld. Count Stromfeldt, botanist; Dr. Arpi, archæologist and philologist; and Herr Flink, mineralogist, will disembark on the coast of Iceland for the purposes of

study and collection. The *Sofia* called at Thurso for coal on Sunday and left on Tuesday.

In connection with Prof. Fries' suggestion of colonising Greenland by mountain Lapps, to which we referred last week, we learn that Baron Nordenskjöld takes with him to Greenland two Lapps from Jockmock, to give their opinion of the country. One of them is thirty, and the other thirty-three years of age.

We learn from the last annual report of the East Siberian branch of the Russian Geographical Society that this Society, which has contributed so largely to the increase of our knowledge of Siberia, is beginning to recover from the losses it sustained during the great fire at Irkutsk. Private subscriptions have been raised for the reconstitution of the library and museum to the amount of 2170*l.*, and both are in a fair way of development. The library already has about 4000 volumes, but is in great want of foreign geographical publications, and makes an appeal to the geographical societies throughout the world to send their publications and, if possible, series of former publications, which ought to be addressed to the Secretary of the East Siberian branch at Irkutsk. The chief occupations of the Society were: the geological exploration east of Lake Baikal, by M. Chersky, who has already published a map of the western coast of the lake; archæological researches as to the prehistoric inhabitants of Siberia, by MM. Agapitoff, Khangaloff, Witkovsky, and Bogolubskiy; and the part it took in the organisation of the Arctic Meteorological Station at the mouth of the Lena, and of a series of four intermediate stations between Irkutsk and this station. This last scheme could not be realised in full, but two stations have already been opened at Verkholensk and at Preobrajenskoye. The last number of the *Journal* of the Society contains, besides the annual report and the proceedings, a list of new determinations of latitudes and longitudes in Transbaikalia; a notice on Shamanisur with Yakuts; a paper on the populations of the basin of the Amur, according to Prof. Schrenck; a paper on the inscriptions on stones and rocks in the district of Minusinsk; and several notes, on the Lena Meteorological Station, on the Usuri region, &c.

Petermann's Mittheilungen for May contains a paper by Mr. Carl Bock describing a journey recently made by him from Bankok to the frontiers of the independent Shan States. He travelled along the Menam River in a boat given him by the Siamese Government, as far as Raheng, where he diverged into the Me Ping. He then proceeded partly by the river, partly by land through Lakon and Lampun, to a town which he calls Tschendmai, but which is more generally known as Kiangmai, or Zimmé. This place, which is the capital of the Shan States tributary to Siam, is an important point in Mr. Colquhoun's proposed railway from Rangoon and Moulmein, into south-western China. It formed the proposed terminus, too, of that gentleman's recent journey through Yunnan and the Shan States. Mr. Bock described it as a fortified town of about 700,000 people, lying in a fertile plain of uninterrupted rice fields, about 500 yards from the Me Ping, which is here 400 feet wide. Even now it is of great political and commercial importance, as it controls the trade of these regions both with Siam and with British Burmah. The teak forests of the States he describes as almost inexhaustible, especially higher up near the Meikong, where, however, it is not yet known whether the lumber can be easily floated down to the sea. For this purpose Mr. Bock recommends a careful survey of the various rivers and their tributaries. From Zimme he continued his way higher up to Kiangtsen, in the valley of the Meikong, and on the borders of the independent Shan States. It was his original intention to travel through these States into Yunnan, as it was Mr. Colquhoun's to travel through them from Yunnan, southwards. Failing this, he returned to the Me Ping, with the object of tracing this river to its source. He was prevented from carrying out either project by the native hostility, which, we regret to say, Mr. Bock himself did much to intensify, if not arouse, by his indiscreet behaviour. It would be inconceivable, if we did not have it on his own testimony, that any traveller among a people who, as he was specially warned, disliked even the Siamese, and absolutely hated any white man, should so far forget all discretion as to enter a populous town and "out of his own hand," as he describes it, take possession of the court of justice, and assault with a stick the official who endeavoured to prevent this unjustifiable trespass. He was punished by several days' imprisonment, but it is unfortunate for the cause of science that the hostility thus carelessly and wilfully aroused should have put a speedy termination to a journey full of promise. Mr. Bock, however, has shown beyond doubt that a railway from Bankok to the Shan frontiers is a possibility. It would pass through populous and rich districts in the valleys of the Menam and Me Ping. He says that no one who has not visited Zimmé can understand how extensive the trade of the place is, and his proposed railway would place the Laos States in direct communication with the sea, and attract the commerce not only of the Shan States, but also of Yunnan. These are exactly the arguments by which Mr. Colquhoun supports his scheme for a railway to Rangoon. Let us hope that in days to come, when this colossal project is an accomplished fact, there may be no dispute as to the originator of the idea of attracting the trade of south-western China to the sea by means of a railway through the Shan States.

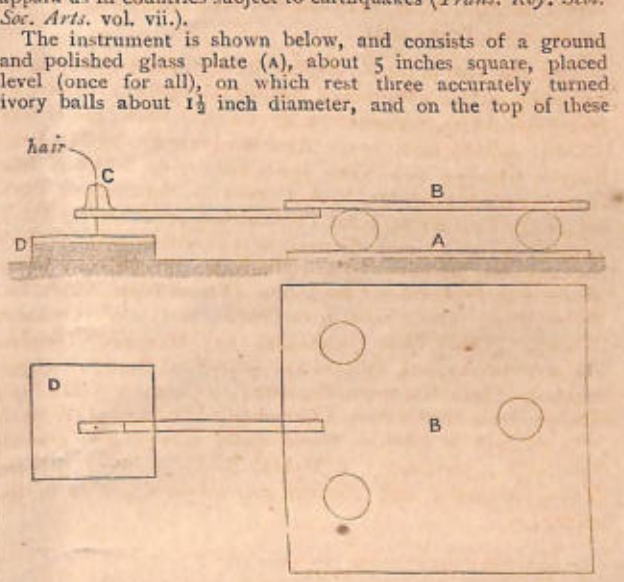
A NEW FORM OF SEISMOGRAPH[1]

NUMEROUS forms of seismometers have from time to time been invented, and having these various instruments, it may be asked why there is any necessity for a new form, and I can best answer this by quoting from a report of a committee of the British Association of 1872, as follows:—"Some simple and cheap method of indicating earthquake movement is thus much to be desired—any apparatus for the purpose should occupy small space, be little liable to derangement, capable of being put up in any apartment not of special construction, and its indications such as any intelligent person could easily interpret and readily note."

Now none of the instruments yet invented fulfil these conditions, and hence I bring before you one which is of the very simplest nature.

The idea of the instrument I propose was suggested to me by the aseismatic arrangement designed by my father, Mr. David Stevenson, for averting damage to buildings and lighthouse apparatus in countries subject to earthquakes (*Trans. Roy. Scot. Soc. Arts.* vol. vii.).

The instrument is shown below, and consists of a ground and polished glass plate (A), about 5 inches square, placed level (once for all), on which rest three accurately turned ivory balls about 1½ inch diameter, and on the top of these



balls is placed a plate (B) similar to the lower, but having attached to it a projecting arm with a long vertical hole pierced through it. Through this hole passes a steel needle (C) with a fine point, which rests by its own weight on a lampblack surface formed on the plate D. A hair about 2 inches long should be fixed to the eye of the needle to assist in adjusting it. The instrument thus becomes a pendulum of infinite length, so that whenever there is any movement of the ground, and therefore of the lower plates, the top plate with its arm and needle attached remain practically steady, and the point of the needle therefore marks on the lampblack surface the amount of motion and the direction in which the lower plate is moved. This instrument, it will be observed, fulfils all the requirements mentioned in the report of the committee of the British Associa-

[1] Abstract of paper read before the Royal Scottish Society of Arts, February 13, 1882, by Charles A. Stevenson, C.E., Edinburgh.

tion, and can be made more or less sensitive. It is impossible from a mere description to form any conception of the efficiency of the apparatus, nor has it been tried by any earthquake, but the instrument before you having been erected on the gable of a dwelling house during the past year, repeatedly registered the shaking of the gable to the amount of 1–16th of an inch.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

CAMBRIDGE.—The Museums and Lecture Rooms Syndicate have just issued their annual report, in which they mention the high value of the present of the late Prof. Balfour's scientific instruments and library to the University by his family, and again emphasise the necessity existing for a new chemical laboratory. Mr. Clark records the mounting of the fine male Indian elephant's skeleton in the Zoological Museum, received in an exceedingly complete state last year in consequence of its careful preparation by Mr. A. Haly of the Colombo Museum. The animal was shot by Mr. Le Mesurier of the Ceylon Civil Service; its height was nine feet. A specially interesting skeleton of the adult Gangetic Dolphin has been presented by Sir J. Fayrer. Mr. A. P. Maudslay, M.A., of Trinity Hall, has deposited in Mr. Clark's care a large portion of his ethnological collection made in Fiji and adjacent islands; these are almost certain to be presented to the University at no distant date. The Curator in Zoology (Mr. A. H. Cooke) has catalogued and arranged the British species in the MacAndrew collection. Its completeness may be judged by the facts that of 6 recorded species of Brachiopoda this contains 5; of 159 marine Conchifera this contains 146; of 248 marine Gasteropoda this contains 208; of 125 land and freshwater shells this contains 114. A recent appeal to add missing species has already resulted in the presentation of fourteen species by Mr. J. T. Marshall.

Dr. Michael Foster reports an average class of about 100 in Elementary Physiology, and of over 20 in advanced Physiology, in the three terms of the past year. Additional lecture room accommodation is much needed for these large classes.

The morphological work begun by the late Prof. Balfour has been continued on the same lines by Mr. Adam Sedgwick, Mr. W. H. Caldwell and Dr. Hans Gadow as lecturers, and Mr. Walter Heape and Mr. W. F. R. Weldon as demonstrators. In the Lent Term of this year 63 students attended the elementary class, and 26 the advanced classes. Five students have been engaged in original work. Mr. A. J. Balfour, M.P., has offered to give annually a sum sufficient to defray the cost of the complete series of scientific journals taken in by his late brother.

Dr. Vines has carried on practical instruction in Vegetable Anatomy and Physiology under considerable difficulties owing to the small space available; he has had to repeat all the work four times. The numbers attending his practical classes in the Michaelmas Term, 1882, were 19; in the Lent Term, 1883, 37; in the present Easter Term, 35.

Prof. Hughes reports that the whole of the geological library, consisting of 800 volumes and 1000 pamphlets, of the late Mr. E. B. Tawney, have been liberally presented to the Woodwardian Museum by his brother, Mr. C. H. Tawney, late Fellow of Trinity College. Opportunity has been taken in the past year to largely improve the foreign Tertiary collections in the museum.

Prof. Stuart reports the addition of a number of machines and a large development of his classes; a foundry begun as an experiment has proved one of the most successful parts of his undertaking.

The Philosophical Library in the new Museums has been largely increased by the valuable presents made by the family of the late Prof. Balfour, by Mr. J. W. Clark, by Prof. Darwin, Prof. Humphry, Prof. Newton, and others.

It has been recommended by the Special Board for History and Archæology that a separate Board be created for Archæology, distinct from that of History. This has been concurred in by the General Board of Studies.

The Botanic Garden Syndicate have reported many improvements in the collections of trees, of rock vegetation, and in the Plant Houses. The largest specimens in the Palm House have been safely lowered to about 2½ feet below the ground level. All the genera of carnivorous plants in cultivation and most of the species are now in the collection. *Vitis gongylodes* has been flowered for the first time in this country. The Curator, Mr. Lynch, was deputed to visit the Botanic Gardens at Dublin, Manchester, and Liverpool, and has also visited Chatsworth with the result that much valuable information has been obtained in all departments of management and cultivation, and many important exchanges have been made.

The Adams Prize, for a general investigation of the action upon each other of two closed vortices in a perfect, incompressible fluid, has been awarded to Mr. J. J. Thomson, M.A., Fellow of Trinity College.

Messrs. W. H. Besant and E. J. Routh are the first to be notified as "approved by the general Board of Studies for the Degree of Doctor in Science."

Candidates for the Professorships of Physiology and Anatomy are requested to send their names to the Vice-Chancellor on or before June 7.

THE Institute of Agriculture, South Kensington, will give an extended series of lectures next winter, beginning on October 1. The following courses are arranged for:—Mr. Bernard Dyer, Chemistry in Relation to the Soil; Mr. F. Cheshire, Practical Course on the Use of the Microscope (these two courses to be delivered in the Lecture Theatre of the Museum of Geology, Jermyn Street). The next series will be given in the Lecture Room of the Natural History Museum, South Kensington: Mr. Bettany, Vegetable Physiology; Mr. Worthington Smith, Diseases of Farm Crops; Prof. J. W. Axe, Animal Physiology in Relation to Farm Stock; Miss E. A. Ormerod, Farm Insects; Mr. W. Topley, Geology and Physical Geography in Relation to Agriculture. The remaining courses will be given in the Lecture Theatre of the South Kensington Museum: Prof. Tuson, the Chemistry of the Food of Farm Stock; Prof. Buckman, Farm Seeds; Prof. Tanner, Agriculture; Mr. R. Holland, Management of Grass Land; Mr. Gilbert Murray, Breeding and Management of Horses; Mr. W. Housman, Cattle; Mr. H. Woods, and Mr. J. A. Clarke, Sheep; Prof. J. W. Axe, Preventable Diseases of Farm Stock; Farm Implements and Machinery, Mr. W. R. Bousfield and Mr. W. W. Beaumont. A distinct course of lectures will be given on Poultry, Dairy, and Bee Management. The arrangements made enable students to give their undivided attention to one subject at a time, two lectures being given daily till the subject is completed. The fees being at the rate of half a guinea for each week's course of ten lectures, and any student being allowed to attend a single course, the greatest facility exists for persons choosing their work according to their needs or convenience. Thus it is believed, after the success of the tentative courses of the past winter, that many sons of tenant farmers will find this a most valuable and available mode of acquiring an agricultural education.

SCIENTIFIC SERIALS

THE *American Naturalist* for March, 1883, contains:—On the extinct dogs of North America, by E. D. Cope.—On the plains of Michigan, by V. M. Spalding.—Organic physics, by Charles Morris.—Indian music, by E. A. Barber.—On the occurrence of fossiliferous strata in the lower Ponent (Catskill) group of Middle Pennsylvania, by E. W. Claypole.—Pitcher plants, by Joseph F. James.

April, 1833, contains:—The Naturalist Brazilian Expedition, No. 1, from Rio de Janeiro to Porto Alegre, by H. S. Smith.—Unnatural attachments among animals, by J. D. Caton.—Butterfly hunting in the desert, by W. G. Wright.—The extinct Rodentia of North America, by E. D. Cope.—Heterogenetic development in Diaptomus, by C. L. Herrick.—A study of the immature plumage of the North American shrikes to show their descent from a common progenitor, by Thos. H. Streets.

May, 1883, contains:—Wampum and its history, by E. Ingersoll.—The Naturalist Brazilian Expedition, No. 2, by H. S. Smith.—The Polar organisation of animals, by C. Morris.—On the classification of moths, by A. R. Grote.—Heterogenetic development of Diaptomus, by C. L. Herrick.—On the morphology of arteries, especially those of the limbs, by F. Baker.—The hairy woodpecker, by A. G. Van Aken.

Archives Italiennes de Biologie, tome ii. fasc. 2, November 30, 1882, contains among the original articles the following:—On the minute anatomy of the muscles which move the wings of insects, by G. V. Ciaccio.—On the structure of striated muscular fibre in some vertebrates.—On the development and the morphology of the kidney of osseous fish, by C. Emery.—On the substance preventing the coagulation of the blood and lymph whilst these contain peptone, by Jules Fano.—On the germs and lower

organisms found in ordinary and malariac earths, by A. Ceci.—Transfusion of blood and its effects on nutrition, by P. Albertoni.—On the pathological anatomy of the cornea in the glaucomatous eye, by F. Tartuferi.—On the presence of a cordon or slip on the Uncus of the Hippocampus in the brain of man and some other animals, by C. Giacomini.—On the chemical composition of the egg and its envelope in the common frog (*Rana temporaria*), by P. Giacosa.—Anatomical considerations of the doctrine of cerebral localisations, by C. Golgi.

Tome ii. fasc. 3, February 1, 1883, contains anatomical considerations of the doctrine of cerebral localisations, by C. Golgi (continued).—On compensative hypertrophy of the kidney, by C. Golgi.—Experimental studies on hypnotism, by A. Tamburini and G. Seppili.—The origin of the mesoderm and its relations to the vitellus, by G. Romiti.—On the anatomy of a fœtal Otaria (*O. jubata*), by L. Camerano.—On the physiology of smooth muscular tissue, by A. Capparelli.—On the physiological action of certain substances on the vesical muscles, by P. Pellacani.—On the anemia of miners from a parasitological point of view, by E. Perroncito.—On the change in form of uric acid by the action of glycerine, by J. Colasanti.—On Ptomaines, by J. Guareschi and A. Mosso.—On some endoparasitic Protista, by Dr. Grassi.

Tome iii. fasc. i., April 15, 1883, contains:—On the sanitary improvement of the Roman Campagna, by C. Tommasi-Crudeli.—On the anemia of miners (conclusion), by E. Perroncito.—On some endoparasitic Protista (conclusion), by Dr. Grassi.—On the presence of a secretive tissue in vertebrates, by C. Emery.—On vibratile endothelium in mammals, by J. Paladino.—On the attenuation of charbon virus, and on its transmission from mother to fœtus, by E. Perroncito.—On the acoustic epithelium, by A. Tafani.—On the termination of nerves in the striated muscles of torpedo, by J. V. Ciaccio.—The general physiology of smooth muscular tissue, by E. Sertoli.—On a new morphological element of the blood, and its importance in thrombosis and coagulation, by J. Bizzozero.—New studies of the chestnut disease, known as the ink disease, by J. Gibelli.

THE *Bulletin de l'Académie Royale des Sciences, des Lettres, et des Beaux-Arts* for 1883, part i., contains papers by F. Henrijean, on the part played by alcohol in nutrition; by MM. Valerius and Van der Mensbrugghe, on M. Delaurier's observations on the concentration of solar rays and the transformation of electricity into heat; by W. Spring, on the colour of marine, lacustrine, and fluvial waters; by C. Le Paige, on the homography of the third order in algebra; by Baron Northomb, on the political relations of the Netherlands during the seventeenth century.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, February 1.—"On the Affinities of Thylacoleo." By Prof. Owen, C.B., F.R.S., &c.

Since the communication of the paper "On Thylacoleo," in the *Philosophical Transactions* for 1871, further explorations of the caves and breccia-fissures in Wellington Valley, New South Wales, have been made, by a grant for that purpose from the Legislature of the Colony, and carried out by E. B. Ramsay, F.L.S., Curator of the Museum of Natural History, Sydney. The present paper treats of the fossils contributing to the further restoration of the great carnivorous Marsupial (*Thylacoleo carnifex*, Ow.) They exemplify the entire dentition *in situ* of the upper and lower jaws of a mature individual; the bones of the forelimb, of which those of the antibrachium and the ungual phalanges are described, are compared with those of other Marsupials, and of placental, especially feline, *Carnivora*. An entire lower jaw with the articular condyles adds to the grounds for the determination of the habits and affinities of the extinct Marsupial.

Figures of these fossils of the natural size accompany the paper.

Geological Society, May 9.—J. W. Hulke, F.R.S., president, in the chair.—Rev. William Spiers and H. A. Williams were elected Fellows of the Society.—The following communications were read:—The age of the newer gneissic rocks of the Northern Highlands, by Mr. C. Callaway, D.Sc., F.G.S., with notes on the lithology of the specimens collected, by Prof. T. G. Bonney, F.R.S. The object of the author was to prove that the eastern gneiss of the Northern Highlands, usually regarded as of "Lower Silurian" age, was to be placed in the Archæan. While admitting that this gneiss frequently overlies the quartzo-dolomitic group of Erriboll and Assynt, he held that this relation was due to dislocation accompanied by powerful thrust from the east, which had squeezed both formations into a series of folds, thrown over towards the west, so as to cause a general easterly dip. In Assynt the "Upper Quartzite" was first discussed. The author described several sections which he considered to prove that this band was the ordinary quartzite repeated east of a great fault, which brought up the Hebridean; in one place, Glen Coul, the quartzite being conformably succeeded by the brown flags and dolomite. The "igneous rocks" of Nicol ("Logan Rock" of Dr. Heddle) were regarded as the old gneiss brought up by a fault and thrown over on to the Assynt group to the maximum breadth of more than a mile. The "Upper Limestone" of authors was described as either outliers of the dolomite or a part of the Caledonian series. The "Caledonian" rocks were seen in Glen Coul to be immediately overlying the Hebridean, the Assynt group being caught in the angle between the two gneisses, and bent back in overthrown folds. The mountain groups of Assynt were described as usually consisting of cores of Hebridean gneiss swathed in or capped by sheets of quartzite. In the former case the quartzite on the western slopes was contorted into overthrown folds by the thrust from the east. In the Loch Erriboll district, the "granulite" of Nicol was considered to be a lower division of the Caledonian gneiss, though bearing some resemblances to the Hebridean. In other respects the views of Nicol were regarded as substantially correct. Along the entire length of Loch Erriboll, a distance of about twelve miles, the thrust from the east had bent back the Assynt group into overthrown folds, and pushed the Caledonian gneiss on the top of the inverted quartzite. This had produced the appearance of an "upper" quartzite passing "conformably" below the eastern gneiss. The superior antiquity of the Caledonian was confirmed by the occurrence of outliers of quartzite upon the Arnaboll (Lower Caledonian) series, and by the fact that the granite, which sent numberless veins into the gneiss, never penetrated the quartzite and associated rocks.—On a group of minerals from Lilleshall, Salop, by C. J. Woodward, B.Sc., F.G.S.—Fossil Chilostomatous Bryozoa from Muddy Creek, Victoria, by A. W. Waters, F.G.S.

Chemical Society, May 17.—Dr. W. H. Perkin, president, in the chair.—Capt. W. de W. Abney, F.R.S., delivered a lecture on photographic action studied spectroscopically. The lecturer said he wished that all chemists were photographers; photography occupied the borderland between chemistry and physics; he was firmly convinced that photographic action was interatomic. The action of a developer was then experimentally illustrated; this action is physical. Light causes the liberation of iodine in a film of silver iodide, and the developer precipitates metallic silver. The silver so reduced is infinitesimal, and must be in many cases derived from the film. The positive pole of the electric arc was found to be the best source of light. Gratings could not be used for quantitative work, as they varied so much in their ruling; a glass prism was therefore used to form the spectrum. A film of silver chloride absorbs only the violet end of the spectrum; silver iodide absorbs more, and the bromide most of all; accordingly when a photograph of the spectrum was taken on these three films it was seen that the portion of the chloride acted upon was very much less than when bromide of silver was used. It was shown that a sensitiser essentially takes up the halogen liberated by the action of light. One salt of silver may act as a sensitiser to another salt of silver. Photographic action is completely prevented by the presence of oxidisers, as bichromate, &c. Reverse photographs were discussed, and the action of sodium sulphite in preventing the evil effects of over exposure. The peculiar green condition of silver bromide which is sensitive to ultra-red rays was explained. In conclusion the lecturer said that his principal object was to warn chemists of some of the numerous pitfalls which they might encounter in scientific photography.

Meteorological Society, May 16.—Mr. J. K. Laughton, F.R.A.S., president, in the chair.—F. A. Bellamy, T. A. Mercer, Rev. H. J. Poole, and A. Wise, M.D., were elected Fellows of the Society. The following papers were read:—Composite portraiture adapted to the reduction of meteorological and other similar observations, by G. M. Whipple, B.Sc., F.R.A.S. It has often been remarked that one of the main, if

not the chief, of the difficulties the meteorologist has to contend with, is the enormous amount of preliminary labour which has to be expended in the not very pleasing task of forming the observations he may wish to discuss into tables, casting the columns of figures so obtained, and then computing the means. With the view of arriving at results by a shorter cut, the author has been led to consider the possibility of employing a method, suggested by a consideration of the highly ingenious system of composite portraiture, invented by Mr. Francis Galton, F.R.S., and utilised in his anthropological studies.—Note on atmospheric pressure during the fall of rain, by H. Sowerby Wallis, F.M.S. The author discusses the condition of atmospheric pressure while rain was falling, during 1882, and finds that, out of a total of 136 rainy days (which were available for his purpose), on 54 per cent. the rain was accompanied by diminishing pressure, on 27 per cent. by increasing pressure, and on 19 per cent. by steady pressure.—New method of reading a thermometer and hygrometer at a distance by means of electricity, by Arthur W. Waters, F.G.S.—An integrating anemometer, by W. F. Stanley, F.M.S.—Observations on the force of the wind at sea, by D.W. Barker, F.M.S.—Meteorological observations at Zanzibar, east coast of Africa, during 1880 and 1881, by Surgeon-Major C. T. Peters, M.B.—Diurnal rainfall at Bangkok, Siam, by Capt. G. H. Inskip, F.R.G.S.

BERLIN

Physiological Society, April 27.—Dr. Mendel read a paper on the anatomy of the corpus striatum and lenticular nucleus. The older view, which was supported by the valuable anatomical researches of Prof. Meynert, was that the relation of the corona of radiating fibres above the lateral ventricle ("Stabkranz") to the lenticular nucleus and corpus striatum consisted in this, that in it ran bundles of nerve-fibres, which arise from the brain cortex and end in the large ganglia, whereas Dr. Wernicke three years ago propounded the view that a connection did not exist between the brain cortex and the corpus striatum and lenticular nucleus, but that these latter were bodies of the same range as the cortex. Dr. Mendel has for some years past studied the anatomy of these parts of the brain very attentively, and has been brought back to the older view by a series of sections (of the brain) of dogs, monkeys, and men, which series he laid before the Society. He found not only the bundles of out-streaming fibres, which alone were acknowledged to exist by Dr. Wernicke, but also a larger number of in-streaming bundles of fibres which show the connection of these brain-nuclei to the cortex. In the discussion Dr. Wernicke stated that he was not convinced by the paper or preparations of the correctness of the view propounded by Dr. Mendel, whereas Prof. Munk believed that his not-yet-completed physiological experiments afford grounds for Dr. Mendel's view.

Physical Society, May 4.—Prof. Hauck laid before the Society a model of a mechanical apparatus which solves the problem of combining drawings and photograms, which are drawn in two planes into a combination figure in the third plane. Prof. Hauck then explained the principle of the apparatus, and pointed out by means of geometrical figures the conditions which must be fulfilled in order to project any given points of two planes in common points of a third plane. He then proceeded to the complicated problem of bringing points of three planes, which meet in a corner, to a common projection, and applied these figures to the special case of projecting the perspective drawing of a building from its ground-plan and elevation. The model was calculated and arranged for this case, but the apparatus, in which the motions are produced by means of polished lineals, each running upon two pins, can be put to manifold uses in physical space investigations.

PARIS

Academy of Sciences, May 14.—M. Blanchard, president, in the chair.—The following papers were read:—On the pyro-electricity of quartz, by C. Friedel and J. Curie, second part.—On the cultivation of the cacao plant, with an analysis of the constituent elements of the cacao and chocolate berries, which were shown to contain in various proportions albumen, legumine, phosphates, fat, starch, sugar, theobromine, besides the materials entering into the formation of bone.—On the action of birds in flight studied by means of photography, with figures showing the successive positions of a pigeon on the wing at intervals of one-ninth and one-eighth of a second, and a closed curve representing the trajectory of the tip of the wing obtained by means of a special contrivance, by M. Marey.—On a double sulphate of iridium and potassium, by M. Lecoq de Boisbaudran.—On the diminution of virulence in carbon bacterides and their spores under the influence of antiseptic substances, by MM. Chamberland and Roux.—On iodine associated with the sedative alkaloids of opium treated both as a preventative and curative in the case of typhoid fever, by A. Delbovier.—On the immunity against attacks of Phylloxera enjoyed by the vine cultivated in the sandy soil of Algeria, by MM. F. Couvert and L. Degully.—Observations on the new planet 233 Borelly made at the Paris Observatory, by G. Bigourdan.—On the determination of the meridian in low latitudes, such as that of Rio de Janeiro, by M. Cruls.—On the conservation of energy and periodicity of the solar spots, by A. Duponchel.—On the laws of coincidences between the reductions of periodical fractions of the "two modes," by E. de Jonquières (continued).—On the generalisation of Thermat's theorem of numbers due to M. Serret, by M. Picquet.—On the possibility of extending to any electrolytic field the electro-chemical method in the figuration of potential distribution, by A. Guébhard.—On the influence of atmospheric pressure on the eruptions of gas and water in the Montrond Geyser (Loire), by F. Laur.—On the differences in the temperature of the sea and air, by M. Semmola.—On the quantitative analysis of sulphur and carbon in sulpho-carbonates, by A. Müntz.—On the regular surface-fissures in certain rocks, such as the hard eocene limestone used in the construction of the old ramparts of Genoa, by Ch. Contejean.—On new physiological studies of the torpedo, by M. Marey.—On the functions and organs of suction and deglutition in the leech, by G. Carlet.—On a case of purulent ophthalmia produced by the infusion of the seeds of the liquorice plant, by L. de Wecker.—On the fundamental principle of the electric log now in use in the French fleet, by M. G. Le Goarant de Tromelin, who claims priority of invention over the electric log invented by M. Fleuriais.

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THURSDAY, JUNE 21, 1883

"THE NEW PRINCIPLES OF NATURAL PHILOSOPHY"

The New Principles of Natural Philosophy. By W. L. Jordan, F.R.G.S. (London: David Bogue, 1883.)

In the Preface to this large and handsome volume we are told that "The Third Chapter, ... and more especially the first nine Parts of that chapter, are the justification for the title of this work." This sort of intimation is unusual, but timely and useful, as it enables us to go at once to the root of the m tter, and to study "*The New Principles*" in themselves, before we commence the perusal of the formidable array of arguments, examples, and demonstrations which constitutes the bulk of the volume. The chapter referred to is formally dedicated to the memory of Descartes and Newton, "as it shows the connexion between the Cartesian Vortices and the Newtonian Laws (*sic*) of Gravitation."

The peculiar employment of the definite article in the title of the work at once arrests the attention of the reader. New principles in Natural Philosophy, some of them coextensive with our whole knowledge of the subject, have been happily introduced even in comparatively recent times. The Conservation of Energy, and Carnot's principle of Reversibility, are notable examples. But such principles consist of generalisations of our former knowledge, or additions to it, and are in no way subversive of the fundamental Laws of Motion (or *Axiomata*) as they were systematized by Newton.

"*The New Principles*," however, are not of this class. They involve, essentially, somewhat extensive modifications of Newton's Laws, and the consequences we have been accustomed to draw from them. So far as we understand our author, he seems to allow that Newton and his followers have correctly deduced the consequences which follow from the assumption of the Laws of Motion and the principle of Gravitation. But the accordance of the results, so deduced, with observed facts is a remarkable coincidence only:—arising from a compensation of errors, where incorrect ideas as to the laws of motion are rectified (so far as results go) by an omission of some of the more powerful causes which are really at work. Thus the true statement of the First Law of Motion is that a body gradually comes to rest when the motive force ceases to act; while "Gravitation is merely the act by which the material universe resists, or endeavours to resist, the motive forces acting on it. And, therefore, if matter were not inherently inert there would be no such force as gravitation" (pp. 306-7). It follows, of course, from the new principles that some cause, hitherto not taken into account, is required to explain the persistence of the earth's rotation and of its revolution in its orbit. Of course this cause must suffice for the same results in the case of each planet; and it must also maintain the rotation of the sun itself. This is found in "Astral Gravitation." How so immensely effective a factor in all the physics of the universe has hitherto been entirely overlooked it is not for us to say. We content ourselves with a humble effort to disseminate "*The New Principle*" as widely as our circulation permits, if not as widely as its intrinsic importance demands. We would merely premise that our author distinctly points out that, after all, Astrology was wrong only because its votaries recognised "personal influences," whatever these may be, and not the gigantic physical forces exerted on us and our surroundings by the stars:—

"Reason also tells us that whatever the apparent magnitude of any given star may be, the greater its actual distance from the earth, the greater is the force which it exerts on the earth; and therefore the argument that the stars are too far off for their power to be felt, even if it merit the designation of common sense, is absolutely refuted by reason.

"It is not possible in the present state of knowledge to make a reliable (*sic*) estimate regarding the actual force exerted by any star.

"Sir William Herschel assures us that the star Capella has an apparent diameter of 2½ seconds. Its distance as indicated by parallax is 4½ million times greater than that of the sun.

"Accepting these measurements as accurate, and supposing Capella to be of the same density as the sun, the force of gravitation which it exerts on the earth is equal to one hundredth part of that exerted by the sun. If, however, any star of equal apparent magnitude whose distance is so great as not to be indicated by parallax, be as many times more distant than Capella as Capella is more distant than the sun, then the force exerted on the earth by that star, according to the foregoing measurements, would be 45,000 times greater than that exerted by the sun."

"It is argued that none of the stars can have so great an apparent diameter as asserted by Herschel, because Huygens has estimated that the sun gives us 2,000 million times more light than we receive from Capella, and this, supposing the two bodies to be of equal brilliancy, would make Capella much smaller than estimated by Herschel. For a star of the apparent size estimated in this manner to exert as much force on the earth as is exerted by the sun, its distance would have to be 20 million times greater than the actual distance of Capella as indicated by parallax.

"Neither of these estimates can be regarded as better than vague guesses at the real size of the stars; but when, on the one hand, we consider the evidence which seems to necessitate the existence of some great controlling force of gravitation far distant from our solar system, and, on the other hand, the fact that the laws of optics and of gravitation make it quite possible that some one of the visible stars may actually be exerting a force far more than sufficient for the purpose indicated, reason is almost forced to the conviction that stars of the requisite magnitude do exist in the heavens."

How this, and "The" other "New Principles" account for Trade Winds, Ocean Circulation, Comets, The Zodiacal Light, The Secular Acceleration of the Moon's Motion, &c., must be learned from an attentive perusal of the work itself. The careful reader will have an ample and varied treat; for mixed with these weighty contributions to science, we have full details of a more strictly human character, such as "Replies to Critics," "Remarks on the Admiralty Current Charts," "A Public Challenge to the Council of the Royal Society," &c. We quote (from p. 365) a few words to show how very serious indeed is the state of matters with our great scientific Society.

"I last year publicly challenged the leaders of the scientific world in London to open discussion; and, in the second of the public lectures I then gave, I made it clear that the question at issue had ceased to be merely a

question of scientific opinion but had become also one of honour. And until the Council of the Royal Society take measures to refute or to atone for (!) the charges I then made, it is evident that courtesy and chivalrous conduct are at a discount in the scientific world, and it is not surprising that the deterioration of the tone of thought should be such as to have at length attracted the attention of the editor of the *Times*."

At p. 486 we have again the old question of the moon's non-rotation about its axis. This latest follower of Mr. Jellinger Symons gives a new and rather amusing argument in support of the heresy. For he says it would require us to suppose that a ship which sails round the world must have turned a complete somersault, while it is quite obvious that she has not done so!! Here we fear we must part company with our amusing instructor; and, though to many it may appear the blackest ingratitude, we must conclude with a hearty wish that Mr. Jordan's work had been published some twenty years sooner. Had it been then given to the world it would, like a fly in amber, have secured immortality in the pages of De Morgan's unique, because inimitable, *Budget of Paradoxes*.

It has many of the distinctive charms of the celebrated works of Mr. James Smith (of Liverpool), Mr. James Reddie, and Baron von Gumpach. All these great men, in their turn, tilted at Philosophers or Scientific Bodies, the Astronomer-Royal, the Royal Society, the British Association, &c., and complained, as Mr. Jordan now does, of the bigotry and malevolence in high places which depreciated the value of the gifts they were bestowing on the world. Some of them were hopelessly illogical and stupid, others merely ignorant. Mr. Jordan appears to belong to the second category. He is evidently untaught, though presumably not unteachable. But he should not attempt to teach. P. G. T.

THE BRITISH MUSEUM CATALOGUE OF BATRACHIA

Catalogue of the Batrachia gradientia s. caudata, and B. apoda in the Collection of the British Museum. Second Edition. By G. A. Boulenger. (London: By Order of the Trustees, 1882.)

THIS volume completes the second edition of the Catalogue of the Batrachians in the British Museum. The former volume, which appeared in the spring of last year, we have already noticed. The first edition (1850) was prepared by Dr. J. E. Gray, and contained descriptions of 72 species; 132 species are described in the present work, the great majority of which have been actually examined by Mr. Boulenger. The number of species of tailed Batrachians in the British Museum collection now amounts to 78 against 38 in 1850. Several of the species which are wanting in the collection are natives of America, and in the interests of science we hope some of the distinguished herpetologists of the New World will generously supply these desiderata. The number of footless Batrachians in the Collection is 19 against 5 in 1850, and in this group also nearly all the species unrepresented in the national collection are natives of America. In addition to very full synopses of the families and subfamilies, of detailed diagnoses of the genera and species with synonyms, we have appended to this volume a summary of the principal facts of the geographical distribution of Batrachians generally, which adds immensely to the value of this catalogue to the general biologist. Of the various primary geographical divisions which have been proposed, Mr. Boulenger finds that that recognised by Dr. Günther for freshwater fishes into Northern Equatorial and Southern Zones, agrees best with the facts deducible from the study of Batrachians, but with one modification, for a Southern Zone does not exist for Batrachians. Tasmania and Patagonia do not differ in any point regarding their frog-fauna from Australia and South America respectively. The following are the principal conclusions:—(1) In the Northern Zone there is an abundance of tailed and an absence of footless forms. A. In the Old World division (Europo-Asiatic or Palæarctic Region) there are numerous Salamandrinæ, with a single exception an absence of Hylidæ, but Discoglossidæ are present. B. In the North American division we find Sirenidæ, few Salamandrinæ, Plethodontinæ, Amblystomatinæ, and Hylidæ numerous, Desmognathinæ. (2) In the Equatorial Southern Zone there is an absence of tailed Batrachians and an equally characteristic presence of footless forms. Dividing (A) the Old World region into Indian and African, we find in both the frogs numerous (260 species out of 300), an absence of Hylidæ and Cystignathidæ, while in the former there are no Aglossæ or Dendrobatidæ, while in the latter there are Dactylethridæ or Dendrobatidæ. Dividing (B) the New World region into Tropical America and Australia, the former is rich in footless forms (21 species), has very many tailless forms, some small families quite peculiar to the region, but above all is it rich in the Arcifera, it has also a few tailed forms; the latter is divided into three subregions: the Australian proper is chiefly remarkable for a negative character, there are no footless or tailed forms, almost no toads or frogs, its fauna consisting mainly of the two families, Cystignathidæ and Hylidæ; the Austro-Malayan subregion presents an interesting blending of Indian and Australian forms, a curious fact is the occurrence, according to Peters, of a third species of the African genus Phrynomantis in Amboyna and Batavia, New Caledonia does not yield a single Batrachian; the third or New Zealand subregion possesses but a single species, *Liopelma hochstetteri*, very curiously a member of the family Discoglossidæ, which is otherwise restricted to the Europo-Asiatic region. The following new species are described for the first time, and there are excellent illustrations of most of them in the plates accompanying this volume: *Hynobius peropus*, China and Japan, *Spelerpes yucatanicus*, Yucatan, *Uræotyphlus africanus*, West Africa, *Hypogeophis guentheri*, Zanzibar, *Dermophis albiceps*, Ecuador, and *Chthonerpeton petersii*, the Upper Amazon.

The keeper of the Department of Zoology in the British Museum may be congratulated on the Batrachian collection having held pace with the progress made in this branch of science during the last thirty years.

OUR BOOK SHELF

The Cinchona Planter's Manual. By T. C. Owen. (Colombo: A. M. and J. Ferguson, 1881.)

FEW plants have been so fortunate or unfortunate in having so much written about them as the *Cinchonas*. Ever since their successful introduction into India, now

some twenty years since, the Cinchonas have had showered upon them books and pamphlets innumerable, and where we find such voluminous writings, it would be strange indeed were there not matter of varied quality, and some that could be dispensed with altogether. Mr. Owen's book is very complete in the several branches of Cinchona literature, facts gathered from various authentic sources, such as the works of Dr. King, Dr. Bidie, Mr. McIvor, and the reports of the Indian and Javan Governments, all of which are acknowledged by the author.

The book is divided into six parts, the first part being devoted to the physiology of plants, gathered, as we are told, from Church and Dyer's "How Crops Grow." The second part treats of the alkaloids, the species and varieties, to which a large space is given, and the next part on the choice of land, felling, clearing, weeding, planting, &c. In the fourth part manuring and harvesting are considered; and in parts 5 and 6 the diseases to which Cinchonas are liable, and the estimates of Cinchona planting are digested. In all these matters careful details are given.

The book no doubt will be very useful to Cinchona planters, more particularly the practical part. Its greatest fault, perhaps, is the extent of the book, numbering 203 pages, too voluminous for many planters to wade through; but on the other hand it appeals also to those who, though not actual planters, are interested in the progress of the Cinchona culture.

Kallos, a Treatise on the Scientific Culture of Personal Beauty and the Cure of Ugliness. By a Fellow of the Royal College of Surgeons. (London: Simpkin Marshall and Co., 1883.)

THE author desires his book to be taken seriously. He shows that good looks and manners have a commercial value, since those are more likely to succeed in obtaining the prizes of life who can make favourable first impressions than those who cannot. The first start greatly depends on patronage, and obscure youths who have won wealth and position have almost always been helped by their good looks, good address, and good voice. These are aids of considerable importance to every candidate, whether it be for a place behind a counter or for the suffrages of a constituency. The author considers from a medical point of view how ill-favoured individuals may palliate their defects. He treats ugliness as a disease, classifying its various forms and indicating such remedies as he can. His classes are coarseness, thinness, obesity, vulgarity, wrinkles, defects of circulation, of complexion, and of the hair. Then he takes the features in detail, eyes, nose, mouth, &c. His recipes are not numerous. We learn incidentally that what is sold as lime juice and glycerine for the hair contains no glycerine at all, and that a very popular dressing is castor oil and rum. This would have harmonised with the toilette of the Syrian beauty of old times, whose "garments smelt of myrrh, aloes, and cassia," a very apothecary-like fragrance. The book does not contain practical advice of much novelty, but its interest chiefly lies in directing attention to much that we already know but are too apt to forget; such as that dissipation, gross feeding, and indolent ways create ugliness of various forms. We know there are bad schools where the boys are slouching, ill complexioned, furtive in expression, and generally ugly, and we also know that there are good schools where, owing to healthy habits and keen and varied interests, the boys are bright, vivacious, and attractive. Similar differences due to different habits of life exist in men; they are pre-eminently shown by the good effect of drill on a plough-boy or street lounger. We may be sure that those who habitually cultivate a healthy mind in a healthy body, and who study how to please, cannot fail to add to the total happiness of the world and to secure for themselves a better chance of succeeding in it than their more negligent rivals.

The Nat Basket. (Printed for the Editress and Publisher, Mrs. Eleanor Mason, at the Albion Press, Rangoon, Burmah.)

WE hope that the subscribers to this extraordinary publication are content to give to it their money and nothing more. It is designed, we are told, to show the natives that there is no contradiction between Scripture and science, but if they believe that what is presented to them in the *Basket* is science they are much to be pitied. Such a medley of misstatements, absurd etymologies, and false astronomy was never before met with out of Bedlam. If this is the stuff that is taught the Burmese by our missionaries, the sooner the latter return home the better.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Deductive Biology

IN the few remarks which I communicated to this journal (vol. xxvii. p. 554) under the above heading, I protested against the deductive method used in a purely literary manner as a mischievous way of attacking biological problems. Mr. William White objects that if I am right the deductive method must be excluded altogether "as a false and dangerous element of philosophy." I do not myself see that this necessarily follows. The pith of what I said simply amounts to this—the biological sciences not having reached the deductive stage, it is not possible to enlarge our knowledge in them by mere ratiocination. This is I apprehend no more than is laid down by Mr. Mill himself. Writing of the conditions under which the deductive method is applicable, he expressly says that without one indispensable adjunct "all the results it can give have little other value than that of guesswork" ("System of Logic," 4th ed. vol. i. p. 498). The indispensable adjunct is verification, which requires the substitution of the work-table for the desk. When the former has put the stamp of confirmation on the speculations elaborated at the latter we get a scientific result which commands attention. Without this confirmation I am still of opinion that the deductive result is only "a literary performance." It is worth noting that the able writer whose papers and method I took the liberty of criticising so far admitted the validity of what I said, that he promised to have some experiments made which would go a considerable way towards demolishing or sustaining the results at which he had so far arrived only deductively.

As it would be a rather arduous undertaking to follow Mr. White over all the other ground covered by his letter, I will only refer to one point. He asks whether "comparative embryology" is not "founded entirely upon the method of deductive analogy." I am certainly myself under the impression that it would be difficult to pitch upon any area in science in which the knowledge we possess has been more conspicuously gained by persistent investigation or one in which generalisations have more often crumbled under the pressure of fresh results of observation. It is the section-cutter, and not the desk, which has won the victories in this field. At the present moment two of the most skilled of our younger embryologists (with funds furnished by the Royal Society) are on the point of starting, one for the Cape, to study the embryology of *Peripatus*, the other to make a similar attempt in Australia on the earliest phases of the life-history of *Ornithorynchus* and *Ceratodus*. They would hardly perhaps engage in so laborious a quest if it would answer equally well to stay at home with a ream of paper, and, say—without any disrespect to the eminent author—a copy of the writings of Mr. Herbert Spencer as "a base of fundamental truth" to start from in the analogical deduction of the embryology of these organisms.

W. T. THISELTON DYER

YOUR correspondent, Mr. William White, has not, it seems to me, a correct appreciation of the words "deductive" and

"induction," as used in reference to the investigation of the causes of phenomena. The mistake which he makes is a very frequent one, and is due to the ambiguity of the words themselves, and to the inaccessibility of a treatise on modern logic.

The "deductive method," as formulated by John Mill, is one method by which the mental process known as induction—"the operation of discovering and proving general propositions"—is accomplished. An "induction" may be a simple inference from an observation; it must be an inference in which the conclusion is *wider* or *more general* than the premises from which it is drawn. A "deduction" (as the term is commonly used) is a result of ratiocination solely, or, in other words, of a "train of reasoning," by which from a general proposition (not alone, but by combining it with other propositions), we infer a proposition of the *same* degree of generality with itself, or a *less* general proposition. The "deductive method" receives its name from the fact that ratiocination is combined in it with induction.

"In order to discover the cause of any phenomenon by the deductive method, the process must consist of three parts:—(1) Induction; (2) Ratiocination; (3) Verification;" or in common language: (1) A generalisation from observed facts [or a deduction from a previous generalisation]; (2) A deduction from this generalisation [or from an initial deduction]; (3) The testing or verification of the final deduction.

The "hypothetical method" is a special and very usual form of the deductive method in which in place of an induction or primary deduction we have substituted a hypothesis. Under proper safeguards this is the most valuable and fertile method of investigating the causes of complex phenomena. Hypotheses are legitimate or illegitimate. The cause suggested by the hypothesis, if not already known as existing, ought to be capable of being known, and, until the cause suggested is shown to exist, the hypothesis, although verified, constitutes only a plausible conjecture. Further, the hypothesis must be such that no other hypothesis substituted for it would lead to verification.

A hypothesis, as distinguished from a proposition resulting from a complete induction or a correctly formulated deduction, is "a supposition without actual evidence or with evidence avowedly insufficient." The whole value of a hypothesis lies in the final carrying through with it of the deductive method. It must be made the starting-point of deductions, and these must be (one or more) brought to the test of observation or experiment—the final process of verification.

So much by way of preliminary.

The objection which my friend Mr. Thiselton Dyer has made to the essay of Mr. Grant Allen upon the forms of leaves does not, it appears to me, consist in a depreciation of the "deductive method" as Mr. William White is led to believe. Nothing can be further from the real state of the case.

What Mr. Dyer objects to is that the method is *not carried out* by Mr. Allen. Mr. Allen gives us hypotheses—suppositions with insufficient evidence—and deductions from the generalisation of evolution, but he is relatively deficient in "verification." He also fails in the condition insisted on by Mill, who holds that the hypothetical method is valueless (or relatively so) unless it be proved that no other hypothesis than that formulated can be similarly verified. He further, in the case of the supposed exhaustion of the carbonic acid in atmospheric air, appears to fail in another respect indicated by Mill, in so far as he does not demonstrate the actual existence of the cause which he assumes in his hypothesis. His proposition on this head is no more than "a plausible conjecture" at the best, and is not a legitimate conclusion arrived at by the deductive method.

I do not think that there is any ground for discountenancing either a "purely deductive" or a "purely inductive" method in the treatment of biological topics, so long as the method is soundly and thoroughly carried out and its logical results truly and clearly stated. Still less is there any shadow of reason for not fully accepting *the* "deductive method" (so named) as the method of biological research. What we have to deprecate in some modern speculative essays is the tendency to put forward suppositions as though they were propositions which have been demonstrated, and to employ the printing press in launching hypotheses which are neither legitimate inductions nor deductions, and should be kept unpublished until their originator has thoroughly examined them by the accepted "deductive method."

E. RAY LANKESTER

11, Wellington Mansions, North Bank, N.W.

The Peak of Teneriffe *not* very Active again

WITH reference to my notice in NATURE, vol. xxvii. p. 315, stating, on the authority of a native lady in Santa Cruz, that the Peak of Teneriffe was active again, even to the extent of exuding a red-hot lava stream from near its summit, I am informed now from a higher scientific authority, viz. a Cambridge man, and high Wrangler there in his day, but since then resident in Teneriffe, near Puerto Oratava, for fifty years, that that view was exaggerated. I hasten, therefore, to present to your readers exactly what this venerable and experienced man has to say, without altering a word, so far as the extract goes:—

"The facts of the case," says he, "are simply these. On a clear day of south weather, about the latter end of December or the beginning of January last, I happened to be looking at the Peak (as I often do) and observed several distinct and very copious gushes of steam issuing from the summit. In similar weather I had often seen a similar phenomenon, but never to anything like the same extent. I watched these steam gushes several times that day, and very remarkable they were. On going down to Port the following day, I found they had been seen by several people there, who declared that the peak was pouring forth volumes of smoke and flames. The so-called smoke was simply the steam gushes I have mentioned, and what were mistaken for flames I am convinced were nothing but the same steam gushes illumined by the rays of the setting sun. All agreed that after dark nothing was to be seen there, which certainly would not have been the case had there been fire or flames. As for the lava stream, *that* was a pure fiction of an excited brain. I have looked carefully at the Peak through my telescope, and see nothing but the old, black lava streams that I have known for the last fifty years, and I have spoken with one of the guides who has been lately with a party to the summit, and he declares he saw no trace of any eruption, or of anything different from what he has always seen there."

Then follow some other topics to the end of the letter proper; but to that there is appended the following P.S., which may be interesting to intending travellers this summer:—

"Last night (May 27), about an hour and a quarter after midnight, we had a smart shock of an earthquake which woke me out of a sound sleep and rather frightened us all. However, no damage was done, but here people say that eruptions of the volcano are always preceded by earthquakes; so who knows but that our eccentric friend's vision of the three bonfires and the lava stream may come to be verified after all. If the Peak has any intention of erupting again, *I* should be personally obliged to it if it would do so while I am still in the body. It would be a grand sight from our Sitio."

To any of the previously mentioned intending visitors to the island I would beg to recommend that they carry Dr. Marcet's recent neat little book on "Southern and Swiss Health Resorts." His descriptions of Teneriffe, and especially of Guajara on the great crater, and Alta Vista on the high peak, are graphic, and true though terse. Indeed the only point of difference I have with him is his reason for there not being forthwith erected a grand hotel on the elevated Canadas, high above the summer cloud level, in the driest air, strongest sunshine, and most curative conditions for the moist kinds of consumptive disease, which the whole of this planet wot has to offer. The reason he gives is, that there is nothing to interest the invalids, or ordinary lady and gentleman travellers up there.

Yet there have long been mineralogy, geology, a peculiar, though scanty, botany, meteorology of a most commanding type, and astronomy under special advantages, inviting all the readers of NATURE to go there and participate in the mental feast; while now the probabilities each morning of witnessing from a distance a little real eruption, will add an exciting topic to the breakfast conversation and the noonday ramble.

C. PIAZZI SMYTH,
Astronomer-Royal for Scotland

15, Royal Terrace, Edinburgh, June 19

"Devil on Two Sticks"

WHY a game at once so graceful and attractive should have received such a christening I do not know, and I am equally at a loss to imagine how an outdoor sport like this, requiring skill and promoting a healthful exercise of the muscles, should have passed out of sight and become almost forgotten. Like Clerk Maxwell, I have played the game many a time some twenty years since, and hasten without further preliminary to describe it.

Imagine two cones joined together at their small ends like an hourglass, and that a solid of such a shape and size is turned out of walnut or cherry or boxwood or of ebony or ivory: this is the devil. Now take the last half-yard from the taper ends of two billiard-sticks, and let them be connected from these ends by a limp silken cord or string half a yard long: these are the sticks.

To play the game hold the thick ends of the sticks one in each hand, bring the cord under the narrow neck of the solid and try to elevate it in the air: it drops directly on the grass. If a brisk rotary motion is given to it however, it will not only remain on the cord, but several dexterous manœuvres may be accomplished with it: the variety of which and the skill displayed in performing them constitute the game. To produce the revolutions the sticks are moved rapidly up and down alternately, and when the spinning is once established, "Diabolus" may be allowed to run up and down the stick, or he may be projected high up in the air, and, still spinning all the while, be caught on the cord again and again in rapid succession. Two may be engaged in the same game. It was fashionable many years since in these parts, and I recollect seeing a picture in a Tunbridge-ware shop in this place of the lords and ladies engaged in playing this game on the Pantiles at Tunbridge Wells more than half a century ago.

JOHN GORHAM

Bordyke Lodge, Tunbridge, June 4

[This is a nearly complete answer to our correspondent's query. The behaviour of the "devil" is an excellent example of that property of the axis of greatest or least moment of inertia of a body which is utilised in a well-thrown quoit or an elongated rifle-bullet. The mode of producing the rotation is easy to learn by trial, but not very easy to describe. The sticks are kept moving so that one end of the cord is always at a greater tension than the other.—ED.]

Channel Ballooning

As I have shown in my pamphlet, "Les Grands Ascensions Maritimes," the British Channel is the proper field for trying maritime ascents and determining which are the best means for rendering balloons serviceable on sea as well as on land. I have had several conversations with Lhoste, my aëronautical pupil, on the circumstances of his audacious trip. I will confine myself to the scientific teaching of this expedition.

The cone anchor was lowered by Lhoste when he saw that the *Noemi* was running after the balloon, and diminished so much the velocity of the run that it was possible to catch him with a rowing boat. But although the wind was rather mild, the balloon inclined so much that the car was plunged into the water, and the waves drenched the occupant. It is evident that in stormy weather the lowering of the cone anchor would lead to the destruction of the balloon by pressure of the wind. To avoid this it is necessary to reserve the cone anchor for ordinary winds, and to use in other circumstances guide-ropes with wooden nuts passed through them in order to increase friction as much as possible.

The balloon went to the great altitude of 15,000 feet through the activity of the sun, which can be resisted very easily by taking some litres of water out of the sea, when nearing it, with a very simple apparatus that Lhoste has invented.

Any balloon attempting to cross the Channel should be bound to take on board half a dozen carrier pigeons to indicate the place where it has anchored, or has been rescued or landed, so that help could be sent to it without any delay. With such easy precautions, and the throwing out of a sufficient number of pilot balloons, the experiments can be conducted on scientific principles, and exert the most useful influence on the study of aërial currents on these seas, where several such currents combine, and where the constitution of the air is so peculiar that mirages of every description have been frequently seen even at the present season.

Lhoste, on the morning of June 9, saw at an altitude of 1200 feet a regular halo, which surrounded the sun. The fog was so heavy that he could not see the sea, except when it was almost ready to send him to a watery grave. The steam whistling from vessels in different seaports, reached him at every altitude; but he did not know what was the cause of the extraordinary noise.

If it had not been for the fog, Lhoste would have succeeded in a scientific sport which will become fashionable, and ultimately lead astronomy to try the air, and to trust to the winds in spite of Shakespeare.

Lhoste's audacity seems to have given rise to a competition in the Mediterranean from Marseilles, but I believe that the Mediterranean must only be tried when the British Channel has been traversed without difficulty in every direction, and that this part of the ocean will play an important part in the development of aërial navigation in the second century of its existence.

Boulogne, June 17 W. DE FONVIELLE

Geology of Cephalonia

CAN any of your readers kindly inform me whether geological investigations were ever made in the island of Cephalonia, one of the Ionian Islands that were under British protection up to the year 1863?

The following are the names of the fossils that I have been able to determine out of those that were given to me and brought last year from Climatzias, Thermanti, and Leacas, localities in the neighbourhood of Mount Cephalos, in the island of Cephalonia:—

Mitra fusiformis, Brocc.
M. Bronni, Mich.
M. Michelotii, Hörnes.
Buccinum costulatum, Brocc.
B. prismaticum, Brocc.
Chenopus pes pelecani, Phil.
Triton Turbellianum, Grat.
T. corrugatum, Lam.
Murex spinicosta, Bronn.
Murex ——
Murex ——
Fusus mitræformis, Brocc.
F. virgineus, Grat.
F. longirostris, Brocc.
Turbinella subreticulata, d'Orb.
Cerithium ——
Turritella turris, Bast.
Turritella ——
Turritella subangulata, Brocc.
Vermetus intortus, Lam.
Natica millepunctata, Lam.
Natica ——
Dentalium ——
Venus multilamella, Lam.
V. plicata, Gmel.
Chama gryphoides, Linn.
Cardita Jouanneti, Bast.
Pectunculus pilosus, Linn.
Limopsis calabra, Seguenza.

Bucuresti (Roumania), June 2 J. P. LICHERDOPOL

Lightning Phenomenon

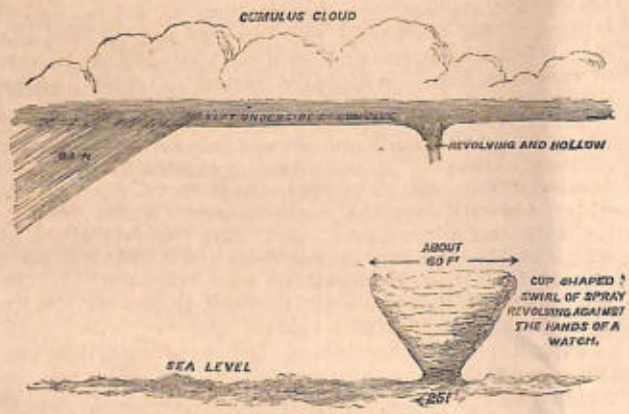
WHILE watching the incessant play of vivid lightning during the progress of a thunderstorm which was raging close by in the country towards Novara, Arona being just on the northern limit, my wife observed the following curious spectacle, the account of which she wrote down immediately afterwards:—At 9.35 p.m. on Sunday, June 3, a meteor-like object was seen to pass apparently from south to north (window facing due east), coming from the side of the storm and disappearing behind a mass of cloud which capped the high hill of Monte Val Grande above Lago Varese. It was oblately spheroid in form and apparently about the size of a fire-balloon, and with the velocity of a rocket was travelling slowly, for it left no visible track. It was of a bright, clear, whitish yellow, with a bright, pale green colour showing on the northern side when it passed behind the dark cloud. It was about three times as high above the horizon as the low hills opposite Arona, and traversed an angle of 45° horizontally from the point where first seen to its disappearance. The next day (June 4) when visiting friends at the Villa Frauzosine, near Tutra, we ascertained that this meteor-like body had also been seen by two or three persons who were sitting on a terrace watching the brilliant lightning to the south; they observed it moving also from south to north, disappearing behind the mountains to the northward. W. H. GODWIN-AUSTEN

Waterspout

ON April 28 last the Cunard steamship *Servia*, in making her outward voyage to New York, fell in with several small waterspouts. Being on deck at the time I made a rough sketch and some notes of the occurrence, which I now venture to send you, having learned that many officers of these steamers have sailed the North Atlantic for years without having witnessed any similar phenomenon there. The ship's position may be easily deduced from the fact that her latitude was 42° 24′ and longitude 51° 3′ at noon on the 27th, while these were 41° 42′ and 59° 53′ respectively at noon on the 28th, at 8.30 a.m. of which day we met the waterspouts. There was hardly any wind at the time, and

the sky, which had been generally overcast, was rapidly breaking up into masses of cumulus clouds separated by wide spaces of blue. About a dozen waterspouts were seen in all, the ship passing right through one of them and thus enabling me to estimate its diameter by direct comparison with the known beam of the *Servia*.

The swirls of spray rose from the sea in a cup-like shape, and revolving rapidly in a direction opposite to that of the hands of a watch. It was only after such a swirl had become well defined that the lower surface of the cumulus cloud above it began to descend as if to meet it, spinning at the same time. Indeed, so inconspicuous was this feature of the phenomenon that many of the passengers, intent on watching the spray-cups sparkling brightly in the sunshine, failed to notice it at all. In no case did the cloud swirl nearly meet the sea swirl, nor did the double-



funneled stem of whirling mist, so generally shown in books, appear. Some spray fell heavily on deck from the swirl through which the *Servia* passed, but the wind, which struck us at one moment on this, at the other on that, side of the face was not brisk enough to carry off any one's hat. The sight was remarkably beautiful whether closely or distantly viewed. In the one case the spray-cup seemed made of rustling jewels which sparkled in the bright sunshine; in the other, the sea horizon appeared as if here and there set with boiling and steaming caldrons, whose rope-like handles hung from the dark undersides of white billowy clouds. D. PIDGEON

Hartford, Mass., U.S.A., May 22

Meteors of June 3

THE large meteor seen by Mr. Hall and others (NATURE, vol. xxviii. pp. 126, 150) was also observed here by Mr. Paul Mathews and myself. We estimated the length of its path while visible as 120° with the middle part due east, the direction of its motion as parallel to the horizon, elevation as 20°, and length of tail as 25°; its apparent brilliance I put at six times, Mr. Mathews at twice, the greatest brilliance of Venus, and the pieces into which it broke up (about six in number) as equal to the brightest planets. The time I should have put at 10.50, but did not note it (Mr. Mathews 10.40 to 10.45). The colour was golden. This was moreover in a very clear and brilliant sky, as about 10 we had observed that the light in the east was so intense that it cast quite a dark shadow as we passed through a somewhat shady part of the road.

Ripon W. W. TAYLOR

IN the correspondence on the large meteor seen on June 3 I have not seen any notice of another curious meteor seen later on the same night. A flash of light in the sky drew attention to it, and when first seen it was moving in nearly a straight line from 102 Herculis to *a* Aquilæ. In five seconds it travelled slightly more than half the distance to the latter star, and then disappeared without any outburst. It was about a lunar diameter in length, and between 3′ and 4′ wide at the widest part, a point distant one-third of its entire length from the head. In fact it was not at all unlike a comet with a bushy tail tapering off to a point. The colour was a pale yellow. P. F. D.

London, W.

Intelligence in Animals

SOME years since, when calling on the late Hon. Marmaduke Maxwell of Terregles, our conversation happened to turn on the subject of intelligence and instinct of animals. Mr. Maxwell said if I would walk down to the stables with him he would show me a curious instance. On reaching the stable he pointed out an empty stall in which five well grown young rats were running about—a board had been fixed at the end of the stall to prevent the rats getting out. Some time before the cat had a litter of five kittens, *three* were taken from her and drowned; the following morning it was found she had brought in *three* young rats, which she suckled with the two kittens that had been left; a few days afterwards the *two* kittens were destroyed, and the next morning it was found the cat had brought in *two* more young rats. While we were looking at this strange foster family the cat came into the stable, jumped over the board and lay down, when the rats at once ran under her and commenced sucking. What makes the matter the more singular is, the coachman told me the cat was a particularly good ratter, and was kept in the stable for the purpose of keeping down rats.

Cargen, Dumfries P. DUDGEON

AMERICAN ETHNOLOGY[1]

UNDER the able management of Major J. W. Powell the Bureau of Ethnology, recently attached to the Smithsonian Institution, has already done much useful work in the wide field of American anthropology. This first annual report, however, of its proceedings for the year ending July, 1880, appears to be somewhat behind time for, although bearing on the title-page the date of 1881, it was not issued to the public till the beginning of the present year. But the delay is doubtless due to the large amount of preliminary work required to be got through in organising the department, and future reports may be expected to appear more punctually. The title, "Annual Report," is itself somewhat misleading, the actual report of the director really occupying no more than thirty-three introductory pages, and consisting mainly of a digest of the rich materials filling a large quarto volume of over 600 pages. Hence this is, strictly speaking, a first volume of the *Proceedings* or *Transactions* of the Bureau, and as such gives fair promise of a long and useful career in an anthropological domain which may be regarded as practically unlimited.

From the director's introductory remarks we gather that, after the fusion in 1879 of the various geological and geographical surveys in the general "United States Geological Survey," the Bureau of Ethnology was created and attached to the Smithsonian Institution for the purpose of continuing the anthropological work which had hitherto been prosecuted in a somewhat desultory way by those Surveys. The management of this newly-organised department was intrusted to Major Powell, who, as former Director of the Survey of the Rocky Mountain Region, had already shown special aptitude for ethnological investigation. The direct object of the Bureau, we are told, is to systematise anthropological research in America, and this it is proposed to effect both by the prosecution of research through the direct employment of students and specialists, and by the general encouragement and guidance of original observers co-operating throughout the continent. "It has been the effort of the Bureau to prosecute work in the various branches of North American anthropology on a systematic plan, so that every important field should be cultivated, limited only by the amount appropriated by Congress" (xiv.).

How closely this wide programme has been so far adhered to is evident from the varied contents of this

[1] "First Annual Report of the Bureau of Ethnology, Smithsonian Institution, 1879-80." By J. W. Powell, Director. (Washington Government Printing Office, 1881).

sumptuous volume, which comprises sundry contributions by the director on the "Mythology of the North American Indians," on the "Evolution of Language," on "Wyandot Government," and on "Limitations to the Use of some Anthropological Data"; a valuable and profusely illustrated treatise on the "Mortuary Customs of the North American Indians," by H. C. Yarrow; a preliminary attempt to decipher "Central American Picture Writing," by E. S. Holden; a paper by C. C. Royce on "Cessions of Land by Indian Tribes to the United States;" Col. Garrick Mallery's important treatise on "Sign Language among North American Indians," which has already appeared as a "Separat-Abdruck"; a "Catalogue of Linguistic MSS. in the Library of the Bureau of Ethnology," by J. C. Pilling; lastly, "Illustrations of the Method of Recording Indian Languages from the MSS. of Major J. O. Dorsey, A. S. Gatschet, and S. R. Riggs." Should the department continue to be administered on these broad lines and in this enlightened spirit, a school of anthropology must soon be developed in America, with which, without liberal State subvention, our European societies will find it difficult to keep pace. But with our petty rivalries, our heavy public burdens and constantly increasing armaments, the prospect of such State subvention seems at present at least somewhat remote.

The papers contributed by the director to this volume touch briefly on several important topics of a general character, and often express views regarding the origin and evolution of speech, mythologies, religious and tribal institutions, which will scarcely go unchallenged in some quarters. That these psychological phenomena have hitherto been studied from a somewhat too subjective standpoint, and that many metaphysical subtleties have consequently been grafted on the theogonies and early philosophies of savage man may readily be admitted. In a paper on the mythology of the Indian Aryans recently read before the English Folk-Lore Society, Mr. Andrew Lang dwelt on the necessity of distinguishing between the old and comparatively modern hymns in the Vedas. He pointed out that the Vedas themselves do not embody the most primitive theories on the origin of man and the universe, that they contain ideas at once very old and very new, very mythological and very philosophical, and he adduced several instances of crude and childish savage myths overlapping the more profound and advanced concepts of the Aryan Hindus. In the same way Major Powell argues that philosophy passes in its upward evolution through two stages—the mythologic, in which all outward phenomena are interpreted by analogy with subjective experience, and the scientific, in which they are treated as orderly successions of events. The mythologic necessarily precedes the scientific stage, for "without mythology there could be no science, as without childhood there could be no ultimate forms." It follows that the views of primitive men are simple, childish, and incoherent, and that it is illogical to credit his theogonies, as is often done, with profound and abstruse concepts of the universe. Here, as in all other evolutions, the progress is from the simple and homogeneous to the complex and heterogeneous; the "unknown known" of savage philosophy antedates the "known unknown" of later science. In the primitive stage all things are known, that is, supposed to be known; later on some few things are really discovered, and these when properly understood throw doubt on all the rest. The era of the known unknown is thus reached; to crude and offhand explanations succeeds the critical period of investigation and discovery; science is born; civilisation begins. This upward growth is illustrated by many examples, such as that of the rainbow—which for the Shoshoni (Snake Indian) is a beautiful serpent abrading the icy firmament to give us snow and rain; which in the Norse myth is the bridge Bifrost stretching from earth to heaven; which in the *Iliad* becomes the Goddess Iris, Messenger of Olympus; in Genesis a witness to the Covenant; in science an analysis of white light into its constituent colours.

North America, it is aptly remarked, presents a magnificent field for the study of savage and barbaric philosophies from this fresh standpoint. Formerly attention was paid almost exclusively to the more advanced peoples, Aryans, Semites, Hamites, Chinese. Now it is felt that the complex mythologic, religious, linguistic systems of these peoples are the outcome of earlier and simpler phases of thought, consequently that the study of barbarous and savage communities can no longer be neglected. But in North America alone we have our seventy-five ethnical groups speaking seventy-five stock languages and more than five hundred well-marked dialects, each linguistic stock with a philosophy of its own, or rather as many philosophic systems as it has distinct languages and dialects.

To account for this astounding diversity of speech, Major Powell holds with one or two distinguished European philologists that the fundamental languages must have been evolved in independent centres, that in fact "mankind was widely scattered over the earth anterior to the development of articulate speech, and that the languages of which we are cognisant sprang from innumerable centres as each little tribe developed its own language" (p. 28). He fails to see that this view, in itself to the last degree improbable, is wholly unnecessary and even inadequate to explain the actual conditions. It is unnecessary because the present diversity of speech may be sufficiently accounted for by its vast antiquity and extremely evanescent character. Time, acting in combination with the phonetic growth and decay inherent in all speech, must inevitably effect an indefinite amount of specific change, even supposing that all languages started from a single centre. No evolutionist can deny this, for he admits that time combined with a tendency to modification in altered environments, has brought about an indefinite amount of specific and generic change in the biological world. But animals and vegetables are certainly more persistent, *cæteris paribus*, than linguistic types. *Ergo*. The theory is moreover inadequate to explain the actual conditions in America alone. Here we have doubtless a vast number of specifically distinct languages; but the mechanism of all is very much alike; all are cast, as it were, in the same mould; all belong to the polysynthetic or at least to the agglutinating order. But if speech had in America been evolved in many different centres, it may be asked how this striking uniformity is to be explained? Why have we not here, as elsewhere, representatives of the isolating[1] and inflecting, as well as of the polysynthetic order of speech? Does not their common structure point at a common centre of dispersion, while their specific diversity within this common groove is amply explained by time and evanescence?

But if Major Powell does not always reason conclusively, he is a good observer, and describes in vivid language the scenes of savage life of which he has been a spectator, as witness the subjoined account of oral narrative in the Indian community:—

"On winter nights the Indians gather about the camp-fire, and then the doings of the gods are recounted in many a mythic tale. I have heard the venerable and impassioned orator on the camp-meeting stand rehearse the story of the crucifixion, and have seen the thousands gathered there weep in contemplation of the story of divine suffering, and heard their shouts roll down the forest aisles as they gave vent to their joy at the contemplation of redemption. But the scene was not a whit

[1] The Othomi of the Anahuac tableland has been cited as an instance of an isolating language in America. But M. de Charancey rightly regards Othomi rather as "une langue primitivement incorporante [polysynthetic], qui, parvenue au dernier degré d'usure et de délabrement, a fini par prendre les allures d'un dialecte à juxtaposition [isolation]" ("Mélanges de Philologie," &c., p. 80, Paris, 1883).

more dramatic than another I have witnessed in an evergreen forest of the Rocky Mountain region, where a tribe was gathered under the great pines, and the temple of light from the blazing fire was walled by the darkness of midnight, and in the midst of the temple stood the wise old man, telling in simple, savage language the story of *Ta-wâts*, when he conquered the sun and established the seasons and the days. In that pre-Columbian time, before the advent of white men, all the Indian tribes of North America gathered on winter nights by the shores of the seas, where the tides beat in solemn rhythm, by the shores of the great lakes, where the waves dashed against frozen beaches, and by the banks of the rivers flowing ever in solemn mystery—each in its own temple of illumined space—and listened to the story of its own supreme gods, the ancients of time" (p. 40).

A detailed notice of the other more important papers in this volume must be reserved for a future occasion.

A. H. KEANE

THE FISHERIES EXHIBITION

WE are gratified to see the very thorough way in which the management of the Fisheries Exhibition are endeavouring to carry out their plans. It is evident that the scientific aspects of the wide and important subject will have a fair amount of attention; and we are glad to think that in this direction advice has been sought in the right quarter. In the Exhibition itself those interested in the science of the subject will find much to attract them. Last week (p. 156 we gave a list of subjects which have been settled for conferences, and among those who have consented to read papers, we find such names as Professor Huxley on Fish Diseases, Professor Ray Lankester on the Scientific Results of the Exhibition, Professor Brown Goode on the Fisheries of the United States, Professor Hubrecht on Oyster Culture and Fisheries, Sir Henry Thompson on Fish as Food, Dr. F. Day on the Food of Fishes, Mr. R. H. Scott on Storm Warnings. Further, we are glad to see that a series of handbooks has been arranged for on subjects cognate to the Exhibition. Among them are a few by men of scientific standing, and likely to be of real scientific importance; we hope it may not yet be too late to secure the preparation of a few more handbooks or reports of a similar character. Among the handbooks arranged for, six will be published this month, and the remainder in July. Those of special interest to science are, "The Life History of Fishes," by Prof. H. N. Moseley; "Fish Culture" and "Indian Fish and Fishing," by Dr. Francis Day; "Food Fishes," by Mr. G. B. Howes; "Marine and Freshwater Fishes of the British Isles," by Mr. Saville Kent; "Curious Sea Creatures," by Mr. Henry Lee.

The conferences were introduced on Monday by an interesting lecture by Prof. Huxley, a report of which we give below, and this was followed on Tuesday by a carefully prepared paper by the Duke of Edinburgh, on British Fisheries and Fishermen, read by the Prince of Wales. The real interest which the leading members of the Royal family take in the Exhibition has no doubt done much to contribute to its success. It was to be expected that the German Ambassador would show his appreciation of the importance of science to an industry of such magnitude as that of fishing, and he aptly pointed out how important was the didactic and scientific work at last commenced.

With the general concurrence of opinion in high quarters as to the value of the scientific aspects of the Exhibition, and of the great services which science may render in bringing about the practical objects which are aimed at, we of course heartily concur. It is admitted on all hands that the haphazard way in which our fisheries have hitherto been carried on has led to the worst results, the extinction almost of some important fishes and mollusks, the bad condition of others, and the dearness of what might be the cheapest and most plentiful of foods. In recent years science has done something to remedy this state of things, and it will be well for our fisheries, and therefore for the welfare of a large portion of our population, if the Fisheries Exhibition leads to still more being done in this direction. So far the Exhibition has been an immense success; half a million of people have already visited it, and thus the educational results are likely to be widespread.

Prof. Huxley, in opening the proceedings, said:—

It is doubtful whether any branch of industry can lay claim to greater antiquity than that of fishery. The origin would seem to be coeval with the earliest efforts of human ingenuity; for the oldest monuments of antiquity show us the fisherman in full possession of the implements of his calling; and even those tribes of savages who have reached neither the pastoral nor the agricultural stages of civilisation are skilled in the fabrication and in the use of the hook, the fish-spear, and the net. Nor is it easy to exaggerate the influence which the industry thus early practised and brought to a considerable degree of perfection has directly and indirectly exerted upon the destinies of mankind, and especially upon those of the nations of Europe. In our quarter of the globe, at any rate, fishery has been the foster-mother of navigation and commerce, the disseminator of the germs of civilisation. Having glanced at the development of the industries connected with fishing, more especially by the Phœnicians, he continued:—These few remarks must suffice to indicate the wide field of interesting research which fisheries offer to the philosophical historian, and I pass on to speak of the fisheries from the point of view of our present practical interests. The supply of food is, in the long run, the chief of these interests. Every nation has its anxiety on this score, but the question presses most heavily on those who, like ourselves, are constantly and rapidly adding to the population of a limited area, and who require more food than that area can possibly supply. Under these circumstances, it is satisfactory to reflect that the sea which shuts us in at the same time opens up to us supplies of food of almost unlimited extent. In reference to the relation which the fisheries bore to the total supply of food of those who had easy access to the sea, he quoted the following paragraph from the Report of the Fisheries Commissioners, 1866:—"The produce of the sea around our coasts bears a far higher proportion to that of the land than is generally imagined. The most frequented fishing-grounds are much more prolific of food than the same extent of the richest land. Once in a year an acre of good land, carefully tilled, produces a ton of corn or two or three hundredweight of meat or cheese. The same area at the bottom of the sea in the best fishing-grounds yields a greater weight of food to the persevering fisherman every week in the year. Five vessels belonging to the same source in a single night's fishing brought in seventeen tons' weight of fish, an amount of wholesome food equal in weight to that of fifty cattle or 300 sheep. The ground which these vessels covered during the night's fishing could not have exceeded an area of fifty acres." My colleagues and I made this statement a good many years ago. I have recently tried to discover what yield may be expected, not from the best natural fishing-grounds, but from piscicultural operations. At Comacchio, close to the embouchure of the Po in the Adriatic, there is a great shallow lagoon which covers some 70,000 acres, and in which pisciculture has been practised in a very ingenious manner for many centuries. The fish cultivated are eels, gray mullet, atherines, and soles; and, according to the figures given by M. Coste, the average yield for the sixteen years from 1798 to 1813 amounted to 5 cwt. per acre—that is to say, double the weight of cheese or meat which could have been obtained from the same area of good pasture land in the same time. Thus the seas around us are not only important sources of food, but they may be made still more important by the artificial development of their resources. But this Exhibition has brought another possibility within the range of practically interesting questions. A short time ago a visitor to the market might have seen fresh trout from New Zealand lying side by side with fresh salmon from Scandinavia and from the lakes and rivers of North America. Steam and refrigerating apparatus combined have made it possible for us to draw upon the whole world for our supplies of fresh fish. In my boyhood "Newcastle" was the furthest source of the

salmon cried about the streets of London, and that was generally pickled. My son, or at any rate my grandson, whenever he goes to buy fish, may be offered his choice between a fresh salmon from Ontario and another from Tasmania. The fishing industry being thus important and thus ancient, it is singular that it can hardly be said to have kept pace with the rapid improvement of almost every other branch of industrial occupation in modern times. If we contrast the progress of fishery with that of agriculture, for example, the comparison is not favourable to fishery. Within the last quarter of a century, or somewhat more, agriculture has been completely revolutionised, partly by scientific investigations into the conditions under which domestic animals and cultivated plants thrive, and partly by the application of mechanical contrivances and of steam as a motive power to agricultural processes. The same causes have produced such changes as have taken place in fishery, but progress has been much slower. It is now somewhat more than twenty years since I was first called upon to interest myself especially in the sea fisheries. And my astonishment was great when I discovered that the practical fisherman, as a rule, knew nothing whatever about fish, except the way to catch them. In answer to questions relating to the habits, the food, and the mode of propagation of fish—points, be it observed, of fundamental importance in any attempt to regulate fishing rationally—I usually met with vague and often absurd guesses in the place of positive knowledge. The Royal Commission, of which I was a member in 1864 and 1865, was issued chiefly on account of the allegation by the line fishermen that the trawlers destroy the spawn of the white fish—cod, haddock, whiting, and the like. But, in point of fact, the spawn which was produced in support of this allegation consisted of all sorts of soft marine organisms except fish. And if the men of practice had then known what the men of science have since discovered, that the eggs of cod, haddock, and plaice float at the top of the sea, they would have spared themselves and their fellow-fishermen, the trawlers, a great deal of unnecessary trouble and irritation. Thanks to the labours of Sars in the Scandinavian seas, of the German Fishery Commission in the Baltic and North Seas, and of the United States Fishery Commission in American waters, we now possess a great deal of accurate information about several of the most important of the food fishes, and the foundations of a scientific knowledge of the fisheries have been laid. But we are still very far behind scientific agriculture, and, as to the application of machinery and of steam to fishery operations, in this country at any rate, a commencement has been made, but hardly more. The relative backwardness of the fishing industry made a great impression on my colleagues and myself in the course of the inquiries of the Royal Commission to which I have referred; and I beg permission to quote some remarks on this subject which are to be found in our Report issued in 1866:—"When we consider the amount of care which has been bestowed on the improvement of agriculture, the national societies which are established for promoting it, and the scientific knowledge and engineering skill which have been enlisted in its aid, it seems strange that the sea fisheries have hitherto attracted so little of the public attention. There are few means of enterprise that present better chances of profit than our sea fisheries, and no object of greater utility could be named than the development of enterprise, skill, and mechanical ingenuity which might be elicited by the periodical exhibitions and publications of an influential society specially devoted to the British fisheries." Taking this Exhibition, the third of its kind, as evidence that the public attention to fisheries for which they hoped had been attained, he remarked that the conference opened that day formed an entirely new feature of such exhibitions, and expressed a hope that there was in them a germ of that which, by due process of evolution, might become a great society, having for its object the welfare and the development of the fisheries of these islands. He presently turned to the question whether fisheries are exhaustible; and, if so, whether anything can be done to prevent their exhaustion. He did not think it possible to give a categorical answer. There were fisheries and fisheries; but he had no doubt that there were some fisheries which were exhaustible. Instancing the salmon rivers, he said it was quite clear that those who would protect the fish must address themselves to man, who was reachable by force of law; and that it not only might be possible, but it was actually practicable, to so regulate the action of man with regard to a salmon river that no such process of extirpation should take place. But if we turned to the great sea fisheries, such as cod and herring fisheries, the case was entirely altered. Those who have watched these fisheries off the Lofoden Isles on the coast of Norway, say that the coming in of the cod in January and February is one of the most wonderful sights in the world; that the cod form what is called a "cod mountain," which may occupy a vertical height of from 20 to 30 fathoms—that is to say, 120 to 130 feet, in the sea; and that these shoals of enormous extent keep on coming in in great numbers from the westward and southward for a period of something like two months. The number of these fish is so prodigious that Prof. Sars, the most admirable authority, from whom I quote these details, tells us that when the fishermen let down their loaded lines, they feel the weight knocking against the bodies of the codfish for a long time before it gets to the bottom. I have made a computation, with the details of which I will not trouble you, which leads to this result, that if you allow the fish each of them four feet in length, and let them be a yard apart, there will be in a square mile of such shoals something like 120 million fish. I believe I am greatly underestimating the actual number, for I believe the fish lie much closer; but I would beg your attention to the bearing of this underestimate, because I do not know that the Lofoden fishery has ever yielded more than 30 million fish in a good season; and so far as I am aware the whole of the Norwegian fisheries, great as they are, do not yield more than 70 millions. So you will observe that one of these multitudinous shoals would be sufficient to supply all the fisheries of Norway completely, and to leave a large balance behind. And that is not all. These facts about the cod apply also to the herring; for not only Prof. Sars, but all observers who are familiar with the life of the cod when it has attained a considerable size, tell us that the main food of the cod is the herring, so that these 120 million of cod in the square mile have to be fed with herring, and it is easy to see, if you allow them only one herring a day, that the quantity of herring which they will want in the course of a week will be something like 840 million. Now I believe the whole Norwegian herring fishery has never reached the figure of 400 million fish—that is to say, one half the fish which this great shoal of codfish eats in a week would supply the whole of the Norwegian fisheries. On these and other grounds it seemed to him that this class of fisheries—cod, herring, pilchard, mackerel, &c.—might be regarded as inexhaustible. But he should not venture to say this of the whole of the sea fisheries—of the oyster fisheries, for example. Here, again, the operations of man become exceedingly important. Regarding the regulation as to close time for oysters as alone absolutely futile for the purpose of protection, he urged that the more logical provisions of government supervision in Denmark, France, and elsewhere, were impracticable of application beyond the three-mile limit of this country. It was under this conviction that the Commission to which he referred recommended the abolition of all restrictive measures. In conclusion, he pointed out how heavily this question bore on the social condition of the fisherman. Every act of legislation with regard to the fisherman created a new offence. If the common welfare and the common interest, said Prof. Huxley, can be clearly shown to render such regulations desirable or necessary, then of course fishermen must put up with this as they put up with anything else—as we all put up with such restrictions. But supposing that no good case is made out, supposing that regulations of this kind are made on insufficient inquiry and based on insufficient understanding of the circumstances of the case, then I am free to confess that I think those who make such laws deserve very much severer penalties than those who break them.

THE SCIENTIFIC WORK OF THE "VEGA"[1]

THE volume we have before us—the first of a series—contains the results of the scientific observations made during the cruise of the *Vega* and to say this is obviously to indicate that it contains a rich supply of most valuable information as to that part of the Arctic Ocean which extends along the coasts of Siberia, which appears in the shape of a series of elaborate papers on different departments of natural history of the Arctic regions. Several parts of this volume are already known. Such are the "Reports to Dr. Oscar Dickson" written by Baron Nordenskjöld during the expedition, and read throughout the civilised

[1] "*Vega*-Expeditionens Vetenskapliga Iakttagelser, bearbetade af deltagare i resan och andra forskare, utgifna af A. E. Nordenskjöld" Vol. i. Part 1. (Stockholm, 1882.)

world as soon as they reached Europe; or his paper on the possibility of navigation in the Siberian Arctic Sea; or, again, his paper on auroræ (recently summarised in the pages of NATURE). The well-known "Reports" appear as they were written on board the *Vega*, but with a map "of the northern coast of the old continent, from Norway to Bering Strait," and with several maps of separate islands and bays. The other papers are quite new, and we find in this volume a medical report, by E. Almquist, on the health of the crew; a paper by A. Lindhagen, on the determinations of latitudes and longitudes, which will put an end to the discussions as to the accuracy of the changes made in the map of the northern coast of Siberia by the astronomers of the *Vega;* a paper by H. H. Hildebrandson (in French), on the meteorological work of the expedition, being a comparison of the climate of Pitlekaj (the *Vega's* wintering station) with the climate of other Arctic stations; several papers on the Chukches; an elaborate paper by A. Stuxberg, on the invertebrate fauna of the Siberian Arctic Sea; and a series of papers by F. R. Kjellman, A. N. Lundström, and E. Almquist, on the vegetation of the region visited.

It is known that the expedition of the *Vega* was—as is, however, always the case with so experienced a traveller as Nordenskjöld, and as it was with Parry—one of the most successful Arctic expeditions with regard to the health of the crew. No death was to be regretted, and all the thirty men of the crew reached Naples in the best condition. It is true that at Pitlekai the sun did not disappear for months under the horizon, and that the crew were not worn out by long sledge journeys. But still the climatic conditions were not at all favourable, on account of the variability of the weather and strong winds which blew with a twenty miles' speed even during frosts $-30°$ strong. The precautions taken for maintaining a temperature as equal as possible in the cabins and for eliminating moisture, as well as for much exercise in the fresh air and the maintenance of cheerfulness among the crew, are not to be underrated. A daily distribution of lime-juice and of jam surely had also their importance as preservatives against scurvy, and this the more as the crew of the *Vega* had no opportunities of having supplies of fresh meat during the winter. The daily baking of fresh bread was a very good innovation; as to the preserved meat, the crew very soon had enough of it, and even salted meat was preferred to corned beef; only the finer and more expensive preserved soups and steaks were appreciated throughout the cruise.

The Chukches were of course the subject of anthropological and ethnographical studies, as far as possible. Mr. Nordquist publishes a most valuable Chukch dictionary, and Mr. Almquist communicates interesting observations on colour-blindness among the Chukches, two hundred men and a hundred women having been submitted to experiments in accordance with Prof. Holmgren's hints as to this kind of research with people whose language is unknown ("Om färgblindheten," &c., Upsala, 1877). The supposition of Helmholtz and Young as to the blindness of the lower races with regard to violet rays has not been confirmed as far as the Chukches are concerned; they certainly distinguish it, but they merely call it red. The same is true with regard to pale green and bright blue; they confound both, but the organs for green are not missing with them. Like other lower races, they use much red colour for their skins, yellow being comparatively rarely used for ornaments. It results also from M. Kjellman's paper on the culinary plants of the Chukches, that, contrary to Wrangel's assertion, they do not despise vegetable food. Their provisions of plants for the winter are as large as their provisions of meat and fat. This feature so much distinguishes them from all other inhabitants of the Arctic regions that one is inclined to admit that the time is not far removed from that when they cultivated some better situated plots of soil on the coast of the Arctic Ocean, or were compelled to leave lower latitudes which had a happier climate than that of the north-eastern extremity of Siberia. Their provisions in vegetables are a very strange mixture of various plants, among which the following are the most common:—*Cinergria palustris*, L., *Petasites frigida*, Fr., *Pedicularis sudetica* and *P. lanata*, Willd., *Rhodiola rosea*, L., *Claytonia acutifolia*, Willd., *Halianthus peploides*, Fr., *Polygonum polymorphum*, L., and *Salix boganidensis*, Trautvetter.

The most important papers in this volume are those devoted to the vegetation of the region visited, that is, to the lichens and mosses of the coast, to the algæ of the Siberian Sea, and to the phanerogamic flora of Novaya Zemlya, of the coast region, and of the Asiatic coast of Bering Strait. The lichens are comparatively rare on the coast; whole stretches are quite devoid of them, and the lichen flora is altogether poor as to the number of species. The Calicieæ are represented by but three species, and the whole group of the Sclerolichens only by five or six species, none of them being spread over the whole of the coast. The Stictaceæ are represented by a couple of species of *Nephroma*; the Pannariaceæ by five to six species; *Pyrenopsis* has but one species which is widely spread, but not very common. The character of the flora is nearly the same on the whole of the coast, but towards the south, where the country becomes inhabited, the flora undergoes a notable change. The Phanerogams become also comparatively rich towards the east, in the land of the Chukches, where the grasses appear in the shape of whole sods, without a mixture of moss. The Algæ are few in the Siberian Sea, the whole number of observed species being but twelve, that is, only one-half of the number of species that are known on the Murman coast and in the Sea of Spitzbergen. The characteristic feature of all Arctic Algæ being their large size, the Siberian Algæ seem to be an exception to this rule. The largest of them was a *Laminaria Agkardii*, 210 centimetres long and 37 cm. wide.

The papers by MM. Kjellman and Lundström on the Phanerogams of the explored region will be read with great interest both by the botanist and the geographer. They are not bare enumerations of plants, but elaborate sketches of botanical geography taking into account former botanical work in neighbouring tracts, and describing the flora in its dependence upon local conditions of climate and soil. The coast-flora of Northern Siberia is altogether poor, as it numbers but 150 species of Phanerogams; this number slightly increases, however, towards the east, where it reaches 221, as well as towards the west, 185 species being known from Novaya Zemlya. The Obi—at least as far as the coast region proper is concerned—is not a separation-line between the Arctic European and the Arctic Asiatic floras, as was expected by Hooker. Only the *Salix rotundifolia* and *Wahlbergella affinis* do not appear to the east of the great West Siberian river. Of the 150 species noticed, only one-third are Monocotyledons. This proportion increases, however, at certain places, and there are monocotyledonous species extending over large areas. However poor as to the number of species, the Siberian coast-flora still affords a variety of forms, as it has representatives of 33 different families and 93 different genera. The families which are the most represented in the coast-flora are those of Gramineæ and Cruciferæ, which number respectively 23 and 20 species. They are followed by those of the Compositæ, Saxifragaceæ, Ranunculaceæ, Cyperaceæ, and Caryophyllaceæ (15 to 11 species). The family of Saxifragaceæ is that which maintains the greatest number of species towards the north, eight species out of fourteen having been found even at Cape Chelyuskin; the Caryophyllaceæ nearly keep pace with the former; whilst the family which spreads least towards the north is that of Compositæ

which was represented only by two species at the mouth of the Taimyr and none at Cape Chelyuskin. So also with the Cyperaceæ and Ranunculaceæ. *Saxifraga oppositifolia* is not the most widely-spread species on the Siberian northern coast, as is the case for other parts of the Arctic region, other species of Saxifraga being as much or more extensively spread than it. The most usual phanerogamous plants on the coast seem to be the *Luzula arcuata*, var. *hyperborea*, and *Stellaria longipes*.

We shall not analyse the valuable paper by M. Kjellman on the flora of Novaya Zemlya, which is a summary of all that is known on this subject, and we shall notice but a few facts concerning the vegetation of the Siberian coast of Bering Strait. It is represented on M. Kjellman's lists by 221 species belonging to 41 families and 109 genera. The Compositæ, Cyperaceæ, Saxifragaceæ, Caryophyllaceæ, and Gramineæ, numbering from 20 to 15 species each, are here also the richest as to the number of species. But we find on the Asiatic coast of the Bering Strait a good many plants belonging to the American flora, as also to the flora of the Altai and Baikal regions, which are not met with elsewhere on the northern coast. No less than 53 species out of 221 appear only to the east of the Kolyma, which appears thus to be, for the coast-region, a more important boundary line than the Obi. This notable increase cannot be accounted for only by the milder character of this region, but it could be explained, in our opinion, if we took notice of the orography of Eastern Siberia, which favours, by the extension of its chains of mountains from south-west to north-east, the spread of both animals and plants in the same direction.

Dredging was very diligently carried on during the whole of the cruise of the *Vega* in the Arctic Ocean; and Mr. Stuxberg's map of dredgings made during the Swedish expeditions of 1875, 1876, and 1878 to 1879, is dotted with 33 spots in the Kara Sea, and with 90 spots along the Siberian coast to Bering Strait. The temperature of water obviously was found to be very low; even at a few fathoms below the surface it was from − 0·9° to − 2·3° at a depth of 50 metres, and it had a normal specific gravity of 1·027. The most uniformly spread animals in the Siberian coast-region of the Arctic Ocean, and in the Kara Sea, are undoubtedly the Crustaceans; the Echinoderms are comparatively few, as also are the Mollusca, Bryozoa, and Hydroids. The Crustaceans *Idothea Sabinei*, *Idothea entomon*, *Diastylis Rathkei*, *Atylus carinatus*, and *Acanthostephia Malmgreni*, are the most usual. The first, as known, has been found nearly everywhere in the Arctic Ocean; whilst the second proved to be specific for the whole of the Arctic coast of the old continent, for a stretch of nearly 160 degrees of longitude; it has been found also in the lakes of Sweden and Northern Russia, even in the Caspian and Lake Aral—Lake Baikal being till now the sole explored great lake of this part of the old continent where it has not yet been found. As to the vertical distribution of the animal forms, no distinct regions can be established. It must be observed, however, that the littoral region—about 30 fathoms deep—on account of its ice and sweet water brought by rivers, is nearly quite devoid of animals; even the littoral forms go to take refuge in the sublittoral region. Not only is the Siberian Sea very rich in forms of animals (the number of described Amphipods being as much as 59 out of 114 Amphipods known in all Arctic seas together); it contains also such a number of individuals of certain species, that Mr. Stuxberg describes about 20 real "formations" (*djurformationen*), each consisting of very large quantities of individuals of one given species, with a comparatively small mixture of other species. Such are the *Diastylis Rathkei*, *Reticulipora intricaria*, *Alcyonidium mammillatum*, *Chiridota lævis*, *Echinus dröbachiensis*, *Asterias Lincki*, *Archaster tenuisbinus*, *Ctenodiscus crispatus*, *Ophiacantha bidentata*, *Ophiocten sericeum*, *Ophioglypha nodosa*, *Astrophyton eucnemis*, *Antedon Eschrichti*, *Yoldia arctica*, and *Idothea entomon*, as also Ascidiæ, Actiniæ, and Hydroids. As a whole, the Siberian basin differs very much in its fauna from the other parts of the Arctic basin, and it has no less than 16 species that are characteristic of it. Novaya Zemlya is the limit of the fauna of the Siberian Sea, being a separation-line for many species.

The foregoing notice will give a general idea of the valuable material contained in the first volume of the "*Vega's* Scientific Work," and the manner it is treated. We have but to express the wish to see, as soon as possible, the appearance of the following volumes of this series. They will surely give a new and powerful impulse to the study of Northern Siberia. P. K.

NOTES

We are glad to learn that Mr. Spottiswoode continues to go on favourably.

Still another well-deserved honour for Sir Joseph Hooker. The Society of Arts' Albert Medal for "distinguished merit for promoting arts, manufactures, or commerce," has been awarded to him for the present year, for the eminent services which, as a botanist and scientific traveller, and as Director of the National Botanic Department, he has rendered to the arts, manufactures, and commerce by promoting an accurate knowledge of the flora and economic vegetable products of the several colonies and dependencies of the Empire.

Among those to whom the Council of the Society of Arts have awarded their silver medals are Mr. Alex. Siemens and Dr. Hopkinson, for their papers on "The Transmission of Power by Electricity," and "The Portrush Railway," and to Capt. Douglas Galton for his paper on "The Economy of Sanitation." Thanks were voted to Mr. W. H. Preece, F.R.S., for his paper on Electrical Exhibitions.

A special extra meeting of the Anthropological Institute was held at the Piccadilly Hall on Tuesday, when the Botocudo Indians and a large ethnological collection from Brazil were exhibited by the kindness of Mr. C. Ribeiro, and Prof. A. H. Keane read a paper descriptive of the Botocudos.

Mr. Mark H. Judge has resigned his position as Secretary and Curator of the Parkes Museum.

M. de Lesseps has declared officially at the Academy of Sciences the intention of the Suez Company to open a new canal. During the works the maritime way will be lighted by electricity, and an appeal has been addressed to physicists to present their several systems. The work will begin as soon as possible.

We have received a favourable report of the National Museum, Bloemfontein, Orange Free State. Considerable collections are being brought together, but the committee should not forget that the chief object of such a museum ought to be to make its collections mainly representative of the interesting country in which it is placed.

Dr. Kerr of Canton is publishing in Chinese a complete work on the theory and practice of medicine, compiled from European standard works upon that subject. The sections on fevers, and diseases of the stomach, have already been published, while those on affections of the heart and lungs have just been issued. Volumes on the kidneys and nervous system are in the press. The translator has omitted the discussion of all unsettled theories and disputed points. The volumes are printed from wooden blocks, clearly and evenly cut, and are sold at a price which brings them within the reach of all.

Mr. J. W. Taylor of Leeds, who is the editor of the *Journal of Conchology*, has issued a prospectus for a "Monograph of the Land and Freshwater Mollusca of the British Fauna," and he invites the assistance of conchologists towards his proposed undertaking. According to the prospectus the work will be very comprehensive, and will include the subjects of variation, geographical and local distribution, synonomy and bibliography, "biological aspect and relation to environment." It would be desirable to add distribution in point of time or the palæontological aspect. Mr. Taylor has given specimens of the work in some of the lately published numbers of his *Journal of Conchology*, and they seem to be carefully and almost exhaustively done. We hope the cost of this work will place it within the means of the numerous and comparatively poor conchologists in the north of England, as so many manuals on the subject have already been published at very moderate prices. Great service would likewise be done to natural history by reducing the excessive number of so-called species fabricated during the last twenty years by some Continental conchologists. The judicious remark made by Hooker and Thomson in the introduction to their "Flora Indica" ought always to be borne in mind, viz. that "the discovery of a form uniting two others previously thought distinct, is much more important than that of a totally new species, inasmuch as the correction of an error is a greater boon to science than a step in advance."

The *Union Médicale* of June 2 announces a discovery of the highest scientific interest, and which, if it turns out to be real, will show that prehistoric man is no longer a myth. On piercing a new gallery in a coal-mine at Bully-Grenay (Pas-de-Calais), a cavern was broken into containing six fossil human bodies intact—a man, two women, and three children—as well as the remains of arms and utensils in petrified wood and stone, and numerous fragments of mammals and fish. A second subterranean cave contained eleven bodies of large dimensions, several animals, and a great number of various objects, together with precious stones. The walls were decorated with designs of combats between men and animals of gigantic size. A third and still larger chamber appeared to be empty, but could not be entered in consequence of the carbonic acid it contained, which is being removed by ventilators. The fossil bodies have been brought up to the surface, and five of them will be exhibited at the *mairie* of Lens; the others are to be sent to Lille in order to undergo examination by the Faculté des Sciences. Representatives of the Académie des Sciences of Paris and of the British Museum having been telegraphed for, are expected to be present.

The Lords of the Committee of Council on Education have sanctioned the addition of hygiene to the list of sciences in which grants are made by the Department. A syllabus has been prepared, and will shortly be issued to science schools and classes.

From the third Annual Report of the Hampstead Naturalists' Club we are glad to see that the society is in a prosperous condition, and is gradually getting together a useful museum.

G. P. Putnam's Sons of New York have published a nicely got up and profusely illustrated Guide to the Yellowstone National Park, by Mr. H. J. Winser, which those proceeding to the States for their holiday would do well to get.

Next month Messrs. Williams and Norgate will publish a new work, entitled "The Natural Genesis," in two volumes, by Mr. Gerald Massey, containing the Natural Genesis and Typology of Primitive Customs; Gesture-signs, Ideographs, and Primordial Onomatopœia; Time and Numbers; the Serpent, Dragon, and other Elementaries; the Tree, Cross, and Four Corners; the Great Mother, Twins, Triads, and Trinity; the Mythical Creations; the Fall in Heaven and on Earth; the Deluges and Ark; and Equinoctial Christology.

A writer in a recent number of the *North China Herald*, referring to fossils in China, remarks that the Chinese have never advanced a theory to explain their existence. In their books references are made to fossil shells, crabs, fish, trees, &c., but no attempt is made to account for their occurrence in solid rock. The little that is said is mostly of the marvellous sort. Ammonites are petrified snakes; fossil brachiopods (lamp-shells) are called "stone swallows," and are said to come to life and fly from their hiding places at the approach of wind and rain, changing again to stones on the return of fair weather. Fossil fish appear and disappear at pleasure, and their appearance prognosticates a plentiful harvest and prosperous times. One author supposes that the figures of birds, beasts, and plants, which he had seen on certain slabs, must be the work of gods or devils, for no human hand could chisel anything so minute and delicate.

Crackers play a large part in the superstitious observances of the ordinary Chinese. It is a popular belief that the evil spirits everywhere inhabiting the air are dispersed by crackling noises, attended by fire and smoke. Accordingly crackers are used on all special occasions to frighten away the demons who are tormenting a sick person, or who crowd around the people at the beginning of the New Year. Bamboo, which when burning emits a crackling sound, is also used for the same purpose.

We have received the Report of the West Kent Natural History, Microscopical, and Photographic Society for 1882-83. It appears to be more bulky than its predecessors, extending to 68 pp. 8vo. The President (Dr. F. T. Taylor) discourses on Bacteria and Vivisection; Mr. J. Glaisher, F.R.S., gives a very instructive paper on the extraordinary meteorological conditions between October 1881 and May 1882, illustrated by two diagrams indicating the mean daily barometric and thermometric readings, and their departure from the mean, as observed at Blackheath; Mr. J. Jenner Weir, F.L.S., discusses on the types of variation in Lepidoptera, in which is embodied much useful information; Mr. Stone alludes to certain points in the economy of wasps; Mr. Heisch's notes on "Adulteration" are of practical interest. In their next Report this old-established Society may perhaps think it advisable to give a tabular indication of the "contents"; the same remark would apply equally to the publications of other local societies.

Under the title of "Lantern Readings" Mr. Lant Carpenter has issued a pamphlet to be used (when necessary) with the first series of the biological lantern slides which we referred to in a recent number. These slides are now ready, and may be obtained from York and Son. The pamphlet and slides are intended to illustrate the results of the voyage of the *Challenger*. There are descriptions of forty-two slides in all, and "preliminary hints" show how the pamphlet is to be used.

The additions to the Zoological Society's Gardens during the past week include two Malbrouck Monkeys (*Cercopithecus cynosurus* ♂ ♀) from West Africa, presented respectively by Mr. L. Morris and Mr. A. M. Moore; a Macaque Monkey (*Macacus cynomolgus* ♀) from India, presented by Mrs. E. J. H. Sprague; a Rhesus Monkey (*Macacus erythræus* ♂) from India, presented by Mr. C. T. Pollock; a Bonnet Monkey (*Macacus radiatus* ♀) from India, presented by Mr. F. Nelson; two Mauge's Dasyures (*Dasyurus maugei*) from Australia, presented by Sir Louis S. Jackson, F.Z.S.; two Earl's Weka Rails (*Ocydromus earlii*) from North Island, New Zealand, a Black-backed Porphyrio (*Porphyrio melanotis*) from Australia, presented by Capt. R. Todd; three Common Kingfishers (*Alcedo ispida*), British, presented by the Hon. and Rev. F. G. Dutton; a Common Night Heron (*Nycticorax griseus*), European, presented by Mr. H. H. Blacklock; a King Penguin (*Aptenodytes pennanti*), two Upland

Nature

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"*To the solid ground*
Of Nature trusts the mind which builds for aye."—WORDSWORTH

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Geese (*Bernicla magellanica* ♂ ♀), two Ruddy-headed Geese (*Bernicla rubidiceps*) from the Falkland Islands, presented by Mr. R. C. Packe; three Common Pheasants (*Phasianus colchicus* ♂ ♀ ♀), British, presented by Mr. H. T. Bowes; an Indian Python (*Python molurus*) from India, presented by Mr. G. E. Shute; a Sykes's Monkey (*Cercopithecus albigularis*), a Philantomba Antelope (*Cephalophus maxwelli* ♀), an Elate Hornbill (*Buceros elatus*), a Jardine's Parrot (*Pœocephalus gulielmi*) from West Africa, an Indian Civet (*Viverricula indica*), two Wandering Tree Pies (*Dendrocitta vagabunda*), from India, a Red-sided Eclectus (*Eclectus polychlorus*) from New Guinea, five Red-bellied Conures (*Conurus vittatus*), a Giant Toad (*Bufo agua*) from Brazil, a Horned Lizard (*Phrynosoma cornutum*) from Texas, four Cornish Choughs (*Fregilus graculus*), British, purchased; a Common Rhea (*Rhea americana*) from South America, received in exchange; two Indian Pythons (*Python molurus*) from India, received on approval; a Japanese Deer (*Cervus sika* ♂), born in the Gardens.

OUR ASTRONOMICAL COLUMN

THE PARIS GENERAL CATALOGUE OF STARS.—In the last Annual Report issued by Admiral Mouchez we find particulars of the progress of formation of this extensive and important catalogue. It is intended to contain all the stars observed at Paris during the forty-five years 1837 to 1881 inclusive, about 40,000, but it is mainly the result of the revision of Lalande's stars in the *Histoire Céleste*; indeed, for several years past, the meridian instruments have been almost wholly occupied upon this work, and upwards of 27,000 observations were made during 1882, the year to which the Report refers. The entire number of observations upon which the Paris General Catalogue will be founded is about 350,000. The positions are referred to three principal epochs; 1845·0 for the years 1837-53, 1860·0 for the years 1854-67, and 1875·0 for the years 1868-82. A specimen of the form in which it is intended to print the catalogue is appended to the Report. The right ascensions and declinations are given for each principal epoch, with the number and mean year of the observations. The precessions are reckoned from the year 1875, with the term depending upon the square of the time. The magnitudes and the differences from the positions of the *Histoire Céleste* are annexed, and where a star has not been observed by Lalande a synonym in some other catalogue is given. In the first column we have the ordinal number, and in the second the star's number in the reduced catalogue of the *Histoire Céleste*. It is mentioned in the Report that M. Bossert had undertaken a new determination of the places of the stars in that work, making use of the reduction-tables of the late Doctor von Asten, which are more exact than the tables of Hansen and Nissen, employed for the catalogue published in 1847. M. Bossert has already effected the reduction of 2,300 stars, a voluntary labour which has occupied his leisure hours. It would add to the value of the columns showing the differences between the new Paris positions and those of Lalande, if the comparisons could be made with places resulting from the application of von Asten's tables, though it might be necessary to supplement M. Bossert's laudable efforts. In the last Greenwich Catalogue (1872) the precessions are given to four places of decimals in right ascension (time), and to three places in north polar distance; the Paris Catalogue gives these quantities with a figure less, which we are inclined to regard as a retrograde step.

This General Catalogue of the Observatory of Paris is to comprise two parts, which will be published simultaneously; the first part forming the catalogue proper, and the second containing details of the observations upon which the mean positions are founded. Each part will be composed of four volumes; the first volume of each is intended to appear during the year 1884.

ENCKE'S COMET IN THE YEARS 1871–1881.—At the sitting of the Paris Academy of Sciences on June 11, M. Tisserand communicated a note by Dr. Backlund, of the Observatory of Pulkowa, relative to the motion of Encke's Comet in the interval 1871–1881. To complete the theory of this comet, it has been necessary to introduce an empirical to the mean motion of the form $\mu' \left(\frac{t}{1200}\right)$. The quantity μ', which was found to be nearly constant during the period 1819-1868, appears to have undergone a considerable variation about the latter epoch. Dr. Backlund bases his calculations upon osculating elements for October 27, 1874, which he considered exact enough for his purpose: they give—

$$\mu = 1079''{\cdot}33355 + \mu'\tau \left(\text{where } \tau = \frac{t}{1200}\right)$$
$$\mu' = + 0{\cdot}051731$$

After having carefully reviewed the computation of perturbations by Asten, and calculated by two different methods the perturbations during the revolution 1878–1881, Dr. Backlund compared the elements with the observations made in the years 1871, 1875, 1878, and 1881. By means of this comparison, he obtains corrections to the elements, and, observing that if there exists a tangential force, which varies the dimensions of the comet's orbit, its effect is not only secular, but also periodic, the periodic terms being always very small, except in the expression for the mean anomaly. This he takes into account, and finally deduces for the corrections of the two quantities above—

$$\Delta\mu = + 0''{\cdot}004745$$
$$\Delta\mu' = - 0{\cdot}0059867$$

Hence, he says, his investigation proves that the acceleration of the mean motion in the period 1871–1881 was less than half the value found by Encke and Asten for the period 1819–1865. Aten's value is + 0·104418.

CHEMICAL NOTES

INTERESTING experiments on the luminosity of gases are described by W. Siemens in *Ann. Phys. Chim.* [(ii.) 18, 311], and by E. Wiedemann [*ib.* 509]. Gases free from solid particles do not become luminous at high temperatures, nor is the luminosity of a flame due to incandescence of the products of combustion; if the gases are strongly heated before being burnt, the flame becomes hotter and shorter than it is when the preliminary heating is omitted, and the luminous flame is seen to be distinctly separated from the non-luminous products of combustion. Siemens seems inclined to regard the chemical action which proceeds as the cause of luminosity: if the existence of an envelope of ether around the molecules is assumed, then the reaction of one molecule on another may be regarded as starting vibrations in this envelope, which vibrations give rise to heat and light rays. Wiedemann especially considers the luminosity of gases under the influence of electric discharges: he thinks that in the process of charging the electrodes the ethereal envelopes of some of the gas molecules are distorted; when discharge occurs these envelopes are set into motion, and hence the luminous effects.

SOME time ago Ostwald deduced the relative affinities of various acids in terms of nitric acid taken as 100; by relative affinity is meant the proportion in which two acids divide themselves between one base, all the reacting substances being in solution. Ostwald has recently investigated this subject by a method different from that formerly employed; he has studied the rates of action of various acids on acetamide, and from the results he has deduced the relative velocities of action, and hence the relative affinities. The following table contains the results. In column II. are placed figures representing the results of his former experiments—

		I.	II.
Hydrochloric	acid ...	100 ...	98·0
Nitric	,, ...	98 ...	100
Hydrobromic	,, ...	98 ...	95
Trichloracetic	,, ...	80 ...	80
Dichloracetic	,, ...	40·8 ...	33
Monochloracetic	,, ...	13·0 ...	7
Formic	,, ...	5·2 ...	3·9
Lactic	,, ...	5·2 ...	3·3
Acetic	,, ...	2·3 ...	1·2
Sulphuric	,, ...	65·4 ...	66·7
Oxalic	,, ...	22·6 ...	—
Tartaric	,, ...	7·5 ...	5·2
Malic	,, ...	4·7 ...	2·9
Succinic	,, ..	2·5 ...	1·5
Citric	,, ...	4·0 ...	—
Phosphoric	,, ...	3·6 ...	—
Arsenic	,, ...	3·5 ...	—

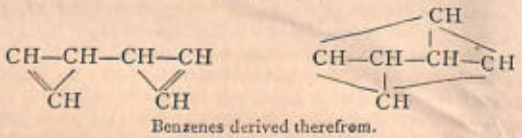
M. SPRING continues his researches into the influence of great pressure upon chemical reactions: at a pressure of about

6500 atmospheres he finds that sulphur combines with magnesium, zinc, iron, cadmium, bismuth, lead, copper, silver, tin, and antimony. Sulphur and phosphorus do not combine when compressed together (*Berichte*, xvi. 999).

BENZENE is perhaps the most important body in the whole range of chemistry, not on account of any intrinsic interest in the substance itself, but because of the immense number of its derivatives. The constitution of these derivatives must depend upon the structure of the benzene molecule itself, and this problem is therefore one of the most interesting that presents itself to the chemist. Any idea that can throw light upon this subject is worthy of attention, and the more so as long as the least doubt exists as to whether benzene can yield more than three di, tri, or tetra derivatives, or more than one mono or penta derivative, the substituting groups being the same. Again, it is possible that benzene may exist in two or more isomeric modifications (disregarding dipropargyl), and the difference found by V. Meyer (*Ber.* xv. 2893) between two samples of benzene, both presumably pure, would seem to point in this direction. The mere fact, therefore, that one formula is good and useful is no condemnation of any other formula that may be proposed. M. Mendeléeff has suggested that benzene may be regarded as a normal butane, in which six hydrogen atoms are replaced by two triad groups, CH. If we allow that benzene is best represented as containing six CH groups, and there seems as yet no reason for departing from this supposition, then this replacement may take place in two ways, as shown by the following formulæ :—

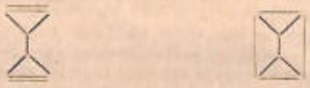
$$CH_3—CH_2—CH_2—CH_3$$

Normal butane.



Benzenes derived therefrom.

These two benzene formulæ may be conveniently written thus :—



and these expressions show at a glance the difference between them. The second is identical with Ladenburg's prism formula, the advantages of which do not need recounting. The first, so far as double and single linkings are concerned, is intermediate between the prism formula and Kekulé's. It lends itself in a particularly ready way to the expression of more complex formulæ, as of naphthaline, &c., but does not show the hexad nature of the benzene molecule. Moreover, it shows possible two mono or penta derivatives, and five each di, tri, and tetra derivatives, a capability that is not yet needed ; and a formula should be a concise expression of facts, and should as far as possible show the limits of those facts. Thus, however valuable the suggestion of M. Mendeléeff may be as showing a possible method of synthesising benzene, it does not appear to be practically useful as indicating its constitution, though the future chemistry of benzene may require such a formula as the one referred to above.

PROF. MENDELÉEFF, to avoid the superheating which takes place during ordinary fractional distillation with a dephlegmator tube, has devised a modified method for the oils from Baku petroleums boiling between 15° and 150°, which consists in passing the vapours from the distilling flask by means of the dephlegmator, or delivery tube, to the bottom of a second similar flask, and from this to a third, and so on ; the heated vapours from the one providing the necessary heat for the distillation of the next, &c. In this manner a great number of fractions at intervals of two degrees were obtained. By comparing boiling points and specific gravities of products the author concludes that Baku oils contain similar hydrocarbons to American petroleum, and in addition a hydrocarbon boiling at 55° and same specific gravity as hexan with the properties of an unsaturated compound. The great bulk of the Caspian petroleums appears to consist, in addition to derivatives of marsh gas, of C_nH_{2n} hydrocarbons, and also some members of the C_nH_n or acetylene series.

SOME interesting results have been obtained by Spring (*Ber. Ber.*) by washing precipitated sulphide of copper for several weeks until all traces of salts were removed. It was then found that the sulphide dissolved to a black liquid, with slight green fluorescence, in water. The solution might be boiled and evaporated without change ; slight traces of salts caused precipitation. The author has also obtained sulphide of tin and oxides of antimony and manganese in a condition perfectly soluble in water. Sulphide of tin on evaporation of its solution in vacuo forms a transparent red glass.

GEOGRAPHICAL NOTES

ON June 6 Baron Nordenskjöld's Greenland expedition arrived at Reykjavik in the steamer *Sophia*. The *Sophia* lay at Reykjavik for a few days, and in the meantime Baron Nordenskjöld and the geologists of the party examined the coal deposits which occur in Bergarfiord. Dr. Arpi, a Swedish philologist, who has resided some time in Iceland and acquired a thorough knowledge of the language, accompanied the expedition thither, and will, along with two other men of science, remain in Iceland after the *Sophia* has left.

WE learn from *Science* that a party for the relief of the observers under Lieut. Greely at Lady Franklin Bay was to leave St. John's, Newfoundland, on one of the steam sealing-vessels belonging to that port, about June 15, probably accompanied by a naval vessel as tender. It will be commanded by Lieut. E. A. Garlington, U.S.A., and composed of twelve men, of whom ten are stated to be old sailors and accustomed to the use of boats. Twenty dogs, native drivers, and a supply of fur clothing, have been secured at Godhavn, Greenland. The party at Lady Franklin Bay will be reached and withdrawn if the state of the ice permits. If not, the relief party is to be landed on Littleton Island, and while part of them are engaged in preparing winter quarters, Lieut. Garlington will endeavour to open communication by sledges with Greely's people. In the failure of the first attempt, another will be made in the spring of 1884. It is to be hoped, if Greely is not reached, that an attempt will be made to leave at Cape Hawkes or Cape Sabine, if not the relief party as a whole, which would be best, at least a boat by which the open water to be anticipated between those points and Littleton Island next year (1884) may be passed by a retreating party, which might well find their own boat unseaworthy after dragging it over many miles of hummocky ice, if, indeed, they did not find themselves obliged to abandon it. Further, the schooner *Leo* is on the point of sailing for Point Barrow to withdraw the signal service observing party under Lieut. Ray, in compliance with the act passed by the last Congress. To utilise the opportunity, Mr. Marr, of the U.S. Coast-Survey, will accompany the vessel with the design of making absolute magnetic determinations, of fixing the astronomical position of the station, and of making pendulum observations.

IN a communication from the Russian Geographical Society we are informed that Col. Prejevalsky is about to start on his fourth journey to Central Asia, accompanied by two officers and seventeen men. The Emperor has granted to the Society 43,000 roubles for the purpose of Col. Prejevalsky's journey. The Society is also sending out a new expedition under M. Potanin, who is now completing the publication of the two last volumes on his journey of 1879-80. He will start in July for South-East Mongolia and the adjacent parts of China ; he will be accompanied by a naturalist and M. Skassi, the companion of Severtzov in his exploration of the Pamir. The funds are being supplied partly by the Society and partly by M. Sookachef, a Siberian merchant.

IN the same communication we are informed that the average temperature of January and February at the Russian Polar station at Sagastyr, on the mouth of the Lena, was about – 50° C. Thanks to the Governor-General of Eastern Siberia there has been organised a special postal service between Jakutsk and Sagastyr once a month. The observing party will most probably remain at the station up to the end of October, *i.e.* until the river is frozen.

THE last number of the *Zeitschrift der Gesellschaft fur Erdkunde zu Berlin* (Bd. 18, Heft 2) contains a paper by Herr van Langegg, entitled "Nara eine alte Kaiserstadt," describing the town of Nara, not far from Kioto, in Japan, at one time the capital of the country, and still much renowned for its temples. The celebrated colossal statue of Buddha there is fully described. The following figures give some notion of its dimensions :—Its weight is 500,000 kilog. ; 3,000,000 kilog. of wood were con-

sumed in making the bronze, which consists of 250 kilog. of gold, 8413·5 of tin, 977 of mercury, and 493 of copper. The present image only dates from 1801.

We have received a German pamphlet by Herr Max Buch, on "Finland and its Nationality Question," being a reprint of papers which have appeared in recent issues of the *Ausland*. In the limited space of seventy-four pages the author gives a short but correct description of Finland, of the prehistoric Finns, according to Ahlquist's researches, of the history of the country, and of the present state of the "national question." He summarises the excellent researches by Retzius on the race-characters of the Finns—as far as can be done in a few pages—and dwells upon the recent efforts of Finnish writers towards the development of the Finnish language and literature as a reaction against the former supremacy of the Swedish language and influence. We notice the interesting fact that although only 7·5 per cent. of the Finns can now read and write, and 70 per cent. read, primary instruction has taken during the last few years a great extension. The number of State schools being too limited, they are supplemented by private instruction. Thus, of the 342,836 children from seven to sixteen years old of the Lutheran Finnish population, only 6983 had not received primary instruction in 1877 (1801 of them on account of disease). But only 26,900 went to the State schools, whilst 116,201 children received primary instruction in private ambulatory schools, and 177,925 at home.

The last number of the *Izvestia* of the Russian Geographical Society contains several interesting papers. M. Veselago gives a sketch of the life and work of the late Count Lütke. Prof. Fr. Schmidt discusses again the claim of Wrangel to the discovery of the land situated north of the Cape Yakan. He tries to prove, against Nordenskjöld, that Wrangel was right in denying the existence of a land which Andréeff said he saw from the fifth island of the Medvyejiy Archipelago; but he did not deny the existence of a land situated north of Cape Yakan. Prof. Schmidt admits, however, that even with regard to this land, Wrangel wrote "in different parts of his report with a varying degree of certainty as to the probability of its existence." M. Karzin, an official of the Verkhoyansk district, having been struck with the terrible fate of De Long, publishes a most valuable list of all settlements and places where human beings can be met with at different seasons on the coast of North-Eastern Siberia. M. Andréeff publishes a brief account of his hydrographical researches in the White Sea and on the Murman coast during the last three years. The flora of the coast becomes very poor north of Archangel. At the Svyatoy Noss lighthouse it consists only of lichens, mosses, and creeping brushes of *Betula nana*. It improves, however, west of Kildin and especially west of the Ribachiy peninsula, offering excellent forests and meadows at the new colony at Pechenga. The yearly average temperature, which is but − 0°·6 Celsius at Archangel, and − 2°·4 at the Svyatoy Noss lighthouse, reaches − 1°·1 at Kola, and + 1°·4 at Vardö. This increase of temperature is due, as is known, to the warm current which flows along the coast. Thus, at Svyatoy Noss, during the hottest days, the temperature of water does not exceed 6°·9; and during the autumn it reaches but 1°·9. To the west of 30° 6′ it suddenly becomes double that. In the spring the warm streamlets reach 4°·3, whilst the cold ones, flowing close by, reach but 1°·9; and during the summer the warm streamlets reach 12°·5, whilst the cold ones, close by, reach 6°·9 to 7°·5. It appears thus that one isolated measurement of temperature of water is of little value, the warm current being not so compact along the Murman coast as elsewhere. Under 33° 6′ E. long. it leaves the coast and flows towards the north-north-east. The positions taken by the warm current at the Murman coast vary with the seasons, and depend upon the prevailing winds. From April to August it touches the coast, but later on it is driven north by the southern winds; in October it already flows off Vardö. Its position varies also for different years, depending upon the prevailing winds. The richness of the fishing depends entirely upon the position taken by the warm current. In 1881, the Norwegians, owing to the current flowing in their waters, had the richest prey, whilst in 1882, the richest prey for a twenty years' series was given by the warm current to the Russian fishers. The same number of the *Izvestia* contains the first sheets of M. Polakoff's reports on his researches in Sakhalin, and M. Mezhoff's bibliographical index of the Russian geographical literature for the year 1880.

M. Thouar, the French traveller, has written a letter from Chili, in which he says that several members of the exploring party under Dr. Jules Crevaux, who was massacred with most of his followers in the early part of last year by Indians while making explorations along the Bolivian part of the Pilcomayo, are believed to be still alive, but prisoners in the hands of the Indians.

THE CAUSE OF EVIDENT MAGNETISM IN IRON, STEEL, AND OTHER MAGNETIC METALS[1]

Neutrality

THE apparatus needed for researches upon evident external polarity requires no very great skill or thought, but simply an apparatus to measure correctly the force of the evident repulsion or attraction; in the case of neutrality, however, the external polarity disappears, and we consequently require special apparatus, together with the utmost care and reflection in its use.

From numerous researches previously made by means of the induction balance, the results of which I have already published, I felt convinced that in investigating the cause of magnetism and neutrality I should have in it the aid of the most powerful instrument of research ever brought to bear upon the molecular construction of iron, as indeed of all metals. It neglects all forces which do not produce a change in the molecular structure, and enables us to penetrate at once to the interior of a magnet or piece of iron, observing only its peculiar structure and the change which takes place during magnetisation or apparent neutrality.

The induction balance is affected by three distinct arrangements of molecular structure in iron and steel, by means of which we have apparent external neutrality.

Fig. 1 shows several polar directions of the molecules as indicated by the arrows. Poisson assumed, as a necessity of his theory, that a molecule is spherical, but Dr. Joule's experimental proof of the elongation of iron by 1/720,000th of its length when magnetised, proves at least that its form is not spherical; and as I am unable at present to demonstrate my own views as to its exact form, I have simply indicated its polar direction by arrows—the dotted oval lines merely indicating its limits of free elastic rotation.

In Fig. 1, at A, we have neutrality by the mutual attraction of each pair of molecules, being the shortest path in which they could satisfy their mutual attractions. At B we have the case of superposed magnetism of equal external value, rendering the wire or rod apparently neutral, although a lower series of molecules are rotated in the opposite direction to the upper series, giving to the rod opposite and equal polarities. At C we have the molecules arranged in a circular chain around the axis of a wire or rod through which an electric current has passed. At D we have the evident polarity induced by the earth's directive influence when a soft iron rod is held in the magnetic meridian. At E we have a longitudinal neutrality produced in the same rod when placed magnetic west, the polarity in the latter case being transversal.

In all these cases we have a perfectly symmetrical arrangement, and I have not yet found a single case in well-annealed soft iron in which I could detect a heterogeneous arrangement, as supposed by Ampère, De la Rive, Weber, Wiedermann, and Maxwell.

We can only study neutrality with perfectly soft Swedish iron. Hard iron and steel retain previous magnetisations, and an apparent external neutrality would in most cases be the superposition of one magnetism upon another of equal external force in the opposite direction, as shown in B, Fig. 1. Perfectly soft iron we can easily free, by vibrations, from the slightest trace of previous magnetism, and study the neutrality produced under varying conditions.

If we take a flat bar of soft iron, of 30 or more centimetres in length, and hold it vertically (giving while thus held a few torsions, vibrations, or, better still, a few slight blows with a wooden mallet, in order to allow its molecules to rotate with perfect freedom), we find its lower end to be of strong north polarity, and its upper end south. On reversing the rod and repeating the vibrations, we find that its lower end has pre-

[1] Paper read before the Society of Telegraph Engineers and of Electricians, on May 24, 1883, by Prof. D. E. Hughes, F.R.S., Vice-President

Continued from p. 162.

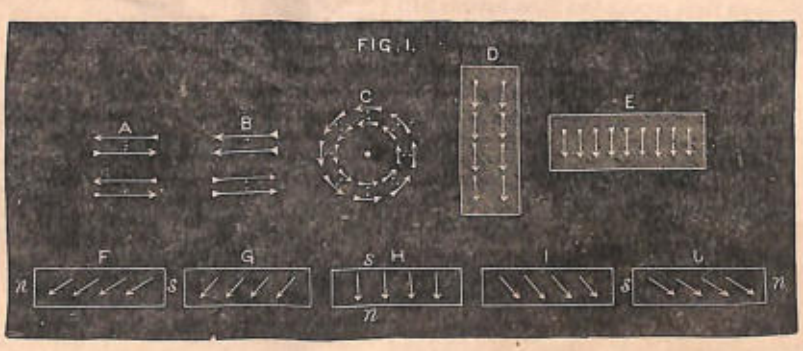
cisely a similar north polarity. Thus the iron is homogeneous, and its polarity symmetrical. If we now magnetise this rod to produce a strong south pole at its lower portion, we can gradually reverse this polarity, by the influence of earth's magnetism, by slightly tapping the upper extremity with a small wooden mallet. If we observe this rod by means of a direction needle at all parts, and successively during its gradual passage from one polarity to the other, there will be no sudden break into a haphazard arrangement, but a gradual and perfectly symmetrical rotation from one direction to that of the opposite polarity.

If this rod is placed east and west, having first, say, a north polarity to the right, we can gradually discharge or rotate the molecules to zero, and as gradually reverse the polarity by simply inclining the rod so as to be slightly influenced by earth's magnetism; and at no portion of this passage from one polarity to neutrality, and to that of the opposite name, will there be found a break of continuity of rotation or haphazard arrangement. If we rotate this rod slowly, horizontally or vertically, taking observations at each few degrees of rotation of an entire revolution, we find still the same gradual symmetrical change of polarity, and that its symmetry is as complete at neutrality as in evident polarity.

In all these cases there is no complete neutrality, the longitudinal polarity simply becoming transversal when the rod is east and west. F, G, H, I, J, Fig. 1, show this gradual change, H being neutral longitudinally, but polarised transversely. If, in place of the rod, we take a small square soft iron plate and allow its molecules freedom under the sole influence of the earth's magnetism, then we invariably find the polarity in the direction of the magnetic dip, no matter in what position it be held, and a sphere of soft iron could only be polarised in a similar direction. Thus we can never obtain complete external neutrality whilst the molecules have freedom and do not form an internal closed circle of mutual attractions; and whatever theory we may adopt as to the cause of polarity in the molecule, such as Coulomb's, Poisson's, Ampère's, or Weber's, there can exist no haphazard arrangement in perfectly soft iron, as long as it is free from all external causes except the influence of the earth; consequently these theories are wrong in one of their most essential parts.

We can, however, produce a closed circle of mutual attraction in iron and steel, producing complete neutrality as long as the structure is not destroyed by some stronger external directing influence.

Oersted discovered that an external magnetic needle places itself perpendicular to an electric current; and we should expect that, if the molecules of an iron wire possessed inherent polarity and could rotate, a similar effect would take place in the interior of the wire to that observed by Oersted. Wiedermann first remarked this effect, and it has been known as circular magnetism. This circle, however, consists really in each molecule having placed itself perpendicular to the current, simply obeying Oersted's law, and thus forming a complete circle in which the mutual attractions of the molecules forming that circle are satisfied, as shown at C, Fig. 1. This wire becomes completely neutral, any previous symmetrical arrangement of polarity rotating to form its complete circle of attractions; and we can thus form in hard iron and steel a neutrality extremely difficult to break up or destroy. We have evident proof that this



neutrality consists of a closed chain, or circle, as by torsion we can partially deflect them on either side; thus, from a perfect externally neutral wire, producing either polarity, by simple mechanical angular displacement of the molecules, as by right- or left-handed torsion.

If we magnetise a wire placed east and west, it will retain this polarity until freed by vibrations, as already remarked. If we pass an electric current through this magnetised wire, we can notice the gradual rotation of the molecules, and the formation of the circular neutrality. If we commence with a weak current, gradually increasing its strength, we can rotate them as slowly as may be desired. There is no sudden break or haphazard moment of neutrality: the movements to perfect zero are accomplished with perfect symmetry throughout.

We can produce a more perfect and shorter circle of attractions by the superposition of magnetism, as at B, Fig. 1. If we magnetise a piece of steel or iron in a given direction with a strong magnetic directing power, the magnetism penetrates to a certain depth. If we slightly diminish the magnetising power, and magnetise the rod in a contrary direction, we may reduce it to zero by the superposition of an exterior magnetism upon one of a contrary name existing at a greater depth; and if we continue this operation, gradually diminishing the force at each reversal, we can easily superpose ten or more distinct symmetrical arrangements, and as their mutual attractions are satisfied in a shorter circle than that produced by electricity, it is extremely difficult to destroy this formation when once produced.

The induction balance affords also some reasons for believing that the molecules not only form a closed circle of attractions, as at B, but that they can mutually react upon each other, so as to close a circle of attractions as a double molecule, as shown at A. The experimental evidence, however, is not sufficient to dwell on this point, as the neutrality obtained by superposition is somewhat similar in its external effects.

We can produce a perfectly symmetrical closed circle of attractions of the nature of the neutrality of C, Fig. 1, by forming a steel wire into a closed circle, 10 centimetres in diameter, if this wire is well joined at its extremities by twisting and soldering. We can then magnetise this ring by slowly revolving it at the extremity of one pole of a strong permanent magnet; and, to avoid consequent poles at the part last touching the magnet, we should have a graduating wedge of wood, so that whilst revolving, it may be gradually removed to greater distance. This wire will then contain no consequent points or external magnetism: it will be found perfectly neutral in all parts of its closed circle. Its neutrality is similar to C, Fig. 1; for if we cut this wire at any point we find extremely strong magnetic polarity, being magnetised by this method to saturation, and having retained (which it will indefinitely) its circle of attractions complete.

I have already shown that soft iron, when its molecules are allowed perfect freedom by vibration, invariably takes the polarity of the external directing influence, such as that of the earth, and it does so even with greater freedom under the influence of heat. Manufacturers of electromagnets for telegraphic instruments are very careful to choose the softest iron and thoroughly anneal it; but very few recognise the importance as regards the position of the iron whilst annealing it under the earth's directing influence. The fact, however, has long since been observed.

Dr. Hooke (1684) remarked that steel or iron was magnetised when heated to redness and placed in the magnetic meridian. I have slightly varied this experiment by heating to redness three similar steel bars, two of which had been previously magnetised

to saturation, and placed separately with contrary polarity as regards each other, the third being neutral. Upon cooling, these three bars were found to have identical and similar polarity. Thus the molecules of this most rigid material, cast steel, had become free at red heat, and rotated under the earth's magnetic influence, giving exactly the same force on each; consequently the previous magnetisation of two of these bars had neither augmented nor weakened the inherent polarity of their molecules. Soft iron gave under these conditions by far the greatest force, its inherent polarity being greater than that of steel.

I have made numerous other experiments bearing upon the question of neutrality, but they all confirm those I have cited, which I consider afford ample evidence of the symmetrical arrangement of neutrality.

Superposed Magnetism.—Knowing that by torsion we can rotate or diminish magnetism, I was anxious to obtain by its means a complete rotation from north polarity to neutrality, and from neutrality to south polarity, or to completely reverse magnetic polarity by a slight right or left torsion.

I have succeeded in doing this and in obtaining strong reversal of polarities by superposing one polarity given whilst the rod is under a right elastic torsion, with another of the opposite polarity given under a left elastic torsion, the neutral point then being reached when the rod is free from torsion. The rod should be very strongly magnetised under its first or right-hand torsion, so that its interior molecules are rotated, or, in other words, magnetised to saturation; the second magnetisation in the contrary sense and torsion should be feebler, so as only to magnetise the surface, or not more than one-half its depth: these can be easily adjusted to each other so as to form a complete polar balance of force, producing, when the rod is free from torsion, the neutrality as shown at B, Fig. 1.

The apparatus needed is simply a good compound horseshoe permanent magnet, 15 centimetres long, having six or more plates, giving it a total thickness of at least 3 centimetres. We need a sufficiently powerful magnet, as I find that I obtain a more equal distribution of magnetism upon a rod or strip of iron by drawing it lengthwise over a single pole in a direction from



that pole, as shown in Fig. 2; we can then obtain saturation by repeated drawings, keeping the same molecular symmetry in each experiment.

In order to apply a slight elastic torsion when magnetising rods or wires, I have found it convenient to attach two brass clamp keys to the extremities of the rods, or simply turn the ends at right angles, as shown in the following diagram, by which means we can apply an elastic twist or torsion whilst drawing the rod over the pole of the permanent magnet. We can thus superpose several and opposite symmetrical structures, producing a polar north or south as desired, greatly in excess of that possible under a single or even double magnetisation, and by carefully adjusting the proportion of opposing magnetisms so that both polarities have the same external force, the rod will be at perfect external neutrality when free from torsion.

If we now hold one end of this rod at a few centimetres distance from a magnetic directive needle, we find it perfectly neutral when free of torsion, but the slightest torsion right or left at once produces violent repulsion or attraction, according to the direction of the torsion given to the rod, the iron rod or strips of hoop-iron which I use for this experiment being able, when at the distance of 5 centimetres from the needle, to turn it instantly 90° on either side of its zero.

The external neutrality that we can now produce at will is absolute, as it crosses the line of two contrary polarities, being similar to the zero of my electric sonometer, whose zero is obtained by the crossing of two opposing electric forces.

This rod of iron retains its peculiar powers of reversal in a remarkable degree, a condition quite different to that of ordinary magnetisation, for the same rod, when magnetised to saturation under a single ordinary magnetism, loses its evident magnetism by a few elastic torsions, as I have already shown; but when it is magnetised under the double torsion with its superposed magnetism, it is but slightly reduced by variations or numerous torsions, and I have found it impossible to render this rod again free from its double polar effects, except by strongly remagnetising it to saturation with a single polarity. The superposed magnetism then becomes a single directive force, and we can then by a few vibrations or torsions reduce the rod to its ordinary condition.

The effects of superposed magnetism and its double polarity I have produced in a variety of ways, such as by the electromagnetic influence of coils, or in very soft iron simply by the directive influence of the earth's magnetism, reversing the rod and torsions when held in the magnetic meridian, these rods when placed magnetic west showing distinctly the double polar effects.

It is remarkable, also, that we are enabled to superpose and obtain the maximum effects on thin strips of iron from a quarter to half a millimetre in thickness, whilst in thicker rods we have far less effect, being masked by the comparatively neutral state of the interior, the exterior molecules then reacting upon those of the interior, allowing them to complete in the interior their circle of attractions.

I was anxious to obtain wires which would preserve this structure against the destructive influence of torsion and vibrations, so that I could constantly employ the same wires without the comparatively long and tedious process of preparation. Soft iron soon loses the structure or becomes enfeebled under the constant to and fro torsions requisite where we desire a constant change of polarity, as described later in the magnetic bells. Hard steel preserves its structure, but its molecular rigidity is so great that we obtain but mere traces of any change of polarity by torsion. I have found, however, that fine cast drill steel, untempered, of the kind employed by watchmakers, is most suitable: these are generally sold in straight lengths of 30 centimetres. Wires 1 millimetre in diameter should be used, and when it is desired to increase the force, several of these wires, say nine or ten, should be formed into a single rod or bunch.

The wire as sold is too rigid to give its maximum of molecular rotation effect. We must therefore give it two entire turns or twists to the right, and strongly magnetise it on the north pole of the magnet whilst under torsion. We must again repeat this operation in the contrary direction, after restoring the wire to its previous position, giving now two entire turns to the left and magnetising it on the south pole. On restoring the wire to its original place it will be extremely flexible, and we may now superpose several contrary polarities under contrary torsions, as already described.

The power of these wires, if properly prepared, is most remarkable, being able to reverse their polarity under torsion, as if they were completely saturated; and they preserve this power indefinitely if not touched by a magnet. It would be extremely difficult to explain the action of the rotative effects obtained in these wires under any other theory than that which I have advanced; and the absolute external neutrality that we obtain in them when the polarities are changing, we know from their structure to be perfectly symmetrical.

I was anxious to show, upon the reading of this paper, some mechanical movement produced by molecular rotation, consequently I have arranged two bells that are struck alternately by a polarised armature put in motion by the double polarised rod I have already described, but whose position, at 3 centimetres distant from the axis of the armature, remains invariably the same. The magnetic armature consists of a horizontal light steel bar suspended by its central axle; the bells are thin wine glasses, giving a clear musical tone loud enough, by the force with which they are struck, to be clearly heard at some distance. The armature does not strike these alternately by a pendulous movement, as we may easily strike only one continuously, the friction and inertia of the armature causing its movements to be perfectly dead-beat when not driven by some external force, and it is kept in its zero position by a strong directive magnet placed beneath its axle.

The mechanical power obtained is extremely evident, and is sufficient to put the sluggish armature in rapid motion, striking the bells six times per second, and with a power sufficient to produce tones loud enough to be clearly heard in all parts of the hall of the Society. As this is the first direct transformation of molecular motion into mechanical movement, I am happy to show it on this occasion.

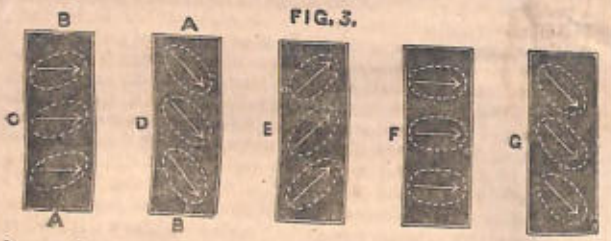
There is nothing remarkable in the bells themselves, as they evidently could be rung if the armature was surrounded by a coil, and worked by an electric current from a few cells. The marvel, however, is in the small steel superposed magnetic wire producing by slight elastic torsions from a single wire, 1 millimetre in diameter, sufficient force from mere molecular rotation to entirely replace the coil and electric current.

Elastic Nature of the Ether surrounding the Magnetic Molecules.—During these researches I have remarked a peculiar property of magnetism, viz., that not only can the molecules be rotated through any degree of arc to its maximum, or saturation, but that, whilst it requires a comparatively strong force to overcome its rigidity or resistance to rotation, it has a small field of its own through which it can move with excessive freedom, trembling, vibrating, or rotating through a small degree with infinitely less force than would be required to rotate it permanently on either side. This property is so marked and general that we can observe it without any special iron or apparatus.

Let us take a flat rod of ordinary hoop-iron, 30 or more centimetres in length. If, whilst holding this vertically, we give freedom to its molecules by torsions, vibrations, or, better still, by a few blows with a wooden mallet upon its upper extremity, we find, as is well known, that its lower portion is strongly north, and its upper south. If we reverse this rod, we now find it neutral at both extremities. We might here suppose that the earth's directing force had rotated the molecules to zero or transversely, which in reality it has done, but only to the limit of their comparatively free motion; for if we reverse the rod to its original position, its previous strong polarity reappears at both extremities, thus the central point of its free motion is inclined to the rod, giving by its free motion great symmetrical inclination and polarity in one direction, but when reversed the inclination is reduced to zero.

In Fig. 3 D shows the bar of iron when strongly polarised by earth's magnetic influence, under vibrations, with a sufficient

FIG. 3.



force to have rotated its elastic centre of action. C shows the same bar with its molecules at zero, or transversal, the directing force of earth being insufficient without the aid of mechanical vibration to allow them to change. The dotted lines of D suppose the molecule to be in the centre of its free motion, whilst at C the molecules have rotated to zero, as they are prevented from further rotation by being at the extreme end of its free motion

If, now, we hold the rod vertically, as at C, giving neutrality, and give a few slight blows with a wooden mallet to its upper extremity, we can give just the amount of freedom required for it to produce evident polarity, and we then have equal polarity no matter which end of the bar is below, the centre of its free rotation here being perfect, and the rod perfectly neutral longitudinally when held east and west. If, on the other hand, we have given too much freedom by repeated blows of the mallet, its centre of free motion becomes inclined with the molecules, and we arrive at its first condition, except that it is now neutral at D and polarised at C. From this it will be seen that we can adjust this centre of action, by vibrations or blows, to any point within the external directing influence.

We can perceive this effect of free rotation in a limited space in all classes of iron and steel, being far greater in soft Swedish iron than in hard iron or steel. A similar phenomenon takes place if we magnetise a rod held vertically in the direction of earth's magnetism. It then gives greater polarity than if magnetised east or west, and if magnetised in a contrary sense to earth's magnetism, it is very feebly magnetised, or, if the rod is perfectly soft, it becomes neutral after strong magnetisation. This property of comparative freedom, and the rotation of its centre of action, can be demonstrated in a variety of ways. One remarkable example of it consists in the telephone. All those who are thoroughly acquainted with electro-magnetism and know that it requires measurable time to charge an electro-magnet to saturation (about one-fifteenth of a second for those employed in telegraphy), were surprised that the telephone could follow the slightest change of timbre, requiring almost innumerable changes of force per second. I believe the free rotation I have spoken of through a limited range explains its remarkable sensitiveness and rapidity of action, and, according to this view, it would also explain why loud sounding telephones can never repeat all the delicacy of timbre that is easily done with those only requiring a force comprised in the critical limits of its free rotation. This property, I have found, has a distinct critical value for each class of iron, and I propose soon to publish researches upon the molecular construction of steel and iron, in which I have made use of this very property as a guide to the quality of the iron itself.

The elastic rotation (in a limited space) of a molecule differs entirely from that known as mechanical elasticity. In perfectly soft iron we have feeble *mechanical* elasticity, whilst in tempered steel we have that elasticity at its maximum. The contrary takes place as regards *molecular* elasticity. In tempered steel the molecules are extremely rigid, and in soft iron its molecular elasticity is at its maximum. Its free motion differs entirely from that given it by torsion or stress. We may assume that a molecule is surrounded by continuous ether, more of the nature of a jelly than of that of a gas: in such a medium a molecule might freely vibrate through small arcs, but a rotation extending beyond its critical limit would involve a much greater expenditure of force.

The discovery of this comparatively free rotation of molecules, by means of which, as I have shown, we can (without in any degree disturbing the external mechanical elasticity of the mass) change the axes of their free motion in any direction desired, has led me into a series of researches which have only indirectly any relation with the theory of magnetism. I was extremely desirous, however, of finding an experimental evidence which in itself should demonstrate all portions of the theory, and the following experiment, I believe, answers this purpose.

Let us take a square soft iron rod, 5 millimetres in diameter by 30 or more centimetres in length, and force the molecules, by aid of blows from a wooden mallet, as previously described, to have their centres of free motion in one direction, the rod will (as already shown) have polarity at both ends, when held vertically; but if reversed, both ends become completely neutral.

If now we turn the rod to its first position, in which it shows strong polarity, and magnetise it whilst held vertically, by drawing the north pole of a sufficiently powerful permanent magnet from its upper to its lower extremity, we find that this rod, instead of having south polarity at its lower portion, as we should expect from the direction of the magnetisation, is completely neutral at both extremities, but if we reverse the rod, its fullest free powers of magnetisation now appear in the position where it was previously neutral. Thus, by magnetisation, we have completely rotated its free path of action, and find that we can rotate this path as desired in any direction by the application of a sufficient directing power.

If we take a rod as described, with its polarities evident when held vertically, and its neutrality also evident when its ends are reversed in the same magnetic field, we find that its polarity is equal at both ends, and that it is in every way symmetrical with a perfect magnet. If we *gradually* reverse the ends and take observations of its condition through each degree of arc passed over, we find an equal symmetrical diminution of evident external polarity until we arrive at neutrality, when it has no external trace of inherent polarity, but its inherent polarity at once becomes evident by a simple return to its former position. Thus the rod has passed through all the changes from polarity to neutrality, and from neutrality to polarity, and these changes have taken place with complete symmetry.

The limits of this paper do not allow me to speak of the numerous theoretical evidences as shown by the use of my induction balance. I believe, however, that I have cited already experimental evidences to show that what has been attributed to coercive force is really due to molecular freedom or rigidity; that in inherent molecular polarity we have a fact admitted by Coulomb, Poisson, Ampère, De la Rive, Weber, Du Moncel, Wiedermann, and Maxwell; and that we have also experimental evidence of molecular rotation and of the symmetrical character of polarity and neutrality.

The experiments which I have brought forward in this paper,

in addition to those mentioned in my paper read before the Royal Society, will, I hope, justify me in having advanced a theory of magnetism which I believe in every portion allows at least experimental evidences of its probable truth.

THE REDE LECTURE

THE following abstract report of Prof. Huxley's Rede Lecture given on Tuesday week in the Cambridge Senate House, to a crowded audience, has been revised, to the extent of removing any errors of importance, by the author. We understand that a full report of the lecture will shortly be published in a separate form.

Professor Huxley said he had undertaken to treat in the course of such time as custom and the patience of his audience might permit, on a very great subject, no less a subject than the origin of all those forms of animal life which at present existed. It had behoved him to restrict what he might lay before them to those considerations which were absolutely essential for his purpose, and he should endeavour to lay before them facts of such an order as appeared to him to be of most importance in reference to his argument. Although he might fail to put those facts before them as clearly as they presented themselves to his own mind, the reasonings which might be based upon them were of so simple an order that he should consider his task performed if he gave them a tolerably clear conception of what those facts were, for he did not think it was the business of a man of science to use the arts of rhetoric or endeavour to procure persuasion. His sole business was to place the facts before those whom he wished to teach, and to leave it to their reason to form such judgment upon those facts as they might think fit. In the present case he should point out to them what judgments such facts had forced upon his mind, but he must leave it entirely to their responsibility to say what judgment they might constrain them to give in their case. They might assume this position at starting, that, whatever in such a matter was true for one animal, was true for the infinite series of the whole animal world; and as he was extremely anxious to avoid everything speculative, everything that could not be directly led back to the matters of fact upon which it was based, he proposed to select one animal particularly, and to put before them facts and arguments by the help of which they might form some probable conclusion as to the origin of that object. He took it for granted that, if the evidence inclined towards a particular conclusion in the case of that animal, they might assume that it would incline in the same direction with regard to all. He had no doubt that a great many of his audience were familiar at any rate with the shell of the animal about which he was going to speak, namely, that of the pearly nautilus, from which, or parts of which, very beautiful ornaments were fabricated. At the present time the nautilus inhabited the warmer parts of the Indian and Pacific Oceans, living at considerable depths and preying upon the hard shelled crustaceans and mollusks that crept along the bottom, and which it found in its way. For that end it was provided with a very curious beak, shaped like that of a parrot, but with each portion covered with a hard calcareous deposit, and which enabled it to be an efficient instrument for crushing its prey. If he were to touch upon the morphological problem which here presented itself, he could occupy far more time than they had at their disposal with the consideration of a multitude of interesting peculiarities which the nautilus presented, for it was one of those forms which at present stood almost isolated and alone in the animal world, separated by a wide gulf from its nearest allies, those animals which they knew as squids and cuttle-fishes. It held the middle place between sea-snails and the group of the cuttle-fishes. It would be, however, entirely out of place at present, and a purposeless waste of time if he were to touch upon any peculiarities except those which would be needed during his further argument. The only points to which he would direct their attention for that purpose were the facts which related to the structure of the shell. There was a diagram beside him showing a part of the nautilus shell in section, but he thought it possible that he could make the matter clearer by roughly sketching on the board the main points as he went on.—Prof. Huxley here described, with the aid of diagrams, preserved specimens, and models, the complicated structure of the shells of the pearly nautilus, or *Nautilus pompilius*. The animal itself was contained in the spacious chamber in the outer part of the shell, which was divided from the rest of the shell by a partition. The rest of the shell resembled a long cone closely coiled up, and divided by partitions at regular intervals into other chambers, which succeeded one another, and in the full-grown animal were full of air. From the hinder part of the animal's body a long tube, the siphuncle, was carried backwards through the whole of the shell, and as it completely filled up the openings in the partitions through which it passed there was no communication between one chamber and another. The first point to be considered was as to what was the origin of the particular nautilus in the bottle before him. Happily there was no dispute upon that point. The female nautilus contained eggs exactly as the hen did. These eggs were small masses of protoplasmic matter, each containing a nucleus in its centre, which was all that was essential. They knew that that pearly nautilus with all its complicated organism, and fitted with the complicated shell he had described, did, in some way or other, proceed from that relatively structureless body which they called the egg or the ovum. As fate would have it, up to the present they had known nothing from direct observation of the process by which that particular animal was produced from this microscopic particle. But they had so large a knowledge of the process in other animals of every description that there was no doubt whatever as to the nature of the process, which he would try to describe to them as briefly as possible, by reference to the process which took place in the case of the domestic hen. Neither by the highest powers of the microscope, nor by other means of investigation which they had at present, could they trace anything in the slightest degree resembling either the chick, which under certain circumstances proceeded from that egg, or the tissues of the chick. There was, however, one spot on the yolk of the egg, a little careful observation of which would show a clear space, which might be a fifth of an inch in diameter. It was very well known by the name of the cicatricula, or little scar. He would suppose that twenty-one eggs were placed together under the hen. If they took one egg day by day and examined it they would know what took place as if they had watched continuously, for what happened in any one egg happened also in the others. That was a process—the wonder of which he must confess never staled in his mind—by which the chick was gradually fashioned out of that transparent rudiment. They saw it make its appearance in the first place on the surface of the yolk, and to the naked eye it looked like a white streak. That white streak gradually assumed the appearance of a sort of elongated body, and that body shaped itself so that it could be seen that it was going to be an animal of some kind, it having a large head, and the rudiments of eyes and vertebræ. On the fifth day they could clearly see what they were going to have. Gradually, step by step, and moment by moment, new differences made their appearance from the original foundation, and until many days before hatching there was an unmistakable bird, and at the twenty-first day there emerged from the shell an animal endowed with all a bird's capacities and structures. That process was the process of development. If they inquired into the nature of the cicatricula, they would find that that was merely a double layer of minute nucleated cells. They would find that that resulted from the splitting up of a protoplasmic mass that had been there before. They could trace the process back into the body of the hen until they came down to a simple nucleated cell, so that it was a matter capable of demonstration that in that nucleated cell which formed a part of the egg organ of the hen—in that particle of, for morphological purposes, structureless jelly, were the same characteristics which were possessed by the very lowest forms of animal life which were known. They knew that in that particle resided a potentiality, capable of developing itself through the stages he had roughly indicated, until it became not only a machine of the highest order from a physiological point of view, but a very remarkable work of art. That particle of protoplasmic matter did that in virtue of the powers inherent in its material nature. That was the point he wished to put before them as clearly and definitely as he could, because it would be fundamental in all further discussion. For it was to the process he had briefly described that the great discoverers of the last two centuries applied the name of "evolution." Singularly enough the persons who first used that name did not use it in that sense in which it was universally used now, because they were under a mistake as to the exact nature of the process. But the whole conception of evolution was now based upon ascertained facts, showing the process of development of the most complicated animal out of a relatively structureless particle, which had no higher organisation than that of the

lowest animal they knew, a process which progressed step by step by means of the gradual addition of small differences, until the animal attained its perfect form. That was what was meant by the process of evolution. At this point he thought it might be desirable that he should deal with what he might speak of as the *a priori* objections to the doctrines of evolution. He had had opportunities of making extensive acquaintance with those objections during the past twenty years or so. He divided them into three categories: (1) That evolution was impossible; (2) that it was immoral; and (3) that it was opposed to the argument of design. Now that was a very heavy indictment, but he thought they must plead "not guilty" upon all three counts. It required no great amount of reasoning to convince one that that which happened could not be impossible; that that which happened thousands and millions of times every hour and every minute in this world as it now was, under certain conditions, could not be held without further evidence to be impossible under somewhat different conditions. Secondly, with regard to the question of morality. He had never understood that argument, and had always been disposed to reply that the morality which opposed itself to truth committed suicide. With regard to the argument of design he would not discuss that point himself, but would beg them to listen for a moment to words that would carry far more weight than any of his own could carry on that topic:—"The philosopher beholds with astonishment the production of things around him. Unconscious particles of matter take their stations and severally range themselves in an order so as to become collectively plants or animals, *i.e.* organised bodies with parts bearing strict and evident relation to one another and to the utility of the whole; and it should seem that these particles could not move in any other way than they do, for they testify not the smallest sign of choice, or liberty, or discretion. There may be particular intelligent beings guiding their motions in each case, or they may be the results of trains of mechanical dispositions fixed beforehand by intelligence or appointment and kept in action by a power at the centre." They might imagine, and not unreasonably, that those were the words of some ultra-evolutionist of the present day who desired to set himself right with the argument from design; but they were not so. They were more than eighty years old, and they were contained in the 23rd chapter of a book which was very much talked about, but, he was afraid, very little read, namely, the "Natural Theology" of Archdeacon Paley. When he was a boy that book was a very great favourite of his, partly for its own merits, and partly because it was one of the few books he was allowed to read on Sundays. He found it much more entertaining than most of the books included in that category. But from what had been since said of the Atheistic tendencies of the doctrine of evolution he began to think that he stood before them a miserable example of the manner in which a man's mind might be poisoned by early instruction, and that his incapacity to understand the force of the arguments against evolution arose from the circumstance that in his early childhood he was indoctrinated with the reasonings of a great divine of the Church.—Professor Huxley now proceeded to consider the next point, the coming into existence of the nautilus species in contradistinction from the origin of a particular nautilus as an individual. He showed that, according to all the evidence that could be gathered, there was every reason to believe the forms of animal life five thousand years ago were practically the same as they were now. If there were no other means of knowing anything about the history of animal life, undoubtedly this experience, resting upon a duration of five thousand years, would have furnished an apparently sufficient basis for a generalisation, tending to the conclusion that the forms of animal life had not changed during that period. Not only had that generalisation been made, but it had been concluded that the forms of animal life were unchangeable, a totally different proposition, the validity of which rested, among other things, on the proportion between our actual experience, supposing it to extend over that time, and our possible experience of the duration of life on the globe. It would, he thought, be absolutely impossible for any of them, however good their vision, to say from actual observation of the hour hand of a watch for four seconds that it had moved during that interval, and in point of fact the space over which it would move was so minute as to be indiscernible, even through a magnifying glass. Yet they knew very well that it had moved, and if they watched it for four or five minutes, the evidence of its movement would be perfectly obvious, even to the naked eye. They would observe, therefore, that a period of observation which extended over the nine-hundredth part of an hour, would give them no conception from which it would be possible to draw a conclusion as to what had happened during the total period. Now geologists told them that the whole depth and extent of the fossiliferous rocks, which composed a considerable portion of the earth's crust, represented a period of time at least one thousand times as great as the historical period. That was a point upon which there could be no room for hesitation. Hence it followed that when they acquainted themselves with the succession of animal forms which were embedded at different depths in the earth's crust, they did exactly what the observer of a watch did when he kept his eyes fixed on it, not for four seconds but for an hour, in which latter case the movement was not only conspicuous, but such as commonly served to indicate the lapse of time. If that analogy held good, the slow procession of events which might be absolutely indiscernible in the course of 5,000 years, would become obvious and plain when the period of observation was extended to a thousand times that period. And that was exactly what happened, for if they went back in the series of stratified rocks they found the genus nautilus, which in the present day was represented by one or two species, represented in the long period of its history by many other species. As far back as the Upper Silurian formation the genus nautilus was represented by an abundant number of shells fabricated by animals having all the essential peculiarities which he had described. In the geological specimens before him, and which were taken from the rich collection in the Woodwardian Museum, there were forms of nautili which no one doubted were to all intents and purposes the same in their general structure as the pearly nautilus of the present day, although they were at least 5,000,000 years old. Now came the main question: were those nautili whose history extended back through such a prodigious range of time identical in character with the modern species? So far as he knew there was nothing in the nature of things to show why a succession of generations which remained unchanged through 5,000 years should not remain so for 50,000 or 50,000,000 years. The facts, however, showed that there had been rather more than 100 distinct species of nautilus, each having as good a title to be called a species as *Nautilus pompilius* itself. No one of these species had endured for more than a portion of the duration of the whole genus, and many species had existed contemporaneously, those species, however, except perhaps two, were now extinct, so that now they were brought face to face with the heart of the question: by what hypothesis could they account for those phenomena? They were driven into hypothesis of some kind or other, because it was impossible to have any evidence of contemporary witnesses of facts which went so far back into the past. So far as he knew there were only two possible alternative hypotheses by which they could pretend to account for those facts. One of these hypotheses was what he ventured to call the hypothesis of construction. That hypothesis was that every one of those species was put together. It was making a needless difficulty to suppose that each species came out of nothing, because they knew that the body of the nautilus was made up of materials which were familiar to them in an inorganic state on the earth's surface; so that by the hypothesis of construction some agency had put together those materials a hundred times or so during the period that had elapsed from the formation of the Silurian rocks to the present day, as an artist constructed his work, or as a mechanician put together the parts of his machine. That was one hypothesis. For his part, he had not a word to say *a priori* against the possibility of that hypothesis. It was certainly conceivable and therefore, according to Hume's maxim, it was possible. But they must bring it, like all other hypotheses, to the test of facts and inquire how far it stood that test. He thought the hypothesis of construction presented two large and almost insuperable difficulties. The one was that it was absolutely opposed to everything that they had received traditionally concerning the origin of animal forms, and the second was that it was no less opposed to every doctrine which might reasonably be held upon grounds of sane science. It stood to reason and common sense that they could have recourse only to those causes for the assumption of which there was some ground of analogy. The business of science would be extremely easy if for every event one were permitted to invent special causes having no analogy in nature. The difficulty of science was in tracing every event to those causes which were in present operation. That difficulty was being so constantly overcome that it had become a canon of

physical science no less than it was a canon of historical science that speculation should confine itself to construing past events by the analogy of those of the present time. The hypothesis of construction seemed to him unacceptable, because it led them into contravention of tradition on the one side and into contravention of scientific logic on the other. The only other alternative hypothesis was that of evolution, which meant that the different forms of animal life had not arisen independently of each other in the great sweep of past time, but that the one had proceeded from the other; and that that which had happened in the course of past ages had been analogous to that which took place daily and hourly in the case of the individual. That was to say that just as at the present day in the course of individual development the lower and simple forms, in virtue of the properties which were inherent in them, passed step by step by the establishment of small successive differences into the higher and more complicated forms, so, in the case of past ages, that which constituted the stock of the whole ancestry had advanced grade by grade and step by step until it had attained the degree of complexity which was seen at the present day. No objection could be brought against this hypothesis on the ground of analogy, because in putting it forward they were not bringing in any kind of causation which was not abundantly operative at the present time. The question was whether the history of the globe in past time coincided with this hypothesis, and to that point he would next address himself. What did they find if they considered the whole series of these forms? Unquestionably, as he had said, nautili were found as far back as the Upper Silurian age. Before that time there were no nautili, but there were shells of the *Orthoceratidæ*—of which there were magnificent examples before him—which resembled those of the nautili in that they were chambered, siphoned, &c., with the last chamber of such a size that it obviously sheltered the body of the animal. He thought no one could doubt that the creatures which fabricated these still earlier shells were substantially similar to the nautili, although their shells were straight, just as a nautilus shell would be if it were pulled out from a helix into a cone. Then came the forms known as the *cyrtoceras*, which were slightly curved. Along with these they had the other forms which were on the table, and in which the shell began to grow spiral. The next that came were forms of nautilus, which differed from the nautilus of to-day in that the *septa* were like watch-glasses, and that the whorls did not overlap one another. In the next series, belonging to the later palæozoic strata, the shell was closely coiled and the *septa* began to be a little wavy, and the whorls began to overlap one another. And this process was continued in later forms, down to that of the present day. Looking broadly at the main changes which the nautilus stock underwent, changes parallel with those which were followed by the individual nautilus in the course of its development, he considered that there could be no doubt that they were justified in the hypothesis that the causes at work were the same in both cases, and that the inherent faculty, or power, or whatever else it might be called, which determined the successive changes of the nautilus after it had been hatched, had been operative throughout the whole continuous series of existence of the genus from its earliest appearances in the later Silurian rocks up to the present day. What the whole question, in whatever way it might be put, came to, was this: Successive generations of animals were so many cycles of evolution that succeeded one another. Within the historical period, there was no doubt that, speaking roughly, those succeeding cycles had been identical, that was to say, without discernible difference. But when the period of observation became proportional to the slow rate of change they found, so to speak, that the hour hand had moved; for, in the successive cycles of evolution which had occupied the whole period, successive cycles had differed from one another to a slight extent. If they might assume that, then the whole of the phenomena of palæontology would fall into order and intelligibility. If not, they had to adopt an hypothesis which, as he had pointed out, had no support in tradition, and which was he held absolutely contradicted by every sound canon of scientific research. This was his case for evolution, which he rested wholly upon arguments of the kind he had adduced. From the time when he first read Charles Darwin's "Origin of Species," now some twenty-four years ago, his mind had fixed itself upon the tenth chapter of that book, which treated of the succession of forms in geological times, for it appeared to him that that was the key of the position; that if the doctrine of evolution was correct, the facts of palæontology, as soon as they became sufficiently known, must bear it out and verify it in every particular. On the other hand, he believed that, if the facts of palæontology or the historical facts of life on the globe were against evolution, then all the rest of the argumentation in its favour would be vain and empty, because the difficulty of adopting it would be in that case absolutely insuperable. He would venture to repeat that the occurrence of evolution was a question of history. He did not know whether Sir Henry Maine was not more competent to speak on that point than he was. It was a question as to whether they would interpret the facts of animated nature scientifically, or whether they would open the door to every description of hypothetical vagary. He came to the conclusion that that was a point worth testing in every possible way, and for some twenty years he had given what leisure he had been able to beg, borrow, or sometimes steal, to the investigation of these questions. He had endeavoured to ascertain for himself how the doctrine of evolution fitted with the facts of palæontology with regard to the higher vertebrated animals, and with regard to the chief varieties of invertebrate animals, and all he could tell them was that the farther his own investigations had gone, the more complete had appeared to be the coincidence between the facts of palæontology and the requirements of the doctrine of evolution. The conclusion he had come to was that at which every competent person who had undertaken a similar inquiry had arrived, and if they would pay attention to the writings of such men as Gaudry, Rütimeyer, Marsh, Cope, and others, who had added materials upon which to form a judgment such as were not dreamt of when Darwin first wrote, they would find that they all without hesitation attached themselves to the doctrine of evolution as the only key to the enigma. In deciding the issue between the two hypotheses, serious inquirers would not trouble themselves about any collateral points as to the how and the why, or as to any of the subordinate points at issue. He thought he was entitled to entreat those who by their calling or by their position in society, or by the fact that they possessed any influence, might be led to express an opinion upon this matter, to look into the arguments which formed the foundation of the case for evolution. Happily, he might address that recommendation to members of the University of Cambridge with a perfectly good conscience, for at this present time he knew not where in the world any one could find better means of passing through all those preliminary studies which were essential to a comprehension of this great question, or where any one could find more amply displayed the means of testing the arguments which he had laid before them. He ventured to say that the members of this University were without excuse if they gave opinions on this question of evolution without having prepared themselves, by as diligent study as they would for the purpose of approaching questions of literary or theological criticism, to express an opinion upon it. These were the considerations which he had wished to set before them that day. It would be understood that they would not suffice to enable any one to form a judgment upon the doctrine of evolution, but he hoped that they had sufficed, brief and insufficient as they were, to show that if judgment on this question was to be worth anything intellectually, if it was to be creditable to the moral sense of those who formed it, it would first be necessary that the facts should be clearly comprehended, and that the conclusion—whatever it might be—should be one which right reason would admit might be justly and perfectly connected with the facts.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

OXFORD.—The term that has just concluded has been chiefly noticeable for the interest drawn towards Oriental studies in the University by the building of the new Indian Institute. The visit of the Prince of Wales to the Chancellor of the University served to draw national attention to the work which Oxford, and especially Balliol College, has undertaken in respect to the training of the selected candidates for the Indian Civil Service. In spite of the failure of the late attempt to induce the University to relax its rule requiring three years' residence as a qualification for a B.A. degree in the case of the Indian Civil Servants, a considerable proportion of the selected candidates come into residence at the University; Balliol, by providing teachers and tutors in Oriental subjects, attracts by far the greatest number.

With the exception of two debates there has been little excitement during the term in the Convocation House. The two questions that roused general interest were, first, the proposal that

examiners in the Pass Schools should allow merit in one subject to compensate for a deficiency in another; and secondly, the decree to grant 10,000*l.* to Prof. Burdon Sanderson for the equipment of the new Physiological Laboratories. In the first debate the proposal was only carried by the casting vote of the Vice-Chancellor, and some doubt has since been raised on the qualification of one of the voters. In the second debate the opponents of vivisection, allied with those who oppose any large expenditure by the University on economic grounds, sought to throw out the decree and force the University to make special provision against the Professor experimenting on living animals. Prof. Burdon Sanderson in his speech disclaimed any intention of introducing vivisection into his courses or demonstrations, but declared that he would experiment on living animals in his own researches if he deemed it necessary for the discovery of truth. The decree was carried in a large house by 88 votes to 85.

The Commemoration in the Sheldonian Theatre passed off with less uproar this year. Among the recipients of honorary degrees Lord Rayleigh and Sir Frederick Abel represented Science.

In November next Balliol and Christ Church will hold examinations for electing to Natural Science Scholarships. The subjects at both colleges will be Chemistry, Physics, and Biology, with an essay and an optional paper in Mathematics.

CAMBRIDGE.—The Senior Wrangler in the Mathematical Tripos (Parts I. and II.) is Mr. Mathews, of St. John's College; Mr. Gallop is second; Mr. Lachlan, third; Messrs. Chevallier and A. N. Whitehead, bracketed fourth—all of Trinity College. One lady, Miss Perrin, of Girton, is placed between the 20th and 21st Wranglers. Three ladies are senior optimes; one is junior optime.

The Special Board for Mathematics have recommended that the Smith's Prize be awarded for the best essay on any subject in Mathematics or Natural Philosophy, to be sent in about a year and a half after the candidates are of standing for Parts I. and II. of the Mathematical Tripos; and that the adjudicators be the Vice-Chancellor, the Mathematical Professors, and the Cavendish Professor.

Prof. Foster has given notice of a revision class in Physiology during the Long Vacation, to be held at the Physiological Laboratory.

The proposed regulations for the Balfour Fund have been formally adopted by the senate.

The annual report of the Observatory states that, owing to the great progress made with the zone observations it has been possible to give much attention to the comets of recent years, and important contributions to the computations of their orbits have been made by the Observatory.

The General Board of Studies have published their recommendations with regard to new teachers, buildings, and appliances, and it is at once evident that several times the whole amount of the new income of the University could readily be spent in supplying the distinct wants of the several departments. They confine their recommendations as to Readerships within narrow limits owing to the extreme pressure upon University Funds, but they recommend the appointment of the present Readers in Indian Law, Classical Archæology, and Talmudic Literature as University Readers at 300*l.*, not 400*l.* as proposed by the Statutes, a Reader in Comparative Philology at 300*l.* and a Reader in Botany at 100*l.* a year, tenable with a College Lectureship. As to University Lecturers, that is College Lecturers who throw open some of their lectures to the University and give advanced lectures, they recommend, as regards Medicine, four University Lecturers; Mathematics, five; Biology—one in Botany, one in Zoology; three in Physiology; two at least in Geology; and others in other departments.

More Demonstrators, the senior to be better paid than at present, are further recommended to be appointed in various departments of Natural Sciences.

The appointment of a Professor of Pathology is again pressed as urgent; provision to be made for a temporary laboratory.

As to buildings, the Board have placed among urgent requirements the extension of the buildings for Physiology and Comparative Anatomy, Chemistry, Botany, Mechanism, and for Geology, to be partly supplied by the Sedgwick Memorial Fund. As extra to the latter fund, 10,000*l.* is asked.

A special grant of 500*l.* for physiological apparatus is recommended. Further, the Museum and Lecture Rooms require at least 350*l.* more annually from the new funds, in addition to 500*l.* asked for from the ordinary income of the chest.

The cost of a chemical laboratory is provisionally estimated at 15,000*l.*, and the purchase of Prof. Stuart's plant, with which he has at his own risk developed the flourishing school of engineering in the University, at 2,500*l.* Then permanent buildings for the school of Mechanism would cost 3,500*l.* more.

The recommendations of the General Board, after sifting and reducing the recommendations of the Special Boards, will entail annual charges of 4,360*l.*, in addition to at least 2,500*l.* required by new professorships or new elections to existing professorships.

Capital expenditure will be required for buildings 31,200*l.*, and for special grants other than building, 4,810*l.*; but it is proposed to borrow for these purposes. The Board have been informed that the special sum (500*l.*) asked for physiological apparatus will be provided by the liberality of a private donor who wishes to remain anonymous.

No voting can take place on these proposals till next term.

The special reports, on which the detailed Report of the General Board of Studies is founded, contain much interesting information about the present state of Natural Science studies in the University.

The Medical Board ask for provision for teaching in Medical Jurisprudence, Sanitary Science, Mental Diseases, and Elementary Medical and Surgical Methods. The number of students at present is about 200.

The report of the Classical Board contains an elaborate account of the present provision for studying Philology, Antiquities, Ancient Art, Topography, &c. which we cannot here reproduce.

The Board of Oriental Studies ask for University Lectures in Hebrew and Sanskrit, and for a Reader in Syriac, and that the Lord Almoner's Professor of Arabic be secured, if possible, as a resident professor by the augmentation of his stipend. They also urge the importance of establishing teaching in the departments of Egyptology and Assyriology.

The Mathematical Board estimate the resident students for Mathematical Honours as between 300 and 400. There are thirty-four College Lecturers in Mathematics, much of whose time is occupied in preparing candidates for the pass examinations. It will be impossible to secure adequate teaching of the subjects of Part III. of the Mathematical Tripos unless at least University Lecturers are appointed, and the Board ask for five at 50*l.* a year, two courses of advanced lectures being required from each lecturer every year.

The Board for Physics and Chemistry in addition to the laboratory claims press for additional means of catechetical teaching in Chemistry and instruction in Technical Electricity; in Mechanism a Superintendent of the Workshops, and in Mineralogy a Curator of the Museum. The number of students in Chemistry in the University is nearly 200; in Physics (Cavendish laboratory only), average for last two years, 54; Mechanism 42; Mineralogy 9.

The Board urge the advisability of Colleges permitting their lecturers in Chemistry and Physics to lecture in the University lecture rooms and laboratories, to allow more efficient organisation of teaching, as well as economy in expenses.

Lord Rayleigh asked for 600*l.* additional for Demonstrators in Physics, but the General Board of Studies have only recommended 220*l.* to be granted.

Professor Stuart's workshop has not more than half supported itself by fees of students as yet, but by employing the workmen in the manufacture of apparatus, &c., for other University departments after their hours of teaching, he has made a profit sufficient to pay the deficit, except the cost of demonstrators. A superintendent of the workshops is absolutely necessary if the University keeps up the school of Engineering, and a demonstrator in each department. Thus the Professor would not as now, be required to teach in the workshops and act as general manager as well as demonstrator. A considerably larger foundry is required, as the department has proved most useful.

The Board for Biology and Geology recommend the appointment of a Professor of Animal Morphology, and, failing this, three University Lecturers in this subject, one of whom shall direct the laboratory.

In relation to Physiology, Dr. Foster made an elaborate report, describing the organisation of, and work done in, his laboratory. He asked for a head demonstrator and four assistant demonstrators. As to advanced lectures, mention was made of the very largely unpaid work undertaken by Dr. Gaskell (entirely

unpaid), and Messrs. Langley and Lea, and University recognition of their work was asked for. Elementary Biology and Physiology of the Senses were also mentioned as needing a special lecturer.

With regard to Botany, teaching in Vegetable Morphology and in Physiology is urgently required, with lecture rooms and laboratories.

It is further asked that University teachers be eventually appointed in Agriculture, Anthropology, Geography, Metallurgy, and Mining.

In Geology it is pointed out that since Prof. Bonney left Cambridge, no College has given any assistance towards geological teaching, and that Dr. Roberts and the other demonstrators have received no University or College payments for the continued work they have done in lecturing and demonstrating.

The average number of students at present in the various departments of Biology and Geology is—Botany, 80; Geology, 40; Zoology, 75; Physiology, 120; Human Anatomy, 100 each term.

Donald MacAlister, M.D., M.B., Fellow and Medical Lecturer of St. John's College, Cambridge, was on Thursday, June 14, elected a member of the Council of the College.

The following awards have been made at St. John's College for proficiency in Natural Science:—Foundation Scholarships to Andrews, Kerr, Phillips (R.W.); Exhibitions to Goodman (already Scholar), Cooke, (E. H.), Fenton, Jones (H. R.), Watts; a Proper Sizarship to Gepp. Goodman obtains a Wright's Prize with augmentation of emoluments to 100*l.*, and a Hughes' Prize, as one of the two most distinguished third-year students in the College. The Open Exhibition was awarded to Rogers.

Mr. J. V. Jones, Principal of Firth College, Sheffield, has been elected by the Council to be the first Principal of the University College for South Wales and Monmouthshire.

SOCIETIES AND ACADEMIES

London

Royal Society, May 24.—"The Effects of Temperature on the Electromotive Force and Resistance of Batteries, II." By William Henry Preece, F.R.S.

In the discussion on the previous paper read on February 22, 1883, it was suggested that observations should be made on the influence of temperature to the case of secondary batteries. One of Mr. Tribe's cells was used.

The negative element of this cell consisted of pure peroxide of lead in the form of a plate 4 inches square carried in a grooved frame, from one end of which projected the necessary conductor. This element was placed between two plates of finely divided lead likewise 4 inches square. These were joined together, and formed the positive element of the cell. Each half of the positive plate was about a quarter of an inch distant from the negative, and all three plates were incased in a thin specially prepared fabric. The elements were contained in a leaden case, and the liquid was sulphuric acid of the strengths given in the various experiments. This cell was placed inside the cylindrical copper vessel used in the previous experiments, and precisely the same method of observation was adopted. The influence of heat on secondary cells was the same in kind as in the Daniell cell, but it differs very much in degree. The electromotive force practically remains constant for all degrees of temperature, but the internal resistance diminishes as the temperature increases at a very steady rate, increasing again as the temperature is lowered. The effect of varying the percentage of acid in solution is not very marked, though as might have been anticipated from Kohlrausch's observations, the 30 per cent. proportion gives the lowest resistance. The mean average reduction in resistance between 0° and 100° C., is 59·6 per cent.

Chemical Society, June 7.—Dr. Perkin, president, in the chair.—The following papers were read:—Laboratory notes by J. H. Gladstone and A. Tribe: (1) On the action of light and heat on cane and invert sugars; cane sugar solution, when heated, forms a small quantity of a substance which is not alcohol, but which gives the iodoform reaction. (2) On hydroxylamine; the copper zinc couple reduces this substance, ammonia being formed. (3) On the recovery of iodine from organic iodide residues; the residues are poured on to an excess of the couple, and the iodide of zinc formed, extracted with hot water; iodine is obtained in the free state by the action of hydrochloric acid and bleaching powder on the iodide. (4) A residual phenomenon of the electrolysis of oil of vitriol; the formation of Berthelot's persulphuric acid was noted. (5) On an alleged test for alcohol; Davy suggests that alcohol can be detected by the blue colour produced with a warm solution of molybdic anhydrid in oil of vitriol. The authors find that other reducing substances and sugar give the same reaction. (6) Reaction of the couple on nitric oxide; ammonia is formed, but no protoxide. (7) On the reducing action of spongy lead.—Note on a basic ammonio-copper sulphate, by S. U. Pickering.—Notes on Loew and Bokorny's researches on the probable aldehydic nature of albumin, by A. B. Griffiths.—Note on the action of sulphuric acid, sp. gr. 1·84, upon potassium iodide, by H. Jackson. The author has investigated this reaction quantitatively; he finds that two reactions occur, one with an excess of sulphuric acid when iodine and sulphur dioxide are formed; the second when just sufficient sulphuric acid is used to satisfy the potassium iodide; iodine and sulphuretted hydrogen are then liberated.—The action of nitrous anhydride on glycerin, by O. Masson. The author obtained the trinitrite of glyceryl; it is an amber-coloured liquid boiling at 150°, burns with a white flame, but does not explode under the hammer. It is decomposed by water, and cannot be preserved. In sealed tubes it generates sufficient gas to shatter the glass.

Linnean Society, June 7.—Sir John Lubbock, Bart., president, in the chair.—Mr. R. J. Clarke and Mr. Frank Matthews were elected Fellows of the Society.—Mr. W. T. Thiselton Dyer exhibited a series of Copals: some from Inhambane, near Mozambique, the product of *Copaifera Gorskiana* of various sorts, with a melting point from 310° to 360° Fahr.; others from Lagos (obtained by Capt. Moloney), used by the natives for burning, and powdered by the women as a body perfume. These last are supposed to be from a species of *Daniellia*, the native name being "Ogea."—Mr. Hiern drew attention to specimens of *Quercus Ilex*, var. *Fordii*, from Barnstaple, Devon, showing remarkable alteration in the leaves after pruning. There was exhibited for Mr. Stansfield R. Rake a burdock leaf with numerous excrescences, supposed to be the result of insect irritation.—Mr. G. Murray exhibited specimens of dace killed by the fungus disease (*Saprolegnia ferax*), the result of inoculation, and said to be the first recorded experimental proof of the communicability of the disease to those fish.—Dr. Cobbold showed shrimps sent by Dr. Burge of Shanghai. They contained immature flukes, which it was thought might prove to be the larval state of one or other of the three species of human fluke known to infest man in eastern countries. He proposed to call the parasite *Cercaria Burgei*.—A paper was read by Mr. H. N. Ridley, on new and rare monocotyledonous plants from Madagascar, among which may be mentioned species of *Dimia* hitherto only known from Africa, several curious orchids, one remarkable for possessing only one or two very large, handsome green, white, and purple flowers. Of Cyperaceæ one form well known as an Indian plant, another of the genus *Fimbristylis*, supposed to be confined to Brazil; he also describes a new genus, *Acriulus*, allied in some respects to *Cryptangium*.—A communication was read from Mr. George Lewis, on Japan Brenthidæ and notes of their habits. These beetles form part of the collection made by the author in his visit to Japan during the summers of 1880-81. He observes that there is no geographical barrier sufficient to exclude tropical forms from Japan, but their environment, when they reach it, prevents them from establishing themselves, to any great extent at least, in the northern parts. In the southern islands of the Japanese Archipelago the warmer climate enables a fair number of beetles of a truly tropical type to exist. The fact that each genus is only represented by one species nevertheless points to some physical check in their spread and numbers. A new genus, *Higonius*, is characterised, and several species of this and other genera described and illustrated.—Mr. T. H. Corry read a paper on the fertilisation of the Asclepiads, chiefly bearing out views noticed on a former occasion.—A short record of observations on the White Ants (Termites) of Rangoon, by Dr. Robert Romanis, was read by the Secretary. He details what he saw in what may be termed the swarming of a nest.

Edinburgh

Royal Society, June 4.—Mr. Thomas Gray, vice-president, in the chair.—Mr. Buchan read a second paper on the oscilla-

tions of the barometer, the conclusions of which were based largely on the *Challenger* observations. It appeared that the greatest diurnal oscillation occurred in regions over the sea where the air was very moist, being least indeed in those oceanic regions north and south of the equator where the average height of the barometer was greatest; whereas over land the contrary was the case, the greatest oscillations occurring where the air was driest. The explanation given was that over the ocean, whose surface changes very slightly in temperature throughout a whole day, the main effect results from the direct heating of the air and its contained moisture; while over the land the effect due to surface changes preponderates, being less, of course, the better the air acts as a screen to the solar rays, that is, the moister it is. Mr. Buchan then proceeded to account for the double maximum in the diurnal oscillations of the barometer. Beginning at six o'clock in the morning, an hour at which in general the barometer shows its daily mean, we find that the first effect of the sun is to heat the air, which tends to expand and rise. This tendency is of course somewhat resisted, so that the pressure is in the first instance increased; but by and by this resistance is overcome, the air flows freely upwards, the morning maximum is reached, and the pressure begins to fall. After noon this diminution in pressure is accelerated by the cooling of the air, for the same reason that the first effect of heating is to increase the pressure. Hence the barometer falls to its afternoon minimum. But as this is going on the region to the west is in its turn being heated, and an eastward movement of air overhead takes place towards the first locality, arresting the diminution of pressure, and then bringing it to a second maximum. This action, however, ceases as midnight comes on, the cooling of the air being then left to have its own effect, and the pressure falling to its second minimum till the approach of the sun on the east makes itself felt, and the same cycle of operations begins again. The modifications introduced by special conditions, such as the distribution of land and water, were also discussed, and explanations given of the retardation in certain places of the maxima and minima in time, and of the very slight, almost imperceptible, second minimum which in such cases frequently is found.—In presenting the last report of the Boulder Committee, Mr. Milne Home, the convener, intimated that the Committee purposed giving a general report in a form in which it could be readily compared with the British Association reports.—Mr. W. E. Hoyle read a paper on a new Entozoon from the mesentery of *Proteles cristatus* (Spairman). It is closely allied to *Pentastomum Dieringii* described by Van Beneden, belonging indeed to the same genus, but distinguished by its size, the number of its segments, and a slight difference in shape. The most curious point in its anatomy is that when the animal is encysted in the mesentery of its host the cirrus-sac is empty, and there is a stoppage in the vas deferens. The name proposed for the parasite is *P. Protelis*.

Mathematical Society, June 8.—Mr. J. S. Mackay, F.R.S.E., president, in the chair.—Mr. Thomas Muir, F.R.S.E., communicated some mathematical notes of interest to teachers and a new proof of Prof. Tait's problem of arrangement.—Mr. A. V. Fraser read a paper on the fundamental notions of the differential calculus; and Dr. C. G. Knott, F.R.S.E., discussed the singularities of plane curves.

BERLIN

Physiological Society, May 11.—Prof. Brieger reported on the further results of his study of the violent poisons formed by decomposition of animal bodies. In continuation of the communication he made a short time ago to the Society he described the process by which he had obtained from decomposing masses of flesh a substance which crystallised in acicular forms, and which he obtained by repeated crystallisation in such a degree of purity that he was able to analyse it. It afforded the empirical composition $C_5H_{14}N_2H_2Cl_2$, and consequently was a hydrochloric salt of a new base, which did not in its constitution resemble any known combination. This diamin-base had no longer the toxic properties of the extracts of the decomposition products. It was extremely easily decomposed, and could only be prepared from decomposing meat, and could neither be obtained in the later stages of putrescence nor from decomposing fibrin or other albuminous substances. Neither could it be demonstrated to be a constituent of meat. A second substance was obtained from the mother-liquor that remained after the crystallisation of the diamin-salt. This body, after purification by recrystallisation, showed the composition of $C_5H_{11}NCl$. This base proved to be a very virulent poison; 1 mg. in solution injected subcutaneously into a rabbit very soon produced the set of symptoms characteristic of fish-poison, *i.e.* salivation, quickened respiration, and diarrhœa, followed in a short time by death in convulsions. Even after the isolation of these exceedingly poisonous bases the mother-liquor contained other bases which have as yet not been more closely studied, and which belong to the group denominated by Prof. Brieger, "peptotoxine." They are more or less poisonous in their action upon the living organism, are decomposed with extraordinary facility, and are not only formed in the first stages of decomposition of masses of flesh, but are also contained in neurin in peptones.—Dr. George Hoppe-Seyler, who was present as a visitor, reported the results of the experiments that he made, starting on the basis of the chemical relation of nitrophenylpropiolic acid to indigo, which was studied by Herr Baeyer, in order to determine the physiological action of this acid, hoping thus to advance a step in the comprehension of the formation of indol and oxindol in the living organism. He found that oxindol appeared in the urine of rabbits into whose stomachs he had introduced solutions of nitrophenolpropiolate of soda, the animal in the meantime evincing no morbid symptom. When the solution was subcutaneously injected, blood appeared in the urine along with the oxindol, and when the treatment was continued for some time the constitution of the rabbits was injuriously affected so that they finally died, without, however, manifesting any characteristic symptoms. Dogs behaved quite otherwise when even a third part of that which the rabbits bore without inconvenience was introduced into their stomachs, increasing quantities of albumen and sugar appeared in their urine, and the animals succumbed to emaciation and loss of power. This very remarkable difference in the action of nitrophenylpropiolic acid on dogs and rabbits was not conditioned by the different diet, because when rabbits were driven to take to albuminous diet, by inanition or milk diet, until the reaction of the urine was acid, or when, on the other hand, the urine in dogs was made alkaline by giving them acetate of soda, the differences of the action remained unaltered, and their study promises a key to the comprehension of the origin of albuminuria and glycosuria.

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THURSDAY, JUNE 28, 1883

THE LINKS OF THE ANIMAL WORLD

Les Enchaînements du Monde Animal dans les Temps Géologiques. Fossiles Primaires. Par Albert Gaudry, professeur de Paléontologie au Muséum d'Histoire Naturelle. (Paris: F. Savy, 1883.)

M. GAUDRY, a distinguished palæontologist, contributed five years ago a very interesting volume on the important and much debated question of the mammalia of the Tertiary epoch (see NATURE, vol. xviii. p. 537). The volume which he now publishes relates to the same *quæstio vexata*, but takes into consideration only the fossils that are to be found in primary strata. The author's proposed task is the same in both cases; he undertakes to find the links and connections that may exist between the animals which have successively or simultaneously inhabited the lands and seas of past epochs.

A great deal has been written on the transformism-theory of Lamarck and Darwin, and it must be expected that much more will be written. One of the principal objections made to it is that if man is really the descendant of the ape, and the ape that of other mammalia, if, generally, there exist links between all animals, living and extinct, so that all animals trace their origin to a common ancestor, how is it that no link really exists between man and ape, or between fish and frog, or between vertebrate and invertebrate? Embryological considerations, it is said, show a real connection between very different animals: a frog for instance is a fish for some time during its youth, and amphioxus looks very much like an ascidian.

But, notwithstanding numerous arguments to support Lamarck's theory, no transformist can show any species gradually losing its peculiar characters to acquire new ones belonging to another species, and thus transforming itself. However similar the dog may be to the wolf, no one has found any dead nor living animal or skeleton which might as well be ascribed to wolf as to dog, and therefore be considered as being the link between the two. One may say exactly as much concerning the extinct species; there is no gradual and imperceptible passage from one to another. Moreover, the first animals that lived on this earth are not, by any means, those that one may consider as inferior and degraded.

M. Gaudry in the first pages of his work states very clearly that he prefers the theory according to which links do exist between the extinct animals of different groups, but he does not show that facts support it yet very strongly.

The opinion one may entertain as to this question being entirely dependent upon facts and the manner of understanding them, let us now turn over the leaves of M. Gaudry's book and see whether we can find in it some firm support to Lamarck's and Darwin's theories.

According to J. Barrande's numerations the number of the species contained in the Silurian strata, comprising the Cambrian, is the following:—

	Species.
Sponges and Protozoa	153
Corals	718
Echinodermata	588
Worms	185
Trilobites	1579
Other Crustaceans	348
Bryozoans	478
Brachiopods	1567
Lamellibranchs	1086
Heteropods and Pteropods	390
Gasteropods...	1316
Cephalopods	1622
Fishes	40
Of uncertain relations	4
	10,074

Thus, in the first of the primary strata nearly all the invertebrates are to be found—excepting insects—and the first vertebrate animals appear.

Generally speaking, and leaving *Eozoon canadense* and *Archæospharina* out of the question, worms are the oldest fossils to be found in the Silurian strata. It is so in England (Caerfay), in Scandinavia, in France, in Bohemia (Przibram), in America (St. John). In Russia, Crustaceans (Obolus) are the first fossils found. It must be therefore acknowledged that life has not begun on earth with the lowest forms. M. Gaudry, it must also be said, does not believe that such has been the case. M. Gaudry's principal aim is to show that, for instance, all Polyzoa somewhat resemble each other, and that the species are so very similar that the links between them are evident; but he does not pretend to show the relations between Worms and Crustaceans for instance, and try to find links between them.

M. Gaudry shows easily enough that, according to Nicholson's, Verril's, and Moseley's views, the arrangement and classification of Polyp corals, Zoantharia, Tubipora, Tabulata, and Rugosa is a very difficult thing inasmuch as by some points of their anatomy the species under consideration should come in one group, and by others come in another group. The same thing may be said of Echinoderms.

Between Starfishes and Crinoids there are many links; others exist between Sea-urchins and Starfish, and Holothurians. As concerns Brachiopods, M. Gaudry remarks that the oldest species of animal on earth is the Lingula, so abundantly found in the Lingula Flags of the English Silurian. It exists to this day, being the best example of "fine old age" yet recorded. Links between the various genera of Brachiopods are not yet very firmly established. Mr. Davidson is opposed to the idea, but Mr. N. Glass's (of Manchester) researches give on the contrary some support to it, by showing how much the arms of the Brachiopods vary and differ in different animals of the same species. Mr. Davidson says that it is often very difficult to ascertain exactly the species of the individuals. Though not having seen, generally speaking, a great number of individuals of the same species, I may say however that of about twenty *Atrypa reticularis* often seen and handled by myself in one of the Parisian geological collections, not two were strictly alike. I have no doubt that the same is true of all, or at least most, species.

Of Mollusks and Crustaceans, M. Gaudry says what he has already said of Brachiopods and other animals. As Dr. Woodward remarks, although Trilobites are always easily distinguishable from other Crustaceans (extinct, of course), one wonders at the astonishing variety afforded by this group of animals.

Let us pass over the Mollusks to see what M. Gaudry says of fishes and other Vertebrates.

Fishes begin in the Upper Silurian strata; they are abundant enough in the Ludlow beds (Pteraspis), and in the Downton grit (bone-bed—Pteraspis, Thelodus, Plectrodus, and Ctenacanthus). The fishes of the first ages of our globe were very singular animals; for instance, *Didymaspis grindrodi* wore on the back a *scutum* very much like that of a queer little Crustacean—at present very abundant in some places around Paris—named *Apus productus*. Pterichthys wore a yet stranger *scutum* which has been ascribed to insects, to Crustaceans, and to turtles before one could understand its meaning; it was incased in a bony helmet, and its fore-fins were also incased in a similar envelope, somewhat like the limbs of a crayfish or a lobster. Those primary fishes were sometimes devoid of a vertebral column, and nothing similar to these animals can be found among the living species. It must be therefore conceded that some links are missing, or that they have not existed.

As to Reptiles, they begin after fishes, in the Carboniferous and Permian strata, at the same time that Batrachia appear. The first of these, *Protriton petrolei*, has been discovered by M. Gaudry in the Permian strata of Autun, in France. *Pleuronoura pellatii*, *Branchiosaurus*, *Apateon*, and many others resemble somewhat the Protriton, and M. Gaudry remarks that these little animals are generally abundant in the same strata where Labyrinthodonts and similar animals are to be found. It may be that some of them are young Labyrinthodonts. Among Reptilia M. Gaudry seems inclined to consider Archegosaurus and Actinodon as the primitive type. They had no real vertebral column, the brain was imperfectly developed, and the limbs were rather imperfect. It is easy to perceive, by careful study of other Reptilia, that they differ from these only very slightly in some cases.

M. Gaudry comes to the following general conclusions:—

There are certainly links between the Silurian, Devonian, Carboniferous, and Permian species, and links exist between these and the actually living species of the same groups. Primary Foraminifera, for instance, are very similar to the actual species of our seas and oceans. This is true also of Brachiopods, Polyps, Mollusks, and Trilobites, but less so of Echinodermata. Brachiopods perhaps illustrate this general theory best, since they are, of all animal groups, the only one that has lasted from the beginning of animal life (Lower Silurian) to the present day.

As d'Omalius d'Halloy says, "It is scarcely credible that the Almighty Being whom I consider as the Author of Nature has, at different times, killed all living animals, to give himself the pleasure of creating new animals, which, very similar to the preceding ones, present successive differences, and display a marked tendency to blend with the actually living forms."

HENRY DE VARIGNY

COLIN CLOUT'S CALENDAR

Colin Clout's Calendar; The Record of a Summer, April-October. By Grant Allen. (London: Chatto and Windus, 1883.)

OF all the writers in this country who seek to render the facts and the theories of modern science attractive to the general public, Mr. Grant Allen is in our opinion among the most successful. We know that he does not profess to be in any serious manner an original investigator of these facts, and we are far from being always ready to accept his theories; but in most of his writings we meet with a characteristic ingenuity of thought, and perhaps a still more characteristic grace of style, which together render his essays the most entertaining in the kind of literature to which they belong.

It has recently been said in these columns, with express reference to Mr. Allen, that this kind of literature does more harm than good to the cause of science and to the advancement of the theory of evolution. But here, we think, the most that can be fairly said is that his zeal may sometimes be in danger of outrunning his discretion, so inducing him to trespass upon the domain of scientific questions which a more technical biologist would feel to be precarious ground. We should remember, however, that the function of a popular writer is to make his material attractive to the general reader, and if he succeeds in doing this for science, we think that he deserves to be encouraged by scientific men, even if they find that in running somewhat too fast over the grounds of theory he occasionally trips over matters of fact. Now, as we have said, Mr. Allen, considered as a literary man, is certainly a man of unusual ability, and he devotes his ability to diffusing an interest in biology among readers of periodical literature, who certainly could not be reached by any less attractive means. Moreover, he is a man of originality, both as regards thinking and observing, and if he were to devote less time to spreading out the sweets of science for popular consumption, there can be little doubt that he might do good work in collecting them.

But, be this as it may, we think that there should be no difference of opinion touching the service which Mr. Allen has rendered in his own province, even if we do not all go so far as to say with Mr. Wallace that he "certainly stands at the head of living writers as a popular exponent of the evolution theory." The book which we have now to notice is restricted to this province, and in its main features resembles those previous volumes which from time to time have been favourably reviewed in these pages. It consists of thirty-nine short papers republished from the *St. James's Gazette*, the greater number of which are devoted to botanical subjects. As the title of the collection suggests, these papers embody a number of observations and reflections on the natural history of plants and animals commonly met with in English country life; and as the essays are written in the least technical and most graphic language, they might be read with profit by all who take any intelligent interest in these things.

We may now give a few quotations, which will serve to show the general nature of the book:—

"But what is most interesting of all about the butterwort is the fact that it is peculiarly adapted for attracting

insects from two distinct points of view—for food, and as fertilisers. While it lays itself out to catch and eat miscellaneous small flies with its gummy leaves, it also lays itself out to allure bees with its comparatively large and handsome mask-shaped flowers. . . . Why should these totally distinct plants [butterwort and sundew], living in precisely similar circumstances, have acquired this curious and uncanny habit of catching and devouring live flies? Clearly, there must be some good reason for the practice: the more so as all other insect-eating plants —Venus's fly-traps, side-saddle flowers, pitcher-plants, bladderworts, and so forth—are invariably denizens of damp watery places, rooting as a rule in moist moss or decaying loose vegetation. Now, in such situations it is difficult or impossible for them to obtain those materials from the soil which are usually supplied by constant relays of animal manure; and under such circumstances, where the roots have no access to decaying animal matter, those plants would flourish best which most utilised every scrap of such matter that happened to fall upon their open leaves."

"The bird which came northward at the close of the glacial period, to inhabit the now thawed plains of northern Europe, much as the American partridge might take possession of Greenland if all its glaciers were to clear away in a more genial era, was doubtless a more or less southern and temperate type of grouse-kind. Coming into Britain, it would soon be entirely isolated from all its allies elsewhere; for it is of course a poor flyer for distance, and it inhabits only the northerly or westerly parts of our island which lie furthest from the Continent, separated from Holland and Scandinavia by a wide sea. Here it could not fail to be subjected to special conditions, differing greatly from those of the European mainland, partly in the equable insular climate, partly in the nature of the vegetation, and partly in the absence of many mammalian foes or competitors. These conditions would be likely first to affect the colouring and marking of the feathers, the spots on the bill, the naked scarlet patch about the eye, and so forth: for we know that even freer-flying birds in the south, which cross often with Continental varieties, tend slightly to vary in such ornamental points; and a very isolated group like the red grouse would be far more likely to vary in similar directions. Meanwhile, the main branch of the family, separated on the great continents from this slightly divergent group, would probably acquire the habit of changing its plumage in winter among the snows of the north, by stress of natural selection, just as the Arctic fox and so many other northern animals have done; for in a uniform white surface any variation of colour is far more certain to be spotted and cut off than in a many-coloured and diversified environment. Thus it would seem probable that the Scotch grouse has slowly become accommodated to the heather, among which it is so hard to discover; while the willow-grouse has grown to resemble the snow in winter, and the barer grounds of its northern feeding-places in the short Scandinavian and Icelandic summer.

"If this be so, we must regard both birds as slightly divergent descendants of a common ancestor, from which, however, our grouse has varied less than its Continental congener. Of course, it is just possible that the common ancestor had already acquired the habit of changing its coat in winter before the divergence took place; and if so, then it is the Scotch grouse which has altered most: but this is less probable, because the usefulness of the change would certainly be felt even in a Scotch winter, and the white suit is not, therefore, likely ever to have been lost when once acquired. Though the winter is not severe enough in Scotland to make such a change of coat inevitable where it does not already exist, it is yet quite severe enough to preserve the habit in animals which have once acquired it, as we see in the case of the varying hare, a creature which in colder ages spread over the whole of northern Europe, and which still holds its own among the chillier portions of the Scotch Highlands. Hence we may reasonably infer that if our grouse had ever possessed a winter coat it would have always retained it for an alternative dress, as the ptarmigan still does in the selfsame latitudes. Accordingly, analogy seems to point to the conclusion that the Scotch grouse is a truly native breed, slightly altered by the conditions of its insular habitat from a closely allied Continental species, whose representatives elsewhere have now all assumed the guise of Scandinavian willow-grouse. In other words, the two isolated groups into which the species has split up have altered each in its own way, but the Continental variety has moved faster away from the primitive type than its British congener."

But in thus recommending Mr. Allen's latest work, we do not wish to appear unduly tolerant of inaccuracy. All we should wish to say is that, assuming Mr. Allen or any other expositor of science to be an amateur not thoroughly versed in technical matters, and therefore liable to fall into technical errors, we do not feel on this account that he need be precluded from publishing his observations and his theories for whatever they may be worth. Sooner or later these are sure to be duly winnowed, and even though they may contain more chaff than Mr. Allen has been in the habit of presenting, they may also contain some seeds of germinative value.

GEORGE J. ROMANES

AGRICULTURE IN INDIA

Field and Garden Crops of the North-Western Provinces and Oudh. Part I. With illustrations. By J. F. Duthie, B.A., Superintendent of the Saharanpur Botanical Gardens, and J. B. Fuller, Assistant Director of Agriculture and Commerce, North-West Provinces and Oudh. (Printed at the Thomason Civil Engineering College Press, 1882.)

THIS *brochure* is the first of a short series in which it is proposed to describe the cultivated products of the North-West Provinces of India. With the exception of an introduction of considerable length, treating generally of the physical, social, and agricultural peculiarities of the North-Western Provinces, the volume is chiefly devoted to a description of farm crops. Many of these, such as wheat, barley, oats, maize, hemp, tobacco, millet, and poppy, are as familiar to European cultivators as to Asiatics. Others, such as opium, rice, sugar-cane, and cotton, betoken the tropical nature of at least a portion of the season. The botanical descriptions of the various crops are contributed by Mr. Duthie in the usual language of the text-books, affording little room for original remark of any kind. By far the greater portion of the work has been compiled from the reports of Settlement Officers and other Government records, or contributed by Mr. Fuller. The agricultural information is of a highly interesting character, and the illustrations are particularly excellent. The work is, however, in a manner disfigured and rendered obscure by the peculiar views of the authors as to the first rule of arithmetic. Sixty-seven millions, &c., are expressed as 6,79,06,496, and six millions, &c., as 64,96,567. Ten millions, &c., are written in figures as 1,09,57,837. This principle of notation renders the statistical portion of the work difficult to follow, and it is not easy to see why it has been adopted.

The text is not free from remarks betokening a want of knowledge as to the progress of research on certain points. When, for example, treating of the enemies which affect the wheat crop, the author (presumably Mr. Fuller) writes as follows :—"But by far the most extraordinary disease to which wheat is liable is *sehwan*, in which the young wheat-grains are found to be filled with minute worms . . . The most extraordinary fact connected with this disease is, however, that the worms can retain their vitality for a long time," &c. A footnote is then added as follows :—"Since the above was written, the worms have been identified as belonging to the order *Nematoidea*, and are apparently of the genus *Tylenchus*"! This is really too gross and wilful ignorance. The well-known and often-described "pepper brand" or "ear-cockle" attributable to the *Vibrio tritici*, now known as the *Tylenchus tritici*, is paraded as a "most extraordinary" disease, the precise nature of which has been ascertained "since the above was written." If such is the fact, the figures 1882 should be withdrawn from the title-page, and 1828 be substituted in their place. Neither do the authors appear to be at home in treating of the varieties of the cultivated plants. The varieties of rice, we are told, are more numerous and more strongly marked than those of any other crop. Forty-seven distinct varieties are announced, in support of this statement, as existing in Bareilly, although the writer proceeds somewhat naïvely to add, "Probably in the Provinces their number considerably exceeds 100." Now, as 300 varieties of wheat have been propagated by one naturalist, the forty-seven varieties of rice do not strike us as bearing out the statement as to the extraordinary variability of the plant.

Another point we cannot forbear to notice is the evident carelessness on the part of the authors as to whether their work should be understood by Englishmen in England. What is a "lakh," a "maund," or a "seer"? In vain we look for an English equivalent, and yet it is "maunds" *per acre* which are constantly spoken of, and which we long to translate into bushels or some intelligible unit of measure or weight. Viewing the volume as a whole, we cannot but pronounce it interesting and readable. The introduction is especially rich in information regarding the climate, irrigation, and cultivation of the North-West Provinces, a vast district comprising, in the quaint method of enumerating employed by the authors, 6,79,06,496 acres.

An alluvial soil and a climate by which the year is divided into two complete seasons certainly are conditions highly favourable to vegetation and to agriculture. In the colder season, wheat, barley, and oats are brought to perfection, while in the kharif, or hot season, rice, cotton, sugar-cane, and maize thrive. Not only do these highly-favoured provinces enjoy a temperate and a tropical climate, but each half of the year is again divided into two definite sub-seasons fitted for producing crops peculiar to it. We cannot but wonder whether the strange climatal vagaries to which the western world has latterly been exposed have disturbed the pleasant division of the year into kharif and rati in the North-West Provinces of India; but on this point our authors are silent.

JOHN WRIGHTSON

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Auroræ of October 2 and November 17, 1882

THE article of Mr. Rand Capron in the *Philosophical Magazine* on the aurora of November 17, 1882, has proved to me that my observation of that phenomenon has not been without value, but it appears that Mr. Capron has been obliged to consult a not quite accurate abstract or translation of my description which I had inserted in the *Utrecht Journal*; it was from lack of time that I did not immediately send translations of it to foreign periodicals.

Perhaps you will still be so kind as to admit into NATURE a translation of both my articles on the auroræ of October 2 and of November 17; they were written immediately after observing the phenomenon.

J. A. C. OUDEMANS

Utrecht, June 9

Translation of the Article in the "Utrecht Journal" of October 3, 1882

Yesterday evening, between seven and eight o'clock, there appeared here a brilliant aurora. Before seven there was visible a white arc running from west to east. This arc grew clearer and clearer, and rose slowly, till at 7h. 5m. (mean time) there emanated brilliant beams from its whole circumference.

At 7h. 10m. there rose also a bright column from the east-north-east, which phenomenon was repeated now and then also from the west.

In the southern part of the sky a magnificent sight was the large white spots, looking like brightly illuminated white clouds, but proving by their variable brilliancy to be no clouds, but to be connected with the auroral light. At 7h. 15m. such a large white spot was just before Aquila, *i.e.* north of the equator, but the spots showed a movement to the south, so that at 7h. 30m. the zone formed by them was some degrees south of the equator.

At 7h. 42m. there arose a large beam through the quadrangle of Ursa Major to β Ursa Minoris; this beam moved to the west and was dissolved a few minutes afterwards.

At 7h. 50m. there appeared again a cloudy patch in Aquila.

At 7h. 54m. a brilliant shooting star descended almost vertically from the space between Lyra and the Dragon through Hercules; this observation was, however, not made with accuracy.

At 8h. the last cloudy patches in the south vanished; in the north also I saw no more upward radiating beams.

Long afterwards the auroral light remained visible in the north. After 8h. 30m. I did not longer look for the phenomenon; it seemed to me to have come to an end.

Utrecht, October 3, 1882 J. A. C. O.

Translation of the Article in the "Utrecht Journal" of November 18, 1882

Yesterday we had again a brilliant aurora, differing entirely in its details from that of October 2.

In the east as well as in the west there appeared at 6h. mean time a red gleam like that of a distant fire. Both these extended red patches were united through the north by an arc much resembling that which appeared in the aurora of October 2. The splendid vertical beams remarked on that occasion were now fewer in number and not so intense; they were of the same reddish hue as the above-mentioned patches visible in the east and in the west.

At 6h. 23m. there appeared suddenly in the east a bright featherlike [1] appearance, which in the beginning showed some resemblance to a comet; the end of it was exactly above Aldebaran. In no more than two minutes this feather had lengthened over an arc that passed above Saturn, through the quadrangle of Pegasus, and south of the Aquila stars, and as the front or western end proceeded, the eastern end followed. With the aid of a star-map we see that the arc, covered successively by this featherlike appearance, was elevated 20° above the

[1] By this word I did not mean that the borders or edges were not fairly defined.

celestial equator, and that its intersections with that equator had R.A. 110° and 290°. The horizon was intersected by this arc about 18° N. of E. and 18° S. of W., *i.e.* in two points situated nearly 90° from the magnetic meridian (Mr. R. Capron cites 20° erroneously; a repeated calculation gives me 18° 14′ N. of E).

When the arc was visible over more than 90° (which lasted no longer than a few seconds), its middle part, having a breadth of about 3°, was separated by a dark rift, 10° long and ½° broad, sharp at both ends.

At 6h. 25m. the whole arc had vanished.

At 6h. 27m. a bundle of red rays appeared between Ursa Major and Ursa Minor; it extended from Polaris to γ Ursæ Minoris, *i.e.* over a width of almost 20 degrees; this bundle moved steadily to the west, just as had been the case with several vertical beams of the former aurora.

At 6h. 30m. clouds began to appear in different parts of the sky: first in Auriga, in the E.N.E.; a moment afterwards another in Aries. In the beginning it was not evident if they were clouds or auroral phenomena, but they proved soon to be clouds floating off southwards by the north wind.

The aurora had now lost much of its splendour; I noted still a few beams, viz.:—

At 6h. 32·5m. a beam about 10° east of Polaris; at 6h. 33m. the red beam, discovered at 6h. 27m., had so far shifted to the west that it went through the head of the Dragon; at 6h. 37m. there was still a faint beam extending from α Coronæ (visible in the twilight) to Vega, which stood higher and more to the south.

At 6h. 38m. there was a white band between α Aurigæ and α Persei; I was not sure if it was auroral or a cloud.

At 6h. 40m. there appeared two new very broad beams in the north-west.

Now the east becomes more and more covered; from the north to the west there extends itself a whitish glow to a height of 40 degrees; in the north-east this glow is much feebler and lower; therefore I estimate it as not higher than from 20 to 30 degrees. It flows off very indistinctly. The polar light seemed now to have passed its greatest intensity; the observations were no longer continued.

A part of the night a white auroral light remained still visible over the northern sky; at 9h. the sky was entirely overcast.

At midnight it was clear again, and the whole northern sky was always covered by an auroral light; I am told that it was still there at 5h. in the morning.

It is said to have been then in the north-west, of a purple colour, in the north-east more pale yellow. J. A. C. O.

Utrecht, November 18, 1882

Effects of Lightning

PERHAPS your readers will be interested in the following remarkable incident of the action of lightning which has just occurred at the village of Great Lumley, near Chester-le-Street, in Durham.

A severe thunderstorm passed over the valley of the Wear on Saturday, June 9. Great Lumley is situated on the top of an elevated plateau on the east side of this valley, and its houses are conspicuously exposed to the weather. At about 11 a.m., during the progress of the storm, an old stone house with steep pantiled roof, known as the "Old Hall," was struck by a thunderbolt. The house is only of moderate height, having three low stories; but it is on some of the highest ground in the village, and is also one of the loftiest houses therein. It is on the south side of the principal street forming the village, and it has two gable ends, which are, however, partially concealed at their lower portions by lower houses joining on. There is a yard in rear of the "Old Hall," separating it by a few yards from some smaller houses mostly inhabited by pitmen. The coal lies about seventeen fathoms below the surface, and there are several pits in the neighbourhood.

The principal damage done to the house is in the internal woodwork; but a hole has also been made in the tiled roof, the upper courses of a brick chimney-stack built over the centre of a brick partition wall which divides the building into two separate dwelling-houses have been thrown down, and some broken bricks and tiles were hurled as far as sixty feet distant to the southward. The building is about 220 years old, and is externally in a rather dilapidated condition. The masonry is rough sandstone rubble, the surface of which has in places been much disintegrated by age and weather. The building is Lord Durham's property, and is let in tenements. At the time of the accident the whole of its eastern dwelling-house was unfurnished and uninhabited. This was where the principal injuries were effected. The western compartment is occupied; and the inmates were knocked down by the concussion, various pieces of furniture being also damaged by the same cause; but nothing serious occurred. The building, being so old, has also experienced considerable damage externally from the tremendous concussion which appears to have accompanied the explosion of the thunderbolt. Loose tiles and stones have been dislodged; an old wooden frame in a window-opening in the loft at the east gable has been thrown to the ground below, and a considerable patch of surface masonry immediately above the window-opening has been shaken down. Falling bricks and stones have also made some holes in adjoining roofs; and the signs of injury shown by all these causes led the people in the adjacent houses to believe that these damages constituted the extent of the harm done by the explosion. I inspected the scene just three days after the event. The eastern and unused portion of the "Old Hall" had not then been examined internally, and the doors were still locked.

On visiting the interior I found that great injury had been done to a wooden rafter of the roof, close to the hole already mentioned; to a wooden upright post resting on the second floor and supporting this rafter; and to a wooden girder and joist sustaining the foot of the post. The plaster on the walls adjacent to the post and girder was also torn off; but below the first floor no certain trace of the explosion could be found. The damaged rafter is on the south side of the roof, and the nearest one on the east of the chimney. Its scantling is 6 inches broad by 3 inches deep; and at about one-third of its length from the eaves it is supported by a longitudinal purlin (on a level with the floor of the loft), and also (just inside the purlin) by the upright post already mentioned. From the level of the upper side of the purlin, for a length of about 6 feet upwards, the rafter is completely shivered, and two large pieces are torn out of it. One, the lower piece, is 3 feet 6 inches long, 3 inches wide, and about 1½ inch deep. This has been cut very neatly out of the west side of the under face of the rafter, leaving the new under face almost smooth; and the lower end of the scar thus formed is scooped out in a *dovetailed* form flush with the horizontal top of the adjacent purlin. No broken pieces corresponding to this scar could be found anywhere about the small loft; but some very small splinters of wood were discovered on the floor to the north-east of the rent rafter and in the direction of the window-opening already mentioned. This latter was quite open to the outer air when the accident happened, and immediately after it occurred numerous small splinters of wood were found in the yard to the eastward of the opening. These had evidently been blown out of it with the old window frame. Some were found against a low wall horizontally distant 63 feet from the opening. These splinters were not visible when I arrived, as they had at once been secured for firewood by the spectators; but I brought away one of the fragments found on the loft floor. Its fibres are quite disevered from each other, as if the wood had been completely *permeated* along their direction by the force of the explosion. The other scar in the rafter is cut out of the under face on the east side, partly alongside the first scar, but extending higher up and ending just under the hole in the roof tiles already mentioned. This second scar is 3 feet long by 4 inches by about 2 inches, and two pieces of wood were found lying on the loft floor about 8 feet distant to the north-east, which exactly fitted this scar. The hole through the roof is made at a point where a small iron nail fixed a lath (for the tiles) to the rafter.

The upright post underneath the rafter has a scantling of 8 inches by 3 inches; it is split and torn right up from its foot at the second floor (the next floor below the loft) to its junction with the rafter, to which it was fixed by several 2½-inch iron nails. The exact joint between the principal splits in the post and rafter seemed to be at the position of one of these nails, which was almost laid bare. I brought this nail away. Its upper two-thirds is quite rusty, whilst its lowest third (nearest the point) is clean and, in minute places, nearly bright. A heavy piece measuring 7 feet 6 inches by 6 inches by about 3 inches was torn clean off the north-west angle of this upright, and was found lying on the second floor about 3 feet off to the northward. The upright forms an angle post on the east side of a sort of dormer or porch projecting out of the south side of the roof, and formerly giving access from the outside to the second floor by a step-ladder. The latter, however, had disappeared shortly

before the accident occurred, having (as I understood) fallen down from age. The upright is split vertically in two places, one (where the piece was rent off) following exactly the line of the small iron nails which fixed the laths (for the wall plaster) to the upright, and the other about 2 inches to the east of this line. At this east split the outer portion of the upright is forced outwards about 4 inches from the centre portion, and all but separated from it. Along the line of the first split the lath nails are forced out of the upright, and the lath ends pushed outwards, and some broken off. A great patch of the plaster that covered them (the centre of it being about 4 feet above the floor) is torn off and thrown violently against the north wall of the building on the opposite side of the room 21 feet distant. The wall is dotted (high and low) with white powdery marks, and the floor at the foot of it is covered with broken and powdered plaster, as also is (more or less) the space of intervening floor. The splits in the upright unite upwards, and pass through the loft floor at a comparatively small orifice, and the piece rent out tapers considerably from the bottom upwards. The rent surface of this piece is minutely *fretted* in a curious manner. In the bottom of the rent off piece (which comprised the greater part of the sectional area of the post at this end) were three or four 2½-inch iron nails, probably used for fixing the post to the floor. These appeared to be driven and bent into the wood; there was no sign of fusing on them, and the surface of the floor immediately underneath the bottom of the post was not in the slightest degree damaged, so far as could be detected. On each side of the slight brickwork forming the east wall of the dormer or porch, a small patch of plaster (about 6 inches square or so) was broken off close to the floor, and about 3 feet southward of the post, but no other marks attributable to the explosion (and these small patches might have been due only to the concussion) could be discovered at the second floor.

On entering the room below (on the first floor), which has no ceiling, it was found that the wooden girder (9½ × 4½), on which the post rested, had a splinter 16 inches long and about 1 inch square (on an average) torn horizontally off the east side of it, 3 or 4 feet to the southward of the position of the bottom of the post, and about 6 feet from the south wall. The girder extends across the room, and rests on the north and south walls. Below the splinter, on the same girder, there was a horizontal crack, extending through the breadth of the girder, and proceeding about 8 feet along it northward, but stopping short of the position of the post above. It seemed, however, *possible* that there might be yet another horizontal split in this girder *close* to the under side of the floor above, and extending right up to the position of the post. Along the west side of the girder is a short joist (7 inches by 3 inches) passing through the south wall, extending about 4 feet into the room, and fixed to the girder by three large iron trenails; and just below it there was some more slight woodwork bedded in the wall, and apparently rotten. This joist was forced out from the other about 1 inch, and had a horizontal split passing exactly along the line of the heads of the three trenails, and completely separating it into two layers. A small piece of the rotten wood underneath (about 6 inches by 2 inches by ½ inch) was broken off, and thrown about 4 feet into the room on the floor to the north. An irregular patch of damp plaster about 4 feet by 18 inches, which had formerly concealed this rotten woodwork, was torn off, and most of it was (as in the room above) thrown hard against the opposite north wall, to which portions were still adhering. These portions are chiefly high up the wall, and near the floor above. Some larger pieces were also spread over the intervening space. The wall where this plaster was torn off was almost saturated with moisture, and the plaster round the rent piece was quite wet and discoloured.

There are no certain traces of the thunderbolt visible on the outside of the south wall, where, however, it most assuredly must have been present. The ground at the foot showed no signs of rending. There was a small lean-to outhouse nearly below, the roof of which was damaged; but I was led to understand that this had been done before; and as the place was locked, and my time was limited, I did not go inside it. At the angle made by one of the side walls of this outhouse with the main building, and not far from a point vertically below the position of the end of the girder, was a wet piece of ground habitually used for emptying slops at; and this seemed by no means an unlikely place for the thunderbolt to have originated. Here and there on the face of the dilapidated masonry some rather new looking abrasions were to be seen; but not even just abreast of the end of the girder could I detect for certain any traces of the explosion; and no metal of any kind was visible. In this connection it may be mentioned that there were no eaves-gutters, rain-water pipes, or metals of any sort on the outside of the house or on the roof.

Perhaps the most noteworthy feature of this accident was the *complete absence of any sign of burning or charring* at the rents in the girder, joist, post, and rafter. The nails struck also showed no symptoms of fusing; and, for all the *traces* that were left by the stroke, it might have been quite unaccompanied by heat. The work of the explosion seems to have taken altogether the form of mechanical violence. The wood of the post, rafter, and girder is sound, dry, old fir, and this would seem peculiarly liable to be set on fire.

The almost perpendicular bend that the course of the stroke seems to have taken from the girder to the post is also very curious. That the direction and force of the stroke was *upward* appears to me a conclusion quite irresistible. I have but little doubt in my own mind, from the traces left by the thunderbolt, that it sprang from the ground outside the building, at or near the wet south wall; passed up its outer face, entered the building through the wall at the rotten wood, and passed through or close to the joist and girder; then, attracted by the nails in the bottom of the post, it took a sudden turn upward (for there were no other marks of its course in the first floor room than those described), cleft right through the heart of the post, altered its course obliquely to gouge out the lower part of the rafter as far as the small nail, broke through the tiles, knocked off the chimney-top, and thence rushed to join the complementary force that had already started from the thundercloud to meet it.

A. PARNELL

13, Windsor Terrace, Newcastle-on-Tyne, June 14

The Soaring of Birds

IN NATURE, vol. xxvii. p. 535, Lord Rayleigh gives what he suggests as a possible explanation of the soaring of "pelicans and other large birds in Assam" mentioned by Mr. S. E. Peal. My own observations correspond so exactly with the theory advanced that I venture to give them for whatever they may be worth.

I have never indeed observed the flight of pelicans; but the Indian kite, the turkey buzzard, and perhaps all vultures, have the same habit of soaring in great circles. The *sandhill crane*, as it is commonly called in the United States, a large migratory crane, possesses this characteristic in a most remarkable degree. These birds will go soaring about for hours at an immense height, never seeming to move a pinion except once in a great while to steady themselves a little. They always move in irregular circles at such times, and there is always a drifting with the wind; but at such a great distance above one it would be impossible by mere ordinary observation to detect the obliquity of the circles if it existed.

A short time since, however, I had a fine opportunity of witnessing the soaring of some kites; the advantageous circumstances being that they were not far away, and that I saw them commence when they were so low that there was little chance of being mistaken in what I saw. I was sitting before an open window one day about eleven o'clock. There was a gentle breeze blowing from the south-east at the time. Presently my attention was attracted by several kites over the village to the north-west. The motions of two in particular I followed for some time. After moving their wings to attain an elevation above the houses and trees they began soaring, and continued upward in this manner to a height of perhaps two thousand feet, apparently making no exertion with their wings except to steady themselves a little occasionally. The method of accomplishing this was evidently to circle away to leeward in a great curve which inclined downward a little, thus acquiring considerable momentum; then turning toward the wind and adjusting the surfaces of the wings to the proper angle, they would shoot upward to a point considerably higher than the one from which the circle began. By the time the momentum was exhausted the bird was circling around again for another sweep to leeward.

There was considerable drifting with the wind, so that in attaining an elevation of some two or three thousand feet the birds had moved away nearly a quarter of a mile. Their consequent upward motion was in an irregular spiral, the highest parts of the curves being on the windward side.

Ongole, India, May 21 W. R. MANLEY

Geology of Cephalonia

In answer to the inquiry of your correspondent in the last number of NATURE (p. 173) I beg to inform him that the shells of the Pliocene formation in the Morea have been long since investigated, as is shown by the great and well-known work of Hörnes. And Dr. Fischer has published a list of the fossil shells from the same formation at Rhodes. These subapennine beds extend over the whole of the south of Europe. For many of those species which are still living I have given the localities of the Morea and Rhodes as fossil in the *Proceedings of the Zoological Society*.

J. GWYN JEFFREYS

June 25

On the Chemical Characters of the Venom of Serpents

Dr. WEIR MITCHELL calls my attention to an error in the brief notice which I wrote in NATURE recently (vol. xxviii. p. 114), on the researches into the chemical characters of snake poison conducted by him and Dr. Reichart. It is that instead of "They are unable to confirm the statement of Gautier of Paris that an alkaloid resembling a ptomaine exists in cobra poison; *or that of Prof. Wolcott Gibbs, that the poison of Crotalus yields an alkaloid*," it should be, "Prof. Wolcott Gibbs was unable to find an alkaloid."

J. FAYRER

53, Wimpole Street, W., June 26

Earthquake in South-West England

I HAVE just felt and heard the shock of an earthquake. The trembling of the earth was very great and the accompanying noise very loud, comparing it with one or two other slight shocks which I have before experienced in this district. I found the time to be 1.38 p.m. The time it lasted was several seconds. It was longer and louder than an ordinary clap of thunder when the lightning is not far off. A man reports that the slates of the cow-house were made to rattle.

As I now write (2.7 p.m.) a second shock has been felt, a little less severe. The weather is very calm, sky cloudy. This place is close to Dartmoor, on the westward side, about 500 feet above the sea-level.

W. F. COLLIER

Woodtown, Horrabridge, S. Devon, June 25

I BEG to inform you of the occurrence of two slight earthquake shocks here to-day, one shortly before 2 p.m., the other near half an hour later. The direction of progress seemed to be from north-west to south-east—that is along the line of the deep and narrow valley. The tremor was sufficient to cause jangling of glass and earthenware, and of the slates covering the house. The usual rumbling noise accompanied the shocks.

SAMUEL DREW

Penalla Terrace, Boscastle, Cornwall, June 25

ON WHALES, PAST AND PRESENT, AND THEIR PROBABLE ORIGIN[1]

FEW natural groups present so many remarkable, very obvious, and easily appreciated illustrations of several of the most important general laws which appear to have determined the structure of animal bodies, as those selected for my lecture this evening. We shall find the effects of the two opposing forces—that of heredity or conformation to ancestral characters, and that of adaptation to changed environment, whether brought about by the method of natural selection or otherwise—distinctly written in almost every part of their structure. Scarcely anywhere in the animal kingdom do we see so many cases of the persistence of rudimentary and apparently useless organs, those marvellous and suggestive phenomena which at one time seemed hopeless enigmas, causing despair to those who tried to unravel their meaning, looked upon as mere will-of-the-wisps, but now eagerly welcomed as beacons of true light, casting illuminating beams upon the dark and otherwise impenetrable paths through which the organism has travelled on its way to reach the goal of its present condition of existence.

[1] Lecture delivered at the Royal Institution on the evening of Friday, May 25, 1883, by Prof. Flower, LL.D., F.R.S., P.Z.S., &c.

It is chiefly to these rudimentary organs of the Cetacea and to what we may learn from them that I propose to call your attention. In each case the question may well be asked, granted that they are, as they appear to be, useless, or nearly so, to their present possessors, insignificant, imperfect, in fact *rudimentary*, as compared with the corresponding or homologous parts of other animals, are they survivals, remnants of a past condition, become useless owing to change of circumstances and environment, and undergoing the process of gradual degeneration, preparatory to their final removal from an organism to which they are only, in however small a degree, an incumbrance, or are they incipient structures, beginnings of what may in future become functional and important parts of the economy? These questions will call for an attempt at least at solution in each case as we proceed.

Before entering upon details, it will be necessary to give some general idea of the position, limits, and principal modifications of the group of animals from which the special illustrations will be drawn. The term "whale" is commonly but vaguely applied to all the larger and middle-sized Cetacea, and though such smaller species as the dolphins and porpoises are not usually spoken of as whales, they may to all intents and purposes of zoological science be included in the term, and will come within the range of the present subject. Taken all together the *Cetacea* constitute a perfectly distinct and natural order of mammals, characterised by their purely aquatic mode of life and external fishlike form. The body is fusiform, passing anteriorly into the head without any distinct constriction or neck, and posteriorly tapering off gradually towards the extremity of the tail, which is provided with a pair of lateral pointed expansions of skin supported by dense fibrous tissue, called "flukes," forming together a horizontally-placed, triangular propelling organ. The forelimbs are reduced to the condition of flattened ovoid paddles, incased in a continuous integument, showing no external sign of division into arm, forearm, and hand, or of separate digits, and without any trace of nails. There are no vestiges of hind-limbs visible externally. The general surface of the body is smooth and glistening, and devoid of hair. In nearly all species a compressed median dorsal fin is present. The nostrils open separately or by a single crescentic valvular aperture, not at the extremity of the snout, but near the vertex.

Animals of the order *Cetacea* abound in all known seas, and some species are inhabitants of the larger rivers of South America and Asia. Their organisation necessitates their life being passed entirely in the water, as on the land they are absolutely helpless; but they have to rise very frequently to the surface for the purpose of respiration. They are all predaceous, subsisting on living animal food of some kind. One genus alone (*Orca*) eats other warm-blooded animals, as seals and even members of its own order, both large and small. Some feed on fish, others on small floating crustacea, pteropods, and medusæ, while the staple food of many is constituted of the various species of Cephalopods, chiefly *Loligo* and other *Teuthidæ*, which must abound in some seas in vast numbers, as they form almost the entire support of some of the largest members of the order. With some exceptions the Cetacea generally are timid, inoffensive animals, active in their movements, sociable and gregarious in their habits.

Among the existing members of the order there are two very distinct types—the Toothed Whales, or *Odontoceti*, and the Baleen Whales, or *Mystacoceti*, which present throughout their organisation most markedly distinct structural characters, and have in the existing state of nature no transitional forms. The extinct *Zeuglodon*, so far as its characters are known, does not fall into either of these groups as now constituted, but is in some respects intermediate, and in others more resembles the generalised mammalian type.

The important and interesting problem of the origin of the Cetacea and their relations to other forms of life is at present involved in the greatest obscurity. They present no more signs of affinity with any of the lower classes of vertebrated animals than do many of the members of their own class. Indeed in all that essentially distinguishes a mammal from one of the oviparous vertebrates, whether in the osseous, nervous, vascular, or reproductive systems, they are as truly mammalian as any, even the highest, members of the class. Any supposed signs of inferiority are, as we shall see, simply modifications in adaptation to their peculiar mode of life. Similar modifications are met with in another quite distinct group of mammalia, the *Sirenia*, and also, though in a less complete degree, in the aquatic Carnivora or seals. But these do not indicate any community of origin between these groups and the Cetacea. In fact, in the present state of our knowledge, the Cetacea are absolutely isolated, and little satisfactory reason has ever been given for deriving them from any one of the existing divisions of the class more than from any other. The question has indeed often been mooted whether they have been derived from land mammals at all, or whether they may not be the survivors of a primitive aquatic form which was the ancestor not only of the whales, but of all the other members of the class. The materials for—I will not say solving—but for throwing some light upon this problem, must be sought for in two regions—in the structure of the existing members of the order, and in its past history, as revealed by the discovery of fossil remains. In the present state of science it is chiefly on the former that we have to rely, and this therefore will first occupy our attention.

One of the most obvious external characteristics by which the mammalia are distinguished from other classes of vertebrates is the more or less complete clothing of the surface by the peculiar modification of epidermic tissue called hair. The Cetacea alone appear to be exceptions to this generalisation. Their smooth, glistening exterior is, in the greater number of species, at all events in adult life, absolutely bare, though the want of a hairy covering is compensated for functionally by peculiar modifications of the structure of the skin itself, the epidermis being greatly thickened, and a remarkable layer of dense fat closely incorporated with the tissue of the derm or true skin; modifications admirably adapted for retaining the warmth of the body, without any roughness of surface which might occasion friction and so interfere with perfect facility of gliding through the water. Close examination, however, shows that the mammalian character of hairiness is not entirely wanting in the Cetacea, although it is reduced to a most rudimentary and apparently functionless condition. Scattered, small, and generally delicate hairs have been detected in many species, both of the toothed and of the whalebone whales, but never in any situation but on the face, either in a row along the upper lip, around the blowholes or on the chin, apparently representing the large, stiff "vibrissæ" or "whiskers" found in corresponding situations in many land mammals. In some cases these seem to persist throughout the life of the animal; more often they are only found in the young or even the fœtal state. In some species they have not been detected at any age.

Eschricht and Reinhardt counted in a new born Greenland Right Whale (*Balæna mysticetus*) sixty-six hairs near the extremity of the upper jaw, and about fifty on each side of the lower lip, as well as a few around the blowholes, where they have also been seen in *Megaptera longimana* and *Balænoptera rostrata*. In a large Rorqual (*Balænoptera musculus*), quite adult and sixty-seven feet in length, stranded in Pevensey Bay in 1865, there were twenty-five white, straight, stiff hairs about half an inch in length, scattered somewhat irregularly on each side of the vertical ridge in which the chin terminated, extending over a space of nine inches in height and two and a half inches in breadth. The existence of these rudimentary hairs must have some significance beyond any possible utility they may be to the animal. Perhaps some better explanation may ultimately be found for them, but it must be admitted that they are extremely suggestive that we have here a case of heredity or conformation to a type of ancestor with a full hairy clothing, just on the point of yielding to complete adaptation to the conditions in which whales now dwell.

In the organs of the senses the Cetacea exhibit some remarkable adaptive modifications of structures essentially formed on the Mammalian type, and not on that characteristic of the truly aquatic Vertebrates, the fishes, which, if function were the only factor in the production of structure, they might be supposed to resemble.

The modifications of the organs of sight do not so much affect the eyeball as the accessory apparatus. To an animal whose surface is always bathed with fluid, the complex arrangement which mammals generally possess for keeping the surface of the transparent cornea moist and protected, the movable lids, the nictitating membrane, the lacrymal gland, and the arrangements for collecting and removing the superfluous tears when they have served their function cannot be needed, and hence we find these parts in a most rudimentary condition or altogether absent. In the same way the organ of hearing in its essential structure is entirely mammalian, having not only the sacculi and semicircular canals common to all but the lowest vertebrates, but the cochlea, and tympanic cavity with its ossicles and membrane, all, however, buried deep in the solid substance of the head; while the parts specially belonging to terrestrial mammals, those which collect the vibrations of the sound travelling through air, the pinna and the tube which conveys it to the sentient structures within are entirely or practically wanting. Of the pinna or external ear there is no trace. The meatus auditorius is certainly there, reduced to a minute aperture in the skin like a hole made by the prick of a pin, and leading to a tube so fine and long that it cannot be a passage for either air or water, and therefore can have no appreciable function in connection with the organ of hearing, and must be classed with the other numerous rudimentary structures that whales exhibit.

The organ of smell, when it exists, offers still more remarkable evidence of the origin of the Cetacea. In fishes this organ is specially adapted for the perception of odorous substances permeating the water; the terminations of the olfactory nerves are spread over a cavity near the front part of the nose, to which the fluid in which the animals swim has free access, although it is quite unconnected with the respiratory passages. Mammals, on the other hand, smell substances with which the atmosphere they breathe is impregnated; their olfactory nerve is distributed over the more or less complex foldings of the lining of a cavity placed in the head, in immediate relation to the passages through which air is continually driven to and fro on its way to the lungs in respiration, and therefore in a most favourable position for receiving impressions from substances floating in that air. The whalebone whales have an organ of smell exactly on the mammalian type, but in a rudimentray condition. The perception of odorous substances diffused in the air, upon which many land mammals depend so much for obtaining their food, or for protection from danger, can be of little importance to them. In the more completely modified Odontocetes the olfactory apparatus, as well as that part of the brain specially related to the function of smell is entirely wanting, but in both groups there is not the slightest trace of the specially aquatic olfactory organ of fishes. Its complete absence and the vestiges of the aërial organ of land mammals found in the Mystacocetes are the clearest possible indications of the origin of the Cetacea from air-breathing and air-smelling terrestrial

mammalia. With their adaptation to an aquatic mode of existence, organs fitted only for smelling in air became useless, and so have dwindled or completely disappeared. Time and circumstances have not permitted the acquisition of anything analogous to the special aquatic smelling apparatus of fishes, the result being that whales are practically deprived of whatever advantage this sense may be to other animals.

It is characteristic of the greater number of mammalia to have their jaws furnished with teeth having a definite structure and mode of development. In all the most typical forms these teeth are limited in number, not exceeding eleven on each side of each jaw, or forty-four in all, and are differentiated in shape in different parts of the series, being more simple in front, broader and more complex behind. Such a dentition is described as "heterodont." In most cases also there are two distinct sets of teeth during the lifetime of the animal, constituting a condition technically called "diphyodont."

All the Cetacea present some traces of teeth, which in structure and mode of development resemble those of mammals, and not those of the lower vertebrated classes, but they are always found in a more or less imperfect state. In the first place, at all events in existing species, they are never truly heterodont, all the teeth of the series resembling each other more or less or belonging to the condition called "homodont," and not obeying the usual numerical rule, often falling short of, but in many cases greatly exceeding it. The most typical Odontocetes, or toothed whales, have a large number of similar, simple, conical, recurved, pointed teeth, alike on both sides and in the upper and under jaws, admirably adapted for catching slippery, living prey, such as fish, which are swallowed whole without mastication. In one genus (*Pontoporia*) there may be as many as sixty of such teeth on each side of each jaw, making 240 in all. The more usual number is from twenty to thirty. These teeth are never changed, being "monophyodont" and they are, moreover, less firmly implanted in the jaws than in land mammals, having never more than one root, which is set in an alveolar socket which is generally wide and loosely fitting, though perfectly sufficient for the simple purpose which the teeth have to serve.

Most singular modifications of this condition of dentition are met with in different genera of toothed whales, chiefly the result of suppression, sometimes of suppression of the greater number, combined with excessive development of a single pair. In one large group, the Ziphioids, although minute rudimentary teeth are occasionally found in young individuals, and sometimes throughout life, in both jaws, in the adults the upper teeth are usually entirely absent, and those of the lower jaw reduced to two, which may be very large and projecting like tusks from the mouth, as in *Mesoplodon*, or minute and entirely concealed beneath the gums, as in *Hyperoodon*,—an animal which is for all practical purposes toothless, yet in which a pair of perfectly formed though buried teeth remain throughout life, wonderful examples of the persistence of rudimentary and to all appearance absolutely useless organs. Among the *Delphinidæ* similar cases are met with. In the genus *Grampus* the teeth are entirely absent in the upper, and few and early deciduous in the lower jaw. But the Narwhal exceeds all other Cetaceans, perhaps all other vertebrated animals, in the specialisation of its dentition. Besides some irregular rudimentary teeth found in the young state, the entire dentition is reduced to a single pair, which lie horizontally in the upper jaw, and both of which in the female remain permanently concealed within the bone, so that this sex is practically toothless, while in the male the right tooth usually remains similarly concealed and abortive, and the left is immensely developed, attaining a length equal to more than half that of the entire animal, projecting horizontally from the head in the form of a cylindrical or slightly tapering pointed tusk, with the surface marked by spiral grooves or ridges.

The meaning and utility of some of these strange modifications it is impossible, in the imperfect state of our knowledge of the habits of the Cetacea, to explain, but the fact that in almost every case a more full number of rudimentary teeth is present in early stages of existence, which either disappear, or remain as concealed and functionless organs, points to the present condition in the aberrant and specialised forms as being one derived from the more generalised type, in which the teeth were numerous and equal.

The Mystacocetes, or Whalebone Whales, are distinguished by entire absence of teeth, at all events after birth. But it is a remarkable fact, first demonstrated by Geoffrey St. Hilaire, and since amply confirmed by Cuvier, Eschricht, Julin, and others, that in the fœtal state they have numerous minute calcified teeth lying in the dental groove of both upper and lower jaws. These attain their fullest development about the middle of fœtal life, after which period they are absorbed, no trace of them remaining at the time of birth. Their structure and mode of development has been shown to be exactly that characteristic of ordinary mammalian teeth, and it has also been observed that those at the posterior part of the series are larger, and have a bilobed form of crown, while those in front are simple and conical, a fact of considerable interest in connection with speculations as to the history of the group.

It is not until after the disappearance of these teeth that the baleen, or whalebone, makes its appearance. This remarkable structure, though, as will be presently shown, only a modification of a part existing in all mammals, is, in its specially developed condition as baleen, peculiar to one group of whales. It is therefore perfectly in accord with what might have been expected, that it is comparatively late in making its appearance. Characters that are common to a large number of species appear early, those that are special to a few, at a late period; alike both in the history of the race and of the individual.

Baleen consists of a series of flattened, horny plates, several hundred in number, on each side of the palate, separated by a bare interval along the middle line. They are placed transversely to the long axis of the palate, with very short spaces between them. Each plate or blade is somewhat triangular in form, with the base attached to the palate, and the apex hanging downwards. The outer edge of the blade is hard and smooth, but the inner edge and apex fray out into long, bristly fibres, so that the roof of the whale's mouth looks as if covered with hair, as described by Aristotle. The blades are longer near the middle of the series, and gradually diminish near the front and back of the mouth. The horny plates grow from a dense fibrous and highly vascular matrix, which covers the palatal surface of the maxillæ, and which sends out lamellar processes, one of which penetrates the base of each blade. Moreover, the free edge of these processes is covered with very long vascular thread-like papillæ, one of which forms the central axis of each of the hair-like epidermic fibres of which the blade is mainly composed. A transverse section of fresh whalebone shows that it is made up of numbers of these soft vascular papillæ, circular in outline, each surrounded by concentrically arranged epidermic cells, the whole bound together by other epidermic cells, which constitute the smooth cortical (so-called "enamel") surface of the blade, and which, disintegrating at the free edge, allows the individual fibres to become loose and to assume the hair-like appearance spoken of before. These fibres differ from hairs in not being formed in depressed follicles in the enderon, but rather resemble those of which the horn of the rhinoceros is composed. The blades are supported and bound together for a certain

distance from their base, by a mass of less hardened epithelium, secreted by the surface of the palatal membrane or matrix of the whalebone in the intervals of the lamellar processes. This is the "intermediate substance" of Hunter, the "gum" of the whalers.

The function of the whalebone is to strain the water from the small marine mollusks, crustaceans, or fish upon which the whales subsist. In feeding they fill the immense mouth with water containing shoals of these small creatures, and then, on their closing the jaws and raising the tongue, so as to diminish the cavity of the mouth, the water streams out through the narrow intervals between the hairy fringe of the whalebone blades, and escapes through the lips, leaving the living prey to be swallowed. Almost all the other structures to which I am specially directing your attention, are, as I have mentioned, in a more or less rudimentary state in the Cetacea; the baleen, on the other hand, is an example of an exactly contrary condition, but an equally instructive one, as illustrating the mode in which nature works in producing the infinite variety we see in animal structures. Although appearing at first sight an entirely distinct and special formation, it evidently consists of nothing more than the highly modified papillæ of the lining membrane of the mouth, with an excessive and cornified epithelial development.

The bony palate of all mammals is covered with a closely-adhering layer of fibrovascular tissue, the surface of which is protected by a coating of non-vascular epithelium, the former exactly corresponding to the derm or true skin, and the latter to the epiderm of the external surface of the body. Sometimes this membrane is perfectly smooth, but it is more often raised into ridges, which run in a direction transverse to the axis of the head, and are curved with the concavity backwards; the ridges moreover do not extend across the middle line, being interrupted by a median depression or *raphé*. Indications of these ridges are clearly seen in the human palate, but they attain their greatest development in the Ungulata. In oxen, and especially in the giraffe, they form distinct laminæ, and their free edges develope a row of papillæ, giving them a pectinated appearance. Their epithelium is thick, hard, and white, though not horny. Although the interval between the structure of the ridges in the giraffe's palate and the most rudimentary form of baleen at present known is great, there is no difficulty in seeing that the latter is essentially a modification of the former, just as the hoof of the horse, with its basis of highly developed vascular laminæ and papillæ, and the resultant complex arrangement of the epidermic cells, is a modification of the simple nail or claw of other mammals, or as the horn of the rhinoceros is only a modification of the ordinary derm and epiderm covering the animal's body differentiated by a local exuberance of growth.

(*To be continued.*)

THE PERAK TIN-MINES[1]

THIS interesting memoir, which forms part of the *Archives des Missions scientifiques et littéraires*, série iii. vol. ix., gives the result of a seven months' exploration of the Malay State of Perak, made by the author, who was sent by the French Government upon a mission of scientific inquiry into the Malay Archipelago in 1881. Perak, although an insignificant unit among even the smallest States of the world, its extreme dimensions being only 95×50 miles, or an area of less than 5000 square miles, has long been known as a tin-producing country, being mentioned in the narratives of Tavernier, and the Dutch and Portuguese navigators of the seventeenth century; but it is only since the large influx of Chinese miners, consequent upon the suppression of the Taeping rebellion, that it has become of first-rate importance.

[1] "Les Mines d'Étain de Perak." Par J. Errington de la Croix. 8vo. Paris, 1882.)

The success attained by the first-comers led to a rapid increase of the Chinese population, who arrived in such numbers as to be soon beyond the control of the feeble Malay Government, and the mining being carried on without any regulations as to boundaries, the miners became divided into two parties, who made war upon each other with varying success, the Sultan looking on impartially during the contest, but siding with the winners. The defeated party in 1872 having taken to piracy at sea, was suppressed by English gunboats, and a resident was appointed for the purpose of keeping order; but the Malays having revolted in 1875, when the resident was murdered, the country has since been placed under a British protectorate, with a native rajah, under the title of Regent. This has been attended with the happiest results, the country having made great progress during the last six years, under the vigorous and enlightened management of the resident, Hugh Low, Esq., C.M.G., and now bids fair, according to the author, to become the most considerable producer of tin in the world.

The mines worked up to the present are entirely alluvial or stream works, the watercourses being filled with sand and gravel deposits to a depth of 20 or 30 feet, resting upon a floor of pure china clay, apparently derived from the decomposition of the granitic rocks forming the numerous parallel ridges which traverse the country from north to south. The geological description is necessarily imperfect owing to the dense tropical vegetation which covers the entire county; but the author has been able to establish the presence of numerous quartz veins traversing the granites which are coarsely porphyritic in the centre and largely charged with tourmaline at the edges of the masses, in fact reproducing the phenomena observed in the north-western tin districts of Cornwall. No mines have as yet been opened in any of these veins, but the author speaks of blocks of tin ore weighing more than 1 cwt. as having been found in the immediate vicinity of the hills, which are evidently not far removed from their original position. The bulk of the production is, however, derived from smaller rounded crystalline masses and grains contained in the lower part of the alluvial gravel, the workable thickness ranging from 7 to 10 feet, and the proportion of clean ore or "black tin" from about 1 to 4½ per cent. by weight. This is remarkable for its purity, being almost entirely free from wolfram, arsenic, and other foreign substances, which are so troublesome to the Cornish tin-miner. The methods of working, mechanical preparations, and smelting of the ore are of the simplest possible kind, the work, with the exception of a few centrifugal steam-pumps, and of Chinese chain-pumps driven by water-wheels, being entirely carried out by manual labour, with furnaces and other appliances of the most primitive types. This simplicity adds considerably to the interest of the author's detailed and carefully illustrated description, which enables the reader to realise in imagination the conditions prevailing in our western districts in the days when the Phœnicians traded with the Cornish miners for tin at St. Michael's Mount. Under the new British rule, the country has made rapid progress, the output of tin having risen from 2059 tons in 1876 to 5994 tons in 1881, the whole of which is exported through Penang. As at the latter date the cost of production, including revenue charges of about 17*l.*, was estimated at about 61*l.* per ton, while the local selling price was 88*l.*, showing a profit of 45 per cent., the popularity of the business is sufficiently explained. It is not probable, however, that such large profits will continue to be realised after the more productive deposits have been exhausted. It does not appear from the narrative that European labour of any kind is employed, the workpeople belonging to three races, namely, Malay aborigines, Klings or coolies from Madras and the Malabar coast, and Chinese, the latter supplying the whole of the miners, smelters

and other artisans directly employed in producing the metal. The author has a very high opinion of the Chinese miners, who are described as sober, regular in work, and accustomed to cooperative enterprises, against which, however, must be set the defects of being addicted to excess in opium and gambling, besides being very quarrelsome and exceedingly superstitious. The latter failing is, however, of interest as reproducing the old European legends of guardian genii of the mine, the "Kobads" of Germany and "Knockers" of Cornwall, who require to be propitiated by sacrifices and kept in good humour by orderly behaviour on the part of the miners. Infractions of the last rules are punished by the withdrawal of the guardian gnome, who takes all the unwrought ore in the mine away with him.

The execution of the work, both as regards illustration and typography, are exceedingly good, and reflect great credit upon the French National Printing Office.

H. B.

THE SIZE OF ATOMS[1]

FOUR lines of argument founded on observation have led to the conclusion that atoms or molecules are not inconceivably, not immeasurably small. I use the words "inconceivably" and "immeasurably" advisedly. That which is measurable is not inconceivable, and therefore the two words put together constitute a tautology. We leave inconceivableness in fact to metaphysicians. Nothing that we can measure is inconceivably large or inconceivably small in physical science. It may be difficult to understand the numbers expressing the magnitude, but whether it be very large or very small there is nothing inconceivable in the nature of the thing because of its greatness or smallness, or in our views and appreciation and numerical expression of the magnitude. The general result of the four lines of reasoning to which I have referred, founded respectively on the undulatory theory of light, on the phenomena of contact electricity, on capillary attraction, and on the kinetic theory of gases, agrees in showing that the atoms or molecules of ordinary matter must be something like the 1/10,000,000, or from the 1/10,000,000 to the 1/100,000,000 of a centimetre in diameter. I speak somewhat vaguely, and I do so, not inadvertently, when I speak of atoms and molecules. I must ask the chemists to forgive me if I even abuse the words and apply a misnomer occasionally. The chemists do not know what is to be the atom; for instance, whether hydrogen gas is to consist of two pieces of matter in union constituting one molecule, and these molecules flying about; or whether single molecules each indivisible, or at all events undivided in chemical action, constitute the structure. I shall not go into any such questions at all, but merely take the broad view that matter, although we may conceive it to be infinitely divisible, is not infinitely divisible without decomposition. Just as a building of brick may be divided into parts, into a part containing 1000 bricks, and another part containing 2500 bricks, and those parts viewed largely may be said to be similar or homogeneous; but if you divide the matter of a brick building into spaces of nine inches thick, and then think of subdividing it farther, you find you have come to something which is atomic, that is, indivisible without destroying the elements of the structure. The question of the molecular structure of a building does not necessarily involve the question, Can a brick be divided into parts, and can those parts be divided into much smaller parts? and so on. It used to be a favourite subject for metaphysical argument amongst the schoolmen whether matter is infinitely divisible, or whether *space* is infinitely divisible, which some maintained, whilst others maintained only that *matter* is not infinitely divisible, and demonstrated that there is nothing inconceivable in the infinite subdivision of space. Why, even time was divided into moments (time-atoms!), and the idea of continuity of time was involved in a halo of argument, and metaphysical—I will not say absurdity—but metaphysical word-fencing, which was no doubt very amusing for want of a more instructive subject of study. There is in sober earnest this very important thing to be attended to, however, that in chronometry as in geometry, we have absolute continuity, and it is simply an inconceivable absurdity to suppose a limit to smallness whether of time or of space. But on the other hand, whether we can divide a piece of glass into pieces smaller than the 1/100,000 of a centimetre in diameter, and so on without breaking it up, and making it cease to have the properties of glass, just as a brick has not the property of a brick wall, is a very practical question, and a question which we are quite disposed to enter upon.

I wish in the beginning to beg you not to run away from the subject by thinking of the exceeding smallness of atoms. Atoms are not so exceedingly small after all. The four lines of argument I have referred to make it perfectly certain that the molecules which constitute the air we breathe are not very much smaller, if smaller at all, than 1/10,000,000 of a centimetre in diameter. I was told by a friend just five minutes ago that if I give you results in centimetres you will not understand me. I do not admit this calumny on the Royal Institution of Great Britain; no doubt many of you as Englishmen are more familiar with the unhappy British inch; but you all surely understand the centimetre, at all events it was taught till a few years ago in the primary national schools. Look at that diagram (Fig. 1), as I want you all to understand an



One centimetre. One millimetre.

FIG. 1.

inch, a centimetre, a millimetre, the 1/10 of a millimetre, and the 1/100 of a millimetre, the 1/1,000 of a millimetre, and the 1/1,000,000 of a millimetre. The diagram on the wall represents the metre; below that the yard; next the decimetre, and a circle of a decimetre diameter, the centimetre and a circle of a centimetre, and the millimetre, which is 1/10 of a centimetre, or in round numbers 1/40 of an inch. We will adhere however to one simple system, for it is only because we are in England that the yard and inch are put before you at all, among the metres and centimetres. You see on the diagram then the metre, the centimetre, the millimetre, with circles of the same diameter. Somebody tells me the millimetre is not there; I cannot see it, but it certainly is there, and a circle whose diameter is a millimetre, both accurately painted in black. I say there is a millimetre and you cannot see it. And now imagine *there* is 1/10 of a millimetre, and *there* 1/100 of a millimetre and 1/1000 of a millimetre, and *there* is a round atom of oxygen 1/1,000,000 of a millimetre in diameter. You see them all.

Now we must have a practical means of measuring, and optics supply us with it for thousandths of a millimetre. One of our temporary standards of measurement shall be the wave-length of light; but the wave-length is a very indefinite measurement, because there are wave-lengths for different colours of light, visible and invisible, in the ratio of 1 to 16. We have, as it were—borrowing an analogy from sound—four octaves of light that we know of. How far the range in reality extends above and below the range hitherto measured, we cannot even guess in the present state of science. The table before you (Table I.) gives you an idea of magnitudes of length,

[1] A lecture delivered by Sir William Thomson at the Royal Institution, on Friday, February 2. Revised by the Author.

TABLE I.—*Data for Visible Light.*

Line of Spectrum.		Wave-length in Centimetres.		Wave Frequency, or number of periods per second.
A	...	7·604 × 10⁻⁵	...	395·0 × 10¹²
B	...	6·867 ,,	...	437·3 ,,
C	...	6·562 ,,	...	457·7 ,,
D_1	...	5·895 ,,	...	509·7 ,,
D_2	...	5·889 ,,		
E	...	5·269 ,,	...	570·0 ,,
b	...	5·183 ,,	...	
F	...	4·861 ,,	...	617·9 ,,
G	...	4·307 ,,	...	697·3 ,,
H_1	...	3·968 ,,	...	756·9 ,,
H_2	...	3·933 ,,	...	763·6 ,,

and again of small intervals of time. In the column on the left you have the wave-length of light in fractions of a centimetre; the unit in which these numbers to the left is measured is the 1/100,000 (or 10^{-5}) of a centimetre. We have then, of visible light, wave-lengths from $7\frac{1}{2}$ to 4 nearly, or 3·9. You may say then roundly, that for the wave-lengths of visible light, which alone is what is represented on that table, we have wave-lengths of from 4 to 8 on our scale of 1/100,000 of a centimetre. The 8 is invisible radiation a little below the red end of the spectrum. The lowest, marked by Fraunhofer with the letter *A*, has for wave-length $7\frac{1}{2}$/100,000 of a centimetre. On the model before you I will now show you what is meant by a "wave-length;" it is not length along the crest, such as we sometimes see well marked in a breaking wave of the sea, on a long straight beach; it is distance from crest to crest of the waves. [This was illustrated by a large number of horizontal rods of wood connected together and suspended bifilarly by two threads in the centre hanging from the ceiling;[1] on moving the lowermost rod, a wave was propagated up the series.] Imagine the ends of those rods to represent particles. The rods themselves let us suppose to be invisible, and merely their ends visible, to represent the particles acting upon one another mutually with elastic force, as if of india-rubber bands, or steel spiral springs, or jelly, or elastic material of some kind. They do act on one another in this model through the central mounting. Here again is another model illustrating waves (Fig. 2).[2] The white circles on the wooden rods represent pieces of matter—I will not say molecules at present, though we shall deal with them as molecules afterwards. Light consists of vibrations transverse to the line of propagation, just as in the models before you.

[1] The details of this bifilar suspension need not be minutely described, as the new form, with a single steel pianoforte wire to give the required mutual forces, described below and represented in Fig. 2, is better and more easily made.

[2] This apparatus, which is represented in the woodcut, Fig. 2, is of the following dimensions and description. The series of equal and similar bars (B) of which the ends represent molecules of the medium, and the pendulum bar (P), which performs the part of exciter of vibrations, or of kinetic store of vibrational energy, are pieces of wood each 50 centimetres long, 3 centimetres broad, and 1·5 centimetres thick. The suspending wire is steel pianoforte wire No. 22 B.W.G. (·07 of a cm. diameter), and the bars are secured to it in the following manner. Three brass pins of about ·4 of a centimetre diameter are fitted loosely in each bar in the position as indicated; *i.e.* forming the corners of an isosceles triangular figure, with its base parallel to the line of the suspending wire, and about 1 mm. to one side of it. The suspending wire, which is laid in grooves cut in the pins, is passed under the upper pin, outside the pin at the apex of the triangle, over the upper side of the lower pin, and thence down to the next bar. The upper end of this wire is secured by being taken through a hole in the supporting beam and several turns of it put round a pin placed on one side of the hole, as indicated in the diagram. To each end of the pendulum bar is made fast a steel spiral spring as shown; the upper ends of these springs being secured to short cords which pass up through holes in the supporting beam, and are fastened by two or three turns taken round the pins. These steel springs serve as potential stores of vibrational energy alternating in each vibration with the kinetic store constituted by the pendulum bar. The ends of the vibrating bars (B) are loaded with masses of lead attached to them. The much larger masses of lead seen on the pendulum bar, which are adjustable to different positions on the bar, are, in the diagram, shown at the smallest distance apart. The lowermost bar carries two vanes of tin projecting downwards, which dip into viscous liquid (treacle diluted with water) contained in the vessel (*c*). A heavy weight resting on the bottom of this vessel, and connected to the lower end of the suspending wire by a stretched indiarubber band, serves to keep the lower end of the apparatus in position. The period of vibration of the pendulum bar is adjustable to any desired magnitude by shifting in or out the attached weights, or by tightening or relaxing the cords which pull the upper ends of the spiral springs.

Now in that beautiful experiment well known as Newton's rings we have at once a measure of length in

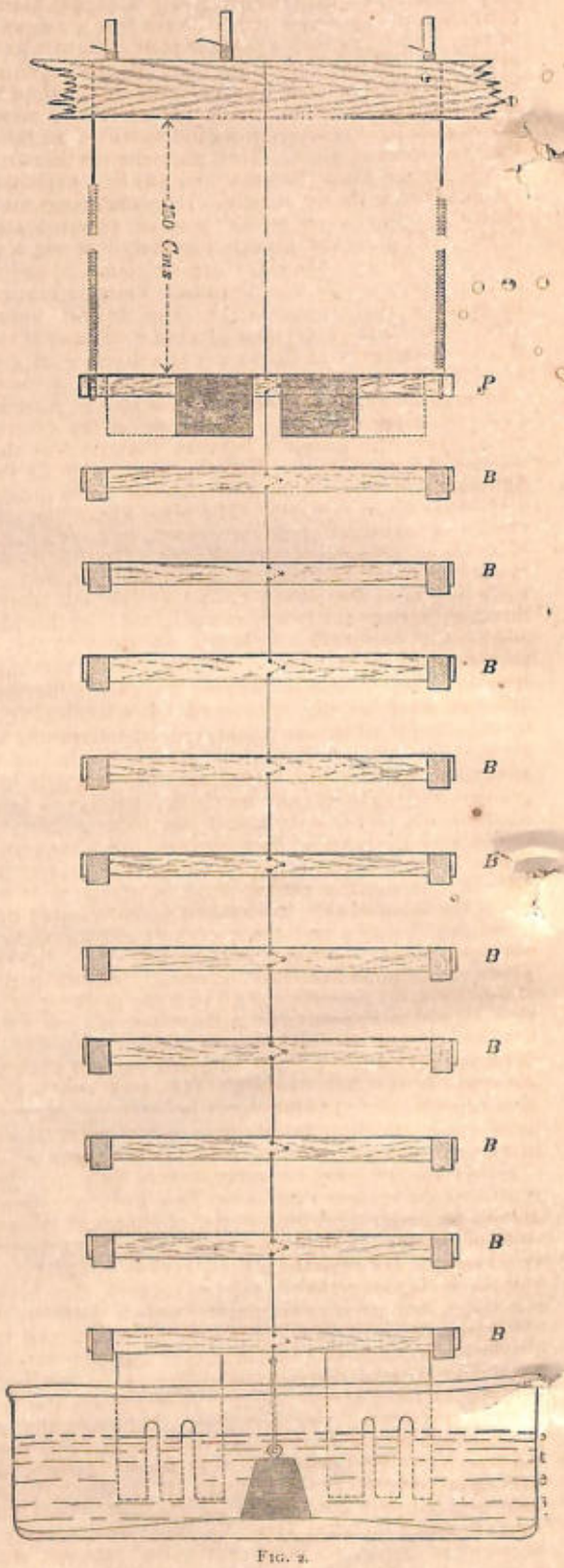


FIG. 2.

the distance between two pieces of glass to give any pa

ticular tint of colour. The wave-length you see, in the distance from crest to crest of the waves travelling up the long model when I commence giving a simple harmonic oscillation to the lowest bar. I have here a convex lens of very long focus, and a piece of plate glass with its back blackened. When I press the piece of glass against the glass blackened behind, I see coloured rings; the phenomenon will be shown to you on the screen by means of the electric light reflected from the space of air between the two pieces of glass. This phenomenon was first observed by Sir Isaac Newton, and was first explained by the undulatory theory of light. [Newton's rings are now shown on the screen before you by reflected electric light.] If I press the glasses together, you see a dark spot in the centre; the rings appear round it, and there is a dark centre with irregularities. Pressure is required to produce that spot. Why? The answer generally given is, because glass repels glass at a distance of two or three wave-lengths of light; say at a distance of 1/5,000 of a centimetre. I do not believe that for a moment. The seeming repulsion comes from shreds or particles of dust between them. The black spot in the centre is a place where the distance between them is less than a quarter of a wave-length. Now the wave-length for yellow light is about 1/17,000 of a centimetre. The quarter of 1/17,000 is about 1/70,000. The place where you see the middle of that black circle corresponds to air at a distance of less than 1/70,000 of a centimetre. Passing from this black spot to the first ring of maximum light, add half a wave-length to the distance, and we can tell what the distance between the two pieces of glass is at this place; add another half wave-length, and we come to the next maximum of light again; but the colour prevents us speaking very definitely because we have a number of different wave-lengths concerned. I will simplify that by reducing it all to one colour, red, by interposing a red glass. You have now one colour, but much less light altogether, because this glass only lets through homogeneous red light, or not much besides. Now look at what you see on the screen, and you have unmistakable evidence of fulcrums of dust between the glass surfaces. When I put on the screw, I whiten the central black spot by causing the elastic glass to pivot, as it were, round the innumerable little fulcrums constituted by the molecules of dust; and the pieces of glass are pressed not against one another, but against these fulcrums. There are innumerable—say thousands—of little particles of dust jammed between the glass, some of them of perhaps 1/3,000 of a centimetre in diameter, say 5 or 6 wave-lengths. If you lay one piece of glass on another, you think you are pressing glass on glass, but it is nothing of the kind; it is glass on dust. This is a very beautiful phenomenon, and my first object in showing this experiment was simply because it gives us a linear measure bringing us down at once to 1/100,000 of a centimetre.

Now I am just going to enter a very little into detail regarding the reasons that those four lines of argument give us for assigning a limit to the smallness of the molecules of matter. I shall take contact electricity first, and very briefly. If I take these two pieces of zinc and copper and touch them together at the two corners, they become electrified, and attract one another with a perfectly definite force, of which the magnitude is ascertained from absolute measurements in connection with the well-established doctrine of contact electricity. I do not feel it, because the force is very small. You may do the thing in a measured way; you may place a little metallic knob or projection on one of them of 1/100,000 of a centimetre, and lean the other against it. Let there be three such little metal feet put on the copper; let me touch the zinc plate with one of them, and turn it gradually down till it comes to touch the other two. In this position, with an air-space of 1/100,000 of a centimetre between them, there will be positive and negative electricity on the zinc and copper surfaces respectively, of such quantities as to cause a mutual attraction amounting to 2 grammes weight per square centimetre. The amount of work done by the electric attraction upon the plates while they are being allowed to approach one another with metallic connection between them at the corner first touched, till they come to the distance of 1/100,000 of a centimetre, is 2/100,000 of a centimetre-gramme, supposing the area of each plate to be one square centimetre.

(*To be continued.*)

DEATH OF THE PRESIDENT OF THE ROYAL SOCIETY

IT is with the profoundest regret that we announce the death of Mr. Spottiswoode, the President of the Royal Society, at 11.15 yesterday morning. The bulletin issued on Tuesday to the effect that although there was no hemorrhage, still that there was no improvement in Mr. Spottiswoode's condition, boded ill because those who knew him best feared that a reserve of strength, which might perhaps have made way against the further progress of the fever through its later stages, was wanting.

As the sad news reaches us just as we are going to press, and as indeed we so recently entered at some considerable length into the lifework of him who is now no more, there is no necessity for us on the present occasion to do more than make the above announcement. This, however, must be said: that there is hardly a man of science in this country, and there are very many in other countries, who will not feel that they have lost a true friend, and one of whose friendship any man might have been proud. There is little doubt too that if he had been more sparing of himself in the various duties which were incumbent upon him as President of the Royal Society, if he had not so freely given all his thoughts and all his exertions to any scientific question which was going on, there might have been more time for relaxation, and there might have been strength to have tided over the illness which has now laid him low.

NOTES

WE regret to have to announce the death of General Sir Edward Sabine, K.C.B., which occurred on the 26th inst. at Richmond, where he had been residing for the last twelve months. He was in his ninety-fifth year, having been born October 14, 1788.

AT the meeting of the Paris Academy of Sciences on Monday last week the following message concerning the eclipse observations from M. Janssen, dated San Francisco, was read:—"*Janssen:* discovery of the Fraunhofer spectrum and the dark lines of the solar spectrum in the corona, showing cosmical matter around the sun. Large photographs of the corona and the circumsolar regions to a distance of 15°, in search for intra-Mercurial planets. *Palisa* and *Trouvelot:* Exploration of the circumsolar regions; no intra-Mercurial planets found. *Trouvelot:* Sketch of the corona. *Tacchini:* Polarisation of the corona and streamers; spectrum of the streamers, showing analogy

with the spectrum of comets; continuous spectrum corona; spectrum of protuberances; plates and drawings of protuberances."

THE American Association for the Advancement of Science will hold its thirty second annual meeting at Minneapolis, Minn., August 15 and following days. The president-elect is Prof. C. A. Young of Princeton, and the following is the list of the sectional vice-presidents of the meeting:—Section A (Mathematics and Astronomy), W. A. Rogers of Cambridge; B (Physics), H. A. Rowland of Baltimore; C (Chemistry), E. W. Morley of Cleveland; D (Mechanical Science), De Volsen Wood of Hoboken; E (Geology and Geography), C. H. Hitchcock of Hanover; F (Biology), W. J. Beal of Lansing; G (Histology and Microscopy), J. D. Cox of Cincinnati; H (Anthropology), O. T. Mason of Washington; I (Economic Science and Statistics), F. B. Hough of Lowville. The permanent secretary is F. W. Putnam of Cambridge; the general secretary (of the meeting), J. R. Eastman of Washington.

THE annual meeting of the American Academy of Arts and Sciences was held in Boston, Tuesday, May 29. The following officers, we learn from *Science*, were elected for the ensuing year:—President, Prof. Joseph Lovering; vice-president, Dr. Oliver Wendell Holmes; corresponding secretary, Prof. Josiah P. Cooke; recording secretary, Prof. John Trowbridge; treasurer, H. P. Kidder; librarian, S. H. Scudder. M. Adolph Wurtz of Paris was elected a foreign honorary member. The list of members of the Academy now includes 192 resident Fellows, 92 associate Fellows, and 72 foreign honorary members. The Academy voted unanimously to confer the Rumford gold medal upon Prof. Henry A. Rowland of Baltimore for his researches in light and heat.

A NEW mode of measuring light was proposed at the last meeting of the Royal Society by Mr. Preece, F.R.S. The standard of reference is a small surface illuminated to a given intensity, and the mode of comparison is the light given by a small glow lamp whose state of incandescence is raised or lowered by increasing or diminishing an electric current. The amount of illumination is measured by the amount of current flowing, so that the number of amperes gives the degree of illumination. The standard surface is that illuminated by a British "candle" at 12·7 inches, and this is the same as that produced by the French "bec" at 1 metre distance. In this way sunlight, moonlight, twilight, fog, and the amount of illumination in any part of a room or building, or that distributed over a street or area at any time of day or night can be measured without any reference to the source of light or its distance from the point lighted. We have in fact a standard of illumination very easily and simply measured.

PROF. BUREAU has been appointed Director of the Jardin des Plantes in place of the late M. Decaisne.

ON Saturday, June 16, a joint meeting of the Essex Field Club and the Geologists' Association was held at Grays for the purpose of visiting the "Deneholes" in Hangman's Wood. From fifty to sixty members and visitors, including many members of the Anthropological Institute, were present, and nearly all had an opportunity of descending one or both of the two holes which were exhibited for the occasion. The meeting was under the conductor hip of Mr. T. V. Holmes, F.G.S., who has written a paper giving an account of last year's preliminary exploration, which will shortly appear in the *Transactions of the Essex Field Club*. Photographs of the interior of one of the holes were successfully taken by Mr. A. J. Spiller by means of magnesium burning in oxygen. The party assembled for tea at the "King's Arms" Hotel, when the president of the Club, Prof. G. S. Boulger, and Mr. Holmes announced that it was the intention of the Club to undertake the systematic investigation of these interesting prehistoric remains, both at Grays and elsewhere along the Essex shore of the Thames. A large fund will be required for this work, and a committee has been formed for the purpose of organising the explorations, which will be carried on under the personal superintendence of Mr. T. V. Holmes and Mr. F. C. J. Spurrell. After some remarks by Dr. Hicks, the president of the Geologists' Association, the meeting broke up. A public appeal for assistance will shortly be made, and in the meantime subscriptions will be gladly received by the treasurer of the Essex Field Club, Mr. Andrew Johnston, J.P., The Firs, Woodford, or by the Hon. Secretary, Mr. William Cole, Laurel Cottage, Buckhurst Hill, to be paid to the account of the "Denehole Exploration Fund."

IT will be seen from our Correspondence Columns that an earthquake was felt in the south-west of England on Monday. The shock seems to have spread very widely over Devonshire and East Cornwall. At Holsworthy, Devonshire, a very perceptible shock was felt at seventeen minutes to two o'clock that afternoon. Floors shook, and doors and windows rattled as from a passing train. No damage is reported. A severe shock was felt at Hartland and Clovelly at 1.30 p.m., and a second shock at Clovelly at 1.40. Houses shook considerably, and the bottles on counters in shops were knocked against each other. A similar statement is sent from Bude. The inhabitants of Princetown and the vicinity of Dartmoor, about two o'clock were startled by two smart shocks, followed by a subterranean rumbling like the passing of a very heavy waggon, or the echo of distant thunder. The first trembling was of sufficient intensity to be perceptibly felt by those who happened to be occupying a chair, and the like effect was produced on small movable objects, but it resulted in no mischief. The disturbances apparently travelled from north east to west. At Launceston at twenty minutes to two a shock was felt, accompanied by a rumbling noise, which lasted at intervals during about thirty minutes. The houses shook, and china and earthenware rattled on the shelves. About an hour afterwards another shock was felt, but not so severe. Similar information comes from Lostwithiel, Liskeard, Lydford, Tavistock, Okehampton, and Bideford.

THE Fine Art Society (148, New Bond Street) have sent us an artist's proof of M. Leopold Flameng's very fine etching after Mr. John Collier's picture of the late Mr. Darwin. The original is admittedly faithful and characteristic, and of high rank as a work of art, and M. Flameng has been perfectly successful in reproducing the artist's intention. The result of both these labours is a portrait of the greatest man of science of this century, which all other men of science should be glad to possess. We believe the number printed is limited. M. Flameng will also undertake a similar etching of Mr. Collier's picture of Prof. Huxley in this year's Exhibition.

WE have received the Report of the Royal Victoria Coffee Hall, where, as may be known to many of our readers, much good work is being done at the present time in the way of providing cheap amusement every night, free from the temptation to drink and other evils common to ordinary music halls. Among other experiments being tried are short lectures of the simplest and most popular kind, generally on some scientific subject illustrated by the oxyhydrogen lantern. We are told that a really good lecturer who understands his audience as well as his subject meets with a most encouraging reception, but that very few men of science give their assistance in this good work. We regret this; but we believe it is largely due to the fact that very little is known of the work in question, and that if a general appeal were made to those men of science who occasionally give an account either of their own work or the work of others, many would be found willing to join in the

effort which is now being made to interest the working classes in science in what was formerly the Royal Victoria Theatre.

OUR Paris Correspondent writes:—An interesting experiment took place on June 24, at the early hour of 3.45 in Paris, for the purpose of testing the capacity of accumulators of the French Storage Company to move tramcars. We travelled up to La Muette and back, a distance of 30 kilometres, in about three hours and twenty minutes, including stoppages and loss of time incurred by several incidents. The road has many steep inclines, which were ascended without difficulty. The mean velocity exceeded 10 kilometres per hour. The electricity was supplied by seventy accumulators, weighing 30 kilograms each, which were placed under the seats. At starting the potential was 140 volts, and having completed its task the current was as high as 126, so that at least 10 kilometres more could have been run if deemed necessary. This run is the longest on record made by electricity. M. Philippart was directing the operations.

THE Balloon Exhibition was closed at the Trocadéro on the 24th inst. It was visited by two officers of the British army, sent by the Government to report. Among the notable objects we may mention the original valve used by Gay-Lussac in his ascent, a new valve used by French aëronauts, the car and net of Lhoste as rescued from the North Sea, a panoramic apparatus for photographing a bird's-eye view of scenery as seen from a balloon at an altitude of 200 metres, several photographs taken from the cars of captive or free balloons in Paris, Boston, and Rouen, a refrigerator by Mignon and Bouard for instantly condensing vapour from clouds, bichromate elements constructed by Trouvé for Tissandier's intended aërial experiments.

THE following are the details of the method by which the fairy-like illuminations at Moscow at the coronation were produced:—The Tower of Ivan the Great and its side galleries were lit up by 3,500 small Edison lamps, fed by eighteen portable engines, which moved a number of dynamo-electric machines of every existing system. The portable engines and machines were kept at the other bank of the Moskwa. The sheds communicated with the tower by seventy aërial electric wires. On the ramparts of the Kremlin towards the river eight large and ten smaller electric suns threw their light over the river. The rest of the illuminations consisted of 200,000 lamps and 30,000 coloured glass globes, 50,000 lanterns of Venetian glass, 600,000 tapers, and 10,800 lb. of fireworks.

THE National Museum at Washington is one of the best examples in the United States of the practical application of electricity. In so large a building it was found advisable to take advantage of the best means of communication, first being its system of telephones and call-bells, by which those in any room can communicate with every room in the building. Twenty-six telephones are connected by a local telephone exchange, which in turn is connected with the main telephone office of the city. The result is that but three messengers are needed in this vast establishment. The photographic laboratory is independent of the sun, owing to the electric light there used. If one of the 850 windows or 230 doors is opened, a bell rings, and an electric annunciator shows to an attendant at the main office which window or door it is. This system is soon to be applied to every case of specimens. The watchmen at night, also, are kept to their posts by hourly releasing an electric current at certain stations, which pierces a dial and records their visit. The sixteen clock dials are likewise run by electric currents.

A MONUMENT to the memory of the celebrated naturalist, Lorenz Oken, will be unveiled at Offenburg on August 1 next. It will be in the shape of a fountain crowned with a marble bust of Oken.

MESSRS. GRIFFITH AND FARRAN have issued a new and cheaper edition of Mrs. Lankester's "Talks about Plants; or Early Lessons in Botany," first published in 1878.

PRINCE LUDWIG FERDINAND of Bavaria, an indefatigable worker in the domain of comparative anatomy, is about to publish a monograph on the tongue. Riedel (Munich) will be the publisher.

A MAGNIFICENT meteor was observed at Giesshübl, near Mödling (Vienna), on June 3, at 9.44 p.m. It seemed to consist of two fireballs, an emerald green one followed by a red one. They both moved apparently at a not very great altitude in the direction south-east to north west. The phenomenon lasted for three seconds. It is remarkable that the meteor seen at the same place on the evening of March 13, moved in almost exactly the same direction. Also at Gau-Algesheim (near Mainz) a fine meteor was seen in the northern sky on the evening of June 3; it left a most vivid trail behind, which shone for some time along the whole extent of its path.

A WRITER in the *North China Herald* gives some curious information respecting the foot-measure in China. At present it varies largely in different parts of the country and according to different trades; thus the foot of the carpenter's rule at Ningpo is less than ten, while that of the junk-builders at Shanghai is nearly sixteen, inches. But a medium value of twelve inches is not uncommon. The standard foot of the Imperial Board of Works at Peking is twelve and a half inches. A copper foot-measure, dated A.D. 81, is still preserved, and is nine and a half inches in length. The width is one inch. The small copper coins, commonly called *cash*, were made of such a size, sometimes, as just to cover an inch on the foot-rule. In the course of two centuries it was found that the foot had increased half an inch, and a difference in the dimensions of musical instruments resulted. Want of harmony was the consequence, and accordingly in A.D. 274 a new measure, exactly nine inches in length, was made the standard. Among the means employed for comparing the old and new foot are mentioned the gnomon of official sundials, and the length of certain jade tubes used according to old regulations as standards. One of these latter was so adjusted that an inch in breadth was equal to the breadth of ten millet seeds. A hundred millet seeds, or ten inches, was the foot. The Chinese foot is really based on the human hand, as is the European foot upon the foot. It strikes the Chinese as very incongruous when they hear that we measure cloth, woodwork, masonry, &c., which they regard as especially matters for the hand, by the foot. Of the jade tubes above mentioned there were twelve, and these formed the basis for the measurement of liquids and solids four thousand years ago. They are mentioned in the oldest Chinese documents with the astrolabe, the cycle of sixty years, and several of the oldest constellations. It is likely that they will be found to be an importation from Babylon, and in that case the Chinese foot is based on a Babylonian measure of a span, and should be nine inches in length.

MR. CHAS. G. LELAND, the writer of No. 4 (1882) of the "Circulars of Information" of the United States Bureau of Education, on the subject of Industrial Art in Schools, after premising that ornamental art is innate in man, and indeed is developed in a race before it attains proficiency in the useful, and remarking that the brains of the Parisians of the thirteenth century, when Gothic art adorned every object, were much smaller than they are now, draws the conclusion that children are more open to art education than to technical training. He finds the sexes equal in ability; urges outline drawing and monochrome as the foundation of further work; recommends the use of various mechanical helps, as of compasses and stencilling as actual incentives to freehand drawing; urges the practice of freehand

from the shoulder, even in the variety of drawing called writing; and gives a wonderful list of artistic effects which pupils who have had only short instruction in these arts are competent to produce. Education of this sort is valuable as simply affording healthy occupation of body and mind to some classes; it opens the eyes of the mind, which will tend to make work popular instead of idleness. He teaches that nothing made by machinery can be artistic; physical comforts may be supplied by it, but works of taste and refinement must be hand-made, and among the poorer classes should be the produce of home art, like the carved oak of Ann Hathaway's cottage. Mr. Leland was one of the first to point out that the decay of the apprentice system must soon necessitate industrial education, and he has prepared a series of cheap art-work manuals on decorative design, ceramic or porcelain painting, tapestry or dye-painting, outline and filled-in embroidery, decorative oil-painting, wood-carving, *repoussé* or sheet-brass work, leather work, papier mâché, modelling in clay, with underglaze faïence decoration, and stencilling.—No. 5 of the "Circulars" is on the subject of Maternal Schools in France, which answer to our Infant Schools. The value of them as laying the foundations of education is urged by the Commissioners. Excellent suggestions for object lessons, whose subjects are supplied by the season of the year, and also for the arrangement of school buildings are given. The result of such schools should be a slight training of the senses by object-lessons; the beginnings of habits and dispositions favourable to future education; a taste for gymnastics, for singing, and for drawing; an eagerness to listen, observe, question, and answer; the power of attention; a generally quickened intelligence, and a mind open to receive good moral influences. In other words, education is a "bringing forth" of the powers of the mind, and not a making it a live cyclopædia. No. 6 is a full copy, with a few useful notes for comparison, of the English Report of the Royal Commission on Technical Education in France, presented by Mr. Samuelson and his coadjutors in February, 1882.

With the May number the *Journal of Forestry* changed both its title and the colour and design of its cover, and it now appears under the simple name of *Forestry*. It is an acknowledged fact that changes of this character are generally inadvisable in a journal of long-established reputation, but under the editorship of Mr. Francis George Heath we have no doubt that *Forestry* will at least maintain the reputation and circulation it had attained under its old management, if it does not increase them, which indeed it is most likely to do. The May number opens with an editorial note entitled "A May Note," in which the glories of spring and summer in woodland glades and forest are set forth. Then Mr. R. D. Blackmore gives us "A Cuckoo Song." Amongst other readable articles may be mentioned "Lord Somerville; a forgotten President of Agriculture," by Mr. R. A. Kinglake; Mr. Boulger's "Beauties of British Trees," and Mr. Guillemard's "Forest Ramble in New South Wales." In the June number the same amount of interest and variety is maintained. Mr. Guillemard gives "A Forest Ramble in Norway." The article on "Epping Forest and its Future Management" will however, we have no doubt, be read by most readers, as any one having the slightest inclination towards any branch of natural history cannot fail to be interested in maintaining the Forest in all its native beauty, and if *Forestry* is able by its advocacy, backed up by the opinions of those who are now taking a lead in the matter, to stem the tide of improvements so-called in Epping Forest, it will have fulfilled a work for which thousands will be thankful.

The additions to the Zoological Society's Gardens during the past week include a Crab-eating Raccoon (*Procyon cancrivorus* ♂) from Brazil, presented by Mr. Theo. Walsh; a Ring-tailed Coati (*Nasua rufa* ♂) from Brazil, presented by Mr. R. G. Hamilton; two Common Hedgehogs (*Erinaceus europæus*), British, presented by Mr. S. Mummery; four Restless Cavies (*Cavia caprera*) from Brazil, presented by Mr. E. H. Draper; a Ring-necked Parrakeet (*Palæornis torquatus*) from India, presented by Mr. W. Quail; two Common Kingfishers (*Alcedo ispida*), British, presented by Mr. T. E. Gunn; three Common Vipers (*Vipera berus*), British, presented by Mr. C. Taylor; two Common Snakes (*Tropidonotus natrix*), European, presented by Lord Arthur Russell, F.Z.S.; a Puma (*Felis concolor* ♀) from South America, a Goffin's Cockatoo (*Cacatua goffini*) from Queensland, deposited; two West African Love Birds (*Agapornis pullaria*) from West Africa, an Indian Python (*Python molurus*) from India, purchased; two Vulpine Phalangers (*Phalangista vulpina*), eight Gold Pheasants (*Thaumalea picta*), six Prairie Grouse (*Tetrao cupido*), a Herring Gull (*Larus argentatus*), bred in the Gardens.

OUR ASTRONOMICAL COLUMN

The Next Total Solar Eclipse.—In Nature, vol. xiv. p. 450, we gave some results of an approximate calculation of the total eclipse of the sun on September 8–9, 1885, wherein the central line traverses New Zealand, but does not encounter land in any other part of its course. The correction required to the moon's place there employed is sufficiently important to render a new calculation of interest, and we shall accordingly present here some of the circumstances of the eclipse, resulting from the substitution of the lunar places in the *Nautical Almanac*, which are founded upon Hansen's Tables, with Prof. Newcomb's corrections. The elements of the eclipse as given in the ephemeris are employed, excepting that in place of Hansen's semidiameter of the moon, we infer the semidiameter from the ratio, 0·2725 of the horizontal parallax.

At a point in longitude 11h. 40m. 0s. E. of Greenwich, with 40° 49′·4 south latitude (nearly on the central line) the total eclipse begins, September 9, at 7h. 44m. 9s. local mean time, and continues 1m. 51s., and this will be about the longest duration of totality available for observation upon land in this eclipse. For any place near the above point, the Greenwich mean times of beginning and ending of totality may be obtained from the following formulæ:—

$$\text{Cos. } w = -116{\cdot}3108 - [2{\cdot}10862] \sin. l + [1{\cdot}64777] \cos. l \cos. (L - 161^\circ\ 16'\ 5)$$

$$t = 8\text{h. } 58\text{m. } 21{\cdot}1\text{s.} \mp [1{\cdot}74578] \sin. w - [3{\cdot}37987] \sin. l - [3{\cdot}85619] \cos. l \cos. (L - 145^\circ\ 49'{\cdot}6).$$

Here L is the longitude from Greenwich, reckoned *positive*, and *l* the geocentric latitude, which may be deduced from Mr. Stone's valuable table in the *Monthly Notices of the Royal Astronomical Society* for January last, a table it might have been worth while to publish separately. The quantities in square brackets are logarithms.

As one result of the introduction of the more accurate place of the moon, it is found that the central line approaches much nearer to Wellington; a direct calculation for that place shows that the total eclipse begins there at 7h. 44m. 23s. a.m., and ends at 7h. 45m. 46s. local mean time, thus continuing 1m. 23s., and the same figures are given by the above equations. At Nelson totality commences at 7h. 37m. 16s. a.m. local mean time, and continues 1m. 3s.

It may be noted that during the totality of this eclipse the planet Jupiter will be situated only 45′ from the sun's limb, on an angle of about 26° with the circle of declination at his centre.

The Annular Solar Eclipse of October 31, 1883.—In May last we had a case where the track of a total eclipse of the sun was almost wholly an ocean-track, and where it was consequently necessary to send expeditions to the Mid-Pacific, to obtain observations. The annular eclipse in October next is similarly circumstanced; excepting possibly one or two mere rocks in the Pacific, it will not be observable on land, elsewhere than on the island of Niphon, Japan. If we calculate from the *Nautical Almanac* elements for longitude 9h. 20m. 48s. E. and latitude 38° 11′ N., we find the annular phase commences at 7h. 28m. 2s. a.m., and ends at 7h. 35m. 23s., a duration of 7m. 21s., and the sun will be at an altitude of about 12°. At the capital, Tokio, the eclipse will not be annular; the greatest phase is at 7h. 28m. a.m., magnitude 0·88 (the sun's diameter being taken as unity).

THE GREAT COMET OF 1882.—In No. 2521 of the *Astronomische Nachrichten* is an elliptical orbit of this comet by Mr. John Tatlock, jun., of Williamstown, Mass., with a period of 1376 years, which, as Prof. Krueger remarks in a note, differs materially from the results of Kreutz, Frisby, and Fabritius. It may be added that the new calculation can have little weight, being founded upon normals for October 8, November 24, and January 29, so that at the date of the first normal the comet was already far past the perihelion, and in fact during the whole interval only described a heliocentric arc of about 5° 10'. Dr. Kreutz has shown the possibility of closely representing by the same orbit the anteperihelion observations and those made subsequently to perihelion passage, though there may be need of much more minute discussion before it can be safely assumed that there was absolutely no appreciable effect from the comet's passage through the solar coronal region.

GEOGRAPHICAL NOTES

Science announces that Lieut. Schwatka, accompanied by Assistant-Surgeon Wilson, C. A. Homan, U.S. Engineer Corps, and three private soldiers, left for Chilkat, Alaska, May 22, from Portland, Or., on the steamer *Victoria*. They are provisioned for a six months' cruise, will employ Indians for packers, &c., and intend to ascend the Chilkat River to its head, make the passage to the head waters of the Lewis River, and descend the same to its junction with the Yukon, and descend the Yukon River to its mouth. It is said to be their intention to survey the course of these rivers; and there is no doubt that a properly qualified and equipped party would find abundance of useful work ready to their hands. The whole route has been travelled before, but not by persons in search of and qualified to obtain geographical information, except in very small part. The explorations of the Krause brothers on the Chilkat and vicinity have been alluded to before. The Yukon has been superficially examined by McMurray, Ketchum, Zagoskin, Dall, Whymper, Raymond, Nelson, and others, and a few points have been astronomically determined; but nothing like an exact map has been attempted, nor do the data for it exist. Astronomical and magnetic observations anywhere along its banks, and especially any data for a map of the Lewis River and its feeders (which are only known from the reports of prospectors and natives), would be of the highest interest.

AT last news has again been received by Dr. Schweinfurth from the well-known African explorer, Dr. W. Junker. He was still in the Nyam Nyam country, and his last news was dated October 16, 1882, from the residence of a chief named Semio some days' journey south of the Mosio district of the present maps. Dr. Junker, who has travelled through vast districts hitherto unexplored, will now soon return home. The last time he had spent principally in various excursions, during which he repeatedly crossed the Uëlle River to the south, and also the third degree north latitude, leaving his provisions in the care of his companion, Herr Bohndorff. On September 27 he again joined the latter after an absence of eighteen months, but found him so poorly that he had to send him home with the collections made up to that time. Bohndorff started with thirty-two porters, who carried the natural history and ethnographical collections. Of special interest for geographers was an excursion of Dr. Junker's, which he made south of the former Munsa district of the Monbuttu. Some seven days' journey (about 60 kilometres) south of this place he reached a large river named the Nepoko, which the traveller identified with Stanley's Aruwimi, one of the main northern tributaries of the Congo in the middle course of the latter.

DR. POGGE has sent a report from the Mukenge station on the Lulua regarding his return journey from Nyangwe, showing that this was not quite as peaceable as the journey to Nyangwe, and that he had frequently to defend himself seriously against the enmity of the natives. From the Lualaba to the Lomani, Dr. Pogge travelled by the same route as he had previously come with Lieut. Wissmann

ONE of the most recent additions to the "Bibliothèque d'Aventures et de Voyages" published by Dreyfous of Paris is a volume containing the letters and journals of La Perouse during his famous voyage round the world in 1785–88, which ended in the disappearance of the circumnavigator among the islands of the South Pacific. The volume is annotated by M. George Mantoux, who also supplies a prefatory memoir of the great sailor.

"IM Reiche des Æolus" is the title of a little book by Adolf von Pereira, published by Hartleben of Vienna, and containing reminiscences of a tour the author undertook to the Lipari Isles. It is profusely illustrated and contains a map.

AUSTRIAN papers report that a mountain in the neighbourhood of Czernowitz, in the Bukovina, is manifesting singular symptoms of disturbance. The ground around its base, to the extent of over 1000 fathoms, has opened out in wide and deep chasms. Most of the houses of a village on the spot (Kuczumare) have fallen down.

THE Thuringian Geographical Society met at Jena on the 17th inst., when Prof. Hæckel read a paper on the flora of Ceylon, and Herr G. Kurze one on the outposts of European civilisation on the way from Zanzibar to Lake Tanganyika.

THE SPECTRUM OF THE AURORA

IN view of the increased frequency of auroras, an inquiry into the present position of our knowledge as to their spectra has seemed to me desirable.

The accompanying table gives in wave-lengths all the observations I could find of the position of the bright lines of the auroral spectrum. J. R. Capron's "Auroræ and their Spectra," goes more fully into the subject than any other work I know, and therefore many of the positions are taken from it, being found on the page or plate indicated in the column headed "Page, &c." The authorities for other observations are given in the notes, but in other cases again I cannot now state whence I obtained them.

They are arranged approximately in order of accuracy,[1] but this is manifestly a very difficult matter to decide: if, as is very likely, I have made mistakes in this respect, I hope I shall be excused. I have gone very carefully into the matter, judging of the accuracy of the observations partly by their internal evidence, and partly by the weights which are in some cases attached to them by the observers themselves. The observers' probable errors are given in the table after the positions of the lines. I consider J. R. Capron has attributed too much accuracy to most of the observations of the auroral spectrum that have hitherto been made; certainly he has to mine. Nearly all the observers have measured the principal line; and, as its position is very well known, the measurements of it are to a considerable extent a guide to the amount of dependence that may be placed on the rest. Of course it may happen to be measured correctly by accident, while the rest are incorrect; but, on the other hand, if it is incorrectly measured, it is not likely that the rest will be correct. It is, therefore, very desirable that observers should measure this line at the same time as they measure any of the others; not necessarily in order to ascertain its position, but as an indication of the correctness of the rest; although it does not always happen that all the lines are by any means equally accurate.

The most probable positions of the lines, given at the foot of the table, are derived from the most accurate of the observations of each. Below are indicated the observations which have been used in the calculation in each case, with the weight given to each; for I have not taken the simple average of those used, but have given higher weights to those that seemed the best. The "Probable Error," as given below the "Probable Average," is partly calculated and partly estimated; it seems rather large; perhaps it should not really be so large.

My second series of observations (No. 18 in the table) are not absolute measurements, but only comparisons with α and γ. I have therefore not used them in the calculation of the general averages. This series is most likely affected by constant errors much larger than the probable errors given in the table from calculation. It seems rather curious that the actual errors of my first series (No. 17) are nearly all so much greater than the probable errors; and possibly the same thing may occur in some other cases.

E. B. Kirk's observation (No. 28) (though a very rough one as regards position) is one of the most striking of all; and, being unique, confirmation of it is very desirable. It will be described under the different lines, &c., concerned.

Where I have attached to an observation a Greek letter with a note of interrogation, it means that it is uncertain whether the

[1] But the observations of each observer are placed together, however unequal in accuracy they may be.

line observed was that named at the head of the column. It is not always possible to identify the lines, and, in some cases, my identifications disagree with J. R. Capron's.

This table shows eleven distinct and well-separated lines or bands, the existence of all of which may, in my opinion, be considered proved, all but λ having been seen by two or more observers.

Seven have been seen by numerous observers; I have myself seen them repeatedly, though none of the other four with certainty; but these (θ, ι, κ, and λ) have all been seen by trustworthy observers, and therefore may be accepted, though of course further confirmation would be advantageous.

I shall now consider each line separately.

β is the only line yet seen in the red. It exists in all auroras that are tinged with red, though the spectroscope does not always show it. I have always seen this line most easily with a single prism; but often the slit has to be so wide that it appears merely as a red border to α. In those reddish auroras where I have not been able to detect it, its invisibility is probably due to moonlight or some such cause. There can be no doubt that this line is the cause of the red colour. It varies greatly in brilliancy with reference to α; I have seen it as bright as α, but never brighter.[1] It sometimes exists when no redness is perceptible in the aurora, it being overpowered by the other rays.

α is almost always the brightest line in the spectrum. The only exceptions I have seen were that once β was as bright, and once ε brighter (see below). α probably exists in all true auroras. In a few very faint ones I have not been able to see it, perhaps because it has been overpowered by the diffused light of the spectrum, which certainly varies in brightness relatively to the brightness of the lines. Very often when there is no decided aurora, a luminosity overspreads the sky, uniform in all directions, though fading gradually towards the zenith; I have several times seen the line α in its spectrum, but at other times it has been invisible, though the light has appeared equally bright.

α and β are slender lines; it is not clear whether this can be said of any of the other lines or bands.

θ. Wijkander's is the only reliable observation of this line, but Vogel has one not far from the same place, and as he states his position is unreliable, there is no reason to doubt its being the same line. There is more doubt whether Peirce observed the same.

ι was observed both by Wijkander and Parent, and probably by Peirce and Copeland.

ε consists of two lines, according to Vogel, which I have called ϵ^1 and ϵ^2; the latter, he says, became very bright whenever β appeared. He is the only observer who describes two lines here (though I have at least once suspected ε to be double), and therefore it is difficult to tell which of them other observers have seen, or whether they have seen both combined as one band. In the table I have assumed that the latter has usually been the case, or at least that a band has been seen in this place; possibly this band has been different from either. I have therefore placed the observations which seem to apply to the band, or to the combined lines, in a separate column from those that seem to refer to the individual lines; but the average of the former includes the observations of the latter. Barker and Procter describe a band here; also E. B. Kirk, who, in 1880, August 12, saw it as a band fading towards the violet; but in 1882, August 4, it faded away on both sides, though quite sharp at the edges, and with a pretty narrow slit it was broken up into lines—his impression is there were six or eight; and that the group was broader than the distance between ϵ^1 and ϵ^2. He used one of Browning's "Maclean star spectroscopes," with an ordinary convex lens instead of the cylindrical one.

I have carefully examined my observations of this band, to see whether it has appeared more refrangible when the red line has been visible or bright than when there has been little or no red; but the result of this examination is inconclusive. However, in 1869, April 16, and 1874, October 3, I noted that ε was relatively brighter in the red part of the aurora than elsewhere, so far confirming Dr. Vogel.

ε exists in nearly all auroras that are bright enough to show any line besides α; perhaps in all. It varies very much in brightness with respect to the other lines. I have sometimes found it the brightest next to α, and once the brightest of all, viz. in 1882, November 20, between 5.50 and 6.5 a.m. There was twilight at the time in addition to the aurora, but I do not see how this could produce the effect. Between 5.40 and 5.50 that morning I estimated α three times as bright as ε, which was the second brightest line; but I see no way of avoiding the conclusion that it was the brightest of all a few minutes later.

ζ may be seen in most bright auroras. It is sometimes brighter than ε.

η is much more seldom visible; but I have several times seen it brighter than either ε or ζ; rarely as bright as γ, or brighter than it; but never so conspicuous as it, as the latter is rendered more visible by its position at the edge of the brighter part of the continuous spectrum.

γ and δ belong, I believe, to all auroras, always being visible when the spectrum is moderately bright. But their brightness varies with respect to each other and to α; indeed I do not know any two auroral lines that always vary together.

γ is usually the brightest line next to α, with my mode of observation. Several observers describe it as a band; Vogel as a double band (if not triple). Capron, on the authority of A. S. Herschel, says it consists of "two lines, the first rather more frequently noted than the second" (the more refrangible). I cannot see that this assertion is borne out by the accompanying table, but if it is correct, the two lines must be about 4700 and 4654. I have several times seen γ as a band fading towards the violet. E. B. Kirk, in 1880, saw it as a band; but in 1882, August 4, resolved it into bright lines—a broader group than ε, less distinctly bounded, and with a less bright centre, and containing, he thinks, about twice as many lines.

κ appears to have been noted by two observers; one being Wijkander, who seems very accurate.

δ is invariably fainter than γ to me.

λ. Seen only by Lemström; but he says that in 1871, November 22, he "observed it with certainty three separate times."

The likelihood of the existence of lines in the violet or blue (such as λ or κ) in addition to those commonly seen, is manifest to me from the fact that I have twice seen purple in auroras. The first time was at Sunderland, 1869, May 13, at 10.55 p.m., when for a minute or two there was a large patch of coloured light—deep crimson, exquisite pink, and most lovely pinkish purple, gradually passing into one another. The crimson was the same colour frequently observed; the pink was very different, and far more beautiful. The crimson lasted after the other colours faded. The second time was in Skye, 1872, August 3, about 10.30 p.m., when for two or three minutes there were large patches of a beautiful, but not deep, pinkish purple. I had no time to observe the spectrum in either case. It is manifestly improbable that these colours would be caused by any of the ordinary lines of the spectrum; probably one line in the violet or blue, in combination with the red line, could account for the various tints.

Colours are closely connected with the spectrum; but I cannot say I ever saw any in the aurora, except the purples and pink just referred to, that might not be readily accounted for by the ordinary lines of the spectrum with or without the red line; as the only other decided colours I have seen are red and the usual greenish colour, varying somewhat in intensity and perhaps tint. I have seen other less decided colours; but, considering the extent to which the colours of the aurora might be affected by mist, smoke, twilight, moonlight, &c., and one's judgment by the effect of contrast, I could not say that they certainly belonged to the aurora.

The continuous light of the spectrum always reaches from α to δ; being very faint from γ to δ, rather faint from α to ε, and sometimes brighter from ε to η or to ζ than beyond. Sometimes when ε is too faint to be detected, the abrupt brightening of the continuous spectrum at that point is plainly visible. Kirk, in 1882, on the occasion already mentioned, when the spectroscope was pointed between the streamers, saw the spaces from α to ε and from ε to γ apparently filled with shifting lines, very numerous and close. Not that the lines really shifted, but their flickering caused them to appear shifting, and possibly also to appear more numerous than they really were. When the spectroscope was pointed on the streamers these lines were obscured by the greater brightness of the rest of the spectrum. I have myself often suspected lines between ε and γ, besides ζ and η.

It has been suggested that some of the lines may vary somewhat in position; but there is no evidence yet that the apparent variations are due to anything but errors of observation.

A flickering of the lines has been observed in certain cases; in all probability this occurs with the whole spectrum when the

[1] In considering the relative brightness of the different bands, it must be borne in mind that it varies considerably with the width of the slit, the dispersion, &c.

	Page, &c.	β	α	θ	ι	ϵ^1	ε	ϵ^2	ζ	η	γ	κ	δ	λ
1. Wijkander, A.	p. 96			5359±3	5289±5	5239±4			4996±9	4871	4692±2	4366	4280±3	
2. Vogel, Dr. H.C., av.	p. 198		5571·3±·9											
3. " "	p. 99, 105	6297±14	5569±2	5390		5233±4		5189±9	5004±3		4663±3 (4694 to 4629)			
4. Parent, Lieut.	p. 96				5280±1			?ε 5207±11		4873	4708±5		4286±16	
5. Zöllner, F.	pl. xii.	6279												
6. Lemström, K. S.[1]			5569			? ε 5250			4959		4686		4256	4112
7. German North Polar Expedition.			5569											
8. Copeland, R., 1880.			5571·6±2		ι? 5275		ε? 5120±22		ζ? 4930±21		4714±20		4320±20	
9. Ångström, A. J.	p. 198		5567											
10. " "							? ϵ^2 5210				4720			
11. Von Oettingen.		6290±70	5548±30								4660±25		4240±12	
12. Barker, G. F.	pl. xii.	6300	5550 (this	series is	±20 to	±6)	(5330 to 5200)		(5050 to 4990)	(4930 to 4850)	4705 (4740 to 4670)		4310	
13. " "	p. 97	6200	5620					?ε 5170	5020	?γ 4820				
14. Rowland, H. A. (Troy).		6283	5543										4250	
15. Respighi, L.			5573						5050					
16. Peirce.[2]			5570	θ? 5450	5315			?ε 5205		4860	4640		4310	
17. Backhouse, T. W.		6010	5605±14				5225		5025±9	4920	4663±10		4302±6	
18. " "		6280±100					5227±6		5002±5	4874±5			4370±6	
19. Struve, O., 1868.			5545±20											
20. Herschel, A. S.			5555±25											
21. Maclear, Capt. J. P.	pl. xi.		5510								4685	4410	4280	
22. Procter, H. R.		6270	5590				(5200 to 5100) 5170				(4840 to 4650) 4760		4240	
23. Smyth, C. P., 1872	p. 98	6350	5580				? ϵ^1 5300		5100	4900				
24. Ellery, R. J. (Melbourne).		6350	5600											
25. Church, A. H.		6180	5600								4870		4480	
26. Lindsay, Lord.	p. 102		5690				5330		5170		4850		4340	
27. Clarke, A., Jun.	p. 97		5690				5320				4850		4350	
28. Kirk, E. B.[3]			5800				5150				4850		4300	
29. T. F., Torquay.	p. 96	6400	5880								4870		4400	
30. Elger, T. G.	p 96	6550	5880						5280		4870			
Probable Average ...		6289	5569·7	5359	5285	5237	5226	5199	5001	4870	4688	4366	4278	4112
Probable Error		±6	±0·6	±15	±8	±8	±8	±8	±8	±8	±9	±12	±8	±20
Observations used for Average		3, w 1. 5, w 1. 12, w ·2. 14, w ·2	2, w 1. 6, w 1. 7, w ·5. 8, w ·5. 9, w ·5.	1	1, w 1. 4, w 1.	1, w 1. 3, w ·5	1, w 1. 3, w ·5. 4, w ·5. 6, w ·3. 10, w ·25. 12, w ·2. 16, w ·15. 17, w ·1.	3, w 1. 4, w 1. 16, w ·3.	3, w 2. 1, w 1.	1, w 1. 4, w ·6. 16, w ·2.	1, w 2. 3, w 1. 4, w 1. 6, w ·5. 11, w ·3. 12, w ·3. 10, w ·1. 17, w ·1.	1	1, w 2. 4, w 1. 6, w ·5. 11, w ·3. 12, w ·3. 14, w ·2. 16, w ·1. 17, w ·1.	6

[1] *Archives des Sciences Physiques et Naturelles*, 1874. [2] Reported by Winlock: *American Journal of Science*, vol. xlviii. [3] *Observatory*, 1880, p. 542; 1882, p. 272.

aurora flashes, though it does not seem to have been remarked. My own observations have all been made with a very wide slit, or, which comes practically to the same thing, with small dispersion. This has been owing to the usual feebleness of the greater part of the spectrum; and many of the other observers have for the same reason also used a wide slit. It may be useful to explain the method by which most of my observations were made, as it seems successful for perceiving the lines and general character of the spectrum, though not for measuring the positions. I have simply used one or two, or in some cases three prisms, usually of Chance's dense flint glass, and for a slit, the space between window-shutters nearly shut, or between two planks placed against the window. I hold the prisms in my hand on a simple stand, not always fixed, but so that they may be easily moved with respect to each other and to the slit, so varying the amount of dispersion. The best results are usually obtained by holding them in the position of almost the greatest deviation possible. Varying the deviation alters the focus. If one plank or shutter is placed rather further forward than the other, the apparent width of the slit is varied at will by simply moving one's head to one side or the other. By these means it is easy to observe all the different features of the spectrum, which require different widths of the slit and degrees of dispersion. A vacuum-tube or other light for comparison may be placed behind the slit, though it is obvious that with small dispersion accurate comparisons cannot be made.

I have made some observations with a Browning's "Miniature Spectroscope," with the diaphragm off, but it gives less light than simple prisms. I have also tried a "half-prism spectroscope," by Hilger, but unsuccessfully; but I find that by taking off the outer lens of the eyepiece and the diaphragm much more light is obtained; I have not, however, had an opportunity of trying this plan yet on an aurora.

The number of nights of aurora on which I have seen each line, between 1871, November 1, and 1883, March 27, is as follows:—

β	α	ϵ	ζ	η	γ	δ
11	83	34	14	7	33	26

On thirteen nights I could not be sure of any line, but on six of these I suspected α, or else there was an abrupt fading away about where α should come. There were other auroras—mostly faint ones—whose spectra I did not observe.

The lines visible in the spectrum often vary in the course of a few minutes, and indeed are not always the same in different parts of the sky at the same time. I have never been able to detect that any particular feature of the spectrum belongs to any particular type or feature of aurora, except that the line β belongs to red auroras.

Dr. Vogel thinks it probable that the auroral spectrum is a modified air-spectrum. The following are the most striking coincidences or approximations between my revised list of auroral lines and Vogel's lists of lines in the spectra of air and its constituents. They are sufficient to make the subject one worthy of consideration; but perhaps this is as much as can as yet be said. There are other approximations to very faint air or gas lines, which he regards as of some importance; but, as the lines in the latter spectra are so numerous, one would naturally expect such coincidences accidentally.

Aurora Spectrum Coincidences.

Aurora.		Air.	
ϵ^1 5237	...	Moist air	5231 dull.
ϵ 5226	...	N	5224 very bright.
ϵ^2 5199	...	O	5189 very bright.
	...	H	5187 very bright.
ζ 5001	...	N	5004 bright.
	...	Air	5008 very bright.
	...		5002 very bright.
η 4870	...	O	4870 moderately bright.
γ 4688	...	N	4704 very intense.
κ 4366	...	O	4372 moderately bright.
	...	N	4363 bright.
	...	N	4357 bright.
	...	H	4358 very bright.
δ 4278	...	N	4275 very bright.

A. S. Herschel has pointed out the proximity of β to the dark atmospheric band α at 6279.

Sunderland THOS. WILLIAM BACKHOUSE

SCIENCE AT KAZAN[1]

THE Kazan Society of Naturalists, which began its *Memoirs* in 1871 with the remarkable work of M. M. Bogdanoff on the birds and mammals of the black earth region of the basin of the Volga, has continued since to publish a series of most valuable explorations of the region of the lower Ural, Volga, and Siberia. We notice thus in the first eight volumes of its *Memoirs* the researches on earthquakes in Siberia, in Turkestan, and on the Ural, by M. Orloff; several valuable papers on the Geology of the Obschiy Syrt plateau, by M. Sintsoff; of the Government of Vyatka, by M. Krotoff; of the Government of Kazan, by Prof. Stuckenberg; and of the banks of the Kania, by M. Zaïtseff; a work on the birds of Caucasus, by M. Bogdanoff; a paper on the Teleostei of the mouth of the Volga, by M. Yakovleff; the history of the development of the *Acipenser sturio*, by M. Zalensky; and mycological researches, by M. Sorokin; several papers on the flora of the Government of Perm, by M. Kryloff; and two papers by M. Levakovsky on the substitution of certain species of plants for others in a given region; as well as several valuable researches into the anthropology of the Bashkirs, Voguls, and Votyaks, by MM. Malieff, Sorokin, and Ostrovsky.

The three last volumes of the *Memoirs*, which we have now before us, contain also many valuable papers. In the department of botany we notice the second part of M. Kryloff's flora of the Government of Perm. It contains a complete list of all Phanerogamæ discovered in this interesting province, which includes the Ural Mountains, completed by special researches into the subarctic and Alpine flora of this region. A map shows pretty well how such plants as the *Viburnus opulus*, the *Cystisus biflorus*, the *Tilia parviflora*, and the cereals are stopped in their extension by the Ural Mountains, reappearing again on their eastern slope; whilst others, like the *Quercus pedunculata*, or the *Acer platanoides*, are stopped in their extension towards the east by the western spurs of the Ural and the lowlands of Siberia, their north-eastern limit meeting nearly together with the south-western limit of extension of the *Pinus cembra*, the *Lonicera cærulea*, the *Spiræa media*, and *Polygonum viviparum*. The whole list contains 948 Angiospermæ, and 8 Gymnospermæ. The Cryptogamæ are represented by 38 Lycopodiaceæ and 124 Lichens.—Dr. Martianoff publishes valuable materials for the flora of the Minusinsk region in Eastern Siberia, comprising a sketch of its climate (according to five years' meteorological observations by A. Krapotkin) and its physico-geographical characters. The flora of Minusinsk is much varied, as it embodies three separate botanical regions: the Alpine, the forest, and the steppe floras, intermingled with one another. Its general character is that of the Altai region, and out of 777 Phanerogams, no less than 714 are Altaic, whilst only 59 belong to the flora of Eastern Asia. The Alpine flora has but 104 representatives; the forest-flora is the most widely spread, and at the same time the richest; it is represented by 579 species. The steppe flora, which covers nearly one-quarter of the Minusinsk district, and appears sporadically even on the plateaux of the hilly tracts, numbers 315 species. We can only notice here the excellent botanical sketches of separate parts of this "Siberian Italy" which we find in M. Martianoff's work. His list of plants, which contains 760 species of vascular plants, is unusually rich also in lower plants, the number of determined Fungi and Myxomycetes comprising 644 species.—An interesting work which has cost much labour to its author, M. Kryloff, is a description of all drugs—mostly plants—used in the popular pharmacies of the Governments of Kazan and Perm. The list comprises about 200 plants, with a description of their use in popular medicine.

The zoological papers in this volume are but two:—On the innervation of the heart of the *Esox lucius* and *Acipenser ruthenus*, by MM. Kazem-beck and Doguel; and on the ear-labyrinth of the Plagiostomi (*Acipenser ruthenus*, *A. sturio*, and *A. schypa*), by M. Sizoff. Both papers have appeared also in the *Archiv für microskopische Anatomie*.

Geology is represented by the following papers:—On the upper part of the mottled marls, by Prof. Stuckenberg; on the Permian in the Governments of Kazan and Samara, by A. Zaitseff; and on the geology of the Volga between Nijni-Novgorod and Kazan, by P. Krotoff. The Zechstein appears in the region situated between the Kama, the water-

[1] *Memoirs of the Society of Naturalists at the Kazan University*, vols. ix., x., and xi., 1880-1882.

shed of the Sok and Sheshma, and the Volga, as an island extending from north-east to south-west, and covered on its borders by mottled marls. The former is closely mingled with the latter, as it extends also in the shape of thinner intermediate deposits among the marls; but on the whole it substantially differs from them by its fauna, undoubtedly belonging to the Zechstein. As to the mottled marls, they contain the *Unio umbonatus*, Fisch., the *Estheria* sp. (*Posydonomia minuta*, Bronn.), the *Lingula orientalis*, Golowkinsky, scales of *Acrolepis acroderma*, Eichw., and Calamites. The Post-Pliocene deposits are spread everywhere, and we notice the find of Caspian shells of *Cardium*, together with *Dreissena*, at the sources of the Cheremshan river (left tributary of the Volga, namely, at Balandino, ten miles from the Cheremshanskaya fort). This important find proves thus that the Caspian formerly extended at least as far north as 54° 40′ north latitude. As to the Permian formation to the west of Kazan, M. Krotoff, who includes in this formation both the Zechstein and the mottled marls, calculates that it has a thickness of 810 to 860 feet. Showing further that the fauna of the mottled marls but slightly differs from that of the Zechstein (a complete list of its fossils being given by the author), and that the fossils that are characteristic of these marls (*Unio umbonatus*, Fisch., *Unio castor*, Eichw., *Estheria* sp., *Cythere* sp., remains of fishes, and Calamites) were found elsewhere, either in company with purely Zechstein forms or in deposits subordinate to the Zechstein deposits, he concludes—perhaps too soon—that there is no ground to consider them as Triassic.

Anthropology and archæology are represented by several interesting papers:—M. Krotoff publishes his researches into the age of the stone implements found in the basin of the Oka, and M. Ivanoff on the Perm region.—M. Malieff publishes the results of his most interesting measurements of the Old Bulgarian skulls dug out from the Babiy Bugor, at the Bulgarian village situated on the left bank of the Volga, close to Tetushi, and his paper is accompanied by sixteen photographs of four skulls. He measured the best preserved twenty-five skulls, all belonging to full-grown males. They are all much like one another, but could be subdivided into three groups: fourteen dolichocephalic, with indexes varying from 71·4 to 77·1; five mesatycephalic, their indexes varying from 77·8 to 79·8; and five subbrachicephalic, whose indexes vary from 81·1 to 82·1. The average size of the horizontal circumference of the twenty-five skulls is 515 millimetres, with a maximum of 555 millimetres and a minimum of 490; the average capacity is 1381 cubic centimetres. They completely differ from the skulls of other inhabitants of the same region: not only Kalmuks, or Bashkirs, but also from the Russian, Tartar, or Mordovian skulls. Without expressing a definitive opinion until a comparison of these skulls with those of Bulgarians from the Balkan peninsula is made, the author points out that they are very much like those of the Koorgan inhabitants of the Government of Moscow, who seem to be Old-Sclavonic, and certainly are not Finnish, as results from an inquiry made on 120 skulls by Prof. A. Bogdanoff. They are similar also to the skulls of the old inhabitants of Kieff and to those of the Scythes of Southern Russia. M. Malieff's companion in these researches, who gives in the same periodical a sketch of the Old Bulgarian burying-place at Babiy Bugor, adds that the skeletons they dug out had their heads towards the west, and were lying on the left side, looking towards the north (towards the Volga). Masses of pieces of earthenware were found together with the skeletons, and the pottery was of the roughest kind, made by hand, and burned very incompletely. He argues with much probability that this burying-place did not belong to a Mussulman people, but to idolaters, and supposes that its antiquity may be traced as far back as the Stone period. In any case, the customs of burying, as shown by this burying-place, seem to have been very much like those of the Sclavonians before their conversion. As to the burying-places at Chulpanovka and Ukrech, in the districts of Christopol and Laishev, explored by MM. Malieff and Vysotsky and described by the latter in his second "Anthropological Sketch of the Explorations of the Year 1880," and by M. Malieff in his just-mentioned paper, both explorers agree in considering them as belonging to Chuvashes. The craniological measurements which M. Malieff made on twenty skulls show that six of them belong to the mesatycephalic type, the average cephalic index of which is 74·5, and the others are either dolichocephalic, or belong to women and children, or afford a most pronounced asymmetry, and cannot thus give reliable figures.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE

THE *alumni* and other friends of the University of St. Andrews have been roused to action by the threat (now withdrawn) of its possible dissolution, in consequence of insufficient endowment. An "Appeal" which has just been issued shows that 2700*l.* (in sums of from 100*l.* to 1000*l.*) has been already subscribed towards the better endowment of the Professorial Chairs; and a scheme has been set on foot amongst the younger graduates for the no less essential object of securing the augmentation of the open bursaries. Upwards of 200*l.* (in sums of from 1*l.* to 50*l.*) has been already promised towards this special fund, and an appeal from the Committee appointed for this purpose will shortly be circulated. There is good reason to believe that the withdrawal of the obnoxious clause has been partly occasioned by the practical shape which the defence of the oldest Scottish University has thus assumed.

SCIENTIFIC SERIALS

THE *American Journal of Science*, June, 1883.—On the nature of the induration in the St. Peter's and Potsdam sandstones and in certain Archæan quartzites, in Wisconsin, by R. D. Irving. The author extends the conclusions already arrived at by Sorby in several important respects.—On the existence of a deposit in North-Eastern Montana and North-Western Dakota, that is possibly equivalent with the Green River group, by Charles A. White. The paper embodies a detailed description of the new extinct genus and species of Percidæ occurring in the Dakota rocks, by Prof. E. D. Cope.—On the peculiar concretions occurring in meteoric irons, by J. Lawrence Smith. These concretions are found to contain sulphuret of iron, schreibersite (phosphuret of iron and nickel), graphite, daubréelite, chromite, lawrencite, aragonite.—On mineral vein formation now in progress at Steamboat Springs compared with the same at Sulphur Bank, by Joseph Le Conte.—Observations on the transit of Venus, December 6, 1882, at the Vanderbilt University Observatory, Nashville, Tennessee, by Olin H. Landreth.—On the fauna found at Lime Creek, Iowa, and its relation to other geological fauna, by S. Calvin. A complete catalogue is given of the Lime Creek fauna which are compared with those of the Niagara, Cinderhook, and other Devonian rocks.—Observations on stratified drift in Delaware, by F. D. Chester.—On the western discharge of the flooded Connecticut, or that through the Farmington Valley to New Haven Bay, by James D. Dana.—Results of some experiments made to determine the variations in length of certain bars at the temperature of melting ice, by R. S. Woodward, E. S. Wheeler, A. R. Flint, and W. Voigt. The experiments are chiefly made with zinc and steel bars, and the authors found that zinc is the least reliable metal for the components of a metallic thermometer and standard of length, while steel, copper, and brass do not vary appreciably at any ordinary temperature.—On Scovillite, a new phosphate of didymium, yttrium, and other rare earths, from Salisbury, Connecticut, by George J. Brush and Samuel L. Penfield.

Journal of the Royal Microscopical Society for April, 1883, contains:—On five new Floscules, with a note on Prof. Leidy's genera, Acyclus and Dictyophora, by Dr. C. T. Hudson (Plates 3 and 4).—The President's (Prof. P. M. Duncan) address.—The action of tannin on the cilia of Infusoria, with remarks on the use of a solution of sulphurous oxide in alcohol, by H. J. Waddington.—Summary of recent literature.—Proceedings of the Society.

Journal of the Russian Chemical and Physical Society, vol. xv. fasc. 4.—On solutions, by W. Alexeyeff; being an inquiry into the mutual solutions of liquids, as depending upon temperature. The experiments carried out on aniline, amyl and isobutyl alcohols, phenol, &c., lead to the following conclusions:—The hypothesis of Person as to the liquefaction of bodies before solution is not confirmed. The solubility depends upon the molecular cohesion, and increases as this last becomes feebler. Thus, at the same temperature, more of liquid than of solid salicylic acid is dissolved. The solutions are quite different from chemical compounds, and the liquid mixtures are different from solutions.—On the specific volumes of elements in liquid compounds; second paper, by M. Shalfeyeff. The conclusions of these valuable researches are:—The compounds of the fat series are derived from the uneven-atomic carbon; and those of the

aromatic series from the even-atomic carbon. The specific volume of carbon is C = 21 in the C_nH_{2n} compounds of the former series, and C = 12 in the C_nH_{2n} compounds of the second series.—On Caucasus naphtha, by M. W. Markovnikoff and W. Ogloblin, being a thorough analysis of it.—On the identity $(\Sigma\sigma_i x_i^2)\Sigma a_i = (\Sigma a_i x_i)^2 + \Sigma a_i a_k (x_i - x_k)^2$, and its meaning in physics, by N. Slouguinoff.—On the focal properties of diffraction-nets, by M. Merching.—On the specific properties of indiarubber, by N. Hesehus. They cannot be explained by the presence of air vesicles.

To the *Bulletin of the Belgian Académie Royale des Sciences*, for 1883, part 2, M. C. Malaise sends a valuable paper on the constituent elements of the Silurian formations of Brabant. An approximate thickness of 12,000 or 14,000 feet is assigned to the various groups constituting the older schistose rocks of this province.—Ed. Dupont deals with the origin of the Belgian Carboniferous limestones; and papers are contributed by M. Terby on the aspect and positions of the great comet of 1882; by M. Chevron, on the inflammable nature of the gases liberated in the decomposition of beetroot; by Baron Kervyn de Lettenhove, on the Conference of Bayonne of 1565; by Alphonse Wauterson, on the origin and rise of the early Flemish school of painting previous to the Van Eycks.—Part 3 contains contributions by J. de Tilly, on Chasles' theorem of central axes; by Ed. Van Beneden, on some additions to the ichthyological fauna of the Belgian seaboard; by M. Genocchi, on the algebraic functions of Prymand Hermite; by Joan Bohl, on the reforms recently introduced into the commercial jurisprudence of Italy.

Rendiconti of the *Reale Istituto Lombardo di Scienze e Lettere*, April 12, 1883.—Some applications of symbolic variability to mechanical problems, by S. C. C. Formenti. This paper is concluded in the next number, April 26.—On springs, head streams, and underground currents in the Italian Alps, by Prof. T. Taramelli.—Experimental researches on the decomposition of adipose substances in water, in damp earth, damp rooms, and in water charged with 10 per cent. of ammonia, by C. A. Tamassia.—A study of microscopic organisms in sweet, salt, and mineral waters, by Prof. L. Maggi.—Remarks on the equivalence of magnetic and galvanic distributions, by Prof. E. Beltrami.—A preliminary inquiry into Zanardelli's proposed Italian penal code, by Prof. A. Buccellati.—On an unpublished letter of Francesco Maurolico, dated September 11, 1571, in connection with the battle of Lepanto, by L. De-Marchi.—On an example of realism in classic art, by Prof. J. Gentile.

April 26.—A comparative study of the aracnofauna of Abyssinia and Shoa, by Prof. Pietro Pavesi. The author determines thirty new species of spiders, for one of which (*Chiasmopes*) he establishes a new order.—On the determination of the coefficients of specific force for iron independently of Wöhler's numbers, by Prof. C. Clericetti.—Suggestions on a substitute for capital punishment in Zanardelli's new Italian penal code, by Cesare Oliva.—Remarks on banking and the cheque system introduced into the new Italian commercial code, by L. Gallavresi.

Atti of the Roman *Reale Accademia dei Lincei*, April 1.—On Finlay's comet (1882), by S. Respighi.—On the first observer of the optical illusion converting convex into concave and concave into convex surfaces, by S. Govi. The priority of discovery usually assigned either to Joblot (1718) or to Christopher Cock (1688) is here credited to Eustachio Divini (1663) on documentary evidence.—On the presence of native cinnabar and sulphide of silver in the Tolfa Hills, by S. Blaserna.

April 15.—Biographical notice of the late Bertrando Spaventa, by S. Ferri.—On the migrations of the ancient peoples of the Armenian Highlands and Asia Minor, studied in the light of the Egyptian monuments and hieroglyphical inscriptions, by S. Fiorelli.—A notice of the archæological discoveries made in various parts of Italy during the month of March, by S. Fiorelli.

Revue internationale des Sciences biologiques for March, 1883, contains:—On the origin of the vertebrates and the principle of the transformation of functions, by Dr. A. Dohrn.—On the excitability of plants, by Dr. Burdon Sanderson.—On dwarfs and giants, by D. L. Delbœuf.—Proceedings of the Academy of Sciences, Paris, of the Belgian Academy, and of the Academy of Amsterdam.

April, 1883, contains:—On the primordial flora, by Louis Crié.—On the origin and relation of sex, by M. Debierre.—On colour and mimicry in insects, by Dr. Hagen.—Proceedings of the Academy of Sciences, Paris, and the Academy of Sciences, Amsterdam.

SOCIETIES AND ACADEMIES

LONDON

Royal Society, May 10.—"Note on the Motor Roots of the Brachial Plexus, and on the Dilator Nerve of the Iris." By David Ferrier, M.D., LL.D., F.R.S., Professor of Forensic Medicine in King's College.

In a communication to the Royal Society (published in the *Proc. Roy. Soc.*, vol. xxxii. 1881) on the "Functional Relations of the Motor Roots of the Brachial and Lumbo-Sacral Plexuses," my colleague Prof. Gerald Yeo and myself gave an account of the results of electrical stimulation of the several motor roots of the brachial and crural plexuses in the monkey. We there described the muscular actions of the upper extremity as resulting from stimulation of the first dorsal up to the fourth cervical nerve.

The careful dissections made at our request by Mr. W. Tyrell Brooks, Demonstrator in the Physiological Laboratory, King's College, and a repetition of the stimulation experiments which I have made, have revealed an error in the enumeration of the roots of the brachial plexus which, in common with Prof. Yeo, I wish to correct. What we took for the first dorsal nerve has proved in reality to be the second dorsal. Hence the results of the experiments must be read as applying to the spinal nerves from the second dorsal to the fifth cervical respectively, instead of from the first dorsal to the fourth cervical, as stated in our paper.

The anterior division of the second dorsal nerve in the monkey apparently invariably gives a well-developed communicating branch to the first dorsal, besides giving off the second intercostal nerve and a branch to the stellate or inferior cervical ganglion of the sympathetic.

The three branches, as seen in a dissection made for me by Mr. Brooks seem pretty equal in size, and all come off from the main trunk together.

The brachial plexis in man is not usually, in text-books of anatomy, considered as deriving any of its component roots below the first dorsal. In "Quain's Anatomy" (ninth edition, p. 619), however, a branch from the second to the first dorsal is given as a variety. On this subject Dr. D. J. Cunningham has published a note in the *Journal of Anatomy and Physiology*, vol. xi. part iii., p. 539, 1877. Dr. Allen Thomson having mentioned to him that he had on one or two occasions seen such a communicating branch in man, he investigated the point, with the result of finding a communicating branch from the second to the first dorsal in twenty-seven out of thirty-seven dissections. Of the ten cases where it was not found, five were so complicated by previous interference in the dissecting-room or by pleuritic adhesions and thickenings, that they may be considered as doubtful. But, even including these, it appears that the second dorsal sends a communicating branch to the first in 73 per cent. of the cases. Hence it should be considered as more than a mere variety. If a perfect homology exists between the roots of the plexus in man and the monkey, the second dorsal root would be the one presiding over the intrinsic muscles of the hand. Presumably in those cases where it is not found, its functions are represented in the first dorsal.

Dilator Nerve of the Iris.—Prof. Yeo and I mentioned in our paper (*sup. cit.*) that in one case in which we directed special attention to the pupil, stimulation of the anterior roots from the first dorsal to the fourth cervical—in reality from the second dorsal to the fifth cervical—caused no change in the pupil, though the movements of the limb occurred with regularity.

I have since investigated this point during the course of another research on which I have been for some time engaged. I have experimented on four monkeys. The animals were thoroughly narcotised with chloroform and kept so during the whole course of the experiments. The posterior roots of the nerves under investigation were cut, and the anterior stimulated within the vertebral canal with a weak induced current from the secondary coil (distant 20 to 15 cm.) of a Du Bois Reymond's magneto-electromotor and one Daniell. As in former experiments, a large flat electrode was placed on the sacrum as a neutral point, the exciting electrode being a hooked needle, by means of which the roots could be easily insulated and separately stimulated.

In the first experiment I failed to obtain dilatation of the pupil from stimulation of the spinal roots from the second dorsal up to the fourth cervical, though the functional activity of the roots was indicated by movements of the limb. In the second I exposed the dorsal roots from the eighth up to the third in-

In the ordinary chemical notation almost every chemical change is represented as much simpler than it really is; no indication is given of the fact that in most cases an *excess* of one or other of the reacting substances must be used. Thus the reaction usually written

$$AgCl + HI\ (gas) = AgI + HCl$$

would more correctly represent the distribution of the reacting bodies were it written

$$xAgCl + x'HI = xAgI + x'HCl + (x-1)AgCl + (x-1)HI.$$

If it is assumed that in the thermal study of a chemical reaction allowance is made for all the purely physical changes which accompany the chemical change, for the influence of the masses of the reacting substances and for the possible formation and decomposition of molecular groups during the reaction, there yet remains the consideration that heat is lost or gained to the system in the decompositions and formations of elementary molecules, which decompositions and recompositions may form parts of the entire change under examination. Thus, take the comparatively simple reaction

$$2H_2O + Cl_4 = 4HCl + O_2;$$

expanded thermally we have

$$r = 4[H, Cl] + [O, O] - 2[H^2, O] + 2[Cl, Cl].$$

Even the apparently most simple case, the union of two elements, is more complex than at first sight appears. $[H^2, Cl^2] = 44{,}000 +$ simply tells that 2 grams of hydrogen combine with 71 grams of chlorine, and that 44,000 gram-units of heat are evolved. But if we wish to apply these data to questions concerning the affinity of chlorine for hydrogen, we must remember that affinity is the name given to the stress between atoms when regarded from the point of view of one kind of the reacting atoms. Hence, remembering that the molecules of hydrogen and chlorine are diatomic, we must amplify the equation $[H^2, Cl^2] = 44{,}000 +$, and write it thus—

$$r = 2[H, Cl] - [H, H] - [Cl, Cl] = 44{,}000 +;$$

but the value to be assigned to two of the terms in this equation are unknown. Until we are able to assign approximate values to the thermal changes accompanying the decompositions of elementary molecules and the combinations of elementary atoms, we shall not be in a position to apply thermal data to the subject of affinity, provided, that is, we use this term in its most precise meaning.

The statement of Berthelot in the "Essai de Mécanique Chimique," that the quantity of heat evolved in a reaction measures the sum of the physical and chemical changes which occur in that reaction, and furnishes a measure of the chemical affinities, is evidently untrue if we assign any precise meaning to the term "affinity." But if we use this term in a wide sense as summing up the various actions and reactions (other than those which are purely physical) which together constitute any given chemical change, then we may perhaps say that thermal measurements of comparable reactions are also relative measurements of the affinities of the reacting substances. It is in some such sense as this that the term "affinity" is used by Thomsen in his thermal researches on the relative affinities of the non-metallic elements (vol. ii.).

It is worthy of remark that Thomsen's arrangement of the commoner acids in order of relative affinities agrees very well with that given by Ostwald as the result of his investigations conducted on altogether different lines and by very different methods.

If thermal measurements of chemical changes really represent the sums of various partial changes, some of which have a positive and others a negative value, then it becomes doubtful whether any practical result is to be looked for from the application of Berthelot's *law of maximum work*, which runs thus:—

"Every chemical change, accomplished without the addition of energy from without the system, tends to the formation of that body or system of bodies the production of which is accompanied by evolution of the maximum quantity of heat."

Thomsen puts this "law" in a somewhat different form: he says, "Every simple or complex reaction of a purely chemical kind is accompanied by evolution of heat." Thomsen explains that by a purely chemical process he means one which is accomplished without addition of energy from sources external to the system, and consists in the "striving of atoms towards more stable equilibrium." But there are, I think, two principal objections to this statement. Actions "of a purely chemical kind," as thus defined, do not actually occur except as parts of cycles of reactions wherein are included changes not of a "purely chemical kind." And, secondly, we have at present no means of measuring the thermal values of those purely chemical actions—*i.e.* on Thomsen's view, atomic actions—but are obliged to include their values in the total value assigned to the complete cycle of operations which we term a chemical reaction.

Thomsen has it is true attempted to assign thermal values to the decomposition of the molecule of carbon into atoms and the recombination of atoms of carbon to form molecules. The pages of NATURE are scarcely suitable for a detailed discussion of Thomsen's methods; it seems to me, and I think to some others who have tried to follow Thomsen's arguments, both in the second volume of his "Untersuchungen" and also in the original papers in the *Berichte* and elsewhere, that these arguments really bristle with assumptions, and that a comparison of the results deduced by Thomsen with the actual calorimetric measurements obtained by himself and others is sufficient to throw grave doubt on the validity of those assumptions on which his arguments are based. One general result which appears to me to follow from Thomsen's investigation is that the time has come when we may with great advantage give up such expressions as "the carbon atom has four bonds," "such or such atoms are held by double links," and indeed the whole of that unscientific pseudo-dynamical nomenclature which has grown up around the vague and indefinable conception of *atomic bonds*.

There are many other points of interest in Thomsen's "Untersuchungen"; but I have said enough I trust to show the importance and the remarkable suggestiveness of these volumes; and also to establish the statement that the great advances of the future in chemistry are to be looked for, not so much in the domain of organic chemistry as in the application of the methods and generalisations of the science of matter and motion to the problems which we call chemical.

M. M. PATTISON MUIR

A SCIENTIFIC CATALOGUE

Bernard Quaritch's General Catalogue. Part II. Natural History and Science. Part III. Periodicals, Journals, and Transactions. (London: 1881-83.)

IN few instances that a political economist could hold up as an example is the function of the merchant in the processes of supply and demand so clearly and simply displayed as in that of Mr. Bernard Quaritch, the wealthy merchant in the book trade. He is especially a merchantman seeking goodly pearls, whose great qualification must be that he knows the exact demand for, and the exact scarcity of, what is to be bought and sold. His catalogue does not aim at completeness as did the one which we noticed lately. Scarcely more than one-tenth of the titles carefully entered in Mr. Friedländer's lists are to be found here; but these make a collection, and a very large one, of books brought together by "natural" selection with the same good results in this case of intelligent working, as in the more automatic world around us. Many eminent men in various branches of science have first selected books bearing upon their own subjects, and then, on the dispersion of such libraries, Mr. Quaritch selects those works which have a higher value through their own superior merit, or the often doubtful though highly-prized recommendation of rarity. Accordingly Mr. Quaritch's catalogue is considerably like the sum total of British legislation. Each item of it was the supply of an existing want according to the best light of the time of its production. While circumstances, however, have changed and fresh laws have been devised to meet the changed circumstances, old laws have remained upon the statute book, and the existing code contains at the same time both inconsistent repetitions and grave deficiencies, and lacks both symmetry and completeness. While the catalogue of Mr. Friedländer shows the German love of both these good qualities and the scientific tastes of the compiler, that of Mr. Quaritch does not profess to be complete in any sense; it is a list of an immense stock of books brought together, as their former possessors ceased to require them, by a shrewd man of business who knew their market value. Hence in examining these bound up volumes which contain the many rich prizes of scientific literature constituting Part II. and Part III. of a new "General Catalogue," one is not surprised to find that a book like Agassiz's "Nomenclator Zoologicus" is to be found in four different places in one of them; that five copies of Owen's "Odontography" are offered, and a variety of copies of many others.

In Friedländer's catalogue we had to complain of too much classifying; not because classification is not of extreme value as a ready guide to the contents of a catalogue or library, but because many books refuse to fall under one head only, however discreet may be the arrangement. Mr. Bernard Quaritch's catalogue is just the reverse. In these volumes there is no attempt at either alphabetical or subject-divisions of the whole collection; different divisions are lists of books purchased at particular sales. A concise index makes up perhaps in the best way for this utter confusion of subjects. The table of contents, to which one would look first in trying to understand such a catalogue, is not printed in a way to clearly express the arrangement of those titles which *are* classified in subjects. A list of thirteen natural history headings follows "Egypt and North Africa" in exactly the same way as a nearly similar list of fourteen follows "The British Isles," but the former has nothing to do with Egypt as the latter has to do with the British Isles.

But Mr. Quaritch is the great connoisseur in a different class of books from the works which draw our attention in his catalogue. This class it would hardly come within our province to notice, were it not for the evidence given here, on the one hand, that costly books are purchased now as much as of old by the "patron" of literature, and on the other, that scientific works of original value and present scarcity are bought up by mere book collectors or bibliolaters, who would in many cases fret while one of their precious volumes was being turned over for consultation, lest it should end in a crack in its beautiful binding! Mr. Quaritch labours abundantly, and not without love, we think, for these purchasers. Here are a few of the feminine pomps and vanities with which he tickles the ears of bibliomaniacs:—"Grolier binding," "variegated leathers," "gold scroll tooling," "purple morocco super extra," "veau fauve," "veau marbré," "arms and cypher of ———," "vellum fly-leaves," "large paper," "tall copy," "magnificent specimen of bibliopegistic skill." Here is a titbit:—"First Aldine edition, very large, fine copy, in blue morocco, gold tooling, silk lining, vellum fly-leaves, gilt gaufré edges, by Bozerian."

A distinguishing feature in Mr. Quaritch's catalogues are the valuable notes appended to nearly all the most important of the works he offers. These notes as to the scarcity, completeness, market value, and often the history of the book testify to both the extent of his business and the minute accuracy of his knowledge of it. They are a mine of valuable information to any one whose business is in books, either commercially or as a librarian intrusted with the care and also the completion of important collections. In few cases will a book only professing to be a stock-list itself command a price in the market. Mr. Quaritch's catalogues command a high price, and the new edition of his general one, of which seven parts are now out, and which will probably not be completed for another year, if it should be the last which our veteran publishes, will doubtless remain for some time to come a standard work upon literature.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Elevation and Subsidence

FOR several months past articles and letters have appeared in NATURE on the subject of subsidence and elevation of the earth's crust by addition and removal of weight. In this connection also much has been said in regard to the history of the idea. I wish therefore to draw attention to the fact that in 1859 I read a paper before the American Association for the Advancement of Science on the subject of the "Formation of Continents

and Ocean Basins," in which, after giving the views of Herschell and Airy, I bring out this idea very prominently, and illustrate it by many diagrams. An abstract of this paper, by Sterry Hunt, was published in the *Canadian Naturalist*, vol. iv., 1859, p. 293, and reference to it will be found in the "Royal Society Catalogue," vol. iii. p. 919.

A very brief outline of the paper is as follows:—I make two assumptions: (1) an internal liquid with floating crust; (2) the crust of continental areas more conductive and therefore cooling and thickening more rapidly than that of oceanic areas.

It is evident that under these assumptions inequalities would commence first on the under surface of the crust by additions there, making convexities beneath the continental and concavities beneath the oceanic areas. But by *flotation* these inequalities on the under side next the liquid would be reproduced on the upper side next the atmosphere, and by this means alone continents would grow continually higher, and ocean beds deeper. Now add to these erosion. By cutting down continents and filling up the seas erosion would tend constantly to destroy these inequalities, while flotation would tend as constantly to reproduce them. Thus according to this view the continents rise partly by additions beneath and partly by removal above, and similarly the ocean beds sink partly by increased concavity beneath and partly by additions above. But evidently if unequal thickening should stop, flotation could only *partly* restore the inequalities destroyed by erosion.

Except the abstract above referred to, the paper was never published, and in February, 1865, it was destroyed, along with much else, by Sherman's army. My reason for not publishing more fully was that I soon became dissatisfied with it; for about that time the views of Hopkins and Pratt on the solidity of the earth began to attract attention, and I became convinced that dynamical geology must be reconstructed on a basis of a solid earth. But now that the idea of a sub-crust liquid or semi-liquid layer is becoming prominent (a condition which would not probably interfere with the substantial solidity of the earth in its astronomical relations), it seemed to me important that this long forgotten paper should be brought forward merely as a part of the history of the subject.

Now a few words on the subject of the communications referred to in the beginning of this letter. It seems to me that some of your correspondents have gone too far in regarding unloading by erosion as a cause of elevation. Evidently there must be some other and more fundamental cause, or erosion could not act. Evidently erosion can only partly restore an elevation produced by some other cause. Erosion is primarily an effect of elevation, only in this as in so many other cases the effect may react as a cause, to maintain the elevation. For example, the Colorado plateau region has been raised since Cretaceous times about 20,000 feet, but the maximum general erosion has been only about 12,000 feet. The erosion has been, therefore, the consequence, not the cause, of elevation, for it is impossible that the cause should lie so far behind the effect. I give this one example because it is on so large a scale, but every mountain range furnishes an example of great erosion as an effect of elevation produced by other causes. That loading and unloading the crust is a cause of subsidence and elevation there is little doubt, but that there are other and far more important causes is certain.

Berkeley, Cal., December 3 JOSEPH LECONTE

Red-deer Horns

IN continuation of my remarks on the eating of shed deer-horns by other deer, I have to add that six shed horns in various stages of erosion have been sent to me from Sutherlandshire. They each bear well defined teeth-marks on the gnawed portions, and this leaves little if any doubt that the popular belief that the horns are eaten by deer is founded on fact. The accompanying interesting letter from Mr. James Inglis, which gives the evidence of two experienced stalkers, both most intelligent and reliable men, is further confirmation of a curious though no doubt very natural habit of the deer, which finds in the lime-salts of the horn a necessary element of nutrition. You will observe that Inglis believes the deer use the molars in eating the bone, and this seems probable enough, as they apparently always begin at the points and eat towards the beam and burr, a method of proceeding by which they can bring portions of the horn within the action of the molars. J. FAYRER

December 27, 1883

"... I send a few red deer horns that have been partially gnawed by deer in the forest. I asked the stalkers to keep a look out and see if they could find any deer eating horns, and am glad to say that they have been able to put the matter beyond all doubt.

"Donald McRae saw with his glass a stag, in Dunrobin Glen, eating a horn; he went to the place where he saw him eating it, and found it partially eaten. I send it with the others. You will find a ticket on it to distinguish it from the rest.

"Duncan McPherson saw with his glass a hind, last week, eating a horn also; he did not find the horn, but he saw her (the hind), quite plainly, with it in her mouth, gnawing away at it near the point.

"Deer have no incisors in the upper jaw, but they have grinders or molars in both upper and lower jaws, formidable enough to eat any horn, and I have no doubt that it is with their molars that the horns are eaten.

"A shepherd in the parish of Lairg has a cow that eats all the bones she can find, and goes miles for them, and eats them up, shank bones and all; ribs are eaten easily, and seem to give no trouble whatever. "JAMES INGLIS

"December 24, 1883"

On the Absence of Earthworms from the Prairies of the Canadian North-West

NOT by any means the least remarkable of the very notable series of works which Mr. Darwin has given to the world is that which came last from his pen but a short time previous to his lamented death. Dealing, as it does, with effects which, when looked at in the detail, are exceedingly small and insignificant, but, when viewed in the aggregate, are shown to be of surprising importance, the "Vegetable Mould and Earthworms" must certainly rank as a most strikingly interesting work.

It is not my desire to call in question the conclusions at which Mr. Darwin has arrived with regard to the action of earthworms in cultivating the soil, but I wish to point out that in one extensive portion of the earth's surface, to which much attention has of late been directed on account of its agricultural capabilities, earthworms do not exist. I refer to the vast region commonly known as Manitoba and North-West Territories. My friend, Mr. E. E. T. Seton, of Carberry, Manitoba, was the first to point out to me that this enormous country must be regarded as forming an exception to Mr. Darwin's generalisations, on account of the total absence from it of every kind of earthworm, and, having lately returned from a visit to these regions, I can add my testimony to his in this particular, as well as in the matter of the amazing, innate fertility of the soil, which has been the wonder and remark of all travellers for years past, but which, in this case, obviously cannot be attributed to the action of worms, since these do not exist there. In addition to my own observations, I have the testimony of numbers of intelligent settlers, most of whom had been several years in the country, but all of whom unhesitatingly assured me that such a thing as an earthworm was unknown. Further, Mr. Leo Rogers, son of Mr. Thos. Rogers of Manchester, who has spent several years with the engineers of the Canadian Pacific Railway, has informed me that earthworms are unknown between Winnipeg and the Rockies. This being the case, it does not seem reasonable to suppose that they exist anywhere in the huge territory still further to the north, and comprising upwards of 3,000,000 square miles of land, or something like one third of the entire North American continent, and which may therefore be regarded as forming an exception to Mr. Darwin's statement (p. 120), that "Worms are found in all parts of the world, and some of the genera have an enormous range. They inhabit the most isolated islands; they abound in Iceland, and are known to exist in the West Indies, St. Helena, Madagascar, New Caledonia, and Tahiti. In the Antarctic regions worms from Kerguelen Land have been described by Ray Lankester, and I have found them in the Falkland Islands. How they reach such isolated spots is at present quite unknown." In connection with the statement (p. 121) that "Worms throw up plenty of castings in the United States," it may be pointed out that the boundary line (the 49th parallel) is to some extent a natural one, from which the rivers run both north and south. Further, I have been assured by friends, and have also seen with my own eyes, that earthworms abound at Toronto and in other parts of Ontario. This being the case, an interesting inquiry arises as to the cause of the absence of worms from the North-West, and I can only suggest two probable reasons—the great cold of winter and the

prevalence of prairie fires in spring and autumn. Personally I favour the latter, though both causes may in part be answerable. If worms abound in Iceland (65° N. lat.), in Kerguelen Land (50° S. lat.), and in Toronto (43·4° N. lat., mean winter temperature 27½° F.), why should they not also occur at Winnipeg (50° N. lat.)? Certainly the mean winter temperature is very low, being about 8° F., and the mean minimum for eleven years – 40½° F. I made special inquiries as to the depth to which the soil in Manitoba becomes frozen in winter. This is often as much as five or six feet, but only, I believe, in the more exposed places, and certainly as a rule it is thawed again in the spring. I do not think this would render the ground uninhabitable by worms when they are able to exist in Iceland. Mr. Darwin says nothing as to the effect of frost on worms except (p. 26) that "worms are sensitive to a low temperature, as may be inferred from their not coming out of their burrows during a frost"; but he states (p. 110) that they are easily able to descend three or four or even seven or eight feet below the surface. It would be interesting to ascertain whether worms inhabit equally cold portions of the Old World.

But the agency which I believe has caused the absence of earthworms from the North-West is, as already stated, the prairie fires which annually sweep over enormous portions of the country, totally consuming the grass, and converting it into a black ash. This, it might well be imagined, would for months together completely deprive any worms that formerly existed of that variety of decaying vegetable matter that composes their food; and assuming that fires have annually passed over large portions of the prairies for scores of generations (as seems in every way probable), it appears to me only reasonable to suppose that this cause would effectually have exterminated the worms from the country or have prevented them occupying it. It is my belief (as I shall elsewhere state more fully) that the very fertile, fine, black, powdery, and almost soot-like soil from one to three feet thick, even the open, treeless nature of the prairies themselves, and the absence from their surface, so far as my observation goes, of every single species of mollusk, while many species abound in all the ponds, lakes, and streams, are all in a large degree, if not entirely, due to the action of the fire. If this view ultimately turns out to be correct, it will be further seen that the very means which has deprived the soil of the North-West of that natural cultivation which the soils of most other countries enjoy has, at the same time, liberally supplied it with a manure resulting from the charred ashes of the grass which is annually burned. My friend, Mr. T. Rogers, who has taken much interest in the absence of worms from the North-West, and is inclined to attribute it rather to frost than to fire, though he suggests that the "alkali" may possibly have had something to do with it, has already brought the subject before the Literary and Scientific Society of Manchester, where he seems to have met with a good deal of incredulity.

As another evidence of the absence of worms, the numerous, large, Glacial boulders that strew the prairies around Brandon and elsewhere may be cited. These, had worms existed, would doubtless have long ago been lowered beneath the surface, as also the skulls and other bones of buffaloes, which so abound on the prairies, and most of which have evidently lain there a long while. Nevertheless some of these have been buried in the course of time, as one gentleman told me that he had sometimes turned them up from a depth of two or three inches beneath the surface when ploughing. Their burial may have been accomplished by the wind drifting soil over them, or by the working of gophers. Of these peculiar little animals two species are very abundant on the prairies, where they make extensive burrows, which it seems possible may to some extent accomplish the natural cultivation of the soil in the way worms are accustomed to do it elsewhere. Some more suggestive remarks on this point may be found in a paper by Mr. Seton, published in the Report of the Manitoba Department of Agriculture for 1882, and which may be studied with advantage. ROBT. MILLER CHRISTY

Chignal St. James, near Chelmsford, December 20, 1883

Magnetic Dip in South China and Formosa

WHILE engaged on a meteorological mission in China I availed myself of the opportunity to make the following determinations of the magnetic dip. The observations in Hong Kong were made at the public gardens, the Observatory being not yet ready. On October 10 I observed at the British Consulate; on November 3 at the English Presbyterian Missions Compound, Swatow. In Amoy I observed at the residence of the Commissioner I.M. Customs, in Takow (Formosa) at the Custom House, and at the South Cape (Formosa), near the magnificent fortified lighthouse. It is to be feared that the observations on the coast of China are slightly vitiated from local attraction, the rocks consisting of ferruginous granite. Southern Formosa is built up of coral, raised in places to a great height, no doubt through volcanic action. Slight earthquakes are of common occurrence in Formosa, whereas along the coast of China they are rare and of no importance except to the seismologist.

Place	Date	Local M.T. h. m.	Dip. ° ′
Hong Kong ...	1883, Nov. 5 ...	5 9 p.m. ...	32 17
,, ...	,, ,, 9 ...	5 4 ,, ...	32 19
Swatow	,, Oct. 10 ...	5 24 ,, ...	34 23
,, ...	,, Nov. 3 ...	11 25 a.m. ...	34 17
Amoy	,, Oct. 14 ...	3 50 p.m. ...	36 45
,, ...	,, ,, 16 ...	5 10 ,, ...	36 50
Takow	,, ,, 24 ...	2 45 ,, ...	32 54
South Cape ...	,, ,, 27 ...	4 0 ,, ...	31·24
,, ...	,, ,, 28 ...	4 30 ,, ...	31 27·5
,, ...	,, ,, 29 ...	3 20 ,, ...	31 24·5

W. DOBERCK,
Government Astronomer

Hong Kong, Nov. 10

THE ORIGIN OF CORAL REEFS

REGARDING this interesting geological problem, which has recently been discussed in NATURE, we are enabled through the kindness of Mr. Murray, of the *Challenger* Commission, to publish a letter which has been addressed to him by Dr. Guppy from the Pacific. The importance of this communication will be recognised in the confirmation it supplies of the inference that coral reefs start upon a platform of limestone composed of the remains of foraminifera, &c., and are themselves of no great thickness. Dr. Guppy will no doubt continue his researches, and we may hope to obtain from him precise data regarding the average thickness of the coral rock, the lithological difference between it and the underlying limestone, the structure of the limestone, whether any succession of organisms can be detected in it, and whether at any point the underlying volcanic rock can be seen which would afford a measurement of the thickness of the calcareous deposits. The effects of denudation and their relation to height above the sea will no doubt also receive his attention.

"*Shortlands Islands, Solomon Group,*
"*August* 7, 1883

"During the twelve months I have spent in this group of islands—serving as surgeon on board H.M. surveying-ship *Lark*—I have been much interested in and have devoted considerable attention to the raised coral formations in various islands; and as my observations may be of service towards confirming the views which you have advanced with reference to coral islands and reefs, I will state briefly the results of my observations.

"Excluding the large continental islands, I will refer for the sake of brevity to the numerous small islands of this archipelago, those of volcanic, and those of calcareous formations. Confining myself to the islands of calcareous formation, I will pass over the numerous small islands which are entirely composed of coral detritus, sand, and shells, and have been formed by the materials thrown up by the waves at the present sea-level; and will restrict my remarks to a very common type of islands in this group, with gently sloping and rounded profile, having an elevation varying perhaps between 100 and 1100, or 1200 feet, and composed in bulk of *an impure earthy or argillaceous limestone*, usually bedded, and almost always *foraminiferous*, now and then rich in other pelagic organisms, such as *Pteropods*. On this rock *rests the*

coral limestone, which forms but a comparatively thin crust, and has been altogether removed from most of the higher regions by sub-aërial agencies. However, I have observed the raised coral rock still preserved at considerable heights above the sea, and in two localities at elevations of 900 feet.

"Amongst the sub-group known as the Shortland Islands, I came upon beds of this *impure calcareous rock* (beneath the raised coral rock) *abounding in Pteropods*, mostly *Hyalæa*, and large foraminiferous tests, mingled with shells, some of them of shallow water habit.

"I am, &c., "H. B. GUPPY"

A FORGOTTEN EVOLUTIONIST

A BOOK has lately come into my hands a few words about which may possibly interest some of the readers of NATURE. Its title is "Histoire Naturelle des Fraisiers"; the author was A. N. Duchesne, and it was published at Paris in 1766. It must be, I suspect, an uncommon book, for there is no copy in the library of the Royal Gardens at Kew. And this library, comprising as it does the contributions of many collectors who allowed little to escape them, is remarkably complete; Mr. Daydon Jackson has in fact found in it more than a thousand publications the titles of which are not to be met with in the last edition of Pritzel's well-known "Thesaurus."

The scarceness of a botanical book is not perhaps in itself a matter of any great moment, and I bought the book out of a provincial sale catalogue without expecting it to be particularly interesting, though I knew Duchesne's name as an authority on the cultivated forms of the strawberry. I very soon, however, came to the conclusion on looking over it that it was a very remarkable production indeed, and in a scientific sense at least a century in advance of its time.

Duchesne's book is in fact the record of a purely biological study of a small group of plants. The significance of work of this sort has only been thoroughly recognised since the publication of the "Origin of Species." Just as with C. K: Sprengel, whose book was also written in the last century (1793), the world has had to roll on far into another hundred years before it was ready to do justice to this kind of research. There is a curious incongruousness between the freshness and modernness of the ideas and the faded type and musty paper in which they are embalmed.

Duchesne plunges at once into the business of his book in the first line of the preface with a straightforward simplicity not unworthy of Mr. Darwin. I will attempt a translation of the first paragraph :—

"The wish to see if it were possible to raise from seed a plant which scarcely ever produces any has led me by a happy chance to the production of a new race, which made its appearance at Versailles in 1761. This circumstance induced me to more closely devote myself to the study of strawberries, and led me to another discovery. I found that they are not all truly hermaphrodite; forms exist, in fact, which are sexually differentiated.[1] And I have succeeded in the past year, 1765, in fertilising, by means of one set of plants, individuals of another sort, which are cultivated as a matter of curiosity, and are constantly sterile. One, amongst others, has produced fruits of great beauty; M. le Marquis de Marigny has obtained for me the honour of having this submitted to the king, and it is to be raised in the Versailles Gardens by my method. This unexpected success has still more redoubled my ardour to make further observations."

The race so produced, which Duchesne called *Le Fraisier de Versailles*, or *Fragaria monophylla*, is undoubtedly a very curious plant. All its leaves are permanently unifoliate; *i.e.* instead of bearing three leaflets, as is ordinarily the case with strawberries, the petioles bear but one. Duchesne observes that this is also the case with the first leaves of all seedling strawberries. *Fragaria monophylla* may be therefore regarded as a form which always retains the juvenile, and never arrives at the adult, foliage, and this peculiarity remains constant in subsequent generations. The effect of crossing, as a potent stimulus to variation, could not but have powerfully impressed Duchesne in so striking a case as this, and further observations seemed to have led him to account for the common characters which otherwise diverging forms exhibited as best accounted for by a common ancestral origin. The study of geographically separated species, however, necessarily led him to see that something more than crossing was needed to account for variation in every case. In discussing *Fragaria virginiana*, a native of North America, which is the origin of the race of Scarlets, Duchesne speculates as to its derivation from the wild *F. vesca* of Europe, and attributes the divergences from this type to the effect of North American soil and climate.

His work on Strawberries, where he was dealing mainly with races, led him to speculate with regard to the higher groups of species, genera, and orders. His results seem to me, for the time, so extraordinarily bold, and therefore historically so interesting, that I quote the first portion of the Recapitulation, pp. 219-221, entire, in the original French :—

"J'ai déjà dit, à l'occasion du Fraisier-ananas, qu'il étoit très-difficile de ranger en ligne droite les diverses Races d'une même Espèce, de manière qu'on pût passer de l'une a l'autre par gradations de nuance. Cela est peut-être aussi impossible, que de ranger en ligne droite les Espèces, les Genres, et les Familles; par la raison que chaque Race, comme chaque Espèce, chaque Genre, ou chaque Famille, a des rapports de ressemblance avec plusieurs autres.

"L'ordre Généalogique est donc le seul que la nature indique, le seul qui satisfasse pleinement l'esprit; tout autre est arbitraire et vide d'idées. J'ai eu soin, à chacune des Races de Fraisiers, d'indiquer ce qui m'a paru vraisemblable à cet égard; mais je n'ose me flatter d'avoir toujours rencontré juste. Il faudroit, pour le bien faire, avoir des connaissances certaines et précises du pays natal de chaque Fraisier, ou bien, du tems où il a été élevé de graine, et de quel autre Fraisier provenoit cette graine; j'ai fait voir combien on manquoit encore de lumières sur tout cela.

"C'est par cette raison que je me suis permis de donner mes conjectures; en voici les résultats; la forme d'Arbre généalogique les rendra encore plus sensibles, et en fera mieux saisir l'ensemble."

It is certainly startling to come upon a phylogeny of the most modern type in a book more than a century old.

It was not till after I had gratified myself with a study of Duchesne's remarkable speculations that it flashed across my mind that attention had already recently been called to them; and I found, in fact, that Prof. Alphonse de Candolle, in a short paper put together with the felicitous erudition of which he seems to possess so inexhaustible a store, had already, in May of last year,[2] stated most of the points on which I have dwelt above. And he mentions that, on the occasion of a visit to Mr. Darwin in 1880 he told him of the existence of the book, which he describes, justly enough, as "a very curious work, older than that of Lamarck, but to which no one had ever referred except for points of secondary interest."

I know little about Duchesne himself. De Candolle says that he was a horticulturist and Professor of Natural History, and that his knowledge was as varied as it was

[1] This must be one of the first observations of the tendency of plants with hermaphrodite flowers to pass into the dioecious state. The fact is now well established. (See Darwin's "Forms of Flowers," pp. 278-309.)

[2] "Darwin considéré au point de vue des causes de son succès," &c., *Archives des Sciences*, May, 1882.

sound. No one, nevertheless, ever seems to have paid the smallest attention to his evolutionary theories. Even Silvestre, who pronounced his *éloge* at a public meeting of the Société Royale d'Agriculture in 1827, abstains from the slightest reference to them.

While in his experiments and his mode of drawing conclusions from them Duchesne strongly recalls the method of Mr. Darwin, the parallel cannot be carried further. In so far as he obtained a glimpse at the modern doctrine of evolution it was in the form afterwards formulated by Lamarck. Of the part played by the struggle for existence in the matter I find no trace in his writings.

W. T. THISELTON DYER

TEACHING ANIMALS TO CONVERSE

MR. DARWIN'S notes on Instinct, recently published by my friend Mr. Romanes, have again called our attention to the interesting subject of instinct in animals.

Miss Martineau once remarked that, considering how long we have lived in close association with animals, it is astonishing how little we know about them, and especially about their mental condition. This applies with especial force to our domestic animals, and above all of course to dogs.

I believe that it arises very much from the fact that hitherto we have tried to teach animals rather than to learn from them,—to convey our ideas to them, rather than to devise any language, or code of signals, by means of which they might communicate theirs to us. No doubt the former process is interesting and instructive, but it does not carry us very far.

Under these circumstances it has occurred to me whether some such system as that followed with deaf-mutes, especially by Dr. Howe with Laura Bridgman, might not prove very instructive if adapted to the case of dogs.

Accordingly I prepared some pieces of stout cardboard, and printed on each in legible letters a word such as "Food," "Bone," "Out," &c. The head master of one of the deaf and dumb schools kindly agreed to assist me. We each began with a terrier puppy, but neither of us obtained any satisfactory results. My dog indeed was lost before I had had him long. I then began training a black poodle, "Van" by name, kindly given me by my friend Mr. Nickalls. I commenced by giving the dog food in a saucer, over which I laid the card on which was the word "Food," placing also by the side an empty saucer, covered by a plain card.

"Van" soon learnt to distinguish between the two, and the next stage was to teach him to bring me the card; this he now does, and hands it to me quite prettily, and I then give him a bone, or a little food, or take him out, according to the card brought. He still brings sometimes a plain card, in which case I point out his error, and he then takes it back and changes it. This however does not often happen. Yesterday morning, for instance, "Van" brought me the card with "Food" on it, nine times in succession, selecting it from among other plain cards, though I changed the relative position every time.

No one who sees him can doubt that he understands the act of bringing the card with the word "Food" on it as a request for something to eat, and that he distinguishes between it and a plain card. I also believe that he distinguishes for instance between the card with the word "Food" on it and the card with "Out" on it.

This then seems to open up a method, which may be carried much further, for it is obvious that the cards may be multiplied, and the dog thus enabled to communicate freely with us. I have as yet, I know, made only a very small beginning, and hope to carry the experiment much further, but my object in sending this communication is twofold. In the first place I trust that some of the readers of NATURE may be able and willing to suggest extensions and improvements of the idea.

Secondly, my spare time is small and liable to many interruptions; animals also we know differ greatly from one another. Now many of your readers have favourite dogs, and I would express a hope that some of them may be disposed to study them in the manner indicated.

The observations, even though negative, would be interesting; but I confess I hope that some positive results might follow, which would enable us to obtain a more correct insight into the minds of animals than we have yet acquired.

JOHN LUBBOCK

High Elms, Down, Kent, December 20, 1883

THE FRENCH DEEP-SEA EXPEDITION OF 1883

I HAVE just returned from a very short visit to Paris, made for the purpose of inspecting the Mollusca which were procured during last summer's deep-sea expedition in the French Government steamer *Talisman*. The expedition was under the scientific charge of Prof. Alphonse Milne-Edwards. For the opportunity of this inspection I was indebted to the kindness of my friend Dr. Paul Fischer, whose reputation as a conchologist is so well known.

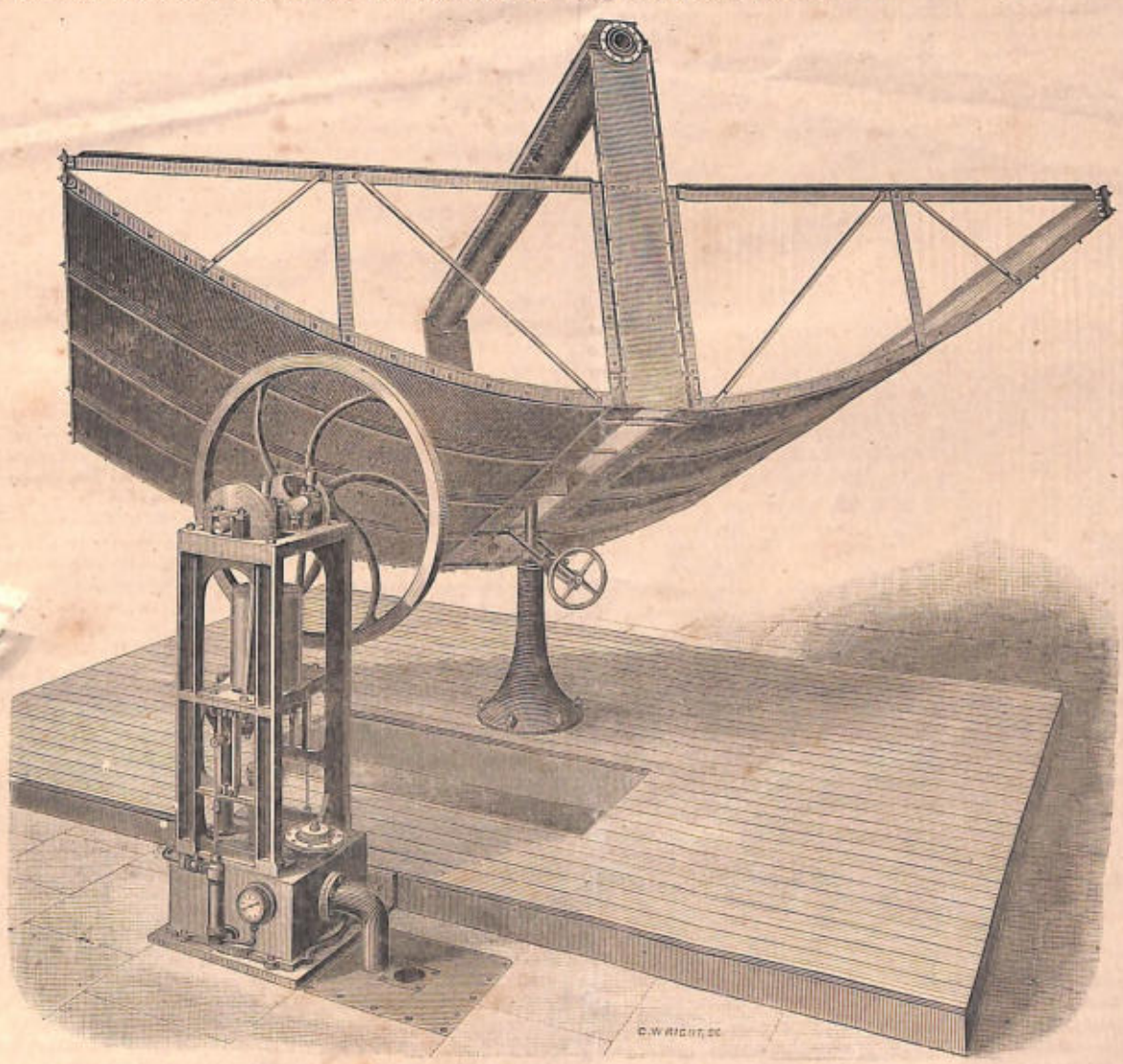
The course of the expedition was along the Atlantic coasts of Spain, Marocco, Sahara, Senegal, Cape Verde Isles, the Canaries, and Azores; and the time occupied was three months. More full and accurate particulars will very shortly be given by Prof. A. Milne-Edwards to the Academy of Sciences, and be published in their *Comptes Rendus*. The collection will be exhibited next month to public view. The greatest depth explored was about 2200 fathoms. The trawl was mostly used. Life was plentiful everywhere. As was the case in the *Porcupine*, *Challenger*, and other expeditions of the same kind, many animals (especially Crustacea) at the greatest depths were highly and brightly coloured, some of them having large eyes, and others being blind or eyeless. There was an abundance of hitherto unknown forms (genera and species) in every department of zoology—fishes, Mollusca, Polyzoa, Crustacea, Annelids, Echinoderms, Polyps, Corals, Foraminifera, and Sponges. Among the Mollusca were some remarkable cases of the wide distribution of species in respect of space as well as of depth. For instance, boreal shells, such as *Fusus islandicus* and *F. berniciensis*, which inhabit northern seas at moderate depths, viz. 50 to 80 fathoms, were found living off the coast of Marocco, and the latter species even below the tropic of Capricorn, at depths of from 450 to 2200 fathoms. *Lima excavata*, considered a peculiarly Norwegian species, was likewise obtained off the Moorish coast, of a very large size; it was recorded by Prof. Seguenza as a Pliocene fossil of Sicily and Calabria, under the name of *Lima gigantea*. In the *Porcupine* Expedition of 1870 fragments were dredged off Cape St. Vincent; and in the *Challenger* Expedition this fine species was obtained from 10 to 175 fathoms off Western Patagonia and Japan. A bivalve (*Scrobicularia longicallus*), which in northern seas inhabits moderate depths, was procured in many places by the *Talisman*, at depths varying from 350 to 1429 fathoms. It occurred living in the deepest dredgings of the *Porcupine* Expedition of 1869, off the coast of Brittany, at a depth of 2435 fathoms. Many Mollusca (*e.g. Pecten vitreus*, *Limopsis minuta*, *Dentalium agile*, *Trochus ottoi*, *Columbella haliæeti*, and *Scaphander punctostriatus*) seem to inhabit the depths of the North Atlantic in every part, from one side to the other. The smaller shells in the *Talisman* collection have not yet been picked out. The Marquis de Folin will, with his usual care and industry, undertake that part of the work, which will occupy some time; he has requested me to examine and name those species which

are known to me. I understand that another deep-sea expedition will be made by our enterprising neighbours next summer, being the fourth in consecutive years.[1]

December 21, 1883 J. GWYN JEFFREYS

THE SUN MOTOR AND THE SUN'S TEMPERATURE

THE annexed illustration (Fig. 1) represents a perspective view of a sun motor constructed by the writer, and put in operation last summer. This mechanical device for utilising the sun's radiant heat is the result of experiments conducted during a series of twenty years; a succession of experimental machines of similar general design, but varying in detail, having been built during that period. The leading feature of the sun motor is that of concentrating the radiant heat by means of a rectangular trough having a curved bottom lined on the inside with polished plates so arranged that they reflect the sun's rays towards a cylindrical heater placed longitudinally above the trough. This heater, it is scarcely necessary to state, contains the acting medium, steam or air, employed to transfer the solar energy to the motor; the transfer being effected by



Ericsson's Sun Motor, erected at New York, 1883.

means of cylinders provided with pistons and valves resembling those of motive engines of the ordinary type. Practical engineers as well as scientists have demonstrated that solar energy cannot be rendered available for producing motive power, in consequence of the feebleness of solar radiation. The great cost of large reflectors and the difficulty of producing accurate curvature on a large scale, besides the great amount of labour called for in preventing the polished surface from becoming tarnished, are objections which have been supposed to render direct solar energy practically useless for producing mechanical power.

The device under consideration overcomes the stated objections by very simple means, as will be seen by the following description:—The bottom of the rectangular trough consists of straight wooden staves, supported by iron ribs of parabolic curvature secured to the sides of the trough. On these staves the reflecting plates, consisting

[1] P.S.—In NATURE of December 20 (p. 172), I overlooked the misprint of India for Sweden.—J. G. J.

of flat window glass silvered on the under side, are fastened. It will be readily understood that the method thus adopted for concentrating the radiant heat does not call for a structure of great accuracy, provided the wooden staves are secured to the iron ribs in such a position that the silvered plates attached to the same reflect the solar rays towards the heater. Fig. 2 represents a transverse section of the latter, part of the bottom of the trough, and sections of the reflecting plates; the direct and reflected solar rays being indicated by vertical and diagonal lines.

Referring to the illustration, it will be seen that the trough, 11 feet long, and 16 feet broad, including a parallel opening in the bottom, 12 inches wide, is sustained by a light truss attached to each end; the heater being supported by vertical plates secured to the truss. The heater is 6¼ inches in diameter, 11 feet long, exposing 130 × 9·8 = 1274 superficial inches to the action of the reflected solar rays. The reflecting plates, each 3 inches wide and 26 inches long, intercept a sunbeam of 130 ×

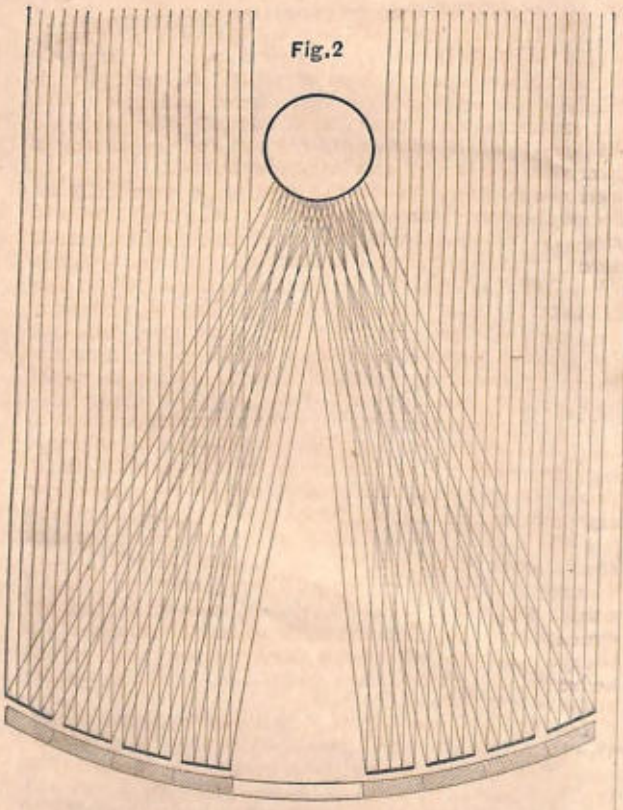


Fig. 2

180 = 23,400 square inches section. The trough is supported by a central pivot, round which it revolves. The change of inclination is effected by means of a horizontal axle—concealed by the trough—the entire mass being so accurately balanced that a pull of five pounds applied at the extremity enables a person to change the inclination or cause the whole to revolve. A single revolution of the motive engine develops more power than needed to turn the trough, and regulate its inclination so as to face the sun, during a day's operation.

The motor shown by the illustration is a steam-engine, the working cylinder being 6 inches in diameter, with 8 inches stroke. The piston rod, passing through the bottom of the cylinder, operates a force-pump of 5 inches diameter. By means of an ordinary cross-head secured to the piston-rod below the steam cylinder, and by ordinary connecting rods, motion is imparted to a crank shaft and fly-wheel, applied at the top of the engine frame; the object of this arrangement being that of showing the capability of the engine to work either pumps or mills. It should be noticed that the flexible steam-pipe employed to convey the steam to the engine, as well as the steam chamber attached to the upper end of the heater, have been excluded in the illustration. The average speed of the engine during the trials last summer was 120 turns per minute, the absolute pressure on the working piston being 35 lbs. per square inch. The steam was worked expansively in the ratio of 1 to 3, with a nearly perfect vacuum kept up in the condenser inclosed in the pedestal which supports the engine frame.

In view of the foregoing, experts need not be told that the sun motor can be carried out on a sufficient scale to benefit very materially the sun-burnt regions of our planet.

With reference to solar temperature, the power developed by the sun motor establishes relations between diffusion and energy of solar radiation which show that Newton's estimate of solar temperature must be accepted. The following demonstration, based on the foregoing particulars, will be readily comprehended.

The area of a sphere whose radius is equal to the earth's mean distance from the sun being to the area of the latter as $214{\cdot}5^2 : 1$, while the reflector of the solar motor intercepts a sunbeam of 23,400 square inches section, it follows that the reflector will receive the heat developed by $\frac{23400}{214{\cdot}5^2} = 0{\cdot}508$ square inch of the solar surface. Hence, as the heater of the motor contains 1274 square inches, we establish the fact that the reflected solar rays acting on the same are *diffused* in the ratio of $1274 : 0{\cdot}508 = 2507 : 1$. Practice has now shown that, notwithstanding this extreme diffusion, the radiant energy transmitted to the reflector by the sun is capable of imparting a temperature to the heater of 520° Fahr. above that of the atmosphere. The practical demonstration thus furnished by the sun motor enables us to determine with sufficient exactness the minimum temperature of the solar surface. It also enables us to prove that the calculations made by certain French scientists indicating that solar temperature does not exceed the temperatures produced in the laboratory are wholly erroneous. Had Pouillet known that solar radiation, after suffering a *two-thousand-five-hundred-and-seven-fold* diffusion, retains a radiant energy of 520° Fahr., he would not have asserted that the temperature of the solar surface is 1760° C. Accepting Newton's law that "the temperature is as the density of the rays," the temperature imparted to the heater of the sun motor proves that the temperature of the solar surface cannot be less than 520° × 2507 = 1,303,640° Fahr. Let us bear in mind that, while attempts have been made to establish a much lower temperature than Newton's estimate, no demonstration whatever has yet been produced tending to *prove* that the said law is unsound. On the contrary, the most careful investigations show that the temperature produced by radiant heat emanating from incandescent spherical bodies diminishes inversely as the *diffusion* of the heat rays. Again, the writer has proved by his vacuum-actinometer, inclosed in a vessel maintained at a constant temperature during the observations, that for equal zenith distance the intensity of solar radiation at midsummer is 5°·88 Fahr. less than during the winter solstice. This diminution of the sun's radiant heat in aphelion, it will be found, corresponds within 0°·40 of the temperature which Newton's law demands. It is proposed to discuss this branch of the subject more fully on a future occasion.

The operation of the sun motor, it will be well to add, furnishes another proof in support of Newton's assumption that the energy increases as the *density* of the rays. The foregoing explanation concerning the reflection of the rays (see Fig. 2), shows that no augmentation of temperature takes place during their transmission from

the reflector to the heater. Yet we find that an increase of the number of reflecting plates increases proportionably the power of the motor. Considering that the parallelism of the rays absolutely prevents augmentation of temperature during the transmission, it will be asked: What causes the observed increase of mechanical power? Obviously, the energy produced by the increased *density* of the rays acting on the heater. The truth of the Newtonian doctrine, that the energy increases as the density of the rays, has thus been verified by a practical test which cannot be questioned. It is scarcely necessary to observe that our computation of temperature—1,303,640° Fahr.—does not show maximum solar intensity, the following points, besides atmospheric absorption, not having been considered:—(1) The diminution of energy attending the passage of the heat rays through the substance of the reflecting plates; (2) the diminution consequent on the great amount of heat radiated by the blackened surface of the heater; (3) the diminution of temperature in the heater caused by convection. J. ERICSSON

A CHRISTMAS VISIT TO BEN NEVIS OBSERVATORY

ALTHOUGH I have no tale of perilous adventure or hair-breadth escape to tell the readers of NATURE, yet I think that they will be interested to hear of the progress that is being made in the first British attempt at the cultivation of high-level meteorology. This interest will be all the greater that the hearty encouragement and support that the Ben Nevis experiment has received from all parts of the United Kingdom has given it the character of a national undertaking.

As most of the readers of NATURE doubtless know, the observatory is at present in the experimental stage. A good road to the top with bridges and waterways has been made, and a part of the building erected sufficient to shelter the observers. It was judged wise to build as little as possible, until experience should have taught us the peculiar difficulties to be contended with in the somewhat novel circumstances presented by the summit of Ben Nevis in winter time. For, although several high level meteorological observatories, and indeed many other human habitations, already exist at much greater heights above the sea, yet there is probably no spot at present inhabited all the year round that presents climatic vicissitudes so remarkable. When winter is over, the directors will have a full report, with practical suggestions from the superintendent, Mr. Omond, to guide them in their further operations. Still it was thought well that some of the governing body should see with their own eyes the state of the observatory, and the work of the observers during the cold season. Accordingly two of them (Mr. John Murray and myself) made a visit of inspection on December 26th, of which I propose to give a few particulars.

Accompanied by Mr. Maclean, the contractor for the road and observatory buildings, we started from Fort William about 9 30 on Wednesday morning. At first the sky was dark and gloomy, and it was thought that Ben Nevis was to give a specimen of his worst weather. It was not cold however; in fact it was oppressively warm during the first thousand feet of the ascent from the farm of Achantie where the new road begins. This, coupled with the fact that the pony which one of the party rode up the first 2500 feet of the hill somewhat forced the pace, made it a little uncomfortable for the two pedestrians. The newly made road, loosened by the frost, and sodden by the rain and melting snow, was in places very heavy. Up as far as the little lake (Loch an Meall aut Suidhe), however, the roadway had suffered no substantial damage, except that the fall of a large stone had carried away a small piece of the margin; and all the bridges and waterways were found in excellent condition. This is very satisfactory, for the snow has already been down to Fort William; and recently a very rapid thaw has carried it so completely away, that on the 26th very little was met with under 3000 feet. The test has thus been tolerably severe and yet up to 2600 feet or so the road on the 26th was in far better condition than it was on the day of the opening ceremony. About the altitude just mentioned, a part of the road had been badly ploughed up by a spate of water from the melting snow; higher up still, the damage seemed to be less, but it was not so easy to judge, as the roadway was there gradually lost in the overlying snow.

As the party rose in height, the temperature of the air and the ardour of the pony alike fell, and then the walkers were left to the full enjoyment of their climb. During the latter part of the first 3000 feet, the mist had been so thick that the pony and its rider could scarcely be discerned a few yards off; but several hundred feet higher, after the road had been finally lost sight of in the snow, and all the party were on foot, we suddenly emerged about noon from the gloom of the mist into the brightest of daylight. Overhead the sky was blue, a fresh light breeze was blowing, and the reflected sunlight was shining in silvery masses on the undulating surface of the frozen snow. We soon reached Buchan's Well, the position of which had been marked by a wooden pole; but the well itself was completely hidden by a deep snow-drift, which filled the hollow in which it lies. From this spot to the top, the ascent was made almost straight over the snow. At times it was steep and slippery, but the surface was so hard that we rarely sank over the ankles. Two of us were rough shod, one having a few cricketer's spikes screwed to the soles of his boots, the other a pair of *steigeisen* (climbing irons), the use of which he had learned several years ago during some excursions in the Tyrolese Alps. Mr. Maclean, who had not taken these precautions, fell once or twice, but fortunately without being hurt in any way. When near the last slope we descried Mr. Omond hacking away most assiduously with an ice-axe to prepare a way for us, a needless precaution as far as the rough-shod members of the party were concerned.

The view from the plateau on the summit was magnificent. All round there floated a billowy ocean of white mist, from which rose masses of the same, piled up in places like mountain ranges, and through which rose here and there black mountain peaks (prominent among these Schiehallion). Away towards Fort William was stretched a black curtain of mist in striking contrast with the snow-whiteness of the upper layer. Down in Glen Nevis a similar mass was seen, rolled and twisted by the air-currents into the most fantastic shapes. So grand was the spectacle that one of our party insisted that we had before us the model from which Dante had drawn his vision of the entrance to hell.

The summit reached, the directors naturally looked around for the building, whose site they had chosen some five months before, and upon whose construction they had expended so much anxious thought. There was, however, nothing to be seen but two small dark-looking stumps rising a little over the surrounding snow flat, and alongside of these a little mound of snow. The stumps turned out to be the chimney and ventilator on the roof of the observatory, and the mound was a portico built by the observers with blocks of frozen snow to protect a snow staircase which had been carried down the side of the house to the doorway. After descending under the translucent canopy and stumbling for a little in the unfamiliar darkness of the passage, we entered the main room of the observatory, which for the present serves as sitting-room, kitchen, and office combined. Here we found the table laid for our lunch; and very soon we were comforting ourselves with hot coffee, cabin biscuits, and excellent Danish butter from the stores of the establishment. The whole ascent had occupied a little over three hours and a half.

The little room in which we sat contained the American

stove which heats the whole observatory, and on which the snow melting and all the cooking is done by John Duncan, the second assistant observer and housemaid. On one of the walls is the combined sideboard and crockery and instrument cupboard; against another stands a small bench with a vice; and on a third is the telegraph instrument, Mr. Omond's desk and book-case, and the drawers in which are kept the records of the observatory. Out of the sitting-room open the three bedrooms for the observers, which resemble very closely the cabins on board a ship; indeed the whole establishment has an intensely nautical air about it, and the visitor steadies himself instinctively now and then, and wonders that the roll never comes.

The rest of the building is occupied with a coal and oil store, and a storeroom in which are kept the cabin biscuits, dried potatoes, tinned soups, meat, and vegetables, lime juice, and medicine chest; which Mr. Omond calculates will support the three observers till June.

The afternoon and evening we spent in watching the observers at work, in dining (which we did very comfortably off the Christmas cheer, viz. roast turkey and plum pudding, provided for the inhabitants of Ben Nevis by a thoughtful friend), and in eager discussion of plans for the present and future work of the Observatory. The routine of the observatory at present consists in hourly observations of the barometer, protected thermometers, dry and wet bulb and maximum and minimum, wind-direction and pressure, rain, snow, sleet or hail, mist, fog or haze, clouds lower and upper, amount, species, and direction, sunshine recorder, miscellaneous, thunder, lightning, haloes, auroræ, meteors, &c., nature and precise time of occurrence of. The self-registering barograph and thermograph now added to the collection of instruments are working very well, and will be invaluable for the record of sudden changes. The protected thermometers and the thermograph are attached to a ladder fixed in the snow. As the level of the snow rises and falls, they are moved from step to step, so as to keep them as nearly as possible to the regulation distance of four feet from the surface. A measurement from the top of the ladder to the surface gives the depth of the snow, which at present varies from six to ten feet at different parts of the summit of the mountain.

Any detailed account of the winter climate of Ben Nevis would be premature and out of place in this notice; but Mr. Buchan has kindly furnished me with an analysis of the meteorological phenomena on Christmas and the following day which were in several respects remarkable.

At 1 A.M. of Christmas day, temperature was 37°·0 from which it steadily fell to 31°·5 at 11 A.M., the air all the time being quite saturated and loaded with dark, gloomy mist, with a barometer steadily rising. The wind was moderate from north-west till 3 A.M., when it changed to west-south-west. About noon the mist pall cleared away and the sun shone out with great splendour. From this hour to midnight, the following most remarkable observations were made (see table).

Except a few cirrus clouds which appeared about one, three, four, and ten o'clock, the sky was cloudless throughout, and during the evening the stars sparkled with unwonted brightness in the dark blue sky.

These remarkable atmospheric conditions were strictly confined to the higher region of Ben Nevis. Fog or cloud covered the lower hills and filled the valleys all the afternoon; it rose sometimes as high as the "plateau of storms," but was mostly below 3000 feet on Ben Nevis, and during the time no other hill showed itself through the sea of cloud. The sunset of the 25th, as well as the sunrise of the 26th, was very beautiful. On the 26th pressure remained high and steady, wind south-westerly, sky generally clear, and temperature and humidity equally

	Barom.	Therm.		Humidity.	
	Inches.	Dry.	Wet.	Calculated.	Air Hygrometer.
Noon	25·822	33°·0	31°·9	88	—
1 P.M.	·834	36°·9	33°·4	70	86
2 ,,	·824	37°·6	33°·1	66	76
3 ,,	·823	40°·9	31°·8	45	64
4 ,,	·819	37°·7	30°·8	50	71
5 ,,	·818	39°·8	31°·8	47	60
6 ,,	·817	40°·0	32°·8	50	56
7 ,,	·811	39°·3	32°·2	51	57
8 ,,	·813	38°·4	32°·1	53	64
9 ,,	·812	32°·8	31°·7	87	77
10 ,,	·813	38°·8	34°·7	69	63
11 ,,	·804	37°·0	36°·1	92	65
Midnight	·809	38°·9	38°·5	97	62

remarkable as on the preceding day. Indeed at 2 P.M. the relative humidity, which was lower than could be calculated from Glaisher's Tables, was only 34. At 3 P.M. temperature had fallen 6°·9, humidity risen to 96°, and a light fog prevailed for the next four hours, the wind having shifted from south-west to west-north-west. About 7 P.M. the sky again cleared, temperature steady, rose from 28°·0 to 36°·0 at midnight, and a humidity as low as 67 was observed. The great significance of these observations on Ben Nevis will be more apparent when compared with the anticyclone which overspread so large a part of north-western Europe at the time, to which, being situated on its west side, we owed the mild weather of Christmas, 1883.

In addition to the hourly observations, the observers have had for some time back to conduct a constant warfare with the rapidly-accumulating snow. Every now and then all hands had to be turned out to clear the doors and windows of the observatory; and it sometimes happened that, when they went out for this purpose, the snow drifted in so rapidly that it was almost impossible to shut the door again. The device of the snow staircase got over the difficulty to a large extent as regards the door, and it is proposed to build tubes with short lengths of rectangular wooden framework, passing from the windows up to the surface of the snow. At the upper end of these will be placed, at night or during heavy snowfalls, light canvas doors, which can be afterwards removed and additional lengths of framework added according to necessity. The chimney will be lengthened in a similar way by means of iron tubes, which have been sent up for the purpose. In this way the difficulties of the present winter will be met. For the future it is proposed to get over the difficulty of the accumulating snow by building an observing tower at some little distance from the living-rooms. In this tower there will be several stories with doors to the four cardinal points of the compass, so that the observers may use for exit and entrance that story which is nearest the snow level, and that door which happens to be on the lee-side of the tower. In the ground-floor of this tower it is proposed to place a seismometer and self-registering magnetic instruments. On the roof will be placed an anemometer for measuring the direction and strength of the wind. It is proposed so to arrange this instrument that its indications can be read inside the tower. This appears to be essential, for during the storm on the 12th ult. it was found impossible to go outside the observatory, so that wind observations are wanting in the daily sheet on that very interesting occasion. The observing-tower will be connected with the rest of the buildings by a covered way of some length fitted with doors to cut off the hot air; and in all probability the accommodation of the observatory will be increased by the addition of an office, or experimenting room, and one or more small

bedrooms for the use of inspectors or others on temporary business, and for the convenience of scientific men who may wish to make a visit to the observatory for the purposes of scientific research.

For reasons sufficiently explained, the staff has scarcely had time as yet to go beyond the mere routine of observations above mentioned; but none of the valuable suggestions which Mr. Omond and the directors have received have been lost sight of. A beginning has already been made in the collection of meteoric dust; in fact Mr. Murray carried down with him a portion of the residue obtained by melting considerable quantities of surface snow. This is now being examined, and we shall doubtless hear by and by whether it is all of purely local, or partly of volcanic or cosmic origin.

It is intended, as soon as proper arrangements can be made, and the concurrence of the Post Office authorities obtained, to commence a series of simultaneous observations on earth currents along the cable from the summit of Ben Nevis to Fort William, and along a telegraph line from Fort William to some other station not far above sea-level. By means of this horizontal and vertical exploration we hope to obtain some interesting data (either positive or negative) regarding the origin of the variations of terrestrial magnetism, auroræ, &c. The cable will also be turned to account for observations on atmospheric electricity. These plans are mentioned partly to show that the directors are fully alive to the manifold uses to be made of their stronghold upon Ben Nevis, partly to incite scientific men generally to favour us with their suggestions for the full utilisation of the observatory, not only for meteorology, but for physical science in general.

It would take too long to dwell at length on all the interesting casual observations recorded in Mr. Omond's log, a detailed account of which will probably be given hereafter by Mr. Omond himself. It may be interesting, however, to allude to the frequently occurring phenomenon which he calls "Glories." The shadow of the head or hands of the observer is frequently seen on the clouds in the valley to the north-east surrounded by a halo of colour. The phenomenon appears to be akin to, or identical with, the mist phantom so well known under the name of the "Brocken Spectre." The occurrence of this phenomenon is by no means so rare in this country as many suppose. The writer of this notice saw it to perfection three years ago in Skye. A party of four or five of us were standing on Sgur-na-Panachtich, or e of the Cuchullin peaks; we were looking down on the dark rock basin of Coruisk, in which was floating a cloud of mist. The sun was low behind us; and, projected on the mist, we saw what appeared to be gigantic dark shadows of ourselves completely outlined with a glory of rainbow colours. Each could see his own spectre best, but also those of his neighbours more or less distinctly. The figures imitated every motion we made, and, when we whirled our alpenstocks over our heads, the antics of the phantoms were most weird and awe-inspiring.

We spent the night of the 26th at the Observatory. During the first watch, that is, up to about one o'clock in the morning, we sat up, and went out with the observer when he made his hourly observations. The air felt quite mild, although the temperature was about the freezing point; the sky was perfectly clear, and the stars shone brilliantly. Mr. Omond brought out his telescope, and we lay down on the snow and examined Jupiter and his satellites, filled our eyes with the beauties of the Pleiades, and exhausted our little stocks of astronomical knowledge by naming such constellations as we happened to know.

The staff had insisted on providing each of us with a bed; we thus had good opportunity of testing their sleeping accommodation, which turned out to be excellent. Next morning we rose to see the sun rise, and were richly rewarded. About eight o'clock a ribbon of bright crimson appeared behind Schiehallion, which deloped a gorgeous succession of tints ending in copper colour and brick red, under the gradually rising sun; to right and left appeared the peculiar green colours so marked in the recent remarkable sunsets, to which the Ben Nevis sunrise showed a great resemblance. The greater part of the horizon was clear, and we had a view of the surrounding mountains seldom, if ever, equalled in summer time for beauty of colour. Ben More, the range of Glencoe, the Perthshire Hills, the whole length of the Caledonian Canal, the Cuchullin Hills, could all be seen with perfect distinctness. The white snow on the black-blue hilltops, and the bright red of the withered heather and bracken lower down, afforded contrasts of colour to be seen at no other season. Some of the hillsides shone in the sunlight like bronze. Others glowed like the richest velvet, and the valleys were filled with the subtle blue haze that gives such a charm to the scenery of the west of Scotland.

We naturally congratulated Mr. Omond on the weather he enjoyed on Ben Nevis; but it appeared that the treat was as great for him as for us. Since he began his seclusion on November 11, there had been just three fine days—the day on which he went up, Christmas day, and the day following, all the rest of the time the most he had seen was an occasional glimpse of a snow-covered mountain-peak through a hole in the mist. Our good fortune had been great; and, although it might have suited the main purpose of our visit better to have been detained by mist and sleet, or to have seen the observatory in the process of being buried in a snow-drift, we resigned ourselves with a very good grace to what the Fates had sent us.

After sharing the regulation breakfast of tinned mutton and coffee, we went out once more to see the observers at work. We then had an opportunity of seeing the precautions which they find it necessary to take in tempestuous weather when they have occasion to go near the edge of the narrow plateau on which they live. For sanitary reasons it is necessary to carry all the refuse of the observatory to a considerable distance, where it is thrown over a cliff. In winter, when this cliff is covered with a treacherous cornice of slippery snow, and the wind blows so hard that the head of a meat tin thrown to windward is often carried right back to leeward of the mountain, the footing at the edge is anything but secure. On such occasions two of the observers go abreast with the pail of rubbish between them, and each is roped to one who goes behind with an ice-axe to steady him in case of accident.

By 11 o'clock the barometer had begun to fall, and the humidity of the air had greatly increased. Mr. Omond therefore warned us that, unless we were prepared to incur the risk of detention, we had better depart. Accordingly we packed up our trophies, consisting of the residue above mentioned, pregnant with the potentiality of cosmic and volcanic dust, a bundle of Mr. Omond's daily sheets, and a little shrew that had been killed on the previous evening, the first of a colony of these animals who, with several weasels, had taken up their abode in the outer dry stone wall of the observatory. As might be expected, animal life is very scarce in winter on the top of Ben Nevis. No deer or ptarmigan had been seen, only the tracks of foxes, which abound in certain parts of the hill. The only living things we had seen in the snow-covered part of the hill were large numbers of a dipterous fly, which we found every now and then crawling on the surface of the snow.

Having bidden farewell to Mr. Omond and his companions, and wished them good luck and a continuance of their present good health and spirits during the rest of the winter, we commenced our descent at 11.30. The bottom was reached, after several halts to enjoy the magnificent view, in about the same time as it had taken us to ascend.

In such weather as we had the ascent of Ben Nevis is decidedly more pleasant and less fatiguing than in summer. It is well, however, to warn the readers of NATURE that our case was exceptional, and that under adverse circumstances such an enterprise is likely to be both unpleasant and dangerous. G. CHRYSTAL

THE REMARKABLE SUNSETS

INFORMATION with regard to these beautiful phenomena and their cause is rapidly being collected, and at the same time the opinions of those who have given most attention to them are being stated, both here and on the Continent. Among the latter we may refer to a memoir presented by Prof. Forel to the Société Vaudoise des Sciences Naturelles, on the 19th of December. At the beginning of the displays in Switzerland, M. Forel ascribed them to those causes which produce the ordinary after-glow so beautifully visible in mountainous countries, and at first he considered that the meteorological conditions were such as to favour this view. Further inquiry, however, he now states has made this hypothesis absolutely untenable. One of his arguments is that the glows which first appeared in November and then decreased to 3rd December, regained a maximum on the 24th and 25th. Now from the 22nd to 26th December, Switzerland was the centre of a maximum of atmospheric pressure, the barometer being higher there than in any of the surrounding countries. Exactly the opposite held in November, and this confirms him in the idea that meteorological factors alone do not suffice to explain the glows. He also describes the dates and tracts of the chromatic phenomena observed, and considers that their origination in Krakatoa is a simple and sufficient explanation. *La Nature* for the 29th ult. contains an interesting communication from M. Van Sandick, an Engineer des Ponts et Chaussées, at Pedang, who was an eye witness of the later stage of the eruption. He was on board the *Governor-General Loudin*, and was close to Krakatoa on August 26th. His communication is accompanied by a very detailed map, showing the changes which have supervened not only in the Straits themselves, but also on the neighbouring coasts of Java and Sumatra, but we shall return to this important letter.

The new observatory on the summit of Ben Nevis has been utilised for the collection of snow, with a view of determining whether or not it contains any dust particles. This has been forwarded to Mr. John Murray of the *Challenger* Commission by Mr. Omond the superintendent of the observatory. We may hope to hear soon whether the results are positive or negative on this special point of inquiry. We have to call attention to the important letter of Mr. Macpherson published below. We learn from the *Weekly British Colonist*, published at Victoria, British Columbia, that the sunsets made their appearance there on November 27th. Long after sunset the light in the sky became more fervent in colour, till at last the waters in the harbour and straits borrowed the splendid crimson. Darting and rapidly moving blood-red rays of light were seen shooting far into the sky, suggesting an aurora. A letter from St. Raphael, on the shores of the bay of San Francisco, dated December 4th, refers to the magnificent sunrises and sunsets. The date of their commencement is not stated. From Kiakhta, on the Mongolian frontier, we learn that the glows there began on December 11th, and terminated on the 25th.

The glows were seen some time before November 6th at Kalim Pong, twenty miles north-west of Darjeeling.

We have received the following further communications on this subject:—

THE body of evidence now brought in from all parts of the world must, I think, by this time have convinced Mr. Piazzi Smyth that the late sunrises and sunsets do need some explanation, more particular than he was willing to give them. With your leave I should like to point out from my own observations and those of others that, "given a clear sky" and the other conditions put by Mr. Smyth, the sunrises and sunsets of other days, however bright and beautiful, have *not* given any such effects as were witnessed, to take an instance, here on Sunday night, December 16th. I shall speak chiefly of the sunsets.

(1.) *These sunsets differ from others, first in their time and their place or quarter.* Sunset proper is, I suppose, the few minutes between the first dipping and the last disappearance of the sun's disk below the true horizen; the pageant or phenomena we call sunset, however, includes a great deal that goes on before and after this. The remarkable and specific features of the late sunsets have not been before or at sunset proper; they have been after-glows, and have lasted long, very long, after. To take instances from your number of the 13th ult., Mr. F. A. R. Russell notices that on November 28th, the sun having set at 3.55, one after-glow lasted till 5.10, and was then succeeded by another "reaching high above the horizon." The day before he mentions the after-glow as lasting to 5.20. On the 29th a "foreglow" is reported as seen in London from 5.30 to 7.30, that is more than two hours before sunrise, which was at 7.43. On December 1st, sunset being at 3.53, Mr. Russell observed an after-glow till 5.35; on December 4th the first dawn at 6.5, the sun rising at 7.50; the next day dawn at the same time, sunrise 7.51; that evening, sunset being at 3.50, he observed not a glow only but "spokes of rays from the glowing bank" at 4.45, thatis to say, sunbeams, visible in the shape of sunbeams, 55 minutes after sunset. Mr. Johnston-Lavis speaks of the after-glow at Naples as *at a maximum* an hour after sunset. Here at Stonyhurst on December 16th, the sun having set at 3.49, the glow was observed till 5.50. Now winter dawns and after-glows do not last from an hour to two hours, and still less so day after day, as these have done. The recent sunrises and sunsets then differ from others in duration.

They differ also in the quarter of the heavens where they are seen. The after-glows are not low lingering slips of light skirting the horizon, but high up in the sky, sometimes in the zenith.

I have further remarked that the deepest of the after-glow is in the south, whereas the sun below the horizon is then northing. I see that other observers take notice of the same.

(2) *They differ in their periodic action or behaviour.* The flushes of crimson and other colours after ordinary sunsets are irregular, not the same now at the same time for two days together; for they depend upon the accidental shapes and sizes and densities of the cloud-banks or vapour-banks the sun is entering or freeing himself from, which vary and can never be alike from day to day. But these glows or flushes are noticed to be periodic before sunrise and after sunset. Mr. Russell furnishes exact estimates of the intervals of time, which he finds to be the same day after day.

(3) *They differ in the nature of the glow, which is both intense and lustreless*, and that both in the sky and on the earth. The glow is intense, this is what strikes every one; it has prolonged the daylight, and optically changed the season; it bathes the whole sky, it is mistaken for the reflection of a great fire; at the sundown itself and southwards from that on December 4, I took a note of it as more like inflamed flesh than the lucid reds of ordinary sunsets. On the same evening the fields facing west glowed as if overlaid with yellow wax.

But it is also lustreless. A bright sunset lines the clouds so that their brims look like gold, brass, bronze, or steel. It fetches out those dazzling flecks and spangles which people call fish-scales. It gives to a mackerel or dappled cloudrack the appearance of quilted crimson

silk, or a ploughed field glazed with crimson ice. These effects may have been seen in the late sunsets, but they are not the specific after-glow; that is, without gloss or lustre.

The two things together, that is intensity of light and want of lustre, give to objects on the earth the peculiar illumination which may be seen in studios and other well-like rooms, and which itself affects the practice of painters and may be seen in their works, notably Rembrandt's, disguising or feebly showing the outlines and distinctions of things, but fetching out white surfaces and coloured stuffs with a rich and inward and seemingly self-luminous glow.

(4) *They differ in the regularity of their colouring.* Four colours in particular have been noticeable in these after-glows, and in a fixed order of time and place—orange, lowest and nearest the sundown; above this, and broader, green; above this, broader still, a variable red, ending in being crimson; above this a faint lilac. The lilac disappears; the green deepens, spreads, and encroaches on the orange; and the red deepens, spreads, and encroaches on the green, till at last one red, varying downwards from crimson to scarlet or orange fills the west and south. The four colours I have named are mentioned in Lieut. G. N. Bittleston's letter from Umballa: "The sun goes down as usual and it gets nearly dark, and then a bright red and yellow and green and purple blaze comes in the sky and makes it lighter again." I suppose the yellow here spoken of to be an orange yellow, and the purple to be what I have above called lilac.

Ordinary sunsets have not this order; this, so to say, fixed and limited palette. The green in particular, is low down when it appears. There is often a trace of olive between the sundown and the higher blue sky, but it never develops, that I remember, into a fresh green.

(5) *They differ in the colours themselves, which are impure and not of the spectrum.* The first orange and the last crimson flush are perhaps pure, or nearly so, but the two most remarkable glows, the green and the red, are not. The green is between an apple-green or pea-green (which are pure greens) and an olive (which is a tertiary colour): it is vivid and beautiful, but not pure. The red is very impure, and not evenly laid on. On the 4th it appeared brown, like a strong light behind tortoiseshell, or Derbyshire alabaster. It has been well compared to the colour of incandescent iron. Sometimes it appears like a mixture of chalk with sand and muddy earths. The pigments for it would be ochre and Indian red.

Now the yellows, oranges, crimsons, purples, and greens of bright sunsets are beautifully pure. Tertiary colours may of course also be found in certain cases and places.

(6) *They differ in the texture of the coloured surfaces,* which are neither distinct cloud of recognised make nor yet translucent mediums. Mr. Russell's observations should here be read. I have further noticed streamers, fine ribbing or mackerelling, and other more curious textures, the colour varying with the texture.

In ordinary sunsets the yellows and greens and the lower reds look like glass, or coloured liquids, as pure as the blue. Other colours, or these in other parts, are distinct flushes or illuminations of cloud or landscape.

I subjoin an account of the sunset of the 16th, which was here very remarkable, from my own observations and those of one of the observatory staff.

A bright glow had been round the sun all day and became more remarkable towards sunset. It then had a silvery or steely look, with soft radiating streamers and little colour; its shape was mainly elliptical, the slightly longer axis being vertical; the size about 20° from the sun each way. There was a pale gold colour, brightening and fading by turns for ten minutes as the sun went down. After the sunset the horizon was, by 4.10, lined a long way by a glowing tawny light, not very pure in colour and distinctly textured in hummocks, bodies like a shoal of dolphins, or in what are called gadroons, or as the Japanese conventionally represent waves. The glowing vapour above this was as yet colourless; then this took a beautiful olive or celadon green, not so vivid as the previous day's, and delicately fluted; the green belt was broader than the orange, and pressed down on and contracted it. Above the green in turn appeared a red glow, broader and burlier in make; it was softly brindled, and in the ribs or bars the colour was rosier, in the channels where the blue of the sky shone through it was a mallow colour. Above this was a vague lilac. The red was first noticed 45° above the horizon, and spokes or beams could be seen in it, compared by one beholder to a man's open hand. By 4.45 the red had driven out the green, and, fusing with the remains of the orange, reached the horizon. By that time the east, which had a rose tinge, became of a duller red, compared to sand: according to my observation, the ground of the sky in the east was green or else tawny, and the crimson only in the clouds. A great sheet of heavy dark cloud, with a reefed or puckered make, drew off the west in the course of the pageant: the edge of this and the smaller pellets of clond that filed across the bright field of the sundown caught a livid green. At 5 the red in the west was fainter, at 5.20 it became notably rosier and livelier; but it was never of a pure rose. A faint dusky blush was left as late as 5.30, or later. While these changes were going on in the sky, the landscape of Ribblesdale glowed with a frowning brown.

The two following observations seem to have to do with the same phenomena and their causes. For some weeks past on fine bright days, when the sun has been behind a big cloud and has sent up (perspectively speaking) the dark crown or paling of beams of shadow in such cases commonly to be seen, I have remarked, upon the ground of the sky, sometimes an amber, sometimes a soft rose colour, instead of the usual darkening of the blue. Also on moonlight nights, and particularly on December 14, a sort of brown or muddy cast, never before witnessed, has been seen by more than one observer, in the sky. GERARD HOPKINS

Stonyhurst College, December 21, 1883

THE remarkable phenomena after sunset which, according to NATURE, were seen in the second half of November in England, Italy, at the Cape, and a little earlier in many parts of Asia, could be observed almost all over Austria and Germany. I saw them myself in an especially distinct appearance here on November 22 and 29. Soon after sunset on November 22 (at 4.30 p.m.), a crimson glow was seen in the direction of south-west, and while everybody was supposing that some large printworks lying in that direction were on fire, the glow was getting more intense, and at 5 p.m. the whole of the western sky assumed a bluish purple hue which rose up to the zenith while the sun was sinking lower, so that the glow could be attributed only to an atmospheric phenomenon. About an hour after sunset the colour of the sky was almost violet, with which the phenomenon disappeared.

According to German papers, a phenomenon of this kind and intensity was never before observed in Central Europe. Dr. Assmann, director of the Meteorological Observatory, Madgeburg, attempts to explain these phenomena by the reflection of sunlight from the upper strata of our atmosphere, highly saturated with aqueous vapour, owing to its comparatively high temperature. The phenomenon could not be attributed to electrical causes, as at that time not the slightest magnetic disturbance could be observed at the Prague Observatory. In the spectrum of this light uncommonly strong "rain bands" were seen. As the sun was about 18¾° below the horizon when the phenomena began (before sunrise) or ceased (after sunset), the reflection was calculated to

have taken place at a height of about fifty English miles.

Does it not strike you that the glow was observed at earlier periods the more we advance towards the east—the source of the late Java eruptions? B. BRAUNER

Bohemian University, Prague, December 18, 1883

THE late splendid sunsets which have so vividly attracted the attention of men of science and of the general public were so remarkable and of so long a duration in the clear atmosphere of the Castilian tableland, where sunsets are usually dull, that they have not failed to impress observers with the notion that they were due to other causes than those of common atmospheric refraction and reflection.

When the phenomena had already lasted four or five days, I read Mr. Symons' letter, published in the *Times* of the 1st inst., and I thought that possibly evidence might be obtained towards the confirmation of this theory if the sediment of fresh-fallen snow was thoroughly investigated; for if the dust of Krakatoa was really reflecting in the higher regions of the atmosphere the sun's rays, some of it must necessarily be descending towards the earth.

Luckily on the 7th of this month, and when the phenomenon was at its height, and had already lasted for about eight days, there was a fall of snow at Madrid, of which I naturally profited, submitting it to a thorough investigation, the results of which, I think, will throw some light on so remarkable a phenomenon.

The snow analysed was obtained from what had fallen on some zinc plates before the exposed windows to the north of my house, which is situated at the extreme north end of the town, where there are no buildings facing it, and also from what my friend Dr. Francisco Quisoga gathered from the windows of his house, situated about ⅛ mile to the south-east of mine; and in both the same substances were found.

The snow yielded about a litre of water, which, when the sediment had collected, was decanted, and the solid part dried at a temperature below that of boiling water. The dry powder was then tested for magnetism and it was found to be extremely magnetic. It was then incinerated on platinum foil to a bright red heat so as to destroy organic substances, and the remaining dust was then submitted to microscopical investigation. The greater part of it is made up of what probably is the natural dust of the atmosphere of Madrid; of particles of mica, generally brown, and similar to that of the Guadarrama range, and in various states of decomposition, splinters of quartz and felspar, the greater part of it orthoclase, some small fragments of tourmaline, magnetic iron, and fragments of diatoms. Besides these mineral substances, which may probably be traced to the rocks forming the vicinity of the capital, some others were found for the presence of which it is difficult to account. The most remarkable are small particles of a foliated mineral of a yellowish colour, perceptibly dichroic, and which between crossed Nichols is extinguished when the cleavage traces are parallel to the principal section of the polarising Nichol; the interference colours being of bright blue, and red, and yellow colours. Treated by boiling hydrochloric acid for twenty minutes, not a trace of action was perceived. These characters are all referable to a rhombic pyroxene, and judging from its dichroism this substance may be taken for a hypersthene, which has besides a most striking resemblance to volcanic hypersthene. In addition to this mineral, small particles are found which appear to be referable to common pyroxene of a yellowish colour, of active action in polarised light, and the extinction not taking place parallel to what seem to be the edges of the prism. Besides these minerals some corpuscles are found of hardly any action on polarised light, and sometimes full of globular concretions and other kinds of microliths, which, if seen in products of a volcanic region, I would not hesitate in considering of volcanic origin.

These are the principal substances which an investigation of the sediment of the snow which fell in Madrid on the 7th inst. have revealed, and though I am far from asserting that what appears to be foreign to the atmosphere of this part of the world is referable to the dust of Krakatoa, if further analyses in other parts of the world should show these same substances floating in the atmosphere, there would be powerful reasons for inferring that the gorgeous sunsets of the past months have been brought about in consequence of that stupendous display of the volcanic forces of our globe.

It is already a remarkable coincidence that hypersthene should have been found both by MM. Daubrée and Renard in their respective analyses of the ashes collected in the vicinity of Krakatoa. JOSEPH MACPHERSON

Madrid, December 22, 1883

COMPLYING with the request contained in your "Notes" of December 13 (p. 157), I would say that the appearances, already fully described by so many of your correspondents, commenced here on December 1. On that day I made an entry in my note-book as follows:—"Perfectly calm at sunset, with a light haze of a rose tint rolling away from overhead towards the west-south-west horizon. The colours of the sky were a very pale green, red, gold, and pink; and, as the light faded away, the south-west was one mass of deep rich red. The crescent moon (a little over eighteen days old) in the refractor was of a pale green colour, and the bright limb seemed to extend to an extraordinary distance round the dark body. Barometer falling."

Again: "December 2.—Sky clouded over by 1 p.m. Sunset, as seen between breaks in the clouds, was again of a deep rich red. Barometer steady."

"December 3.—Rainy and very dull. Barometer steady."

"December 4.—Sunset, as seen through the clouds along the horizon, was again of a deep red colour, gradually shading off into a pale rose tint towards the zenith. The moon, Fomalhaut, and Vega seemed to float in a pale rose sea; whilst thin fleecy clouds as they drifted across the moon's face were of a beautiful pale green. This appearance—as did that on the 1st—lasted for about an hour and a quarter after sunset; the rest of the sky being covered with clouds, some faintly reflecting the various tints. Barometer falling."

I should not omit to mention that the sunrises were also, more or less, of similar character. Since the 4th we have had very bad weather; gales from both north and south, heavy rains, and snow. Yet the sky, when occasionally glimpsed at sunset, seems to bear traces of the same appearances. W. E. J.

Constantinople, December 21, 1883

IN addition to the remarkable sunsets which have led to such a large amount of correspondence in NATURE and elsewhere, there is another and possibly a related phenomenon to which my attention has been directed during the last few weeks. From country friends I learn that the nights, in the absence of the moon, and even when cloudy, have been remarkably light for the time of year. I cannot profess to have witnessed this phenomenon myself, living as I do in the midst of London, where the perpetual glare of gas renders any satisfactory estimate of the atmospheric luminosity quite hopeless. It would be interesting, however, to learn whether other observers more favourably located have noticed this effect. It occurred to me that the phenomenon might perhaps be connected with the volcanic dust theory of the sunsets, being, in fact, a result of the slight phosphorescence of this dust. Whether the latter exhibits any degree of phosphorescence could be readily deter-

mined by those who are fortunate enough to possess a specimen by means of Becquerel's phosphoroscope.

R. MELDOLA

21, John Street, Bedford Row, W.C., Dec. 31, 1883

IN corroboration of what Messrs. Beyerinck and Van Dam noticed at Wageningen in connection with the late storm, I write to tell you that on the morning of December 12, after the heavy rain which accompanied the gale had ceased, the windows of my house, which is isolated and exposed, were covered with a grayish sediment, just as your correspondents describe it. It will be interesting, now that attention has been drawn to the fact, to know if the phenomenon, the result no doubt of dust brought down by the rain, has been observed elsewhere. F. M. BURTON

Highfield, Gainsborough, December 24, 1883

I SUBMIT to you two slides of dust from windows, deposited during the storm of December 12. When the contained salt crystals are dissolved by adding distilled water, the appearance much resembles that recorded in NATURE of December 20. The material, scraped from windows cleaned just before the storm, where the original drop-marks are still unaltered, was put on the cleaned slides, and a drop of distilled water added. Should my surmise be confirmed, and any of your readers desire to have specimen slides, I would forward a limited number on receipt of sixpence each to cover postage and trouble. Descriptions I have received from America, either in letters or newspaper cuttings, show an identical sequence of appearances. At Poughkeepsie, on the Hudson, the fire engines were called out on the morning of November 27, and "this spectacle has been witnessed every clear evening for several days past, generally between a quarter past five and six o'clock." A letter from Dorset, Vermont, November 29, describes "a very unusual exhibition in the skies for the past three or four evenings. It has been clear, and the colouring intense, from flame to a delicate pink, and the clouds off at a distance would look light green. . . . It gave an impression of an intense fire the other side of the West Mountains, and colouring the entire sky." J. EDMUND CLARK

York, December 22, 1883

THE accompanying extract may be of service to you. Sapporo is in the northernmost island of Japan (Yeso), in lat. 43° N., and long. (*circa*) 141° E. As the telegraph ramifies through all parts of Japan, it is improbable that any considerable local eruption would have taken place to account for the phenomenon without news of it having also reached the *Official Gazette*. ROBERT BEADON

11, Lee Park, Lee, Kent, December 14, 1883

Extract from *Japan Weekly Mail* (published in Yokohama) of October 20, 1883. (The *Official Gazette* is the Government gazette published in Japanese.)—"The *Official Gazette* states that, since the 13th inst., a constant haze has pervaded the atmosphere of Sapporo, and that the sun and moon are of a blood-red colour. Clouds of ashes fall continuously. The phenomenon is ascribed to some volcanic eruption."

NOTES

PROF. OWEN has received the honour of K.C.B. as an acknowledgment of his eminent services for sixty years to science and the public interests.

PROF. W. H. MACKINTOSH has been elected to the Professorship of Comparative Anatomy in Trinity College, Dublin, vice Prof. Macalister, F.R.S., who resigned on his appointment to the Anatomy Chair at Cambridge.

BY the death of the well-known mathematician, the Rev. W. Roberts, M.A., the Rev. Richard Townsend, M.A., F.R.S., becomes a Senior Fellow of Trinity College, Dublin, thereby vacating the Professorship of Natural Philosophy held by him since 1870.

THE vacancy in the Professorship of Geology and Mineralogy in the University of Dublin has been filled by the election of Prof. Sollas, of University College, Bristol. This appointment will give great satisfaction, and will afford Mr. Sollas large opportunities for palæontological research; the large collections of fossil plants and vertebrates in the museum in Dublin remaining to this day almost unknown.

THE Swedish Government intend to establish a botanico-physiological station in the north of Sweden for the study of the flora and the diseases of the crops in that part of the country.

THE Finnish Government have ordered a steamer to be specially built in Sweden for the scientific researches about to be prosecuted in the Baltic.

M. HOUZEAU, who was only recently appointed director of the Brussels Observatory, has resigned his post, and it is reported that M. de Konkolly of Gzalla Observatory, Hungary, will succeed him.

PROF. MAURICE LEVY has been nominated member of the Paris Academy of Sciences in the Section of Mechanics.

THE Prince of Wales, as President of the Society of Arts, has transmitted to Lady Siemens the resolution passed after the death of Sir William Siemens, by the Council of that Society, and in doing so has expressed his own appreciation of Sir William Siemens's labours.

SCIENCE had quite a field-day in Perth on December 20, when the Natural History Society of the Fair City formally opened its museum. Prof. J. Geikie of Edinburgh, who was for some time president of the Society, opened the proceeding with an address in which he pointed out what such a local museum should be. Other speakers followed, and from the 20th to the 23rd was an almost continuous *conversazione*, in which exhibitions, demonstrations, and lectures were given. The electric light played a prominent part, and the objects brought together for the instruction and enjoyment of the many visitors represented all departments of science. The enterprise of the Perthshire Society is exceptional, and they have reason to be proud of their museum, reading, lecture, and other rooms, all of which, we have no doubt, will be put to excellent practical uses.

THE meteorological observations taken during October, 1883, at St. Ignatius' College, Malta, by the Rev. James Scoles, S.J., have been received. For the month the means were—pressure, 30·283 inches; temperature, 67°·98; daily range, 10°·2; elastic force of vapour, 0·498 inch, and humidity, 76; rainfall, 2·67 inches, and days of rain, 12; velocity of wind per hour, 8¼ miles; sky, a third covered with cloud; temperature of sea, 72°·0, with a monthly range of 4°·0; and thunderstorms and other electrical phenomena on the 4th, 10th, 11th, 12th, 13th, 15th, 26th, and 30th. Atmospheric pressure was thus fully a fourth of an inch below the mean, temperature 3°·4 lower than usual, and rainfall about half an inch less. This Society has peculiar facilities for prosecuting meteorological and other researches through its widely scattered seminaries and colleges, and we have the greatest pleasure in noting the increasing readiness with which its services are given to science.

MR. H. H. JOHNSTON will give a discourse on "Kilimanjaro, the snow-clad Mountain of Equatorial Africa," at the Royal Institution, on Friday evening, January 25. Prof. Bonney's discourse on "The Building of the Alps," announced for that evening will be given on April 4.

HERR STEINEGER has been so fortunate as to secure eleven crania and numerous bones of the extinct sea-cow, *Rhytina stelleri*, which have been forwarded to the Smithsonian Institution at Washington.

A SPLENDID meteor was seen at Frankfort-on-the-Maine on December 8 at 6.45 a.m. It moved from west to east, and illuminated the whole neighbourhood.

A *Times* correspondent writes from Iceland that reports of a volcanic eruption in the interior were current last year, and were founded on peculiar appearances of the sky, and especially on the observation from some of the remote inland farms of columns of smoke or vapour rising in the far distance. Nothing definite has, however, been ascertained as to these phenomena. An unusually large number of scientific men, geologists, botanists, and philologists, chiefly German and Swedish, have this year visited Iceland and investigated its structure, flora, and language; and at present Prof. Sophus Tromholt, well known in scientific circles by his researches as to the aurora borealis, is pursuing these investigations here, and intends to remain all the winter, as, from the clearness of the atmosphere and the frequency and brilliancy of the aurora, Iceland is exceedingly well suited for his observations.

THE extensive collections of American Coleoptera made by the late Dr. J. L. LeConte, containing an immense number of original types, become the property of the Museum of Comparative Zoology of Cambridge, Mass.

THE French Société des Électriciens has completed its arrangements, and has been divided into six sections:—Theoretical electricity, M. Marie Davy president; Dynamo-electrical machinery, transmission of force to a distance, distribution of energy, M. Tresca president; Electric lighting, M. Du Moncel president; Telegraphy and telephony, M. Blavier president; Electro-chemistry and electrotherapy, M. Jamin president.

WHEN Arago was director of the Observatory of Paris, the dotation of this establishment was less than 4000*l*. a year. This sum was greatly increased when Leverrier was appointed by Napoleon III., and before his death it had reached 10,000*l*. Now the sum allotted is about 16,000*l*., although the meteorological department has been set apart as a special service.

THE Italian Geographical Society awards its great gold medal to Count Pietro Antonelli, in consideration of the important results of his last journey to Shoa.

FROM advanced sheets of the *Proceedings of the Anthropological Society of Washington*, Col. F. A. Seely of the United States Patent Office, we learn from *Science*, publishes a pamphlet entitled "An Inquiry into the Origin of Invention." The author is accustomed, day by day, as new claims for patents come before him, to eliminate the successive steps in the classes of machinery until he reaches the fundamental idea. This is the plan pursued in tracing backward the whole subject of invention to its sources in the mind of primitive man. The subject is illustrated, first, by the story of the steam-engine, and then by the examination of the bow and arrow and other implements of the lower races. The author rejects Prof. Gaudry's Dryopithecus, and affirms, "Obviously, archæology can find no trace of a remoter age than that of stone; but I mistrust that the thoughtful anthropologist will accept the evidence of earlier ages, one of which, taking one of its perishable materials as the type of all, we may call the age of wood. Still farther back must lie an age, as indefinite in duration as any, when man existed in his rudest condition, without arts of any kind, except such as he employed in common with lower animals; and this is the true primitive period."

WE have received the report for the years 1880 and 1881 of the administration of the artistic and scientific collections in the Royal Museums of Dresden. The Zoological and Anthropological Museum was visited by 61,129 persons in 1880, and by 65,455 in 1881. An index to Reichenbach's ornithological works has been prepared by the director, Dr. A. B. Meyer, who has also issued an important work on the picture-writings of the Eastern Archipelago and Pacific Islands. The staff of this museum now consists of the Director, Th. Kirsch, curator, L. Römer and J. C. G. Wilhelm, first and second conservators, C. A. Kippe, preparer of specimens, a scientific assistant, and two attendants. The zoological and anthropological collections were enriched in the years 1880 and 1881 by 2242 specimens of the higher animals, and 17,753 of insects, by 237 anthropological and 1351 ethnographic objects, including 61 crania and 56 photographs and drawings of human types from various quarters. The library attached to this department was increased by 332 works, including donations from the British Museum, Smithsonian, and other sources. The systematic catalogue of the fishes was completed in three volumes, with alphabetical index of the 294 genera, 726 species, and 2901 specimens contained in the collection. The nests, to the number of 800, were also rearranged and catalogued, and progress was made with the catalogues of the birds (from No. 1688 to 2948) and insects (Hymenoptera concluded, Diptera thoroughly revised, of Coleoptera three families arranged and catalogued).

MESSRS. BAILLIÈRE AND CO. of Paris have issued the first number of a new scientific weekly, *Science et Nature*, profusely illustrated.

M. ÉD. MAILLY has brought out, in two volumes, a "Histoire de l'Académie Impérial et Royal de Bruxelles," from which so much good work has emanated. The history abounds in interest. F. Hayez of Brussels is the publisher.

SPAIN does seem to be progressing in the right direction. We have the second volume of Mr. F. Gillman's very useful and carefully compiled "Enciclopedia-Popular Illustrada" (Madrid), with a large atlas of plates. Also the first number of *La Industria Ibérica*, a weekly paper devoted to the industry and science of the whole peninsula, well printed, and, to judge from the first number, judiciously edited.

MESSRS. CHARLES GRIFFIN AND CO. announce the following scientific publications as forthcoming:—"A Manual of Geology," by Robert Etheridge, F.R.S., and Prof. H. G. Seeley, F.R.S.; "A Manual of Chemistry," by Prof. Dupré, F.R.S., and Dr. H. Wilson Hake; "A Manual of Botany: the Morphology, Physiology, and Classification of Plants, for the Use of Students," by Prof. W. R. M'Nab; "A Pocket-book of Electrical Rules and Tables, for the Use of Electricians and Engineers," by John Munro, C.E., and Andrew Jamieson, C.E., F.R.S.E.

THE additions to the Zoological Society's Gardens during the past week include a Rhesus Monkey (*Macacus rhesus*) from India, presented by Miss P. Crabtree; a Campbell's Monkey (*Cercopithecus campbelli*) from West Africa, presented by Mr. Walter van Weede; an Alligator (*Alligator mississippiensis*) from the Mississippi, presented by Mr. Thick; a Ring-tailed Coati (*Nasua rufa*) from South America, deposited.

PHYSICAL NOTES

M. E. REYNIER has described, in *l'Électricien*, a research made by him on the maxima and minima of electromotive force of certain batteries in which polarisation takes place. These he calls "single-electrolyte" batteries, instead of "single-fluid" batteries, following a suggestion of the late M. Niaudet. The

difference consists in the relative size of the electrodes. For example, in the case of a zinc-copper cell containing a single electrolytic fluid, the maximum cell is made with a kathode of sheet-copper folded and curved, presenting 300 times as much surface as the thin copper rod which serves as anode, whilst in the minimum cell the proportion is reversed, so that the polarisation at the surface of the copper attains at once its maximum value. The value of the E.M.F. of the cells when filled with dilute sulphuric acid, and having the zinc amalgamated, was 1·072 volts maximum, and 0·272 volts minimum. Many other electrolytes were examined by M. Reynier. The electromotive force was measured upon a galvanometer of high resistance.

M. REYNIER has suggested a modification of his maximum cell to serve as a standard of electromotive force—namely, a cell having a very large copper electrode, and a very small amalgamated zinc electrode, immersed in a solution of sea-salt. According to M. Reynier, this battery has an E.M.F. of 0·82 volts, and maintains this value within 1 per cent. even when the circuit was loosed for two hours through a resistance of 820 ohms. M. Reynier prefers this combination to one containing sulphate of zinc in solution, because of the liability of the latter salt to contain free acid.

M. HENRI BECQUEREL has been pursuing his researches upon the infra-red rays of the spectrum. For the investigation of this region there are four methods, the first of them involving the use of a line-thermopile and a rock-salt prism; the second, Abney's photographic method; the third, Langley's method, with bolometer and a reflecting diffraction grating; the fourth, that of Becquerel, depending upon the discovery that the infra-red rays have the effect of extinguishing the glow of a phosphorescent body exposed previously to ultra-violet rays. M. Becquerel finds that water, for example, gives in the region to which this method is applicable three well marked absorption-bands, having wave-lengths respectively of 930, 1080, and 1230.

THE newest result of Becquerel's researches is worth more than passing mention. He finds that there exist in this wholly invisible region of the spectrum bright-line spectra—equally invisible, of course—just as in the visible parts of the spectrum, observable in the radiations of hot vapours. Thus, incandescent sodium vapour prints upon the previously "insolated" phosphorescent substance two well-marked lines (wave-lengths 819 and 1098), corresponding to two bright lines hitherto unknown. The extent of the region which is capable of being explored by this novel process is from wave-length 760 to 1300, or exceeding in extent that of the whole of the visible and ultra-violet rays.

AN interesting experiment is described in the *Zeitschrift des elektrotechnischen Vereins*, in Vienna, by Prof. von Waltenhofen, made by means of Noe's thermo-electric generators. If a current from a voltaic battery has been sent for a few moments through one of these generators, it is capable of yielding a discharge like a secondary battery. This effect is so far a mere repetition of a well-known experiment of Peltier, and is due to the change of temperature at the junction, called the Peltier effect. But von Waltenhofen observes that the effects are different according to the sense of the charging current. In one case, with increasing charging currents the discharge currents also increased, and were always in the opposite sense to that of the charging current. But when the charging current was reversed, it was found that with increasing charging currents the discharge currents at first increase, then attain a maximum, then decrease to zero, then actually recommence in the converse sense, namely, in the same sense as that of the charging current. Prof. von Waltenhofen is disposed to attribute this anamalous result to the lack of symmetry in the disposition of the alternate solderings of the generators, and to their alternately unequal resistance causing alternately unequal developments of heat due to resistance.

IN proof of the law of proportion between the thickness of a square vibrating plate and its pitch, Dr. Elsas gives the following neat experiment. Let three plates be cut from the same sheet of material, of the same size and form. Cement two of these together so as to produce a plate of double thickness. Then, on exciting the single plate and the double plate by communicating to them respectively the vibrations of two tuning forks whose pitches are as 1 : 2, the plates will be excited in identical manners, as will be seen by dusting sand upon them, the clang-figures being identical.

LORD RAYLEIGH has reprinted for private circulation in pamphlet form several of his most valuable optical papers, including those on the manufacture, reproduction by photography, and theory, of diffraction-gratings, and those on colour-mixtures.

LORD RAYLEIGH has also reprinted some of his papers on electricity and on absolute pitch, from NATURE and from the Reports of the British Association, in a convenient pamphlet form.

THE question whether condensation of steam is a cause of electrification has been examined afresh by S. Kalischer in the Physical Laboratory at Berlin. According to the views of Faraday, this is a cause of electrification, and upon the alleged phenomenon Prof. Spring has founded a theory of the origin of thunderstorms. Landerer thought he had heard sounds in the telephone due to condensation of moisture on the line wires. Kalischer has in vain repeated the experiment. He has also examined, by means of the quadrant electrometer, whether any such electrification could be observed from the deposit of moisture upon the surface of a vessel containing ice or some artificial cooling mixture. The whole of the results were negative.

AMONGST the many recent suggestions for primary batteries is one due to MM. Lalande and Chaperon, in which oxide of copper is used as a depolarising agent. The oxide, in powder, is placed in or on a sheet of copper or iron. The positive element is zinc, and the exciting liquid caustic potash. A zincate of potash is formed by the solution of the zinc. The cell is absolutely inactive when the circuit is open. When closed, the current is remarkably constant. According to Hospitalier, the electromotive force is 0·98 volt. It must of course be closed from the air, to prevent absorption of carbonic acid by the potash. The reduced copper is reoxidised by simple exposure to the air.

IN a series of studies on the copper voltameter, published in the *Repertorium der Physik* by Dr. H. Hammerl, the following conclusions are formulated:—1. The material condition of the surface of the electrode, that is to say, whether it is covered with a bright copper film or not, has no influence on the amount of the deposit. 2. The changes of concentration of the copper solution, brought about in the voltameter by the current itself, cannot be sufficiently prevented by stirring. 3. Heating the fluid to boiling causes the deposit to come down almost completely in the state of cuprous oxide: it is partially oxidised even at temperatures between 40° and 60° C. 4. The greatest permissible strength of current, for which the deposit may be safely assumed to be a measure of the current, is about 7 amperes per square decimetre of the cathode surface.

THE EVIDENCE FOR EVOLUTION IN THE HISTORY OF THE EXTINCT MAMMALIA[1]

THE subject to which I wish to call your attention this morning requires neither preface nor apology, as it is one with the discussion of which you are perfectly familiar. My object in bringing it before the general session of the Association was in view of the fact that you were all familiar with it in a general way, and that it probably interests the members of sections which do not pursue the special branch to which it refers, as well as those which do; also, since it has been brought before us in various public addresses for many years during the meetings of this Association, I thought it might be well to be introduced at this meeting of this Association, in order that we might not omit to have all the sides of this interesting question presented.

The interests which are involved in it are large: they are chiefly, however, of a mental and metaphysical character; they do not refer so much to industrial and practical interests, nor do they involve questions of applied science. They involve, however, questions of opinion, questions of belief, questions which affect human happiness, I venture to say, even more than questions of applied science; certainly, which affect the happiness of the higher grades of men and women more than food or clothing, because they relate to the states of our mind, explaining as they do the reasons of our relations to our fellow-beings and to all things by which we are surrounded, and the general system of the forces by which we are surrounded. So it has always appeared to me: hence I have selected the department of biology, and have taken a great interest in this aspect of it.

[1] A lecture by Prof. E. D. Cope of Philadelphia, given in general session before the American Association for Advancement of Science at Minneapolis, August 20, 1883. Stenographically reported for *Science*.

The doctrine of evolution, as taught by the biologists of to-day, has several stages as grounds or parts of its presentation. First, the foundation principle is this: That the species of animals and of plants, the species of organic beings, as well as the various natural divisions into which these organic beings fall, have not always been as we see them to-day, but they have been produced by a process of change which has progressed from age to age through the influence of natural laws; that, therefore, the species which now exist are the descendants of other species which have existed heretofore, by the ordinary processes of reproduction; and that all the various structures of organic beings which make them what they are, and which compel them to act as they now act, are the result of gradual or sudden modifications and changes during the periods of geologic time. That is the first phase or aspect which meets the naturalist or biologist.

Another phase of the question relates to the origin itself of that life which is supposed to inhabit or possess organic beings. There is an hypothesis of evolution which derives this life from no-life, which derives vitality from non-vitality. That is another branch of the subject, to which I cannot devote much attention to-day. There is still another department of the subject, which relates to the origin of mind, and which derives the mental organisation of the higher animals, especially of man, from pre-existent types of mental organisation. This gives us a genealogy of mind, a history of the production or creation of mind, as it is now presented in its more complex aspects as a function of the human brain. This aspect of the subject is, of course, interesting, and upon that I can touch with more confidence than upon the question of the origin of life.

Coming now to the question of the origin of structures, we have by this time accumulated a vast number of facts which have been collated by laborious and faithful workers, in many countries and during many years; so that we can speak with a good deal of confidence on this subject also. As to the phenomena which meet the student of zoology and botany at every turn, I would merely repeat what every one knows—and I beg pardon of my biological friends for telling them a few well-known truths, for there may be those present who are not in the Biological Section—that the phenomena which meet the student of biology come under two leading classes: the one is the remarkable fidelity of species in reproducing their like. "Like produces like," is the old theorem, and is true in a great many cases; just as coins are struck from the die, just as castings are turned out from a common mould. It is one of the most wonderful phenomena of nature, that such complex organisms, consisting of so many parts, should be repeated from age to age, and from generation to generation, with such surprising fidelity and precision. This fact is the first that strikes the student of these sciences. The general impression of the ordinary person would be that these things must continue unchanged. When I began to study zoology and botany, I was remarkably surprised to find there was a science of which I had no conception, and that was this remarkable reproduction of types one after another in succession. After a man has had this idea thoroughly assimilated by his honest and conscientious studies, he will be again struck with another class of facts. He will find, not unfrequently, that this doctrine does not apply. He will find a series of facts which show that many individuals fail to coincide with their fellows precisely, the most remarkable variations and the most remarkable half-way attitudes and double-sided aspects occurring; and he will come to the conclusion, sooner or later, that like does not produce like with the same precision and fidelity with which he had supposed it did. So that we have these two classes of facts,—the one relating to, and expressing, the law of heredity; the other, which expresses the law of metamorphosis. I should not like to say which class of facts is the most numerously presented to the student. In the present fauna we find many groups of species and varieties before us; but how many species we have, how many genera we have, and families, we cannot definitely state. The more precise and exact a person is in his definition and in his analysis, the more definite his science becomes, and the more precise and scientific his work. It is a case of analysis and forms. What the scales are to the chemist and the physicist, the rule and measure are to the biologist. It is a question of dimension, it is a question of length and breadth and thickness, a question of curves, a question of crooked shapes or simple shapes,—rarely simple shapes, mostly crooked, generally bilateral. It requires that one should have a mechanical eye, and should have also something of an artistic eye to appreciate these forms, to measure them, and to be able to compare and weigh them.

Now, when we come to arrange our shapes and our measurements, we find, as I said before, a certain number of identities, and a certain number of variations. This question of variation is so common and so remarkable, that it becomes perfectly evident to the specialist in each department that like does not at all times produce like. It is perfectly clear, and I will venture the assertion that nearly all the biologists in this room will bear me witness, that variability is practically unlimited in its range, unlimited in the number of its examples, unlimited in the degree to which it extends. That is to say, the species vary by failing to retain certain characteristics, and generic and other characters are found to be absent or present in accordance with some law to be discussed further on.

I believe that this is the simplest mode of stating and explaining the law of variation: that some forms acquire something which their parents do not possess; and that those which acquire something additional have to pass through more numerous stages than those which have not acquired so much had themselves passed through.

Of course we are met with the opposite side of the case,—this law of heredity. We are told that the facts there are not accounted for in that way; that we cannot pass from one class of facts to the other class of facts; what we find in one class is not applicable to the other. Here is a question of rational processes, of ordinary reason. If the rules of chemistry are true in America, I imagine they are true in Australia and Africa, although I have not been there to see. If the law of gravitation is effective here, I do not need to go to Australia or New Zealand to ascertain whether it is true there. So, if we find in a group of animals a law sufficient to account for their creation, it is not necessary to know that others of their relatives have gone through a similar process. I am willing to allow the ordinary practical law of induction, the practical law of inference, to carry me over these gaps, over these interruptions. And I state the case in that way, because this is just where some people differ from me, and that is just where I say the simple question of rationality comes in. I cannot believe that nature's laws are so dissimilar, so irregular, so inexact, that those which we can see and understand in one place are not true in another; and that the question of geological likelihood is similar to the question of geographical likelihood. If a given process is true in one of the geological periods it is true in another; if it is true in one part of the world it is true in another; because I find interruptions in the series here, it does not follow that there need be interruptions clear through from age to age. The assumption is on the side of that man who asserts that transitions have not taken place between forms which are now distinct.

We are told that we find no sort of evidence of that transition in past geological periods; we are assured that such changes have not taken place; we are even assured that no such sign of such transition from one species to another has ever been observed,—a most astonishing assertion to make *to* a biologist, or *by* a biologist; and such persons have even the temerity to cite special cases, as between the wolf and the dog. Many of our domestic dogs are nothing but wolves, which have been modified by the hand of man to a very slight extent indeed. Many dogs, in fact nearly all dogs, are descendants of wild species of various countries, and are but slightly modified.

To take the question of the definition of species. Supposing we have several species well defined, say four or five. In the process of investigation we obtain a larger number of individuals, many of which betray characters which invalidate the definitions. It becomes necessary to unite the four or five species into one. And so then, because our system requires that we shall have accurate definitions (the whole basis of the system is definitions—you know the very comprehension of the subject requires definitions), we throw them all together, because we cannot define all the various special forms as we did before, until we have but one species. And the critic of the view of evolution tells us, "I told you so! There is but one species, after all. There is no such thing as connection between species; you never will find it." Now, how many discoveries of this kind will be necessary to convince the world that there are connections between species? How long are we to go on finding connecting links and putting them together, as we have to do for the sake of the definition, and then be told that we have nevertheless no intermediate forms between species? The matter is too plain for further comment. We throw them together simply because our

definitions require it. If we knew all the known individuals which have lived, we should have no species, we should have no genera. That is all there is of it. It is simply a question of a universal accretion of material and the collection of information. I do not believe that the well-defined groups will be found to run together, as we call it, in any one geological period, certainly in no one recent period. We recognise, however, that they diverge to a wonderful extent; one group has diverged at one period, and another one has become diversified in a different period; and so each one has its history, some beginning farther back than others, some reaching far back beyond the very beginning of the time when fossils could be preserved. I call attention to this view because it is a very easy matter for us to use words for the purpose of confusing the mind; for, next to the power of language to express clear ideas, is its power of expressing no ideas at all. As we all know, we can *say* many things which we cannot *think*. It is a very easy thing to say twice two is equal to six, but it is impossible to think it.

I would cite what I mean by variations of species in one of its phases: I would just mention a genus of snakes, Ophibolus, which is found in the United States. If we take the species of this snake genus as found in the Northern States, we have a good many species well defined. If we go to the Gulf States and examine our material, we see we have certain other species well defined, and they are very nicely defined and distinguished. If now we go to the Pacific coast, to Arizona and New Mexico, we shall find another set of species well defined indeed. If we take all these different types of our specimens of different localities together, our species, as the Germans say, all tumble together; definitions disappear, and we have to recognise, out of the preliminary list of thirteen or fourteen, only four or five. That is simply a case of the kind of fact with which every biologist is perfectly familiar.

When we come to the history of the extinct forms of life, it is perfectly true then that we cannot observe the process of descent in actual operation, because, forsooth, fossils are necessarily dead. We cannot perceive any activities because fossils have ceased to act. But if this doctrine be true we should get the series, if there be such a thing; and we do, as a matter of fact, find longer or shorter series of structures, series of organisms proceeding from one thing into another form, which are exactly as they ought to be, if this process of development by descent had taken place.

I am careful to say this, because it is literally true, as we all must admit, that the system must fall into some kind of order or other. You could not collect bottles, you could not collect old shoes, but you could make some kind of a serial order of them. There are no doubt characters, by which such and such shoes could be distinguished from other shoes, these bottles from other bottles; but it is also true that we have, in recent forms of life in zoology and botany, irrefragable proofs of the metamorphoses, and transformations, and changes of the species, in accordance with the doctrine which we commenced with.

We now come to the second chapter of our subject. With the assumption, as I take it, already satisfactorily proven, of species having changed over into others—in considering this matter of geological succession or biological succession, I bring you face to face with the nature and mode of the change, and hence we may get a glance, perhaps, at its laws.

I have on the board a sketch or table which represents the changes which took place in certain of the mammalia. I give you a summary of the kind of thing which we find in one of the branches of palæontology. I have here two figures, one representing a restoration, and the other an actual picture, of two extinct species that belong to the early Eocene period. One represents the ancestor of the horse line, Hyracotherium, which has four toes on his anterior feet, and three behind; and the other, a type of animal, Phenacodus, which antedated all the horse series, the elephant series, the hog, the rhinoceros, and all of the other series of hoofed animals. Each presents us with the primitive position in which they first come to our knowledge in the history of geological time.

I have also arranged here a series of some leading forms of the three principal epochs of the Mesozoic times, and six of the leading ones of the Tertiary time. I have added some dates to show you the time when the faunæ which are entombed in those beds were discovered in the course of our studies; and you will easily see how unsafe it is to say that any given type of life has never existed, and assert that such and such a form is unknown; and it is still more unsafe, I think, to assert that any given form of life properly defined, or that a specific intermediate form of life, will not be found. I think it is much safer to assert that such and such intermediate forms will be found. I have frequently had the pleasure of realising anticipations of this kind. I have asserted that certain types would be found, and they have been found. You will see that I attend to the matter of time closely, because there have been a great many things discovered in the last ten or fifteen years in this department. In these forms I give the date of the discovery of the fauna in which they are embraced.

Formation	No. of toes	Feet	Astragalus	Carpus and Tarsus	Ulno-radius	Superior Molars	Zygapophyses	Brain
Miocene	1-1	Digitigrade (Plantigrade)	Grooved (Flat)	Interlocking (Opposite)	Faceted	4-tubercles, crested and cemented	Doubly involute Singly do.	Hemispheres larger, convoluted
Upper ... ... (Loup Fork)	2-2 3-3 4-4 (5-5)							
Middle ... ... (John Day)	2-2 3-3 4-4	Digitigrade	Grooved	Interlocking	Faceted Smooth	4-tubercles, and crested	Singly involute Doubly do.	Hemispheres larger, convoluted
Lower ... ... (White River)	3-3 4-3 4-4	Digitigrade Plantigrade	Grooved	Interlocking	Smooth Faceted	4-tubercles, and crested	? Singly involute	Hemispheres small; and larger
Eocene	3-3	(Digitigrade)	Grooved (Flat)	Opposite Interlocking	Smooth	4-tubercles 3-tubercles, and crested	Singly involute Plane	Hemispheres small
Upper ... ... (Bridger)	4-3 4-5	Plantigrade						
Middle ... ... (Wasatch)	5-5 4-3 4-5	Plantigrade (Digitigrade)	Flat (Grooved)	Opposite Interlocking	Smooth	4-tubercles 3-tubercles, a few crested	Plane Singly involute	Hemispheres small; mesencephalon sometimes exposed
Lower ... ... (Puerco)	5-5 5-5	Plantigrade	Flat	Opposite	Smooth	3-tubercles (4-tubercles), none crested	Plane	Mesencephalon exposed; hemisphere small and smoother

Here we have the White River fauna discovered in 1856; then we skip a considerable period of time, and the next one was in 1869, when the Cretaceous series was found. Six or seven Cretaceous faunæ have been found. Thus we have the Bridger fauna in 1870, the Wasatch fauna in 1874. Next we have, in 1877, the Equus beds and the fauna which they embrace, which also was found in 1878. The Permian fauna, which is one of the last, is 1879; and the last, the Puerco, which gives the oldest and ancestral types of the modern forms of mammalia, was only found in 1881. When I first commenced the study of this subject, about 1860, there were perhaps 250 species known.

There are now something near 2000, and we are augmenting them all the time. I have found many myself: if they were distributed through the days of the year I think in some years I should have had several every day. But the accessions to knowledge which are constantly being made make it unsafe to indulge in any prophecies that, because such and such things have not been found, therefore such and such things cannot be: for we find such and such things really have been and really are discovered.

The successive changes that we have in the mammalia have taken place in the feet, teeth, and brain, and the vertebral column. The parts which present us the greatest numbers of variations are those in which many parts are concerned, as in the limbs and feet. In the Lower Eocene (Puerco) the toes were 5-5. In the Loup Fork fauna some possess toes but 1-1. Prior to this period no such reduction was known, though in the Loup Fork fauna a very few species were 5-5. Through this entire series we have transitions steady and constant, from 5-5, to 4-5, to 4-4, to 4-3, to 3-3, to 2-2, to 1-1. In the Puerco period there was not a single mammal of any kind which had a good ankle-joint, which had an ankle-joint constructed as ankle-joints ought to be, with tongue and groove. The model ankle-joint is a tongue-and-groove arrangement. In this period they were all perfectly flat. As time passes on, we get them more and more grooved, until in the Loup Fork fauna and the White River fauna they are all grooved. In the sole of the foot, in the Puerco fauna, they are all flat; but in the Loup Fork fauna the sole of the foot is in the air, and the toes only are applied to the ground, with the exception of the line of monkeys, in which the feet have not become erect on the toes, and the elephant, in which the feet are nearly flat also, and the line of bears, where they are also flat. As regards the ungulation between the small bones of the palm and of the sole there is not a single instance in which the bones of the toes are locked in the Lower Eocene, as they are in the later and latest Tertiary.

When we come to the limbs, the species of the Puerco fauna have short legs. They have gradually lengthened out, and in the late periods they are nearly all relatively long.

(*To be continued.*)

SOCIETIES AND ACADEMIES

LONDON

Royal Society, Dec. 13, 1883.—"On the Figure of Equilibrium of a Planet of Heterogeneous Density," by G. H. Darwin, F.R.S., Plumian Professor of Astronomy in the University of Cambridge.

If a rotating planet be formed of compressible fluid, the strata of equal pressure are of equal density, and the ellipticity of the strata increases from the centre outwards. Since it is supposed that the earth consolidated into its present form from a fluid or semi-fluid condition, the determination of the arrangement of internal density and of the law of ellipticity in such a planet is often called the problem of the figure of the earth. When the law of compressibility of the fluid is known, the laws of density and ellipticity are determinate, but the differential equations involved are of such complexity that only one solution of the problem is well known, viz. that associated with the names of Legendre and Laplace.[1]

In this solution the modulus of compressibility varies as the square of the density, but the assumption of this law appears to have been dictated more by the necessity of solving a certain differential equation than by physical considerations.

The comparison of the solution of the problem with the observed facts with regard to the earth may be made in several ways. The constant which determines the rate of the earth's precessional motion gives us information with regard to the arrangement of density in the interior, and the ellipticity of the surface is determined by geodesy and by the amount of a certain inequality in the moon's motion. Now, in order that the solution of the problem of the earth's figure may be satisfactory, the same arrangement of internal density must give the observed amounts both to the precessional constant and to the ellipticity of the surface.

Laplace's solution is highly satisfactory in this respect; and at the same time it makes the mean density of the whole earth about twice as great as the density of the surface stratum. The density of rock is about 2·8, and that of the whole earth is about 5·6.

In this state of our knowledge another solution of this celebrated problem possesses some interest, even if its results are not quite so satisfactory as those of Laplace's theory.

In the present paper such a solution is offered. The law of compressibility of the fluid is such, that the modulus varies as a power of the density, which power may range from negative infinity to $\frac{5}{3}$. When the power is zero, we have constant compressibility; and when the power is unity, we have the same law of compressibility as in a gas.

The solution is expressible in a far simpler algebraic form than that of Laplace, and it differs from his solution in placing a far larger proportion of the mass of the planet in the central regions.

It is remarkable that this solution affords for the case of the earth a correspondence between the precessional constant and the surface ellipticity equally good with that of Laplace. To obtain this correspondence we have to assume the compressibility of the fluid to be nearly constant.

The density of the surface layer appears however to be 3·7, and this is considerably greater than that of ordinary rocks. This result tells adversely to the acceptability of the proposed solution, but the discrepancy is not so serious as might appear at first sight. It appears from pendulum experiments on the Himalayan plateau and on the Andes, that there is a considerable deficiency of density underneath those great ranges. This would favour the view that our continents are a mere intumescence of the surface layers. In this case there must be a somewhat abrupt change in the law of density at only a few miles below the surface. The theory of the earth's figure can take no account of a sudden change of density on passing into a swollen superficial layer, and the value of the surface density to be used is that which is to be found immediately below the swollen part.

The author therefore points out that whilst the solution now offered cannot be held to be quite as satisfactory as that of Laplace, yet its inferiority is not of a kind to render altogether unacceptable the contention that it may be somewhere near the truth.

Linnean Society, December 20, 1883.—Alfred W. Bennett, M.A., in the chair.—Messrs. N. Cantley, W. Dobson, F. G. Smart, and Rev. R. Thom were elected Fellows of the Society.—Mr. S. O. Ridley exhibited and made remarks on a series of 177 vertical sections of sponges collected in the neighbourhood of Point de Galle, Ceylon, by Dr. W. C. Ondaatje, F.L.S., and transmitted to England by him in letters. They are in most instances sufficient for the identification of the genera and some species.—Mr. F. Maule Campbell showed the web of a spider (*Tegenaria guyonii*) which had been spun in the centre of a pasteboard cylinder, the peculiarity being the manner in which the solid part of the web was medially swung, whereas in this species of spider it is more usually on the sides of objects.—A paper was read by Mr. F. O. Bower on the structure of the stem of *Rhynchopetalum montanum*. The plant is a native of Abyssinia, growing in districts 11,000 to 13,000 feet above the level of the sea. It differs from its ally Lobelias in being perennial. Internally it is succulent when young, but afterwards the surface becomes scarred as the leaves drop off, and exteriorly is hardened by a thick corky deposit. *Rhynchopetalum*, the author shows in detail, has certain peculiarities in the arrangement of the tissue of its leaf bundles, since the cortical system does not consist of branches of bundles of the leaf trace, but are cauline bundles, in this respect differing widely from such forms as *Lathyrus casuarina*, many *Begonias*, &c. *Rhynchopetalum*, moreover, has the cortical bundles running obliquely, and forming a regular four-sided meshed network related to the leaf bases and bundles of leaf trace. In these respects it approaches *Cycas*, in which latter the bundles of the accessory cortical system are not so regular and are almost vertically arranged. Some Cycads and *Rhynchopetalum* also agree in the exterior appearance of their stem, so that palæontologists might be deceived in their judgment if two well-preserved specimens were examined by them.—A communication was read on the auditory ossicles of *Rhytina stelleri* by Alban Doran. This was based on skeletons obtained by the *Vega* expedition, and shown at the late International Fisheries Exhibition by the Swedish Government. The author arrives at the conclu-

[1] The late M. Roche seems to have also solved the problem in 1848, and his paper is published in the *Memoirs of the Academy of Montpellier*.

sion that the malleus of *Rhytina* is larger than in *Manatus*, and therefore it is the largest and bulkiest malleus to be found in the whole section of the animal kingdom where such a bone exists, that in the characters of its body it resembles *Manatus* rather than *Halicore*, and that in the manubrium it differs from the other Sirenia, and is far more generalised. The incus is of the *Manatus* type, and so is the stapes, which is also the largest and bulkiest stapes to be found in any animal.—A paper on the organs of secretion in the Hypericaceæ, by Mr. J. R. Green, was read. He concludes (1) that the view advocated by Link, Martinet, and De Bary, of the lysigenous origin of the reservoirs of ethereal oil in these plants is the correct one; (2) that there exists in many parts of the plants a series of ducts or passages differing only slightly from these reservoirs, the differences being that they are not globular and isolated, but are generally connected more or less intimately with each other, and that their secretion is not a clear ethereal oil, but a viscid or resinous liquid, the points of agreement being those connected with their development and function; (3) that at least in some species there is also a series of schizogenous ducts confined to certain portions of the phloem; (4) that the dark glands which have been described are in intimate relationship with the fibrovascular system; (5) that the formation of resin and kindred secretions in these plants is confined to the parts where metabolism is active, and where there is a primary meristem. That all such parts give evidence of such formation with the exception of the roots.—A paper on the glands of *Coprosma hauerziana*, by Mr. Walter Gardiner, was read. These glands are externally well developed and very typical. The so-called stipular body is placed immediately behind each leaf, and in the young condition the stipule arches over the leaf, and the glands with which it is provided secrete copiously a mucilaginous fluid, which bathes and surrounds the young leaf structure. As to the development of the glands, they arise as protrusions of the stipule parenchyma, which are covered by an epidermis. Each epidermal cell then rapidly grows out at right angles to the protuberance. In *Coprosma* the glands are situated on the sides of the stipules, but it more usually occurs in other genera that they are distributed over the inner face of the base of the stipular organ.—The last paper taken was on the development of starch grains in the laticiferous cells of the Euphorbiaceæ, by M. C. Potter. It is pointed out that while the discovery of the existence of starch-forming corpuscles had been made by Kruger, yet he had failed to interpret their function, which Mr. Potter's researches now fully prove in the case of the Euphorbiaceæ, where the development of rod or spindle-shaped grains of starch lying within cell protoplasm has been clearly demonstrated.

Chemical Society, Dec. 6, 1883.—Dr. Perkin, president, in the chair.—The following gentlemen were elected Fellows:—F. A. Blair, T. J. Barr, C. J. Baker, L. Briant, R. G. Durrant, Kamchundra Datta, L. L. Garbutt, A. E. Harris, T. Hart, W. Irwin, S. Johnson, R. Jackson, H. C. Lee, W. H. Martin, C. E. Potter, B. M. K. Rogers, C. W. Stephens, P. H. Wright, H. A. Wetzel, and W. G. Whittam.—The following papers were read:—On the constitution of the fulminates, by E. Divers and M. Kawakita. When moist mercury fulminate is treated with much strong hydrochloric acid, hydroxyammonium chloride and hydrocyanic acid are formed; if the fulminate be dry, no prussic acid is formed. The carbon is completely converted into formic acid. No oxalic acid is produced.—Theory of the constitution of the fulminates, by E. Divers.—On Liebig's production of fulminating silver without the use of nitric acid, by E. Divers and M. Kawakita. When nitrous acid is passed into an alcoholic solution of nitrate of silver, crystals separate; these are not, as Liebig stated, fulminating silver, but nitrate of silver.—Note on the constitution of the fulminates, by H. E. Armstrong.—Experimental investigation on the value of iron sulphate as a manure for certain crops, by A. B. Griffiths. The author obtained from an experimental plot of land manured with ferrous sulphate fifty-six bushels of beans; a similar plot in its normal state gave twenty-five bushels. The ash of the plants also contained more iron and phosphoric acid in the first case.

Physical Society, December 8.—Prof. G. Carey Foster, in the chair.—New members:—Major McGregor, R.E., Mr. James Walker, M.A., Mr. W. B. Gregory, B.A.—Prof. Silvanus P. Thompson, D.Sc., read a paper on the static induction telephone as an instrument of research. The author had employed Dolbear's telephone in investigating the action of influence machines such as those of Holtz and Wimshurst or Toepler. This was done by holding the end of a wire (connected to one terminal of the telephone) near the electrified parts of the machine, for example the "carriers" in the Toepler apparatus. The carriers induced a change in the telephone, whose other terminal was to earth, as they passed, and the pitch of the note heard in the telephone increased with the speed at which the machine was driven. Useful results were obtained leading to modifications of some machines. The same telephone was also applied to the measurement of capacities of condensers arranged like the resistances of a Wheatstone balance, and the telephone taking the place of a galvanometer. For the "divided coil" of the balance Prof. Thompson substituted a double condenser, or rather two condensers, so joined that the earth-plates were separate, while the other plates were in one. This device was made from two glass tubes with tinfoil round their outsides and a brass tube sliding into both interiors in such a way that the relative capacities of the two condensers thus combined could be altered by sliding the tube between them. A modification of this plan was suggested by Mr. Starling, the author's assistant, which was analogous to Prof. Foster's arrangement of the Wheatstone balance, that is to say, six condensers were used, the two extra ones being included between the battery connections and the sliding tube. The battery was in this case an induction coil having no condenser, as a discontinuous current is necessary to give sounds. The author also showed that the Dolbear telephone could be used instead of the quadrant electrometer in such experiments as those of Mr. J. E. H. Gordon on specific inductive capacity. The author also showed how he had applied it to explore the equipotential surfaces round conductors charged statically by an induction current. With two wires from the terminals of a telephone silence is produced when both ends are on the same equipotential surfaces; and sounds when they are not.—Prof. Thompson then read a note on a new insulating stem. This consisted of a glass tube with one end blown into a flat foot, which was planted on the bottom of a glass bottle and cemented there by a little wax paraffin. The upper and open end of the tube served to hold the stems of brass plates, or other electrified bodies. Paraffin oil or strong sulphuric acid could be used in the bottom of the bottle. A cap of rubber or percha made to slide up the stem served as a dust cover.—Prof. Thompson next made a communication on the first law of electrostatics, and illustrated his remarks with experiments showing how a series of floating magnet poles of like name repelling one another tend to produce equal distribution of the poles. Prof. Thompson, arguing from the second law of electrostatics (inverse squares), sought to explain the first law in a rational manner, on the hypothesis of self-repelling molecules, which tend to uniform distribution. When there is a surplus in one part and a deficit in another, the molecules are urged towards each other, *i.e.* attract. This was shown by putting a surplus of floating magnets at one part of the basin. By the movements of these magnets when confined by barriers, and with surplus and deficit purposely made, the author imitated the effects of a Leyden jar, induction, a battery current, &c., the motions and arrangement of the poles illustrating the hypothetical behaviour of electricity. The author was led by the hypothesis to infer that either the ether is electricity, or that the ether is electrified, and the former seemed the simpler conclusion.—Dr. Monkman showed some experiments illustrating the attraction and repulsion of bodies in motion. The attraction of a light balanced body to a vibrating tuning-fork was shown; also the attraction between two disks of paper revolving parallel and in the same direction. The author showed that two smoke-rings travelling abreast in the same direction attracted each other, and that two paper rings revolving in the same direction close together attract, while if revolving in opposite directions they repel.—Mr. Walter Baily exhibited his new integrating anemometer in action by means of a small electric motor, which took the place of the Robinson cups. The apparatus sums up, or integrates, the wind velocities on the lines of the four cardinal points. An electric counter is attached.

PARIS

Academy of Sciences, December 17, 1883.—M. Blanchard, president, in the chair.—Preliminary report on the expedition of the *Talisman* to the Atlantic Ocean, by M. Alph. Milne-Edwards.—On the preparation and manner of employing arti-

ficially developed virus attenuated by heat, intended to be used in prophylactic inoculations against charbon, by M. A. Chauveau.—On the remarkable sunsets observed during the months of November and December, 1883, by M. P. de Gasparin. The author considers that these luminous effects cannot be due to falling stars, and must be referred to the solar light acting on an atmosphere charged with particles of matter whose nature has not yet been determined.—On the determination of elastic forces, by M. Fontaneau.—On the processes adopted by M. Mandon and M. Aman-Vigié in the treatment of vines affected by phylloxera, by M. F. Henneguy. The process of Dr. Mandon, which consists in saturating the sap with a solution of phenic acid, appears to have little or no effect on the parasite. That by M. Aman-Vigié, an injection of a mixture of vapours of sulphur and sulphuric acid into the ground, has been tried on too limited a scale to warrant any definite judgment as to its efficacy. but the experiments already made do not appear to have proved very beneficial, because the vapours of sulphuric acid do not penetrate to a sufficient depth into the ground, and evaporate too rapidly.—Observations of the Pons-Brooks comet made at the Paris Observatory with the bent equatorial, by M. Périgaud.—Observations of the planet 235 Carolina and of the Pons-Brooks comet made at the Paris Observatory (west equatoreal in the garden), by MM. Henry.—On the multipliers of linear differential equations, by M. Halphen.—On a point in the theory of elliptical functions, by M. Lipschitz.—On a theorem of M. Liouville in mathematical analysis, by M. Stieltjes. In continuation of his previous paper, the author here shows how the theory of elliptical functions leads to the theorem of M. Liouville.—On algebraic equations, by M. H. Poincaré.—Demonstration of the fundamental properties of the system of geodesic polar coordinates, by M. G. Ossian-Bonnet.—On a method of generating the ovals of Descartes proposed by Chasles in his "Aperçu Historique," by M. Maurice d'Ocagne.—On the measurement of the specific heats and variations of temperature of two bodies in contact, by M. Morisot.—On a practicable method available for the photometric comparison of the usual sources diversely coloured, by M. J. Macé de Lépinay.—On the influence of colour on the sensitiveness of the eye to different degrees of luminosity, by M. Aug. Charpentier.—Researches on the permanence of the solidification of superfused sulphur (continued), by M. D. Gernez.—Second note on chromic selenite; preparation of biselenite, by M. Ch. Taquet. The author has obtained a biselenite of chromium by the action of nitric acid on neutral selenite. It is almost insoluble in water, but soluble in acids, and decomposable by heat.—Note on the action of bromium on pilocarpine ($C_{22}H_{16}Az_2O_4$), by M. Chaasting.—On emetics of mucic and saccharic acids, by M. D. Klein.—Third note to serve as a contribution to the history of the formation of coal; genus *Arthropitus*, Gœppert, by M. B. Renault.—On the artificial reproduction of schistosity and slate layers, by M. Ed. Jannettaz.—Experiment relative to the mode of formation of bauxite and gypsum, by M. Stan. Meunier.—On the glaucous amphibolic schists of the island of Groix, by M. Barrois.—On an anorthite rock discovered at Saint Clément, Canton of Saint-Anthème (Puy-de-Dôme), by M. F. Gonnard.—On the fall of cosmic dust, by M. E. Young.—On the coincidence of the recent phenomenal after-glows with the passage of the cosmic meteors, by M. Chapel.

December 24, 1883.—M. Blanchard, president, in the chair.—The President announced the painful loss sustained by the Academy in the person of M. Yvon Villarceau, member of the Section for Geography and Navigation, who died after a short illness on December 23. Funeral orations on the deceased savant were pronounced by Col. Perrier in the name of the Academy, by M. Faye in the name of the Bureau of Longitudes, and by M. Tisserand in the name of the Paris Observatory.—Separation of gallium (continued): separation from terbium, ytterbium, and the earth provisionally called y_α by M. de Marignac, from scandium and fluor, by M. Lecoq de Boisbaudran.—Observations of the comet Pons-Brooks, made at the Observatory of Algiers by MM. Trépied and Rambaud.—Observations of the same comet made at the Lyons Observatory (Brunner equatorial of 0·160 metre), by M. Gonnessiat.—On a special development of the perturbing function

$$\frac{1}{\Delta'} = \frac{1}{(r^2 - 2rr'\cos v + r'^2)^{\frac{1}{2}}},$$

by M. O. Backlund.—On the purely trigonometrical series connected with M. Linstedt's new solution of the problem of three bodies, by M. H. Poincaré.—On the generation of geometrical surfaces, by MM. J. S. and M. N. Vanecek.—On the gauging of galvanometers, by M. E. Ducretet.—Researches on the permanency of the solidifications of superfused sulphur (continued); production of a new crystallised variety of sulphur, by M. D. Gernez.—On the decomposition undergone in the presence of water by the acid phosphates of the alkaline earthy bases, by M. A. Joly.—Determination of the neutralising heat for the fluorhydric acid of the alkaline and alkaline-earthy bases, by M. Guntz.—On the kreatines and kreatinines, fourth note, by M. E. Duvillier.—Action of ammoniacal gas on the nitrate of methyl, by MM. E. Duvillier and H. Malbot.—Researches on the compound oxygenised ammonias, by M. Reboul.—On some haloid derivatives of ethane, by M. L. Henry.—On the pathologic anatomy of the phlegmon, and especially on the seat of the bacteria in this affection, by M. Cornil.—On the species of Arctic mollusks found by the *Talisman* Expedition at great depths in the intertropical waters of the Atlantic Ocean, by M. P. Fischer.—On the morphology of the plumicole Sarcoptides, by MM. E. L. Trouessart and P. Mégnin.—On a rapid and economical method of treating vines affected by Peronospora, by M. Senderens.—On a parasitic Nematode of the common onion, by M. Joannes Chatin.—On the cultivation of beetroot and some other plants in solutions of organic substances in decomposition, by M. V. Jodin.—On the relations of the Serpentine rocks to saline substances, especially in the Pyrenees, by M. Dieulafait.—On a chlorosilicate of lime, by M. Le Chatelier.—Experimental researches on the velocity of aqueous or atmospheric currents capable of holding in suspense mineral particles, by M. J. Thoulet.—Note on the sunset glows recently reported to the Academy, by M. E. Marchand.—Observation of the after-glows witnessed at Valence on the evening of December 2, by M. P. du Boys.—Remarks on the sunsets observed at Rambouillet on the evenings of December 15 and 18, by M. A. Laugier.—Letter on the sunsets observed at Christiania towards the end of November, by M. Fearnley, director of the Christiania Observatory.

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THURSDAY, JANUARY 10, 1884

AMERICAN GEOLOGY

Twelfth Annual Report of the United States Geological and Geographical Survey of the Territories. In Two Parts, with Atlas of Maps, &c. By F. V. Hayden, U.S. Geologist. 8vo. (Washington: 1883.)

THERE is a singular fascination in American geology. Its features are as a whole so massive and colossal, their infinite detail so subordinated to breadth of effect, their presentation of the great elements of geological structure so grand, yet so simple and so clearly legible, that they may serve as types for elucidating the rest of the world. The progress of sound geology would assuredly have been more rapid had the science made its start in the Far West of America, rather than among the crumpled and broken rocks of Western Europe. Truths that have been gained on this side of the Atlantic by the laborious gathering together of a broken chain of evidence would have proclaimed themselves from thousands of plateaux, cañons, and mountain ranges, in language too plain to be mistaken. No doubt much has been gained by the mere toilsomeness of the search after the truth. A possession is more valued when it has been hard to obtain, and the qualities which its capture has called forth and strengthened could probably be educated in no other way. Nevertheless, no European geologist can visit these western regions without realizing more or less distinctly what an amount of time has been wasted here over questions about which there should never have been any discussion at all. This impression is renewed by every new geological memoir which brings to us fresh revelations of the scenery and structure of the Western Territories. It is especially deepened by a perusal of the volumes of which a brief notice will here be given.

It may be in the recollection of readers of NATURE that after some inquiry and discussion it was discovered by the Congress of the United States that various independent Surveys, under different Government departments, had been engaged among the Western Territories, and, having no connexion with each other, had, to some extent, duplicated the mapping of the same ground; and that at last in the summer of 1879 a law was passed whereby these various geological and topographical Surveys were abolished, and a new single organization was created under the name of the "Geological Survey of the United States." One of the Surveys thus abolished was known as "the U. S. Geological and Geographical Survey of the Territories," under Dr. F. V. Hayden as Geologist in charge. The publications of this Survey comprised a voluminous series of annual Reports and Bulletins, quarto volumes of elaborate and well illustrated Memoirs, and Geological Maps and Sections. Many thousands of square miles of country had been examined by the staff, and had been mapped and described in such a way as to lay out the broad features of wild regions for the first time, not only for the assistance of the geologist or geological surveyor who might afterwards care to fill in the details and improve the mapping, but for the guidance of future settlers in the far west, and of the Central authorities who have charge of the public lands. When, at the bidding of Congress, Dr. Hayden's Survey organization ceased to exist and his staff dispersed in search of other occupations, the work done in the year 1878 had not been published, while several important works were in progress. A small appropriation was granted to enable him to bring out his last Report and to complete other office-work of the Survey. This grant was exhausted in the summer of 1882, leaving five quarto volumes still unpublished though far advanced towards completion. These have been handed over to the Director of the Geological Survey, to be finished and published under his auspices. The final Annual Report, however, being the twelfth of the series, has at last been issued, the delay in its appearance having arisen from the scattering of the staff and their employment in other avocations, but partly perhaps (though he makes no mention of it) to the prolonged indisposition under which Dr. Hayden has been labouring ever since his retirement from official life.

Dr. Hayden's Report for 1878 is a most fitting termination to the series which it closes. It consists of two massive octavo volumes with an atlas of Maps and Panoramas, and is profusely illustrated with plates. It is of course impossible to give any adequate notice of this elaborate work within the limits permissible in these pages. But a mere outline of its contents may afford some idea of the nature and importance of this latest contribution to American Geology.

The first volume opens with a Prefatory Letter from Dr. Hayden himself, stating briefly the arrangement o the work under his supervision during the last year of its progress. One of his parties was charged with the primary triangulation of the entire area to be surveyed, and made satisfactory progress, among the Wind River and adjacent ranges westwards to Henry's Lake, where its operations were unfortunately cut short by Indians who crossing its trail, carried off all its animals and a portion of its outfit. Not far to the north lay the Yellowstone Park—an area perpetually exempted from settlement by special Act of Congress. That wild tract, surrounded by rugged mountains, formed a natural retreat for bands of hostile Indians when pursued by troops. Only the year before, the Nez Percés, retreating from General Howard, broke into the region, killing and plundering as they went. No wonder the surveyors should excuse any shortcomings in their work by pleading "that peculiar mental condition consequent on the uncertain and exaggerated rumours relative to the movements of the hostile Bannacks by whom the country was said to be overrun, but of whose presence we saw no more than the traces of some days' old trails." Next year, the writer of these lines, having previously heard similar wild rumours, passed over some of the same ground, but actually encountered an armed party, and will always remember the "peculiar mental condition," which the dust-cloud of the approaching red-skins awakened.

A second division of the staff made a detailed survey of the Yellowstone Park, obtaining materials for a Map of it on the scale of one inch to a mile. Mr. W. H. Holmes, attached to this party, had excellent opportunity for wielding that facile pencil to which geological science is so much indebted. Dr. A. C. Peale and Mr. Musbach made a detailed study of the thermal springs for which the region is now so famous.

A third division surveyed the previously little known but

magnificent snowy range of the Wind River Mountains, in which three true glaciers were observed—the first known to occur east of the Coast Range of the Pacific border.

The Report of these various surveys and of palæontological and natural history researches connected with previous explorations is divided into two parts. Taking the second part first, we have a stout volume of some 500 pages with 80 plates, besides figures, maps, and sections entirely devoted to the Yellowstone Park. A good deal has been written on the wonders of this region, chiefly in previous Reports of Dr. Hayden's Surveys, and sometimes in considerable detail, as, in Professor Comstock's Report, accompanying Captain Jones' Reconnaissance published in 1875. But no such minutely circumstantial narrative has ever appeared as that now issued.

An exceedingly erroneous general impression is conveyed by the word "Park" which has been applied to this region and which has received the sanction of an Act of Congress. The tract comprises an area of upwards of 3500 square miles, most of it being forest covered and of a rugged mountainous character. Some of the peaks rise to between 10,000 and 11,000 feet above the sea. Between the lower ridges, open glades of park-like woodlands make one half forget for a while the great altitude and remoteness of the region, till the true character of the place is recalled by some pine-trunk deeply scored by a passing bear or by a herd of "antelopes" or an occasional "elk" scampering across the sunshine into the gloom and silence of the surrounding forest. Through this region, the Yellowstone River and its tributaries, draining a series of lakes, flows northward till it enters a profound cañon in which, at times unseen and unheard, it chafes the feet of volcanic precipices until, emerging amid a series of glacier moraines, it passes out of the "Park" into the Territory of Montana.

The Monograph of this deeply interesting region now published by Dr. Hayden is composed of three unequal sections. The first of these, by Mr. W. H. Holmes, treats of the general geology. It is no disparagement to the author to say that the most valuable part of his Report is to be found in his admirable sketches. He adds some interesting particulars, indeed, to what was already known of the geology of the district. For example he has worked out in greater detail the structure of Cinnabar Mountain which forms so striking a feature in the ascent of the Yellowstone above the second cañon, likewise the geology of the remarkable volcanic plateau of which one sees a section from the camping ground at the Mammoth Hot Springs. The beautiful unconformability under the sheet of rhyolite which forms so impressive a feature in that landscape stands out with admirable clearness in Mr. Holmes' drawings. Evidence is supplied of the diminution of the Yellowstone Lake. A reference, tantalizingly brief, to the interesting glacial problems of the district concludes this short Report. The author was too well and busily employed with his pencil to find time for much independent geological observation. But it is matter for hearty congratulation that before he was moved away into the vaster domain of the Grand Cañons of the Colorado, where he has since done such service to the United States Geological Survey, he was enabled to spend long enough time in the Yellowstone region to produce the series of pictoral illustrations which enrich Dr. Hayden's final Report. His trained eye and power of rapid and accurate sketching greatly contributed to the perfection of the map of the Park.

The second and by much the longest section of the book is devoted to the Hot Springs of the Yellowstone Park, and is from the pen of Dr. A. C. Peale, who spent about two months in the district making detailed observations of the geysers and other thermal waters. He describes more than 2000 springs and seventy-one geysers, and illustrates his descriptions with so numerous a series of plates that every minute detail and variety of form in the geysers and sinter accumulations is vividly brought under the eye. Dr. Hayden justly remarks that this preliminary work ought never again to be necessary. Short of an actual inspection of the geysers and basins themselves, nothing could give a clearer idea than these plates do of the extraordinary forms assumed by the deposits from the thermal waters. The strange coralloid and sponge-like aggregations are excellently depicted in lithographs which have obviously been reproduced from photographs. Dr. Peale's Monograph consists of three parts, the first devoted to a description of the geysers and thermal springs; the second to an account of the principal geyser regions of the world for purposes of comparison; the third to thermo-hydrology, in which he discusses the general characters of thermal waters, their chemistry and deposits, and the theories of geyser action. The premature disbanding of the Survey prevented the completion of this essay on the scale originally intended. But Dr. Peale may be congratulated on having made a most useful addition to the literature of the subject. Not the least of its merits is the copious bibliography which is given in an Appendix.

The third section of the volume, by that able cartographer Mr. H. Gannet, deals with the topography, and gives an interesting *résumé* of the various reconnaissances and surveys which have resulted in the present detailed map of the Yellowstone Park.

The other volume, forming Part I. of the Report for 1878 is divided into two sections. One of these, relating to geology and palæontology, contains a series of Reports by Dr. C. A. White on the invertebrate palæontology of the Western States and Territories from the Carboniferous to the Tertiary rocks, and is accompanied by forty-two Plates of Fossils. Some sections have a special interest, in particular that in which the author discusses the fossils of the much disputed Laramie group, and sustains his previously expressed opinion that this group should be regarded as transitional between the Cretaceous and Eocene formations of the West. The abrupt cessation of the Survey, by depriving Dr. White of an opportunity of completing some of his work by further collection, has materially crippled him in the preparation of these further contributions to a subject which he has already done so much to elucidate.

Mr. Orestes St. John supplies a report on the Wind River District Basin, and Mr. Scudder reprints with additions and alterations the report on the Tertiary Lake-basin of Florissant, Colorado, which has already appeared in the Bulletin of the Survey, and which made known the extraordinary abundance of insect remains preserved in the lacustrine deposits of that locality.

If the compounds X Y and Z V react to produce X Z and Y V then

$$r = [X, Z] + [Y, V] - [X, Y] - [Z, V].$$

These equations illustrate the methods by which the thermal value of a chemical change can be indirectly calculated. The total loss of energy by a chemical system in passing from a definite initial to a definite final state is independent of the intermediate states; assuming, as we may do for most purposes, that the total loss of energy is measured by the quantity of heat evolved, it follows that the total thermal change accompanying a chemical change depends only on the initial and final states of the system. Hence, if we have series of reactions beginning with the same materials in the same condition, and ending with the same products in the same condition, and if all the thermal changes in one series may be measured, and all except one in the other series may be measured, it follows that we can calculate the thermal value of the unknown member of the second series of changes. Thus, it is required to determine the thermal value of the synthesis of 46 grams of formic acid (CH_2O_2). Twelve grams of carbon, 2 of hydrogen, and 48 of oxygen combine to produce 44 grams of carbon dioxide and 18 grams of water: but the same quantities of the same materials might theoretically be combined to produce 46 grams of formic acid, and then from this, 44 grams of carbon dioxide + 18 grams of water would be produced. The following are the thermal values of the various parts of these two series of changes :—

$$[C, O^2] = 96,960 \text{ gram-units} + ; \quad [H^2, O] = 68,360 + ;$$
$$[CH^2O^2, O] = 65,900 + ;$$

but

$$[C, O^2] + [H^2, O] = [C, H^2, O^2] + [CH^2O^2, O] = 165,320 +$$
$$\therefore [C, H^2, O^2] = [C, O^2] + [H^2, O] - [CH^2O^2, O] = 99,420 +.$$

Such calculations sometimes become very complex; corrections must frequently be introduced for quantities of heat evolved or absorbed during purely physical changes which form integral parts of the cycle of chemical change under investigation.

The thermal study and comparison of classes of chemical changes leads to the conclusion that a chemical change which is accompanied by considerable loss of energy to the changing system will generally occur, unless prevented by the action of forces external to the system. This generalisation, vague though it be, helps to explain many classes of chemical reactions, *e.g.* the action of concentrated and dilute solutions of hydriodic acid on sulphur, and on many hydroxyl-containing carbon compounds; and the action of sulphuretted hydrogen in precipitating certain metallic sulphides in the presence of acid, and others only form alkaline liquids.

Thomsen has devoted much time and care to the thermal investigation of the mutual actions of acids and bases: the greater part of his first volume is devoted to this inquiry. The "heat of neutralisation of an acid by a base" is defined as the number of gram-units of heat evolved on mixing equivalent quantities in grams of the acid and base in dilute aqueous solution, the products of the action being also soluble in water. Thomsen employs a solution of 2 NaOH (grams) in about 200 H_2O (grams) as the standard base: he measures the thermal values of the following reactions :—

[2NaOH Aq, 2HX Aq]	in the case of a	monobasic	acid.
[2NaOH Aq, H_2X Aq]	" "	dibasic	"
[2NaOH Aq, $\frac{2}{3}H_3X$ Aq]	" "	tribasic	"
[2NaOH Aq, $\frac{1}{2}H_4X$ Aq]	" "	tetrabasic	"

The commoner acids may be broadly dividedrinto four groups, according to the values of the "heats of neutralisation." This value is for Group I. about 20,000 gram-units; II., about 25,000; III., about 27,000; and IV., about 30,000 gram-units. The study of heats of neutralisation has led Thomsen to the conception of the *avidity* of an acid, *i.e.* the striving of one acid to displace another from its combination with a base. Thus, when equivalent quantities of NaOH, HNO_3, and H_2SO_4 are mixed in dilute aqueous solutions, two-thirds of the NaOH combines with the HNO_3, and one third with the H_2SO_4; the *avidity* of HNO_3 for NaOH is said to be twice as great as that of H_2SO_4 for the same base. HNO_3 in aqueous solution is therefore a *stronger acid* than H_2SO_4.

Measurements of the heats of neutralisation of monobasic, dibasic, *n*-basic acids has led Thomsen to classify some of these acids in ways different from those generally adopted in the text-books. His results as regards dibasic and tribasic acids may be thus summarised :—

Dibasic Acids

Group I. Typical formula RH_2 *e.g.* $SiF_6.H_2$
" II. " " $R(OH)_2$ *e.g.* $SO_2(OH)_2$
" III. " " $R(OH)H$. *e.g.* $SO_2(OH)H$.

Tribasic Acids

Group II. Typical formula $R(OH)_3$. *e.g.* $C_3H_5O_4(OH)_3$.
" III. " " $HR(OH)H$. *e.g.* $HPO_3(OH)_2H$

These examples will serve to show the suggestiveness of the results of thermal chemistry. Thomsen's three volumes teem with suggestions: his results throw light on such questions as are connoted by the expressions allotropy, molecular compounds, classification of elements and compounds, isomerism, and affinity.

It is in examining the subject of chemical affinity from the point of view of thermal chemistry that one becomes aware of the complexity of the problems included under this expression.

From the following numbers,

$$[H, Cl] = 22,000 + ; \quad [H, Br] = 8440 + ; \quad [H, I] = 6050 - ;$$

it might be concluded that the affinity of chlorine for hydrogen is much greater than that of bromine, and that the affinity of iodine for hydrogen is much less than that of bromine. But these thermal equations are not comparable; at ordinary temperatures chlorine is a gas, bromine a liquid, and iodine a solid; hence, on this ground alone, no precise conclusions can be drawn from the above data regarding the relative affinities for hydrogen of the three halogen elements. Again, looking at the numbers,

$$[C, O] = 28,600 + ; \quad [C, O^2] = 97,000 +,$$

it might be said that when oxygen combines with carbon in quantities of 16 grams at a time, the union of the second parcel of 16 grams is attended with evolution of much more heat than accompanies the addition of the first parcel of 16 grams. But measurement of the heat of oxidation of carbon monoxide, $[CO, O] = 68,400 +$, at once negatives this conclusion, and rather points to the number 68,400 × 2 136,800 as representing the thermal value of the transaction, $C + O_2 = CO_2$, where C represents 12 grams of gaseous carbon.

THURSDAY, JANUARY 3, 1884

THERMAL CHEMISTRY

Thermochemische Untersuchungen. Von Julius Thomsen, Dr. Phil. et Med., &c. Volumes I., II., and III. (Leipzig: Johann Ambrosius Barth, 1882-83.)

A PAPER was published in this journal a short time ago calling attention to "The Backward State of Chemistry in England" (vol. xxviii. p. 613); the writer regrets that so little attention is paid to the chemistry of the carbon compounds, and that so much time is spent in our chemical schools in elementary and routine instruction. In the second of these regrets I can thoroughly sympathise; our so-called students of chemistry are becoming mere machines which perform, and generally perform badly, mechanical processes known as qualitative and quantitative analyses. We hear complaints from physical laboratories that practical physics is taught in an unsystematic manner; we sometimes have comparisons drawn between the desultory methods of teaching pursued in these laboratories and the orderly and systematic courses of practical chemistry conducted in the workrooms of the sister science. But I am afraid it is rather the chemist who is to be pitied: his method is too methodical; it seems to succeed because it neglects the really scientific aspects of chemistry. Chemistry is a great branch of science; but what is the so-called practical chemistry of the schools or the examination? It is but a weary round of dull repetition; it consists of obtaining black precipitates, and yellow precipitates, and colourless precipitates, precipitates which are soluble and those which are insoluble; it occupies itself with filtering and washing, and drying, and burning, and weighing; it has little or no connection with the problems which belong to the science of chemistry. But when the author of the article to which reference has been made attributes the backward state of chemistry in England to the comparatively small amount of attention which is given to organic chemistry, I find myself unable to agree with him. I think we are apt to be dazzled by such things as the synthesis of indigo, or the artificial manufacture of alizarin: we forget to inquire whether the study of organic chemistry has in recent years added any great general principle to chemical science. The conception of the valency of elementary atoms is certainly an outcome of the study of the carbon compounds, or rather of the application of the atomic theory to this study; but have we not of late made too much of this conception? has it not rather stopped than aided inquiry? is it not time we had given up our "bonds," our "units of affinity," which are chiefly remarkable as being changeable almost at pleasure? Organic chemistry, as pursued in the German laboratories, it seems to me, has almost if not quite entered on the same path as that which has led qualitative and quantitative analysis to so sad a fall: it is in danger of ceasing to be a branch of science and of becoming an art of manufacture. Any student who goes through the course of preparation of organic compounds, systematised so well in the laboratories of the German universities and elsewhere, is ready to manufacture new compounds by the score; the difficulty consists in not making such compounds. There are whispers abroad that he who is not in the trade is regarded by the German professors as "no chemist."

I think the evil lies deeper: we are so anxious to act that we have no time to think. The chemist may gain a kind of reputation by making new compounds; the process requires no thought, no scientific training, no originality. It has also something to be said in its favour. Nature is so vast that we can scarcely hope to gain any accurate knowledge save by attacking the problems in detail. In chemistry, as in other branches of science, we must be content to gain "a series of small victories" over nature. But in fighting nature in detail we are apt to lose sight of general principles by the help of which alone can empiricism become science. I think that in chemistry, and more especially perhaps in organic chemistry, we are specialising too much: we are trying to solve large and complex problems by a series of small attacks all delivered from the same point. What then is the remedy? I would answer: Vary the points of attack; remember that the victory is to be gained only by boldness, and that it is emphatically worth gaining. Do not let chemistry remain the battlefield of the Philistines, but enliven it with the true spirit of science, with that spirit which will not believe that the universe is a "rubbish-heap of confused particulars," but will rather regard it as a vast organism in which while "everything is distinct yet [is] nothing defined into absolute independent singleness."

That the points from which the problems of chemistry may be attacked are many is witnessed by the book before us. Why is there no handbook of thermal chemistry in English? Will not some one at least translate Naumann's "Handbuch"? M. Thomsen has for years been known as one of the two great workers in the field of thermal chemistry; his contributions to this branch of science have been numerous and important; we cannot be too thankful that he has gathered these contributions together, and arranged and digested them in this series of volumes, which must remain as the groundwork of the science. Three volumes have appeared, and a fourth (to treat of organic compounds) is promised. Let me try to give some account of one or two points in Thomsen's work.

The notation of thermal chemistry is simple: Let r = the thermal value (stated in gram-units) of a chemical change; if the change consist in the formation of a definite quantity of a compound $X_a Y_b Z_c$—made up of a parts of X, b parts of Y, and c parts of Z—then

$$r = [X^a, Y^b, Z^c] + ;^1$$

if the same compound is produced in presence of a large quantity of water, then

$$r = [X^a, Y^b, Z^c, Aq] + ;$$

if the same substance already formed is dissolved in an unlimited quantity of water, then

$$r = [X^a Y^b Z^c, Aq] + .$$

The general expressions for the production and decomposition of a compound $X_a Y_b$ are

$$(1)\ X_a + Y_b = X_a Y_b + (X^a, Y^b);$$

and

$$(2)\ X_a Y_b = X_a + Y_b - (X^a, Y^b).$$

[1] Thomsen always writes the indices above the elementary symbols when these symbols occur in thermal equations

GEODESY AND MEASURES OF PRECISION

A Treatise on the Adjustment of Observations, with Applications to Geodetic Work and other Measures of Precision. By T. W. Wright, B.A., C.E., late Assistant Engineer United States Lake Survey. (New York: D. Van Nostrand, 1884.)

THIS treatise will be found a valuable addition to the literature of geodetic operations; the title is, however, misleading,—it implies a discussion of the various corrections required to allow for the effects of temperature, refraction, &c.; such corrections, however, are either omitted or only superficially dealt with, and the principal subject-matter is the adjustment of unavoidable errors by the method of least squares.

The work commences by a discussion of the various causes of error, and several practical hints are given as to how to diminish them. A remark in connection with personal error is worth quoting:—"A good observer, having taken all possible precautions with the adjustments of his instruments and knowing no reason for not doing good work, will feel a certain amount of indifference towards the results obtained. The man with a theory to substantiate is rarely a good observer, unless, indeed, he regards his theory as an enemy and not as a thing to be fondled and petted."

In the second chapter the usual law of error is stated, and the method of least squares is deduced therefrom, together with formulæ for calculating the mean square error, the probable error, and the average error. The author points out that the name "probable error" is unfortunate, and so we think; he is also of opinion that the average error might with advantage be more used than it is at present as a measure of the precision of a set of observations. This chapter is concluded by a most instructive discussion on the laws of error, based on various assumptions as regards the number of sources of unavoidable error. It is first supposed that there is only one source of error, and that all errors between certain limits are equally probable; the curve of error then becomes a finite straight line. The next case considers two independent sources of error, the curve then becomes two straight lines intersecting on the axis of y at an angle of 45°. In the third case three sources of error are assumed, and the curve of error is shown to consist of three parts, which together form a close approximation to the usual curve of error. The method of least squares is further developed in the succeeding three chapters, and applied to the adjustment of the direct observations of one unknown, to indirect and to condition observations. Various methods of solving the numerous resulting equations are given, both rigorous and approximate; amongst the latter the method of solution by successive approximations as used in reducing the primary triangulation of the Ordnance Survey of Great Britain is strongly recommended. The author also recommends the use of a calculating machine, or of Crelle's Tables, in order to diminish the arithmetical labour.

The remainder of the work is devoted to applying the foregoing to triangulation, to base-line measurements, to spirit levelling, to trigonometrical levelling, to the graduation of line measures, to the calibration of thermometers, and to the discovery of empirical formulæ. The application to triangulation is treated very fully, and several methods of solving the necessary equations are given and exemplified by means of examples. One of these examples is the adjustment of the angles of a quadrilateral taken from the Survey of the Great Lakes of North America, executed by the United States engineers; three methods of solution are given, one of them being that adopted by the United States engineers.

The author remarks very truly that it is a waste of time applying the rigid methods of adjustment to tertiary or even to secondary triangulation, and he proposes a method of successive approximations by first adjusting the angles at each station for the local conditions, and then using these adjusted values for the further adjustment in connection with the side and angle equations of the net. It may be mentioned that the reduction of the secondary triangulation of Great Britain, now being carried out, is effected by a graphic method applied after the angles have been locally adjusted; this method is found to give excellent results with far less labour than even an approximate method of calculation. The criticism on the title of the work is well exemplified in the chapters on base-line measurements and on the graduation of line measurements. For instance, there is no mention of the corrections required to be made to a base-line measurement to allow for errors in alignment or of level, for the effects of temperature and for reduction to sea-level. We think that at any rate a sketch of these and other sources of error and their methods of adjustment would not have been amiss.

The adjustment of the errors of trigonometrical levelling is very fully considered, and one of the examples proposed for solution is the adjustment of the levels taken trigonometrically during the triangulation executed to determine the axis of the St. Gothard tunnel.

The following remark is, we think, worth quoting:—"Closely allied to the preceding (elimination of accidental errors) is the common idea that if we have a poor set of observations good results can be derived from them according to the method of least squares, or that if work has been coarsely done such an adjustment will bring out results of a higher grade. A seeming accuracy is obtained in this way, but it is a very misleading one. The method of least squares is no philosopher's stone; it has no power to evolve reliable results from inferior work."

An excellent feature in the work is the illustration of the text by means of examples, embracing almost every possible case that occurs in practice. Some of these examples are fully worked out, others are proposed as exercises. Most of them are derived from geodetic work carried out in the United States. In conclusion we can strongly recommend this book.

OUR BOOK SHELF

On the Higher Teaching of Agriculture. By the Rev. J. B. McClellan, M.A. (Edinburgh: T. and A. Constable, 1884.)

NOT the least among the benefits of the International Health Exhibition was the series of Conferences held in connection therewith; and of these, one of the most valuable was the Conference on Education held in August last. Dr. Armstrong's paper on science-teaching in

scils has been already noticed in NATURE (vol. xxxi. p. , and the paper before us, by the Principal of the Rol Agricultural College, Cirencester, is another produ of the Conferences. The author looks on agriculture bradly, as extending, like the theme of the poet of the "Corgics,"—

"... super arvorum cultu per corumque
Et super arboribus,"

and in a well-reasoned and well-written paper pleads for the teaching of the natural sciences, their facts, laws, methods, and applications to agriculture, to those who have the direction of agriculture in this country, or who seek fortunes in the soils of new countries. Cowley, two centuries ago, asked, "Who is there among our gentry that does not entertain a dancing-master for his children as soon as they can walk? But did ever any father provide a tutor for his son to instruct him betimes in the nature and improvements of that land which he intended to leave him?" Though this reproach is not deserved so much now as when it was written, it is still not wholly unmerited, and will so remain until those who have the possession and management of landed property shall receive some special training such as that sketched out by Mr. McClellan. This training, if fairly common, would do far more to mitigate agricultural depression than any amount of piecemeal legislation. The paper is a useful addendum to Mr. Jenkins's recent report on agricultural education, and it may be commended to the attention of landowners and others connected with agriculture.

The Text of Euclid's Geometry. Book I., uniformly and systematically arranged. With a discussion of Euclid's application of logical principles, copious notes, exercises, and a figure-book. By J. Dallin Paul, R.N. (Cambridge: Deighton, Bell, and Co., 1884.)

THIS is a "prodigious" work of 182 pages demy 8vo, printed on excellent paper, with clearly-drawn figures, devoted to the "painful" elucidation of all the difficulties to be found in the first book of Euclid's Geometry, with such other matter as hath been adumbrated in the above-cited title-page. The road may be an easy one to walk in, all stones of offence being carefully put on one side or so rearranged that the wayfarer may not stumble as he saunters along it, but it certainly is a long road. The tendency of modern agitation a few years ago was to condense our text-books with a view to get up geometry in the minimum of time, but experience has taught us that in the majority of cases junior boys are very tender-footed, and cannot be driven along the geometrical path, and so there has been a reversion to the "grand old" book with many an aid to lure the young into paths not naturally attractive to them. We do not find fault with these attempts—we have recently noticed in these columns two admirable editions of the "Elements,"—but Mr. Paul has taken, we think, an extreme course: at some perhaps not distant date, if this sort of editing is catching, we shall have a similarly got-up work devoted to Euclid's treatment of isosceles triangles with a preliminary chapter on an axiom.

Our author has had so much to do with Euclid that his views of life have possibly got to be Euclid-tinted, and he sees nothing but Euclid! It would be no wonder, for his own words are, in deprecation of the presumption of adding another edition to the many that have gone before, "having been teaching Euclid almost daily for the last twenty years to pupils who, before coming under his tuition, had learnt something of geometry from the different text-books in use during that time, he ventures to think that this experience has made manifest to him the principal advantages and disadvantages of these numerous works, and thereby enabled him to present the propositions in the form most likely to be of educational value to those who are beginning either to learn or to teach the subject." We have allowed the author to put so much in evidence that the majority of our readers may gat[illegible] this is not "just the book they wanted" for the[illegible] and yet may see the scope of Mr. Paul's labours.

We cannot commend the author's action in placing the notes on the propositions in the early part of the book; experience has shown him that when placed in their usual position at the end they are passed by, but their actual position here offends *our* eye, and will not, we fancy, secure the writer's object. We regret that the writer has spent so much time and thought to so little purpose, as we believe, for we cannot imagine who will be the public that will purchase his book, its size and price are a bar to its introduction into school use. We close with remarking that there is a good deal that may be of use to (say) a pupil-teacher, or to one who is not strong in geometry and yet has to teach young pupils; but much, if not all, of this, can be got in handier text-books. A good feature is the placing at the end the particular enunciations of the propositions with the diagrams placed in positions very different from those which they had in the text: this would enable a pupil to test his acquaintance with the subject. R. T.

Das kleine botanische Practicum für Anfänger. Von Dr. Eduard Strasburger. Mit 114 Holzschnitten. (Jena, 1884.)

A BOOK by Prof. Strasburger, entitled "Das botanische Practicum," has recently been reviewed in NATURE, and recognised as a most valuable addition to botanical literature. The same author has now produced a condensed edition of the same book under the heading given above. The more important of the facts distributed through the 600 pages of the first and larger edition are here collected into the smaller space of 250 pages, an arrangement which is obviously better suited to beginners. It was specially remarked in the review of the larger edition that the efficient study of the various types named would occupy the average student a longer time than the author of the book appeared to think. This smaller edition will obviate the difficulty by supplying the elementary student with a shorter course of study, while the larger book will no doubt be found more useful as a book of reference for more advanced students, or as providing a curriculum for those who will make botany their profession. The merits of good type and excellent illustrations are to be found in this smaller book in as high a degree as in the earlier and larger edition. F. O. B.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Dr. Koch and the Comma-Bacterium

THE article published in NATURE of December 4, setting forth Dr. Koch's well-known theories with regard to the connection of a comma-shaped micro-organism with cholera, serves very efficiently as the text for one who desires to point out the deficiencies in Dr. Koch's observations and reasonings on this subject. The article is the most favourable statement which can be made on the side of those who accept Dr. Koch's conclusions, and is to a certain extent not quite fair to his opponents, since his original statements are not clearly separated from the subsequent statements which he has made in reply to criticisms.

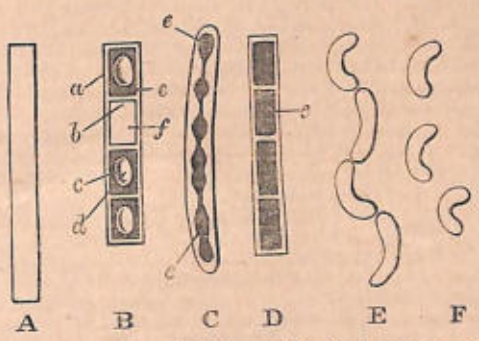
In opposing Dr. Koch's conclusions, it is desirable at the very first to state clearly that those who accept them appear to labour under two important misconceptions, the first being that Dr. Koch is, and has been for a long time, acquainted with *every* form (and the complete history of every form) of Schizomycetes or Bacteria existing, whether in the healthy body or in disease,

or in non-parasitic conditions; the second being that no one, with the exception of Dr. Koch and one or two of his pupils, has more or less real first-hand knowledge of Bacteria which is of any breathment. It is hardly necessary to insist in the pages of a scientific journal upon the fact that these really are misconceptions. Our knowledge of the Bacteria is in its infancy—and Dr. Koch's knowledge of them is no more than that which an industrious worker may be expected to have gained in the course of very special observations in regard to a limited class of these organisms (the pathogenic class) extending over a few years. On the other hand, the study of Bacteria has been prosecuted from three separate points of view during the past fifteen years by a number of observers, who may be grouped according to their point of view as the botanists, the chemists, and the pathologists. It is undeniably the fact that neither the chemists nor the pathologists have given much heed to the work of the botanists, and that the results attained by the three groups of workers have not been brought into harmony. To the medical world the special investigations of the pathologists alone are familiar, and undue weight has been given on the one hand to generalisations which ignore the more widely-based conclusions of the botanists, and on the other hand to the introduction into the pathological arena of methods of study which are not new or original, but have been borrowed from the botanists, whose opinions are nevertheless ignored or dismissed with little consideration. As examples of these tendencies I may quote the reiterated assertion by Dr. Koch, and the pathological school, of the conclusion (upon which they base many very momentous arguments) that the forms and the activities of Bacteria are absolutely fixed and limited—that micrococci only produce micrococci, bacilli only bacilli, spirilla only spirilla, and that none of these forms vary from generation to generation, or can be produced from another of these forms, and that a micro-organism producing a particular disease or a particular ferment cannot in the course of generations lose the property of producing that disease or that ferment, and *vice versâ* that one not having such properties cannot, in the course of human experience, acquire them. This axiom of the pathologists as to fixity of form and property, is entirely opposed to the conclusions of the botanists, who reason from a much larger area of observation. Such authorities as Nägeli, Cienkowski, and de Bary are amongst those who maintain, in opposition to the pathological specialists, that wide range of form and wide range of physiological activity are possible in one species or "race" of Bacteria. To this subject I propose to revert in detail, on a subsequent occasion. As an example of the borrowing of methods by pathologists from botanists, I may quote the fact that it is customary in the writings of pathologists to attribute the gelatine method of cultivation to Dr. Koch, and to attach some additional weight to his conclusions on the ground that he has originated this and other ingenious methods of research. As a matter of fact, the gelatine method of cultivation, which is only a modification of the potato-slice method, is due to the botanist Brefeld (as acknowledged by Koch himself).

Whilst it appears that there has not been on the part of the pathologists engaged in the investigation of Bacteria such an acquaintance with, and appreciation of, the work of the botanists as would be conducive to sound conclusions, it is true that the chemists also have frequently failed in the same way. Much of the work of M. Pasteur on Bacteria is difficult, if not impossible, to verify or to use in any way, on account of the fact that he has not, in prosecuting his studies on these minute plants, made correct use of the conceptions and terminology of the botanists, and has on the other hand used that terminology erroneously and in a special sense.

Dr. Koch has given a very remarkable proof of the isolation of his knowledge and work from that of the botanists (among whom without question the most trustworthy conclusions in this department of knowledge are likely to be found) by his use of the term "spore" in his description of the tubercle-bacillus discovered by him. The "spore" of a bacillus, as shown more especially by the minute studies of the botanist Oscar Brefeld, is a very special structure formed within the filament of the bacillus by a modification of a *part* of its protoplasm, and provided with its own special capsule. Koch actually describes the whole of the constituent protoplasm of a tubercle-bacillus which has a moniliform arrangement as a series of "spores," although it is quite clear that there is nothing in common between the arrangement of the entire protoplasm of a bacillus in the form of a string of micrococci and the periodic and special elaboration of the spores of the hay and anthrax bacilli. The so-called "spores" of the tubercle-bacillus are spores only in the sense that all segments of bacteria which can be detached and multiply are spores (Arthrosporeæ of de Bary) and do not justify the distinction which Koch makes when he states that the tubercle-bacillus is characterised by producing spores, whilst stating that spirilla, such as the spirillum of relapsing fever (which breaks up into segments capable of growth), do not produce spores.

Bearing in mind these facts as to the attitude of different schools of bacteriologists, let us examine the claim put forward



A, Outline of the bacillus of glanders (which Koch says resembles the comma F). B, Diagram of *Bacillus subtilis* of hay infusion during sporulation: *a*, sheath of the bacillus; *b*, transverse septum; *c*, coat of a spore; *d*, content of a spore; *e*, protoplasm surrounding the spore, which disappears entirely when the latter is fully formed; *f*, empty or sterile segment. C, Tubercle bacillus; the protoplasm is arranged in moniliform masses (*e*), which are erroneously called "spores" by Koch. D, Diagram of hay bacillus in vegetative state; the protoplasm is arranged in block-like masses (*e*), comparable to the moniliform masses of C. E, Spirillum dividing into commas. F, Commas (stated by Koch to be identical in form with the glanders bacillus, fig. A).

by Dr. Koch, and on behalf of Dr. Koch, by the writer in NATURE of December 4, p. 97, to have discovered that a certain comma-shaped bacterium is the cause of cholera. The writer in NATURE gives a summary of the various peculiarities of growth, form, and properties which Dr. Koch states he observed to be characteristic of a micro-organism occurring in the intestine of persons dead of cholera. He then observes: "Micro-organisms presenting all these characteristics are the bacilli described by Koch; organisms presenting only some of the characteristics, such as microscopical appearance, but differing in other points, are not Koch's comma-bacilli." To this conclusion, it is quite impossible in our present state of knowledge to assent. Its acceptance by the writer of December 4 renders it improbable that he will ever be convinced that Dr. Koch has formed an erroneous conclusion. The pretension put forward on behalf of Dr. Koch amounts to this, viz., that he has ascertained all the properties of this organism, that he cannot possibly have made any mistake, and that it is more probable that this organism has, since Dr. Koch left India, disappeared from existence, and been replaced by another very much like it, but not quite the same, than that any subsequent observer should be able to correct the hurried observations made by Dr. Koch when he was there. Such a pretension, were it advanced in regard to an animal or plant belonging to a group of exceedingly well-defined and highly-organised species would be unreasonable, but when put forward in relation to a representative of a group consisting of such minute, unstable, protean, and ill-understood species as are the Bacteria, must lead us to question altogether the impartiality and critical faculty of those who make it.

Admitting, however, for a moment that Dr. Koch's comma-bacillus is as peculiar as he supposes, admitting, as Dr. Koch originally implied by his silence as to the existence of other comma-shaped bacteria, that it is utterly unlike anything at present known in shape as well as in its action on gelatine, Dr. Koch has not proved or even rendered it greatly probable that this comma-bacillus is the cause of cholera, even when we accept his statement that "he has always found the comma-bacilli constantly accompanying cholera, and that he has never found them elsewhere." In the first place, it is quite certain both from Dr. Koch's reports and from the observations of others, that cases of cholera occur in which these commas are not abundant, in fact are insignificant in quantity; and in

the second place, great as has been Dr. Koch's activity in the study of Bacteria, the fact that he and others with whom he is in relation have not found the "comma-bacilli" elsewhere does not render it at all improbable that other observers might find them elsewhere. This fallacy, viz., as to the perfection of Dr. Koch's knowledge of all possible forms and modes of occurrence of Bacteria, I have already pointed out above.

On the supposition that these comma-bacilli *never* occur except in the choleraic process it is of course impossible to maintain (see the article in NATURE of December 4, section (7), p. 98) that the choleraic process merely favours the growth of the commas. But Dr. Koch admits that they occur and flourish outside the human body, in immediate connection with cholera dejecta; also that, when artificially cultivated, they flourish on substances not derived from the human intestine. What proof is there that they do not naturally continue so to flourish? Dr. Koch offers none—he merely tells us that he has failed to show that they do. It is not at all impossible, on Dr. Koch's own showing, that they do—and if they do, what becomes of the argument as to the impossibility of their introduction from external non-choleraic conditions into the human body?

The suggestion is also considered by Koch (and is cited in the section of the article already mentioned) that, "as a result of the disease (cholera), conditions arise which cause the transformation of some ordinary bacterium into comma-bacilli." But, say Koch and his English disciple, there is no evidence of such rapid transformation of one form of bacterium into another. Here we meet with the special axiom of the pathologists to which I have already referred. The opinion of those who are entitled to the very greatest consideration, namely, the botanists Nägeli, Cienkowski, and de Bary, is that there is evidence of such rapid transformation of one form of bacterium into another. Without going further than the case cited by the writer in NATURE as "merely" an alteration in pathogenic action, we have the instance of the attenuation of the virulence of anthrax bacilli, and we have also the case of the complete change of form of that same bacillus into nostocoid chains of spherical elements when cultivated on pork broth as shown by Klein. These two cases are by no means isolated ones (see my own researches on *Bacterium rubescens*, and also those of Zopf), but were they so they would be sufficient to establish the possibility of such changes in other Bacteria and to destroy the argument based on the assumption that such change is impossible.

The "only conclusion which remains" (see paragraph (*c*), section (7), in the article referred to) is therefore NOT that these bacilli and the cholera processes stand in the relation to each other that the commas are cause and the cholera effect. On the contrary, the only conclusion which remains is that WE DO NOT KNOW whether the commas although not detected by Koch may not be present in some parts of the healthy body, or flourishing outside it on organic matter, or may be the result of the transformation of some other bacterium, or may be the cause of cholera.

And the only way in which that ignorance can be removed has been very clearly recognised by Dr. Koch and all other recent writers, previous to the attempt made by Koch in 1884 to persuade the medical and scientific world that he had discovered the cause of cholera. The obscurity and uncertainty surrounding the Bacteria is such that no value can be attached to any asserted connection of a micro-organism with a disease as the cause of that disease, which is not based upon the experimental production of the disease by the inoculation into healthy animals of "pure cultures" of the suspected micro-organism. Dr. Koch's earlier statements on this subject are so precise and apt that I cannot do better than quote them here. He says in a pamphlet published in 1882, entitled "Ueber die Milzbrandimpfung":—

"The position which I take up is briefly as follows:—It is not yet proved that all infectious diseases are caused by parasitic micro-organisms, and consequently in each particular disease the proof of the parasitic character of the disease must be furnished. The first step towards this proof consists in the careful investigation of all those parts of the body affected by the disease, in order to establish the presence of the parasites, their distribution in the diseased organs, and their relation to the tissues of the body. . . . It is not until a thorough knowledge has been obtained in this way as to whether micro-organisms are present in the diseased parts, at what points they are present in perfect purity—whether, for instance, in the lungs, spleen, heart's blood, or elsewhere—that the attempt can be made to obtain the proof that these micro-organisms are of a pathogenic nature, and that they are more especially the cause of the disease in question. With this object in view, they must be isolated by [illegible] 'pure cultivation,' and when they have been freed in this [illegible] from all particles of the diseased body originally adhering to them they must be introduced by inoculation into the same species of animal in which the disease was observed, or, if that should not be possible, into animals in which the disease in question is known to occur with unmistakable symptoms. . . . An example is afforded by the disease known in man as erysipelas. It has been known for a long time that in this disease micrococci constantly are found in the lymph-vessels of the skin. *But by this knowledge it certainly was not proved that the micrococci are the cause of erysipelas.* Now, however, that Fehleisen has recently succeeded by excision of portions of skin from erysipelas patients (with every precaution against contamination by other bacteria which might be accidentally present on the skin) in rearing these micrococci in 'pure cultivations,' and in producing typical erysipelas by inoculating the human subject with these isolated micrococci, there can no longer be any doubt that the micrococci are, in fact, the cause of erysipelas, and that the latter is to be regarded as a parasitic disease."

This is the kind of proof which we require in the case of the comma-bacillus, and its supposed causal relationship to cholera. Dr. Koch has not succeeded in obtaining that proof. He has tried, and has failed, to produce cholera by inoculation of "pure cultivations" of his "comma." Cholera, at present, is not known as a disease in animals. Nevertheless, Dr. Koch has urgently and persistently declared that he considers it to be proved that the comma-bacillus is the cause of cholera! After repeated and public declarations of this conclusion, he is now making experiments by introducing his "comma-bacillus," not through the mouth, but by fistulæ into the intestine of rodents. Those who know the history of experiments on the production of cholera in mice and other rodents will not be convinced, even should Dr. Koch succeed in producing choleraic symptoms in this manner, since the readiness with which cholera-like processes are induced in these animals by abnormal conditions is such as to render them unfit subjects for these researches.

II. We may now revert to some of the statements made by Dr. Koch, which in the preceding remarks we have accepted without criticism. Even when this method is pursued, we find Dr. Koch's conclusions unwarrantable; they will appear still more so when we examine his position in detail. The writer of the article in NATURE of December 4 has omitted to notice a very important charge brought by Dr. Lewis against Dr. Koch, after the publication of Dr. Koch's address to the Medical Conference at Berlin in last August. Dr. Koch, also, has remained entirely silent in regard to this matter. It would be a very important thing if he would even now frankly reply to it. Dr. Koch and his defender assert that the "comma-bacilli" were found by Dr. Koch in cholera cases in Egypt, and also in specimens of intestine sent to him from India previous to his going there. Dr. Timothy Lewis, on the other hand, asserts that Dr. Koch had *not* recognised the "comma-bacillus" previously to his visit to India, and that in Egypt Dr. Koch attributed the causing of cholera to a totally different organism from that which he put forward after his arrival in India, and that, although he had thus shifted his ground, Dr. Koch did not admit at the time, and has not since admitted, that he was at one time convinced that cholera was caused by one organism, and a few months after was convinced that it was caused by another.

This charge is of importance for two reasons. If true, it must tend to lessen the confidence reposed by some in Dr. Koch's conclusions; and, secondly, it must also lessen our belief in the candour with which he states all the circumstances attending his observations and inferences.

The following quotations from the official reports sent home at intervals by Dr. Koch, coupled with the fact that he has not replied to Dr. Lewis on this point, though he has replied to him on other points, seems to leave little room for doubt that Dr. Lewis is perfectly correct in the very grave charge which he has brought against Dr. Koch.

In his report from Alexandria, September 17, 1883, Dr. Koch says:—"These bacteria are rod-shaped, and belong accordingly to the genus bacillus; they resemble most nearly in size and form the bacilli found in glanders" (which are straight; see woodcut, fig. A). In his report from Calcutta, dated January 9, 1884, he says:—"The microscopic examination demonstrated the presence of the same bacilli in the cholera intestine as had been found in Egypt." In a further report, dated February 2,

more or less at last get the following remarkable statement:—"The breath-cells are not quite rectilinear, like other bacilli, but slightly curved, like a comma. The curvature is sometimes sufficient to give the bacillus a semicircular form" (see woodcut, fig. F).

I think that it is abundantly clear that the organism selected by Dr. Koch in Egypt as the cause of cholera is not the same organism as that which he selected when in India, and that, although he is aware of that fact, he has not explicitly stated it, but has on the contrary (as does the writer in NATURE) endeavoured to give the impression that they are the same organism.

A further point of great importance as affecting the validity of Dr. Koch's theories, with regard to the connection of what he calls the comma-bacillus with cholera, is the statement of Dr. Lewis which is abundantly confirmed, and is not disputed by Koch, viz. that comma-bacilli, indistinguishable in appearance from those occurring in cholera cases, are quite common in the mouths of healthy persons. There is no doubt whatever that this is the case, although no record of the fact is to be found in any published treatise or paper on Bacteria, and that it was not commonly known to bacteriologists previously to Dr. Lewis's announcement of it in last September. The writer of the article in NATURE of December 4 hardly gives full effect to the importance of this point, since he cites Dr. Koch's reply to Dr. Lewis at the same time that he records Dr. Koch's earlier statements. Setting aside for the moment Dr. Koch's reply to Dr. Lewis, let us examine Dr. Koch's statements bearing on this subject, at the time when he announced his supposed discovery of the cause of cholera. He wrote from India that the organisms which he identified as the cause of cholera were of *peculiar* form, and "on account of its peculiar form, I have given to it the name of comma-bacillus." Throughout his subsequent writings, previous to the publication of Dr. Lewis's report by the Army Medical Department, Dr. Koch speaks of his cholera-organism as *the* comma-bacillus. He does not mention that any micro-organism similar to it in form is known to him. Had he been acquainted with one commonly occurring in the mouth, he would certainly have said, "The cholera comma is very like one occurring in the mouth, but differs in such and such ways." So far from this, he expressly says that no similar organism occurs in the human body, and states that he has failed to find an organism like the comma-bacillus in (amongst other places) the human mouth. No subsequent statement (after Lewis's publication) can affect the evidence which we have here that Dr. Koch was not acquainted with the "comma" which occurs in the human mouth.

After Dr. Lewis had shown that a "comma-bacillus" indistinguishable from Koch's "comma-bacillus" occurs in the healthy human mouth, and that accordingly—if we may suppose, from their identity of form and close association, that the two organisms are identical in every respect—the fundamental proposition of Koch as to the exclusive association of his comma-bacillus with cholera utterly breaks down, Dr. Koch replied as follows—(1) that the occurrence of a comma-bacillus in the mouth had long been familiar to bacteriologists (he did not say, it is to be noted, that it had long been familiar to him); and (2) that this comma-bacillus of the mouth will not grow upon neutralised cultivating-gelatine, whereas that from the intestine will, and that accordingly there is no ground for regarding them as identical species.

It seems to me in the highest degree improbable that Dr. Koch was acquainted with the mouth-comma when he published his conclusions as to the cause of cholera. If he was acquainted with it, it is undeniable that he committed a very grave fault in not drawing attention to it, and pointing out then and there the differences presented by cultures of the two commas. I have fairly conclusive evidence before me of the fact that Dr. Koch was not acquainted with the comma-bacillus of the mouth two years ago, when he published his large report and coloured plates on the tubercle-bacillus. In one of the drawings in that work he gives a delineation of the chief forms of micro-organisms occurring in the mouth, in order as he says to enable other observers to guard themselves against any confusion of the tubercle-bacillus with the micro-organisms which are normally present in sputa. *No comma-like organism is figured in that drawing or mentioned by Dr. Koch.*

As to the cultures of the "comma" from cholera intestines on the one hand, and from the healthy mouth on the other, differing in respect of their properties or their sensitiveness to conditions of alkalinity and neutrality, I venture to say that, *taking into consideration the whole history of the case*, it is not sufficient for Dr. Koch to tell us in an abrupt way that such differences exist. There is no reason to accept as final and perfect Dr. Koch's account of the characters of the comma associated with cholera, and I should greatly prefer to have the comparison of the conditions of growth of the commas from these two sources made by some one who is not, as Dr. Koch must unfortunately be, so very seriously biased in one direction.

I think there is some reason to expect that we shall hear from Dr. Klein as to the result of his impartial experiments, now being carried on in Calcutta, that the comma which occurs in the healthy mouth behaves in precisely the same way under cultivation, and is in fact as in appearance the same organism as the comma which occurs in the intestines of cholera patients.

Lastly, I may record a protest against Dr. Koch's extraordinary term "comma-bacillus." I have already pointed out that Dr. Koch uses botanical terminology loosely. The word "bacillus" has been by common consent restricted to the description of such rod-like forms as Koch first associated with cholera as the result of his Egyptian work. To prefix the word "comma" to this, was perhaps a method of avoiding unpalatable explanations. At the same time it is utterly inconsistent with the sense of the words. What Koch calls "comma-bacilli" may for convenience be termed "commas." They are well known to botanists as the segments of a spirillum (see woodcut, fig. E), the result of the breaking up of a spirillum into little pieces, one corresponding to each turn of the spire. They have been clearly figured and their nature recognised by Zopf. The "commas" of the human mouth and intestine are undoubtedly related to a spirillum which is frequently found in association with them, and would not have caused any astonishment or been stigmatised as "peculiar" in form by an observer who had that adequate knowledge of the natural history of the Schizomycetes in general which Dr. Koch has in many ways shown that he does not possess.

E. RAY LANKESTER

[We desire merely to make one remark with regard to the foregoing letter. The article referred to was prepared at the request of the Editor with the view of putting before the scientific public a fair and complete statement of Dr. Koch's case. The writer of the article requests us to state that he did not, except in the last paragraph, give any views of his own, and holds himself perfectly neutral in the matter, his mind not being at all made up on the subject.—ED.]

On the Distribution of Honey-Glands in Pitchered Insectivorous Plants

THE four genera of pitchered insectivorous plants at present in general cultivation are Nepenthes, Sarracenia, Darlingtonia, and Cephalotus. Attention was drawn to the minute structure and physiological action of the first three of these by Sir J. Hooker in his celebrated presidential address to the British Association in 1874, while the structure and morphology of the last was treated of by my master, Prof. Dickson (*Journal of Botany*, 1878, 1881). Both observers pointed out an *attractive surface* studded with honey-glands, which constituted the lid part, a *conducting surface*, either of an exceedingly smooth nature (Nepenthes), or beset with small downward-directed hairs (Sarracenia, Darlingtonia, Cephalotus), and in most cases a *glandular surface* (Nepenthes, *S. purpurea*, and Cephalotus), the secretion from which directly or indirectly assisted in digestion of animal products. In Sarracenia and Darlingtonia there was found in addition a *detentive surface*, covered with long deflected hairs.

A year ago Prof. Dickson further drew attention to a set of magnificent attractive glands along the free edge of the corrugated rim in Nepenthes, which he named "marginal glands."

My attention has recently been directed to all the genera, and I propose stating here the main results. A detailed account of the comparative results obtained by examination of the different species in the young and adult condition will shortly be presented to the Royal Society of Edinburgh.

Nepenthes.—Examining a pitcher of Veitch's beautiful hybrid, *N. Mastersiana*, I observed on its outer surface what seemed to be the small openings of honey-glands. When microscopically examined, they were found exactly to resemble those on the inner lid surface, except that the gland fossa was deeply hollowed out, and opened externally by a small orifice, while its inner surface was clothed to within a short distance of the orifice

by the gland tissue, very much as in sphæriaceous fungi the cavity of the perithecium is lined by asci. But even in this they agreed with the lid glands noticed by Dickson in *N. lævis*, and termed by him "perithecioid." Careful study of the outer lid surface revealed a few similar glands. On comparison of the species and hybrids grown in the Royal Botanic Garden, Edinburgh, a like condition was found to occur in all. The presence of these on the outer pitcher surface of *N. ampullaria* is interesting, since in it the lid is rudimentary, directed back, and destitute of glands on its inner surface.

At Prof. Dickson's suggestion I then examined the expanded lamina, and was agreeably surprised to find that glands were scattered rather sparingly over its upper, but pretty abundantly over its under, surface, especially near its junction with the stem. The tendril intervening between the lamina and pitcher also possessed them, and in some cases they were of very large size. Passing to the stem, it was found that some species had them very sparingly, others in considerable number, but while resembling those on the leaf externally, they were sunk much deeper in the tissue of the cellular layer, and strikingly reminded one of a simple animal gland.

After a comparative study of the different species I was induced to look at the sepals, as our garden curator, Mr. Lindsay, had mentioned to me that a very copious secretion of nectar took place in flowering. A complete pavement of glands the same in size and appearance as those on the inner lid surface of the pitcher, was spread over the upper epidermis of each. In Hooker's elaborate monograph of the genus ("De Cand. Prod.," vol. xvii.) these are mentioned, though their complete resemblance to the latter is not indicated. A few large "peritheicoid" glands may also be seen on the lower epidermis, and in flowers of *N. bicalcarata* (for opportunity of examining which I am indebted to Mr. Courtauld of Braintree), these attain relatively a gigantic size.

We see, therefore, that in Nepenthes, with its diœcious flowers, the same structure, which by their secretion attract insects for aiding in fertilisation, also lure them to the pitcher, so that their dead bodies may help in the nutrition of the plant.

Sarracenia.—Mellichamp pointed out (*Gardener's Chronicle*, 1874) that honey-glands are present not only on the lid, but also on the external projecting wing of the pitcher. I find, however, that, as in the last genus, they are diffused over the whole outer surface, including the lid; further, that in some of the species (*S. variolaris* and *S. rubra*) there are external upward directed hairs, as in some of the Nepenthes. On the outer surface of the three bracteoles and of the sepals the glands are likewise numerous, and will undoubtedly be insect attractors for promoting cross-fertilisation.

Darlingtonia.—This genus agrees with the last, except that the glands are very simple, being one- or at most two-celled. I have not as yet examined the flower, though there can be little doubt but that in it a like condition will occur.

Cephalotus.—Prof. Dickson, in studying this genus, noticed glands not only on the lid and outer pitcher surface, but even on the ordinary foliage leaves. I therefore required to deal only with the flowers. Scattered among the "encapsulating" hairs on the peduncle, bracts, and six sepals, were many glands identical with those of the leaves, though rather smaller; but further, the peculiar glandular processes intervening between the stamens and carpels seem to be the same mounted on cellular outgrowths of the receptacle.

Nepenthes, Sarracenia, Darlingtonia, and Cephalotus are therefore found to agree fundamentally in their morphological arrangements for physiological purposes, though referable to orders widely separated systematically.

J. M. MACFARLANE

Botanical Laboratory, University of Edinburgh

Earthquakes in England, and their Study

As no record of the most recent earthquake shock in England has yet found a place in the pages of NATURE, perhaps I may be permitted to give the following slight details, collected from the daily papers of Lancashire and London for November 15:—

A shock of earthquake ("severe," yet causing no actual injury) was experienced at Clitheroe, and in the neighbourhood, on the evening of November 14. At about 5.10 p.m. a terrific report, resembling loud thunder, was heard, instantly followed by a strong vibration of the earth, sufficient to induce the inhabitants to run out of their houses into the streets in a terrified state.

At Low Moor, where the shock seems to have been felt strongly, the wife of a man named Wilkinson fainted with fright. A waggoner on the road states that his two horses were nearly thrown to the ground. Much excitement prevailed throughout the borough and neighbourhood of Clitheroe, especially at Low Moor.

A lurid glare noticed in the sky at the time of the disturbance—5.10 p.m., sun set at 4.10—is mentioned in connection with the occurrence, but that appearance was, in all probability, only one of the sunset-glow effects with which we have lately become so familiar, and had nothing to do with the shock.

The circumstance that this particular part of Lancashire is much subject to earthquake disturbances, makes it specially important that no details of their occurrence be lost to science. Within the last fifty years at least six well-authenticated shocks have been recorded,—in 1835, 1843, 1868, 1871, 1873, and 1884,—and this list might easily be extended. Lancashire, indeed, may be considered as one of the chief areas of disturbance in England, and after Comrie, in Perthshire, perhaps the most important centre of seismic action in Great Britain.

While writing upon this subject, perhaps I may be allowed to offer the suggestion that, as the study of seismology is now one of such growing importance, it would be of considerable interest to many if a small space were set apart in the columns of NATURE every month, devoted specially to the record of current earthquake action, and kindred convulsions, in a scientific manner. It is my experience, as one who has for some time been engaged in collecting certain facts of these phenomena from various sources, that no sufficiently precise and complete records of the necessary facts, as may thus be readily transferred to the annals of exact science, are anywhere available. The general observations of seismic disturbance as heretofore described, are usually not only scanty in the matter of their detail, and often dressed up still with a superstitious flavouring, but also, for lack of *the right class* of observation, are too frequently merely vague and useless statements of wrong facts, generally in favour of doubtful hypotheses; and these are allowed to take the place of a well-ordered treatment of the real state of the case, upon a proper scientific basis.

WILLIAM WHITE

55, Highbury Hill, N., December 9

The Cacao-Bug of Ceylon

THE note by Mr. Distant in your number for October 30 (p. 684) may perhaps lead its readers to think that the insect which has lately been the subject of a report to the Ceylon Government has been wrongly identified by me as *Helopeltis antonii*, Sign. As that report will, however, before this have reached England, the matter will probably have been set right. I am not an entomologist, nor have I here the opportunity of reference to Signoret's original description or to other descriptive works; but the insect is, without any doubt at all, that which is well known—too well known—in Assam and in Java as *Helopeltis*. In the former country it is the destructive tea-bug or "mosquito-blight,"[1] and in the latter it is the notorious pest of the cinchona plantations.

As to the fragments which reached Mr. Distant, they were apparently insufficient for identification, further than with the family *Reduviidæ*. The cacao-tree harbours a host of Hemiptera, and planters are very apt to confound the innocent with the guilty. Its only formidable enemy in this order of insects, however, so far as I have seen, is the *Helopeltis*.

HENRY TRIMEN

Royal Botanic Garden, Peradeniya, Ceylon,
November 21

The "Messenger of Mathematics"

I THINK it is right that attention should be publicly directed to the exceedingly irregular appearance of the *Messenger of Mathematics*. In the case of a magazine of its size and character there is no reason whatever why it should not be published on the first of each month. The "heavy" mathematical journals may be permitted to turn up when their editors please; but the case of a *monthly* meant to foster a taste for mathematical investigation among junior mathematicians is entirely different; indeed, the good such a magazine is calculated to do is almost nullified by irregular publication. The *Messenger* is always

[1] Since my report was written, Mr. Wood-Mason's short treatise on the tea-bug has reached us here.

more or less irregular: just now, however, it is drawing so long a breath that one fears that its last message has been carried. We are now in the middle of December and the *October* number has not yet been heard of! ANGELUS

The Pronunciation of Chinese Names

SOMEWHAT after date, I beg to return to the subject of Anglo- and Franco-Chinese orthography, referred to in NATURE, vol. xxx. p. 592. In a short paper of mine published in the *Proceedings* of the Royal Geographical Society, vol. xxii. No. 6, 1877, I alluded to the desirability of a uniform or fixed "Roman equivalent" for Chinese characters standing for names of places, &c. I inclose a copy of this paper for insertion if desirable. To my mind the Italian vowels, &c., come nearest to the sounds of the Chinese characters. *Tung-King*, meaning "Eastern Capital," is the usually accepted form of *Tonquin*, or *Ton-Kin*, the terminal *g* being but slightly sounded. *Shang-hai*, the "Upper Sea," or the place "of going up to the sea," should be pronounced with the *g*, and is so spoken (Shanghai) by English and American authorities. Dr. Wells Williams has, I believe, in manuscript a standard Chinese Gazetteer of the World, in which all proper names likely to be used in telegraphy, newspapers, &c., are smoothly transliterated into Chinese characters. For translations from Chinese it is very necessary to adopt some such plan as Dr. Hunter has suggested for Indian names. Although his plan has come too late into the field to induce people to spell Calcutta as Kolkata, this is hardly the case as yet with Chinese names. The old native names of places should always be literally preserved. How much more beautiful is the old Franco-Indian name Stadaconda than Quebec for the scene of the death of Wolfe! I should be glad to co-operate or correspond with any interested in this matter, so prominent and important at the present juncture. F. PORTER SMITH

Hillworth House, Shepton Mallet, December 12

EXPLORATIONS IN ICELAND[1]

THE LAVA DESERT OF ÓDÁÐAHRAUN

III.

THE second part of my programme included the exploration of the western and southern portions of the Ódáðahraun Desert. In this journey I spent a fortnight during the latter half of August, a thoroughly rough and arduous time, on account of the very unsettled weather alternating between cold and rain, tempestuous gales, snowstorms, and sand-hurricanes. My journey extended to 240 English miles, but only two oases of grass were discovered the whole way. Along the skirts of Vatnajökul, throughout the whole extent of the lavas and sand plateaus which form the northern fringe or border intersecting it from Ódáðahraun, not one single blade of grass, nay, not even signs of mosses or lichens, are anywhere discoverable, hence we were obliged to provide ourselves with fodder for the horses in the shape of hay, oats, and maize dough.

The results of the journey are in every way as good as, under the circumstances, I could have anticipated. Now at last the whole of Ódáðahraun, with its surrounding wildernesses, has been explored. The weather was often enough sufficiently clear and fair to give me an opportunity to note all that required surveying. The few who have travelled over various parts of these deserts before me have seen next to nothing, on account of bad weather. Ódáðahraun, as stated in a former letter, is the largest lava-desert not only in Iceland, but in all Europe; the main portion of it has been formed by volcanic activity in Iceland in prehistoric times; but since the discovery of the island, even down to our own day, the region has witnessed a succession of eruptions. The various lava flats form one plateau, the bounds of which are determined on the east by Jökulsá in Axarfjörd, south by Vatnajökul, west by Skjálfandafljót, north by Mývatn. At its southern extremity it rises to 3200, at its northern to from 1400 to 1500, feet above the level of the sea. Altogether I took there about two hundred barometric and trigonometrical elevations and surveys. The separate lava-flats are due to about twenty separate volcanoes, honeycombed by hundreds of craters. Several of the separate lavas are, to the extent of many tens of square miles, one unbroken flat lava-field as it were; others, again, all torn up and disrupted, in some cases almost, in others entirely, impassable. The substratum of Ódáðahraun is palagonite-tuff and breccia, over the top of which is spread the doleritic lava, the origin of which dates from before the Glacial period. Above all the modern lavas have flowed. All the mountains that tower above the lava consist of palagonite breccia; along their roots and spurs are frequently found rows of craters, as well as those shield-fashioned volcanoes from which the lavas have welled out. The largest volcanoes have been built up entirely by lava-floods, which have flowed successively over each other, so as to form enormous convexities presenting an equal inclination to every side, but so slight as to amount to only a few degrees. This kind of volcano, which in the north country is generally designated by the name of Dyngja, reaches in Iceland nowhere such dimensions as in Ódáðahraun, as for instance Kollótta-Dyngja, Tröllad-yngja, Kerlingar-Dyngja, Ketil-Dyngja. In some places many rows of craters are ranged together along rifts from north-east to south-west, as on Reykjanes, and in Dyngjufjöll, where the craters around Askja and along the slopes of the mountains are practically innumerable. In Ódáðahraun proper hardly any water is found; rain sinks through the lava, and emerges again from under its edges in many small rivers and springs. The southernmost portion of Ódáðahraun has already been buried under glacial mud and sand from Vatnajökul, incessantly poured over its edge towards the north by innumerable glacial rivulets, that mostly vanish into the underlying sands and the lavas over which they are spread. Some of the larger streams, however, find their way eastward to Jökulsá in Axarfjörd, and a few into Skjálfandafljót. In consequence of the elevation of Ódáðahraun above the level of the sea, and of its waterless condition, it is a region almost barren of vegetation. On the drift-sand a few tufts of *Elymus arenarius* or stray specimens of *Statice armeria* and *Cerastium alpinum* may be found. Round the skirts of Ódáðahraun, where the water wells forth, a good deal of vegetation shows in some places, especially along the western fringes, in the valleys of Skjálfandafljót, where summer-pastures form the sheep-walks of the inhabitants of Bárðardal. On the eastern side of Ódáðahraun there are only two oases—Herðubreiðarlindir and Hvannalindir, and here the vegetation is confined to the banks of springs, its most distinguishing feature being the *Angelica archangelica*, which grows in small clusters or bushes everywhere along the banks of the brooks. There occur likewise some species of the slighter kinds of willow, such as *Salix glauca*, *S. phyllicifolia*, *S. herbacea*, as well as a few species of heather. Over the watered shingle-flats about Herðubreiðarlindir there are spread in parts red carpets of the lovely French willow-herb (*Epilobium angustifolium*). Insect life is very poorly represented, hardly anything being visible, save a few *Diptera*. To the south of Ódáðahraun not a plot of grass is to be seen, except at Gæsavötn, in Vonarskarð, where the vegetation is of the scantiest kind, comprising indeed little more than the *Salix herbacea*. Along glacial streams no sign of vegetation is ever apparent here; what little occurs grows along fresh-water springs.

It might be imagined that such a volcanic region as Ódáðahraun would be rich in hot springs, solfataras, &c. But such is not the case. The main portion of the lava is now so old, that all such volcanic phenomena seem to have died out. Of warm springs only two may be said to be still in existence, both on the western side of the lava; yet they are only lukewarm (respectively 33½° and 35½° C.) About Gæsavötn such springs obviously once

[1] Continued from vol. xxx. p. 585.

existed, but they have now almost entirely vanished (their temperature having sunk to from 5° to 7° C). Dyngjufjöll, especially the valley of Askja, are the only localities in these regions, where volcanic manifestations of this character are now to be seen; and there hot springs, clay-pits, sulphur-mines, and fumaroles of every kind are well developed. But these appearances are to be connected with an enormous eruption which occurred as late as 1875. Throughout the whole of Ódáðahraun I have come upon no traces of subterranean heat, except at the places here mentioned. About the peninsula of Reykjanes which I explored last year, many more signs of activity were found, which seems to show that in that locality the volcanic disturbances are to be referred to a later period than those of Ódáðahraun.

The northern edge of Vatnajökul has never been examined before. In my journey I was enabled to take the various elevations of this glacier, and found that at its western extremity, in the neighbourhood of Vonarskarð, it rises to its greatest height, over 6000 feet. East of this point it becomes lower, until it rises again about Kverkfjöll, where an upheaval is perceptible right across it from north to south. From the hollow, or lowest point, the largest glacier in Iceland has taken its course. It is important that this glacier should be carefully examined, but its exploration would require a long time, for it is almost impossible to make a lengthened stay here, on account of the utter barrenness of the region, and the roughness of the weather.

In this journey I succeeded in solving the geographical riddle, which of the many rivers of Iceland is the longest. It has hitherto been assumed that Jökulsá in Axarfjörd was the longest, 100 English miles; and that next to it came Þjórsá, 96 miles long; but I have now ascertained that Þjórsá is by far the longest river in Iceland, its course being about 120 miles, while Jökulsá is only 95. Hitherto, also, it has been supposed that the sources of Jökulsá were situated in the spurs of Kistufell; they are really twenty miles further to the east, under the western slopes of Kverkfjöll. The sources of Þjórsá are situated in the north-westerly portion of Sprengi-andr, to the north-west of Fjórðungsalda. Þjórsá, too, carries a greater volume of water than Jökulsá. On a July day the latter carries, midway between its source and its mouth (viz. at Grímsstaðir) 14,500 cubic feet of water per second, but Þjórsá at the proportionate point (at Þjórsárholt) carries 17,600 cubic feet in the same space of time.

Akreyri, September 7 TH. THORODDSEN

AMERICAN SUMMER ZOOLOGICAL STATIONS

IN the United States there has been during the past ten years a great increase in the advantages for the study of zoology. Not only has this increase been manifested in the colleges, but also by the facilities for summer study at the sea-shore. At present we have on the Atlantic coast five stations where there are facilities for students to carry on investigations. These laboratories are of two kinds—one where only the advanced student is allowed to study, the other in which any one manifesting a sufficient interest in Nature may be allowed a chance to work upon the marine animals; these latter are themselves divisible into two classes—one in which regular instruction is given, and the other where the student is supposed to study for himself under the direction of an efficient instructor.

The laboratory at Beaufort, North Carolina, connected with Johns Hopkins University, is intended as a place where students of the University, and somewhat advanced students from other colleges, can spend the summer in advanced work. It has attained for itself a reputation equalled by no other laboratory of its character in the country, because of the excellence of its work. Being supported by a regular fund, there are advantages connected with it which one will not find in other laboratories which are dependent upon subscriptions. Some excellent specialists spend their summers at this station, and the character of their work is shown in the bulletins published from the laboratory. Although Beaufort is not remarkably rich in variety of forms, still this is counterbalanced by the abundance of certain very interesting animals, for the study of which no better place than Beaufort can be found. As the Gulf Stream strikes on this coast, there are many interesting embryos found in the water. The building is a two-storied house made to serve as a laboratory, and it is placed within a few feet of high-water mark. The location is a low sandy shore in a rather warm climate, but this is necessary on that coast where nothing else is found. For collecting purposes a steam-launch and sail-boat are used. It is under the direction of Prof. W. K. Brooks, who has done much towards making it what it now is.

Much further north, at Newport, Rhode Island, is another laboratory of a somewhat different character. It is under the charge of Prof. Alexander Agassiz, who, with a few assistants and some advanced students from Harvard College, carries on his investigations on the sea-shore. Dr. F. Walter Fewkes and C. O. Whitman study regularly at this laboratory. Because of its private character it should rather be classed with the former private laboratories which investigators were accustomed to establish at some favourite place on the sea-shore than with the general laboratories for students, though a certain limited number are admitted each summer. The advantages for study are limited, and the locality rather poor.

In the southern part of Massachusetts, at a place called Wood's Holl, the chief marine station of the United States is stationed. This is the Laboratory of the United States Fish Commission. Since 1871 the Fish Commission has each year been located at some point on the New England coast, investigating principally the specific characters of the marine fauna. Prof. Baird, the Commissioner, has had the direction of the Commission since it was first originated, and with the assistance of such eminent American naturalists as Goode, Bean, Verrill, Smith, and Sanderson Smith, the previously unkown New England fauna has been thoroughly studied, and certain parts of the North Atlantic deep sea carefully studied. For many years all the work has been done by specialists employed by Government, in a poorly adapted laboratory; but now a new building is being erected for the express purpose of serving as a laboratory, and it will be fitted up with all the modern conveniences for zoological and microscopical study. Being supported by an ample Government fund, it is expected that there will be a good library connected with it, and we know that there will be a supply of large aquaria, and that all necessary chemicals will be supplied. In addition to the tables for regular *employées*, there will be room for a limited number of students from some of the larger colleges, who will thus be offered the finest advantages for zoological study to be found in America. For the use of the laboratory there is a steam-launch, and many small boats, while the two steamers *Albatross* and *Fish Hawk* are constantly bringing in material from the deep sea and surface of the ocean. Wood's Holl is excellently adapted for the purposes of a summer laboratory, both because of climate and variety and abundance of animal forms. The work already done from the old laboratory is of world-wide renown.

This ends the list of those laboratories intended solely for advanced students. Of the other class, the Summer Institute at Cottage City, Mass., is an example. This is a summer educational institution covering a wide variety of subjects, and intended for teachers who are willing to

spend their summers in quiet study. Courses of lectures are given in various subjects, one of which is natural [illegible] and the students can, if they choose, supplement [illegible]arse by laboratory study. It is exceedingly elementary, and none but beginners attend.

Of a similar character, but of more importance, was the Summer School of Natural History at Salem, Mass., under the direction of Prof. E. S. Morse. The principle upon which this school started was wrong. The originators seemed to have the idea that courses of lectures were essential to the success of the school. Such lectures, if delivered by men of reputation, were costly, and to meet the expenses of the school a large attendance was necessary. But in America the sciences are not studied by a sufficient number of people to supply such a school, dealing in a limited branch of science, with enough students to defray the cost of lectures; and few students can afford to pay large tuition fees. So it was that the Salem School had to depend entirely upon outside aid for its continuance, and this being withdrawn, the school was obliged to break up a few years ago. It is, indeed, unfortunate that it was obliged to do this, because it was filling an important place in American scientific education by originating an interest in teachers of the public schools for this branch of study, and thus raising the standard of scientific teaching in the lower schools. If a regular fund could be placed at the disposal of some body of scientific men for the purpose of giving instruction to teachers in this way, it would be an important thing; but unless such regular support be established, other less expensive means of instruction in natural history for beginners must be looked to.

At Annisquam there is another laboratory, under the direction of Prof. Alpheus Hyatt, which has an entirely different plan for teaching beginners. At this laboratory both beginners and advanced students are allowed to study upon paying a merely nominal sum. No special instruction is given, but there is an instructor, Prof. J. B. Van Vleck, who helps the beginner over hard places in his studies. The student is given some animal to make a study of, and he is advised to examine it critically, dissect it, and make drawings of the parts, all without the aid of a book; and then, having found out all he can without aid, he is given some book to verify his observations. In this way the student goes through all the important groups of marine invertebrate animals, often learning for the first time that he can really see things for himself without the aid of books. The powers of observation are brought into play, and the first foundation of a successful student of Nature are thus laid. How much practical benefit this method of instruction will have in making original investigators cannot be told at present, because the school has been in operation for such a short time. The amount of knowledge possessed by the students at the end of the summer, compared with that with which they started, is certainly encouraging. That this is the proper method of teaching natural history has been satisfactorily demonstrated to those in charge by the results. Both sexes are admitted, and preference is given to those who are going to make use of the facts which they learn, either in teaching or in special investigation. The building is a plain one-story-and-a-half house, situated at the water's edge. It is well lighted and firm, and aquaria on each table are furnished with water from a tank filled by a windmill. For collecting purposes there are common boats, and Prof. Hyatt has a schooner yacht, in which he frequently takes parties from the laboratory upon dredging [illegible]editions. Fifteen was the average number of students [illegible] summer, and they came from all parts of the country, [illegible] mostly teachers in small colleges and schools, and [illegible]w medical students and special investigators. In its inception it was intended for beginners, but advanced students are welcomed and given the best tables. The one unfortunate thing about this laboratory is that it is not established on a firm money basis, depending each year upon a grant of money from the Woman's Educational Society of Boston, which each year, so far, has generously given the funds for its maintenance. Neither the director nor the instructor receive salaries for their work, but furnish their summers free to the cause. For the purpose of making collections there is no better place on the eastern coast of the United States, with the exception, perhaps, of Eastport, Maine. The variety of animals is immense, and their abundance is also great, every condition necessary to an extensive fauna being present.

The last laboratory which we shall notice is the one which has long since passed out of active existence, in fact which died with its founder, the elder Agassiz. It was an immense building of wood on the island of Penikese, in Massachusetts, the outermost of the chain known as the Elizabeth Islands. The location was poorly chosen, for the fauna in the vicinity is poor, and there was no regular communication with the mainland, which was twenty miles distant. At one time during its brief existence it had a very large attendance, beginners particularly being attracted by the name of the eminent director. Lectures were given and laboratory practice was allowed each student. At this school such men as Fewkes, Faxon, Brooks, Whitman, and Alexander Agassiz, who have since become eminent in American science, received some of their first instruction in natural history. The death of Agassiz ended the institution, which if it could have been kept up under his direction would no doubt have equalled if not excelled any similar institution in the world. It is doubtful if even under Agassiz's direction this stupendous school could have been carried on, for we understand that the money basis was very insecure, and certainly the expenses were very heavy, and the tuition charges light.

RALPH S. TARR

ON A NEW METHOD FOR THE TEACHING OF SCIENCE IN PUBLIC ELEMENTARY SCHOOLS[1]

THE desirability of imparting to children some knowledge of the principles of science is now so generally agreed upon that this paper will be devoted not to the argument that science-teaching is necessary, but to a description of a method by which it may be successfully and thoroughly carried out.

In the "Code" under which the system of Government education is carried on in this country, science is mentioned under two heads:—

(1) As a "class-subject" (optional) which may be taught to any or all of the seven "Standards" under which the children are classed, and

(2) As a "specific subject" (also optional) which may only be taken by the children in Standards V., VI., and VII. The specific subjects named are—

1. Algebra.
2. Euclid and Mensuration.
3. Mechanics.
4. Latin.
5. French.
6. Animal Physiology.
7. Botany.
8. Principles of Agriculture.
9. Chemistry.
10. Sound, Light, and Heat.
11. Magnetism and Electricity.
12. Domestic Economy (*Girls*).

Either one or two (but not more than two) of these specific subjects may be taken by a child. The course in each subject is divided into three parts, so that a child must remain at school for three years in order to complete the study of any one subject.

The grants paid are at the rate of 1*s.* for a "fair" or 2*s.* for a "good" pass in class-subjects, and 4*s.* per pass in the specific subjects.

[1] By W. Jerome Harrison, F.G.S., Science Demonstrator for the Birmingham School Board. The greater portion of this article was read as a communication to the International Conference on Education, held at the Health Exhibition in July last, and is here reprinted by permission of the Executive Council.

To be successful in a public elementary school any scheme of instruction must be based upon the conditions of the Code. To these conditions, as they now stand, the following exceptions may be taken :—

(*a*) The teacher is forced to choose between geography and science as a class-subject. He may take *either*, but he cannot take *both*. As a rule he takes geography. It is to be hoped that in the future this restriction may be removed, and that a simple course of object-lessons on plants, animals, manufactures, &c., which would fulfil the requirements of science as a class-subject, will be given *in addition* to those lessons on geography which are really indispensable.

(*b*) The three years' course in a specific subject is too long, now that the child does not begin the study until it enters the Fifth Standard. Taking the case of the boys and girls presented for examination during 1883 in the Birmingham Board schools, we find in Standard V. 1864 children ; in Standard VI. 482 ; in Standard VII. 85.

Tracing back the eighty-five Seventh Standard children, we find that they are the residue of 427 Sixth Standard children of 1882, and of 1223 who passed the Fifth Standard in 1881. It would probably be better to reduce each specific subject to a two years' course, and to allow Seventh Standard children to be examined in the work of the *two* previous years.

CHOICE OF SUBJECTS.—In considering what science

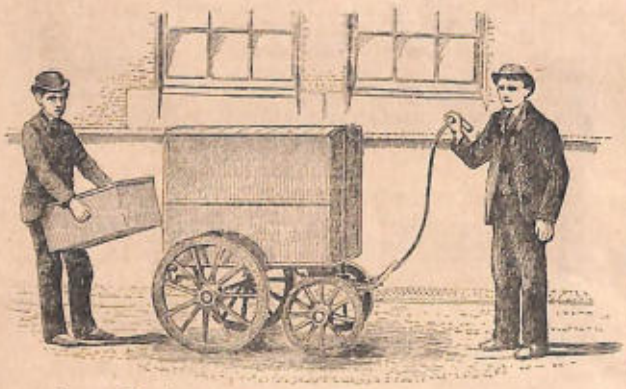


FIG. 1.—Hand-cart used for conveying apparatus from school to school.

subjects to select from those named in the Code, much will depend upon local conditions. Generally speaking, for boys' schools mechanics should be chosen, and for girls' domestic economy. As a second subject in town schools, either chemistry or magnetism and electricity may be recommended for boys, and animal physiology for girls. In country schools, principles of agriculture for boys, and botany for girls, will be found very suitable.

In the new Seventh Standard School, lately opened by the Birmingham School Board, there is an excellent workshop, fitted up with carpenters' benches, forge, lathe, &c., for forty boys. For this school I have drawn up a syllabus of a (proposed) new specific subject, entitled "Principles of Tools and Properties of Materials."

OBJECTIONS TO SCIENCE-TEACHING.—In time past three principal objections have been urged to the introduction of science-teaching into public elementary schools. These objections are :—

(1) *Want of Qualified Teachers.*—The ordinary teachers and pupil-teachers of our schools have not, as a rule, the sound knowledge of principles and practised powers of manipulation which are necessary in order to teach science with power and effect.

(2) *Want of Time.*—To *prepare* for a science-lesson, and to properly *clean and put away* the apparatus, requires more time than our closely-worked school-teachers are able to give. Some have also urged that "time" cannot be spared from the study of the "three R's," in which they consider incessant mechanical practice to be necessary.

(3) *Cost of Apparatus.*—To teach science practically, and it should be so taught to be of any value—a considerable sum must be spent in the purchase of apparatus. Thus the apparatus required for the three stages of mechanics costs about 75*l.*, and for domestic economy 65*l.*, and this is a considerable expenditure for a single school.

THE ITINERANT METHOD OF SCIENCE-TEACHING.—A method by which the principal objections urged against science-teaching in elementary schools may be overcome was suggested a few years ago by Col. Donnelly and Prof. Huxley, and it is not the least of the many services which these gentlemen have rendered to science and to education. This method has been carried out on a large scale, and with the most gratifying success by the School Boards of Birmingham and Liverpool, and the object of the present paper is to describe the manner in which the work is done in the former town.

The principal features of the itinerant method of science-teaching are as follows :—

(1) A science demonstrator is appointed, who should combine a practical knowledge of school-work and power to teach large classes with a thorough acquaintance with the branches of science which he is to teach.

(2) A "centre" is chosen in connection with some particular school, where a class-room may be set apart, or (better) a subsidiary building erected, where apparatus can be kept and the experiments prepared.

(3) A hand-cart must be provided (Fig. 1), into which the boxes containing the apparatus fit, and can so be conveyed from the science-centre to school after school by a strong youth. In this way one set of apparatus will serve for many schools. In each school department there must be a trestle-table, which should be placed in front of the class as the time for the science-lesson draws near. The hand-cart is brought to the school, the youth carries in the boxes, unpacks the apparatus, and places it upon the table. Then the science demonstrator walks in and gives the lesson. Afterwards the youth packs up the apparatus in the boxes, replaces them in the hand-cart, and marches off to the next school.

(4) A time-table is drawn up showing the exact time at which the science-lesson is given at each school, and its duration (forty-five minutes will be found suitable). A syllabus of each year's course of lessons must also be prepared (which should be distributed to the class-teachers and children), so that the subject may be gone through in a systematic way. As a rule it will be found possible for each science demonstrator to give four lessons per day, or twenty per week.

Each class should receive a lesson from the demonstrator at least once a fortnight. At each science-lesson the ordinary teacher of the class is present, and takes full notes of the matter given. During the intervening week the class-teacher *recapitulates* the science-lesson, giving such additional or new illustrations as he or she may be able to provide. The children then either write a general account of the lesson or answer three or four questions upon it, and the papers worked are submitted to the science demonstrator when he next visits the class.

It is plain that the itinerant system fairly meets the objections which have been urged against the introduction of science-teaching on the grounds of want of qualified teachers, want of time, and cost of apparatus. It also secures systematic and continuous teaching throughout the school year. The teaching is practical, and every fact or law is demonstrated experimentally. Wherever eight or ten schools are within a reasonable distance of each other, this plan may be carried into effect. Voluntary schools may combine with Board schools (as is done in Liverpool) to secure the services of a science demonstra-

tor, or small towns near to one another (as in Lancashire and Yorkshire, or in the Black Country), may unite for the same end.

APPLICATION OF THE ITINERANT SYSTEM OF SCIENCE-TEACHING IN BIRMINGHAM.—It was in June 1880 that I received my present appointment from the Birmingham School Board. Since that time the work in which I have been engaged has received the unanimous approval of the Board, but I ought especially to acknowledge the encouragement received from the Chairman—Mr. George Dixon—and from Dr. Crosskey, and the valuable advice given by the able and experienced Clerk to the Board, Mr. G. B. Davis.

Three assistants have been appointed, with a junior laboratory assistant, and two youths who work the two hand-carts which we now employ. The regular science staff thus includes seven individuals, whose salaries amount to 750*l.* per annum. In connection with the new Icknield Street School an admirable laboratory has been erected, at a cost (with fittings) of 1450*l.*, including a lecture theatre to seat eighty, a chemical laboratory and store-room, and a demonstrator's room (Fig. 2). About 400*l.* has been expended in the purchase of apparatus.

There are now thirty schools under the Birmingham School Board, attended by nearly 40,000 children.

In each of the thirty boys' departments Mechanics is taken as a specific subject by every boy in the Fifth and higher Standards; six departments take magnetism and electricity as a second specific subject.

In each of the thirty girls' departments Domestic Economy is taken as a specific subject by every girl in the Fifth and higher Standards; three departments take animal physiology as a second specific subject.

At the request of the teachers a few Fourth Standard children of exceptional ability are allowed to attend the science-lessons, since it is found not merely to do them good mentally but to induce them to remain longer at school.

The total number of children now receiving instruction in science in the Birmingham Board schools is, in round numbers:—

Mechanics	2400	boys
Magnetism and Electricity ...	300	,,
Domestic Economy	1800	girls
Animal Physiology	100	,,

In the framing of the syllabuses a wide interpretation has been given to these subjects; thus under the head of domestic economy as much elementary chemistry and physiology are taught as will enable an intelligent girl to comprehend the familiar facts of home life.

As a rule two science-teachers and two youths go with each hand-cart, so that the lessons to boys and girls go on simultaneously in each school. By this plan each hand-cart can visit four schools (eight departments) daily, while with a single teacher only two schools (four departments) could be visited.

The same lesson is given to class after class throughout the week. It is previously very carefully prepared by the science-demonstrator, is written out in full by him, and the experiments are tried over and the apparatus packed on Saturday morning, so that everything is ready for the start on Monday morning.

In each science subject there is but *one stage* taught in each school. Children entering on the subject join in at the second or third stage, as the case may be, so that all the children in any one department form one class, working at the same stage of the same subject. This plan simplifies the work wonderfully, and it is found in practice that the science subjects taken may be as conveniently commenced at any one of the three stages into which each is divided in the Code as at any other. Each stage stands quite by itself, and each may be considered in turn as forming an introduction to the other two.

RESULTS OF THE SCIENCE-TEACHING IN BIRMINGHAM.—The visits of the science demonstrator have been welcomed both by the teachers and children of the Board schools. The teachers have earnestly co-operated in the work, and much of its success is due to their efforts. With the children, the science-lessons have proved extremely popular. There is invariably a good attendance on the day of the science-lesson. Among the boys the half-timers then muster strongly, often getting leave to come in for that lesson only, and sitting with bare arms and rolled-up aprons, just as they have run from their work. In the same way big girls, who cannot escape from tyrannical babies, beg leave to bring their charges into the class-room; and I know of many a case where "mother" has been persuaded to change her "washing-day" because it

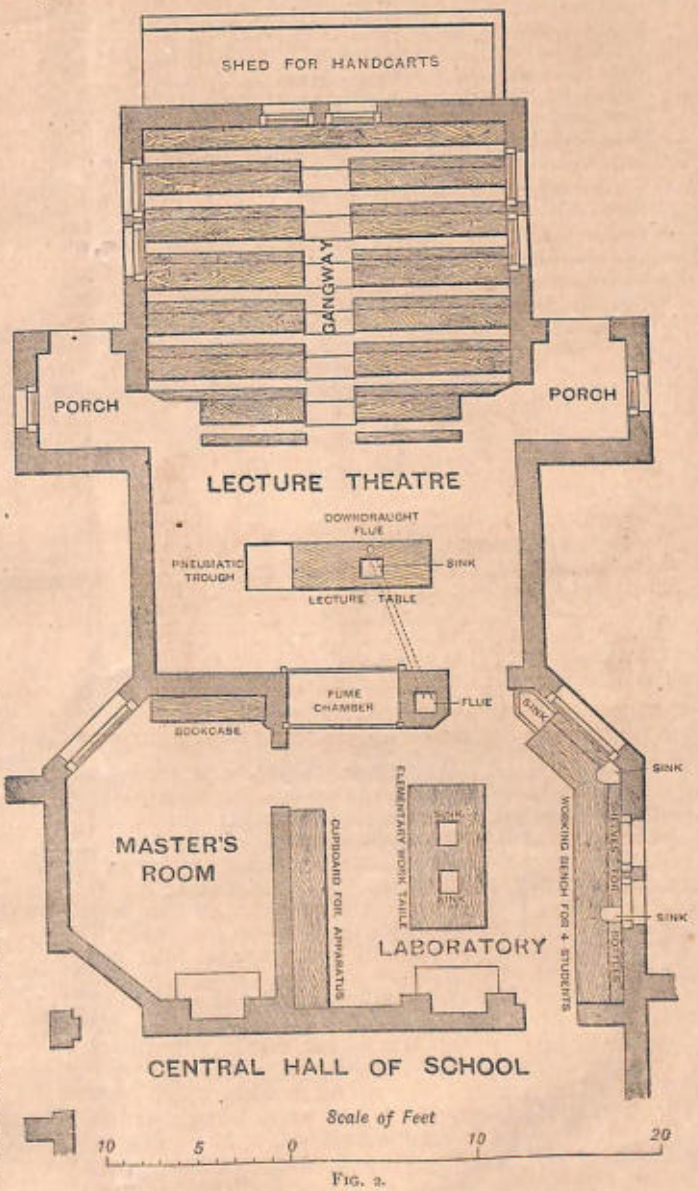


FIG. 2.

clashed with the day of the demonstrator's lesson in domestic economy. The teaching has evidently been carried home, for an irate landlord visited one school to "know what they meant by teaching children that his houses were not fit to live in!" the said houses being built "back to back," a practice the evils of which are pointed out in one of our domestic economy lessons. The large number of papers, essays, mechanical drawings, models of apparatus, &c., exhibited by the Birmingham School Board at the Health Exhibition will give some idea of the results of the work and of the eager manner in which it has been taken up by the children. So far from the science-lessons having interfered (by taking up time which would otherwise have been spent on the three R's) with the ordinary school work, the unanimous testimony of the teachers is that the increased intelligence of the children enables them to do their Standard work more easily. The idea has been very prevalent that by incessant mechanical practice excellence in the "three R's" can be secured; but the fact is that unless the intelligence be cultivated, no subject can be properly learnt. True education is culture of the mind, and mechanical acquirements have nothing in common with culture.

Applying to the matter the practical test of the Government examinations by Her Majesty's inspector, the results come out in a very satisfactory way.

Year	Number of passes in specific subjects	Percentage of passes in the three R's
1878	121	81·3
1879	424	82·0
1880 [1]	841	84·7
1881	1724	88·4
1882	3114	92·6
1883 [2]	3150	89·6

Another pleasing fact is the much larger number of children now found in the upper Standards. In 1879 (the year before the introduction of science-teaching) the percentage of children examined in Standards IV. to VII. was only 19·5; it is now 33·7.

The following extracts from the published reports of the Birmingham School Board prove that, in the estimation of those best able to judge, the teaching of science has proved a success.

1880.—"An important addition to the work of the Board schools has been the introduction of experimental lessons in elementary science. A science demonstrator has been appointed, and has now commenced work."

1881.—"In June 1881 the Board decided to appoint an assistant science demonstrator. The lessons in elementary science had proved so successful and attractive, that it was felt to be unfair that such advantages should be denied to some schools while they were afforded to others."

"These science-lessons are fully answering our expectations; the children are very attentive and much interested in the work; and, in addition to the useful knowledge they gain, their general intelligence is being developed."

1882.—"The success of the science-teaching has been strongly marked, both by the papers worked by the candidates for the science scholarships, and by the greater development of intelligence shown in regard to other subjects."

"As the teaching of science in Board schools has now become exceedingly popular, and many of the children have made considerable progress, six scholarships of 10*l.* each have been founded in connection with the Science and Art Department."

"Upwards of one thousand boys are now receiving admirable lessons in elementary science in the Board schools, and the result of this teaching is little less than marvellous."

1883.—"The teaching of elementary science in Board schools has developed considerably during the year, the scholars taking great interest in it, and the results shown by the examinations being such as to prove that the knowledge imparted has been largely retained."

"Two great steps in advance have been made by the present Board. One is the establishment of science classes. The remarkable success which has attended these classes has been frequently alluded to, and is generally known."

Science Scholarships.—Twelve science scholarships of 10*l.* per annum have now been established in connection with the Science and Art Department. The boys who obtain these scholarships, together with an equal number selected as showing special aptitude for science, spend each Friday afternoon at the science laboratory in the study of analytical chemistry. All those hitherto examined have passed (and a large number in the first class) at the May examinations of the Department.

There are also two valuable science scholarships by which boys may pass from the Board schools to King Edward's Grammar School, and thence to the Mason Science College, their parents meanwhile receiving allowances of 15*l.* and 25*l.* per annum for their support. These scholarships are very keenly competed for, the usual number of boys examined being over 200. The examiner, Prof. Poynting, M.A., of Mason College, reports as follows:—

1882.—"Hardly any of the questions in my paper could have been answered without independent thought on the part of the candidates, and I had but very few answers showing a want of such thought. The boys showed that they had seen and understood the experiments which they described, that they had been taught to reason for themselves upon them, and that they were not merely using forms of words which they had learnt, without attaching physical ideas to them."

1883.—"The paper worked by the boy who stands highest on the list was an excellent one, and showed considerable power. The next five boys also deserve special mention as having done very good work. I think the general style of work sent in was very satisfactory. The average was not so high as last year, as the third stage of the subject was far more difficult, and the paper set was also much harder, but I think that quite as much ability was shown on the part of the candidates, and that the evidence of careful teaching was quite as strong."

Mr. Richard Tangye—the head of the great firm of Tangye and Co.—has taken a warm personal interest in the work, and his aid and countenance have been most valuable. He testifies strongly to the great improvement of his young "hands" since the introduction of the School Board system in Birmingham.

Summing up the matter, the results which we hope to obtain from this science-teaching, and which indeed have already manifested themselves, are:—

(1) The general quickening of the intellectual life of the school.

(2) The imparting of scientific knowledge and method to children which will be useful to them in after life, and which will cause many of them to continue their science-studies in evening classes.[1]

(3) The discovery of children of exceptional ability, and their support by means of scholarships.

(4) The instruction of the school-teachers in scientific principles, which they may apply to the general work of the school.

Evening Work in Science.—The work done among the teachers by means of evening science classes in connec

[1] Science demonstrator appointed June 1880.

[2] Mundella Code introduced, by which Literature (in which 1435 passes were made in preceding year) was removed from list of specific subjects. The general requirements of this Code being higher, there was a slight drop in the percentage of passes for this year.

[1] The last Report of the Birmingham and Midland Institute speaks of the influx of youths into the evening science classes—"the result doubtless of the science-teaching now carried on in the Board schools."

with the Science and Art Department has been of an ...tant character. The Birmingham School Board employs about 800 teachers, and it now provides education, by means of training classes, for about 450 (the pupil-teachers and uncertificated assistants). The growth of the science work in this direction will appear from the following table:—

Year	Number of Certificates obtained	Number of First Class Certificates awarded	Gross Grant
1881 ...	24 ...	0 ...	£18
1882 ...	91 ...	18 ...	£108
1883 ...	100 ...	24 ...	£124
1884 ...	173 ...	33 ...	£197

It is very important to elementary school teachers to do well in science, since (by a regulation of the Education Department) those who have passed in science have a certain number of marks added to those which they obtain for other subjects at the Queen's Scholarship and Certificate Examinations, through which all these teachers have subsequently to pass.

Electricity and magnetism has been taught to the pupil-teachers, and physiography to the assistants.

When evening science lectures are given, however, no school-work can be done by the demonstrators in the afternoon of the same day, as the time is taken up with the preparation of the experiments, &c., for the evening lectures.

The Board possesses an excellent optical lantern presented by Messrs. R. and G. Tangye as a token of their appreciation of the science-teaching and with its assistance the science demonstrator gives popular evening science lectures in the various schools, taking subjects such as will be likely to awaken the interest and increase the intelligence of the children, as "Wild Animals in the Zoo," "The Star-lit Sky," "Two Days in London," "A Voyage to the Moon," &c. Occasionally, on fine evenings, the elder children are shown the moon, planets, double-stars, &c., through a three-inch achromatic telescope (refractor). These expositions tend to attract children to school, and to improve the regularity of the attendance.

COST OF THE SYSTEM.—The following rough balance-sheet for the year 1883-84 shows the very small cost at which the work of science-teaching is carried on in Birmingham:—

	£	s.	d.
Receipts.			
Half of Government Grant on specific subjects ...	160	0	0
Grant from Science and Art Department	150	0	0
	£310	0	0
Expenditure.			
Salaries	750	0	0
Interest on cost of buildings and apparatus ...	70	0	0
Renewal of apparatus and cost of materials ...	50	0	0
	£870	0	0
Net cost to the Board	£560 per annum.		

As a penny rate yields 6000*l.*, it will be seen that the cost of this system, by which more than 4000 children, distributed over sixty school departments, receive regular and practical science-lessons, amounts to only one-tenth of a penny in the pound, or to 9*l.* 10*s.* per annum for each school department. It must be remembered also that the full benefit of the system has not yet been reaped, and that the grants will certainly continue to increase. Credit has only been taken for one-half of the grant for the specific subjects.

TEXT-BOOKS.—Failing to meet with works exactly suitable for the wants of the children, the science-lessons in mechanics and in domestic economy have been written out in full, and are now published by Messrs. T. Nelson and Sons. Similar works on magnetism and electricity and on chemistry are nearly ready for issue. Each work consists of three small volumes corresponding with the three years' course prescribed by the Code. These books have already been adopted by the School Board for London, the Irish Intermediate Education Board, and other important educational bodies.

SCHOOL MUSEUMS.—For use in object-lessons, and as a constant source of pleasure and instruction, a small collection of typical objects stored in a glass-fronted cupboard ought to be placed in every school. Such cupboards are now being supplied to the Birmingham Board schools, and it has naturally fallen to the lot of the science demonstrator and his staff to assist in the mounting, naming, and classification of the objects with which the cupboards are, at little or no expense to the Board, to be filled.

CONCLUSION.—Since the commencement of this system of practical instruction in science in Birmingham, many eminent men have visited the schools to see it in operation, and they have been unanimous in their approval. In the "Instructions to Inspectors" issued by the Education Department, the system receives official sanction and commendation:—"You will often find that these (specific) subjects are most thoroughly taught when a special teacher is engaged by a group of schools to give instruction in such subjects once or twice a week, his teaching being supplemented in the intervals by the teachers of the school."

The Commissioners for Technical Education visited the Icknield Street Centre a few months ago, heard science-lessons given, and examined fully into the work. In their valuable Report, recently issued, they say:—"We could hardly overstate our appreciation of the value of the plan of giving instruction in natural science by special teachers as carried out in the Board schools of Liverpool and Birmingham, where the employment of a well-qualified science demonstrator insures the sound character of the instruction, whilst the repetition of the lesson by the schoolmaster enables him to improve himself in the methods of science-teaching."

Within the present year the work has been crowned by the opening of a Technical School for Seventh Standard boys, situated in the centre of the town, and fitted with an admirable laboratory (for forty boys), lecture-theatre, workshop (for forty, with three lathes), room for drawing, class-rooms for the ordinary subjects, and a capital dining-hall, &c. The building has been adapted, fitted (at a cost exceeding 2000*l.*), and presented rent-free to the Board by Mr. George Dixon. This school will constitute the last link of the chain of elementary education supported by the town, and who can doubt that in it will be laid the foundation of many a good work, both for the individual and the community.

NOTES

MR. J. J. THOMSON has been elected to fill the post of Cavendish Professor of Experimental Physics in the University of Cambridge in succession to Lord Rayleigh. A numerously signed requisition to Sir Wm. Thomson to become a candidate was declined.

CAMBRIDGE was *en fête* on Monday. Peterhouse, the oldest collegiate institution in the University, was celebrating the six-hundredth anniversary of its foundation. It was stated at the dinner that one-third of the present Fellows were Fellows of the Royal Society.

IT is announced that the International Sanitary Conference, which Signor Mancini proposed some time ago, will meet at Rome in February or March. Later on another Conference, also suggested by Signor Mancini, will meet to consider the possibility of some agreement for the mutual execution by the Signatory Powers of legal judgments.

THE monthly weather review of the Signal Service, prepared, as announced for the first time in the August number of *Science*, by Second Lieut. W. A. Glassford, has come to be a quarto of twenty-eight pages, with five charts. This is a good growth from the four small pages and three charts of the first issue, eleven years ago. Then, the headings were storms, anti-cyclonic areas, temperature, precipitation, peculiar phenomena and facts, rivers, and cautionary signals: now, all these subjects are treated in much greater detail; and among the many additional topics there may be mentioned atmospheric pressure and its range (illustrated by a new style of chart), Atlantic storms and ice, range of temperature, frosts (illustrated by a chart for August 9 and 25), winds, local storms, tornadoes and thunder-storms, sunsets, drought, two and a half pages on the earthquake of August 10, meteors, and notes of State weather services for Alabama, Nebraska, Tennessee, Missouri, Louisiana, Ohio, and Georgia. The storm-tracks for the month are remarkably regular, and, with insignificant exceptions, all lie north of the Great Lakes and St. Lawrence: no tropical cyclones were felt along the sea-coast. Nine tornadoes are reported, and many violent thunderstorms. Some of the results of the special studies of the latter, undertaken by Mr. H. A. Hazen during the past season, take form in a brief summary, from which it appears that the mean distance and direction of the 900 thunderstorms reported in August, from the centre of the broad cyclonic storms in which they occurred, was 515 miles, a little west of south. A full account of these studies will be of much value and interest. Most of the observations on meteors are of small value, and, at best, they have but an etymological connection with a weather-review.

THE completion of the Lick Observatory is stated by *Science* now to depend upon the successful making of the disk of glass for the objective of the large telescope. The main dome cannot be made till the focal length of the large equatorial has been determined.

A MEMBER of the Institute of France has brought forward a scheme for the foundation of a number of annuities, of the value of 80*l*., 160*l*., and 240*l*., to aid scientific men in the prosecution of experimental work, offering to subscribe 200*l*. towards the realisation of the scheme out of his own pocket. If the Government, who will soon have to decide on the application of Giffard's legacy of over 200,000*l*., thought fit to patronise this scheme, they have the means of giving it practical embodiment on an extensive scale.

IN commemoration of the services to astronomy rendered by the French observers of the transit of Venus in 1874, the French Government have placed in the National Library of Paris a large monumental vase, designed and manufactured at Sèvres, bearing the following inscription: "La République Française à MM. Janssen, Bouquet de la Grye, André, Fleuriel, Hérault, Mouchez: Passage de Venus sur le Soleil en MDCCCLXXIV. Hommage du Gouvernement Français au Science." This vase, about 2 m. high by 1 m. broad, at present standing at the entrance of the Reading Room of the Library, will remain there for public inspection for some time, after which it will be removed to the Gallerie Mazarine, which contains a collection of rare manuscripts and other treasures.

A NUMBER of scientific men in Paris having founded a club called "La Science," for the purpose of dining together at stated times, recently entertained M. Chevreul at a banquet. The toast of the occasion was proposed by M. Jamin, the new Perpetual Secretary of the Academy of Sciences. M. Pasteur has been nominated Chairman of the next banquet. A similar club was instituted six years ago under the name of "Banquet de la Presse scientifique."

IN his discourse on re-election to the Presidency of the Zoological Society of Paris, M. Paul Bert stated that he had intended endeavouring to summarise the work of the Society during the preceding five years that he had held the office. But he found the task so difficult on account of the mass of facts presented by the publications of the Society, and the brevity of the papers, that he decided to abandon the idea. He promised, however, in future, at the commencement of the annual sessions, to sum up rapidly the progress realised during the preceding one. The scientific world will doubtless look forward with interest to the annual statements of the advance of biological research thus promised.

THE Museum of the International Association at Brussels has just received a large collection of birds of all kinds, sent from Karema by Lieut. Storms; and also a collection made by Mr. Stanley during his last visit to the Upper Congo, consisting of utensils, furniture, musical instruments, arms, &c.

THE laying of the foundation-stone of the new Sorbonne buildings will take place in a few days, the houses which covered the site intended for the new edifice having been all pulled down and the ground around the old Sorbonne to the extent of several acres having been levelled. The new buildings are to be pushed on rapidly, and the plans connected with the undertaking contemplate giving quite a new aspect to this part of the Quartier Latin. The enlargement of the Sorbonne was projected by Napoleon III. some years before 1870, and he had so far made a beginning with the work as to pull down several houses, and with all due state lay the first stone towards the additional structures in contemplation. The "first of the first stones" so laid down has been removed, though there is a rumour current that after search this first stone has not been found, and people are at a loss to know what has become of it. At all events the laying of the second of the first stones of the new Sorbonne will shortly be celebrated with becoming ceremony.

THE International Society of Electricians has decided to hold an exhibition in January next, on the occasion of the first general meeting. The exhibition, which will last several days, is to be held in the rooms of the Observatory of Paris, which have been lent for the purpose by the director.

ACCORDING to the *Oxford Magazine* there have been several interesting additions lately to the collection of casts in the University Museum. By the side of the skull of a Dinotherium now stands the skull of a Mastodon. Casts of the complete skeleton of Halitherium, the curious Miocene Sirenian which possesses distinct though small hind limbs, and of the hind and fore feet of the gigantic *Iguanodon Bernissartensis*, the original of which is one of the chief features of the Natural History Museum at Brussels, have also been added.

AN exhibition of the arts, industries, and natural productions of the Malay Archipelago was opened under the patronage of the Government of the Dutch East Indies at Batavia last month. The productions of Penang, Singapore, North Borneo, and Sarawak are largely represented.

PROF. MELL, director of the Alabama Weather Service, announces that through the liberality of the chief signal officer and of several railways; daily weather signals predicting changes of weather and temperature, will in future be displayed at upwards of 100 telegraph stations in the State of Alabama. The predictions will be received by the director at an early hour every morning from the Signal Office at Washington, and then promptly distributed along the railways. On paying about six dollars, the cost of the signal flags, any town or telegraph station will receive free telegraphic warning of the daily weather changes. Only about five minutes are required to set the flags. A similar

...eather signals has been for some time in operation ...portion of Pennsylvania.

...lar Report from China, published as a Parlia... ...k (China 6A Trade Reports), contains the ... annual report of the English Consul at ...eal with the animal, fossil, mineral, and ... of the Ichang district. A considerable part ...s to be employed only for medicinal purposes. ... Mr. Gardner's diary of his travels through the ...vince ...metimes extremely interesting; his account of a ...t to the fossil quarries is especially so. Three kinds of ...sils found in the district are staples of trade, the pagoda stones (*Orthoceras*), kosmos stones (*Ammonites*), and the "stone swallows." The first is found in the slate, and is cut, and either framed as a picture, or made into ornamental furniture. The *Ammonites* receive the name of "kosmos stones" from their resemblance when polished to the Chinese symbol for kosmos. The so-called stone swallows are ground down, and, like much else in that region, used as medicine. These are fossil bivalves, and the name is given to them because the natives believe that they fly about underground in the same way that the swallow flies in the air. The fossil cutters appear to be a separate guild, and mostly converts to Christianity. The tools are merely a saw and a chisel. They prod about the slate until they find an *Orthoceras*, which they think will be perfect; they then cut out a slab, which they saw into two or three thin planks, so that the fossil looks like a white picture of a pagoda on a black ground. These various fossils are close together in a region at least thirty miles long, and Mr. Gardner thinks that there is hardly a cubic foot of the limey slate which does not contain a fossil or the fragment of one.

THE most recent link in the long chain of telegraph lines which is spreading with such rapidity over China is the land line from Shanghai to Canton. A line from Pekin to Tientsin was opened a few months ago, and the capital of China was connected directly with London. Now the capital of Southern China is joined with the metropolis in the north; and as Canton was put in communication by telegraph with the frontier of Tonquin at the outbreak of the present political troubles in the latter district, the telegraph now stretches in an unbroken line from Pekin in the north to the most southern boundary of the Chinese Empire, and a message either from London or Pekin might reach the head-quarters of the Chinese forces on the Tonquin frontier in a few hours. Four years ago the only telegraph line in China was one about six miles in length, stretching from Shanghai to the sea, and erected to inform the mercantile community of the arrival of vessels off the mouth of the river. The next important line constructed by the Chinese Government will probably be one uniting Pekin with the great northern lines across Siberia at Kiachta. This will have to cross the whole of Mongolia, and will give the capital of China a third alternative telegraph route to Europe, a matter to which some political importance is believed to be attached in China. As already pointed out in NATURE, this extraordinary development is due solely to political considerations.

A COMMISSION appointed by the French Government to consider the best method of developing the mineral wealth of Annam and Tonquin has just issued its report. It lays down a programme for a mining mission, which it has been decided to send out there, and suggests the appointment of two separate missions. The duty of the first of these would be to ascertain whether the metalliferous deposits stated by Annamite documents to exist in two north-western provinces of Tonquin do actually exist there, and how far it would be possible to work them profitably. The second should investigate the copper deposits of the delta, and subsequently extend its labours into Annam. A draft mining law for these regions has also been proposed. Its special provisions are those relating to the mutual rights of the owners of the soil and those who have been granted concessions to work the mines; to administrative intervention (which it is recommended should be as rare as possible) with private mine owners. The broad policy laid down by the Commission is very liberal, not only to the natives, whose rights or alleged rights are to be scrupulously respected, but also to other nations, whose subjects are, for mining purposes, to be placed on the same footing as Frenchmen. Work, it is said, can take place at once on the coal-measures known to exist on the coast of Tonquin, as well as in the adjacent islands.

THE several correspondents of *La Lumière Electrique*, who have been sent to report on the progress of electricity in America, have returned to Paris, and are preparing their reports, which will be published next year.

THE site for the Centennial French Exhibition has been selected. It is to be held on the Champ de Mars, which belongs to the War Office, but will be given up to the city of Paris. A part of the Champ de Mars will be sold for building purposes. The Central Palace of the Exhibition will be made permanent and used for yearly exhibitions like those held in London at South Kensington.

PROF. T. C. MENDENHALL, *Science* states, has been appointed Chief Electrician of the U.S. Signal Bureau.

THE additions to the Zoological Society's Gardens during the past week include a Silvery Gibbon (*Hylobates leuciscus* ♀) from Java, presented by Mr. C. H. A. Hervey; a Bonnet Monkey (*Macacus sinicus*) from India, presented by Mrs. J. N. L. Boljahn; a Common Roe (*Capreolus capræa*), British, presented by Mr. C. Hambro; a Common Rhea (*Rhea americana* ♂) from South America, presented by Lady Brassey, F.Z.S.; a Tawny Owl (*Syrnium aluco*), European, presented by Mr. W. P. Clark; a Greater Sulphur-crested Cockatoo (*Cacatua galerita*) from Australia, presented by Mr. R. O. S. Ogilby; a Greater Black-backed Gull (*Larus marinus*), British, presented by Mr. T. E. Gunn; a Herring Gull (*Larus argentatus*), a Common Gull (*Larus canus*), three Greater Black-backed Gulls (*Larus marinus*), three Black-headed Gulls (*Larus ridibundus*), British, presented by Mr. W. H. Fielden, C.M.Z.S.; a Vervet Monkey (*Cercopithecus lalandii*) from South Africa, a Brush-tailed Kangaroo (*Petrogale penicillata* ♀) from New South Wales, a Hairy-fronted Muntjac (*Cervulus crinifrons* ♂), two Michie's Tufted Deer (*Elaphodus michianus* ♂ ♀) from China, a Tawny Owl (*Syrnium aluco*), European, a Hobby (*Falco subbuteo*), British, deposited; two Common Guillemots (*Lomvia troile*), a Razor-bill (*Alca torda*), British, purchased.

OUR ASTRONOMICAL COLUMN

OCCULTATIONS OF ALDEBARAN.—The next series of occultations of Aldebaran visible at Greenwich commences on February 22, 1885, and terminates on October 6, 1887. The dates and mean times of immersion and emersion are as follow:—

			Immersion h. m.		Emersion h. m.
1885	February	22 ...	5 17	...	5 50
	March	21 ...	11 43	...	below the horizon
	November	22 ...	9 48	...	10 57
1886	January	16 ...	7 48	...	8 49
	April	8 ...	5 8	...	5 54
	November	12 ...	18 27	...	19 16
1887	January	6 ...	12 17	...	13 15
	March	2 ...	5 47	...	6 4
	October	6 ...	15 20	...	16 2

There are therefore eight occultations in this series in which both immersion and emersion are visible, and one in which only the immersion occurs while the star is above the horizon at Greenwich. In the last series, which commenced September 28, 1866, and ended on August 2, 1869, ten occultations were wholly visible.

Occultations of Aldebaran are on record as far back as the year A.D. 491; it is stated in the Chinese Annals that the star was occulted at Nankin on March 29. Apparently the first occultation observed in Europe was found by Bullialdus in a Greek manuscript, which thus describes it:—"Anno 225 Diocletiani, Phamenoth 15 in 16, vidi Lunam sequentem claram Hyadum post accensas lucernas, digiti unius ad summam semisse. Videbatur autem occultasse ipsam. Stella quippe apposita erat parti, per quam bisecatur limbus Lunæ illuminatus." Bullialdus makes the date A.D. 509, March 11, and an approximate calculation shows that he is correct. New moon fell about 7h. G.M.T. on March 6.

ENCKE'S COMET.—This comet at its present return will be observable in these latitudes in the early evening hours before perihelion. The following ephemeris is for 6h. G.M.T.:—

1885	R.A. h. m. s.	Decl. ° ′	Log. distance from Earth	Log. distance from Sun
Jan. 1	22 55 25	+3 57·8	0·1526	0·1309
2	22 56 18	4 0·5		
3	22 57 12	4 3·4		
4	22 58 7	4 6·5		
5	22 59 3	4 9·8	0·1500	0·1120
6	23 0 1	4 13·3		
7	23 1 0	4 16·9		
8	23 2 0	4 20·7		
9	23 3 2	4 24·7	0·1462	0·0917
10	23 4 5	4 28·9		
11	23 5 10	4 33·2		
12	23 6 17	4 37·7		
13	23 7 25	4 42·4	0·1410	0·0699
14	23 8 35	4 47·2		
15	23 9 46	4 52·2		
16	23 10 58	4 57·3		
17	23 12 12	5 2·6	0·1344	0·0463
18	23 13 27	5 8·0		
19	23 14 44	5 13·5		
20	23 16 2	5 19·1		
21	23 17 20	5 24·8	0·1261	0·0206
22	23 18 40	5 30·7		
23	23 20 1	5 36·6		
24	23 21 24	5 42·6		
25	23 22 47	5 48·7	0·1158	9·9926
26	23 24 11	5 54·8		
27	23 25 37	6 1·0		
28	23 27 4	6 7·2		
29	23 28 31	6 13·5	0·1033	9·9619
30	23 30 0	6 19·7		
31	23 31 29	+6 25·0		

The intensity of light expressed in the usual manner is 0·27 on January 1, and 0·51 on the last date of the ephemeris.

GEOGRAPHICAL NOTES

AN interesting project was laid before the Associated Swiss Societies of Geography at their meeting at Berne last month, by M. Müllhaupt. He suggested the formation of an international geographical bureau for the following purposes:—(1) To carry out the resolutions arrived at by the International Geographical Congresses. (2) To make exchanges every month, or oftener if need be, between the eighty odd geographical societies; in place of each society sending its own publications in eighty different directions, it would only have to send them all at once to the bureau, which would do so. This, he claims, would save both time and money. (3) To publish, in the four or five principal languages, a summary of the contents of the publications of the various geographical societies; instead of each society being forced to do this for itself, a single examination would suffice to put them all *au courant* with what has been done all over the globe. There would in this way be the further advantage of knowing what was published by societies like the Geographical Society of Japan, the publications of which are in a language not generally known in Europe. M. Müllhaupt thought that the idea was not a difficult one to be carried out; the expenses would be shared by the numerous societies interested. These contain approximately 38,000 active members, and doubtless the countries having an interest in the progress of the geographical sciences would take part in a central organisation of the nature here suggested.

THE last number (Band xi. No. 8) of the *Verhandlungen der Gesellschaft für Erdkunde zu Berlin* contains two papers on West Africa: one accompanied by an excellent map of the Flegel, of his recent journey along the Niger to [illegible]y of the con-other, by Herr Reichenow, on the Cameroons, [illegible] had interests colony there. Dr. Lopez writes on the Anger[illegible]ety during the the importance of the German element in the [illegible]ut he found the there. [illegible]esented by the

THE investigation of the subterranean co[illegible] River has been actively pursued for some time [illegible]e papers, the Section of the German and Au trian Alpine Soc[illegible], however, i is that mysterious river which, coming from t[illegible]ons, to sum u Carniola, loses itself in the caves of the Karst, [illegible]ng one. [illegible] terranean course of more than thirty kilometres, [illegible]s out the ground near San Giovanni di Duino, is then called t Timavo, and eventually flows into the Bay of Monfalcone. Already, on March 30 last, a part of this subterranean course was investigated by a party starting from the village of St. Canzian, where a celebrated cave is situated, into which the Reka falls with thundering noise when the water is high. In September a second exploration was made. The first subterranean cave is called the Rudolfshöhle; it was from here the explorers started in two boats. First, they passed a canal about sixty metres in length, very narrow, and bounded by rocky walls one hundred metres in height; then a large cave was reached, where the party landed and fastened the boats, as waterfalls and rapids prevented further progress in boats. The underground journey was now continued on the rocky banks, the river being crossed several times on ladders. Thus six waterfalls were passed, and a seventh was reached. Altogether the explorers penetrated to a distance of between two and three hundred metres underground.

BULLETIN No. 5 of the U.S. Geological Survey is, *Science* remarks, a dictionary of altitudes in the United States, compiled by Henry Gannett, chief geographer of the Survey. It is essentially an extension of the "Lists of Elevations," prepared by the same author for Hayden's Survey; but, with the present broader organisation of the Geological Survey, the lists now appropriately include the whole country, while the earlier editions were concerned chiefly with the region west of the Mississippi. A list of authorities fills eight pages, and railroad abbreviations occupy eight more; then the States and stations follow alphabetically, the number of altitudes given being about 18,000. It is stated that the collection of railroad profiles for Pennsylvania is exceptionally complete and admirably adjusted, making the portion of the dictionary referring to that State by far the fullest and most satisfactory. By an unfortunate oversight, it is not stated whether the base level is high, mean, or low tide.

AT the recent meeting of the Ethnological Section of the Imperial Russian Geographical Society a paper was read describing Adrianow's journey through the Altai Mountains in 1881. The traveller was only able to take four companions, on account of the meagre funds at his disposal; nevertheless he was able to obtain excellent results, and to penetrate hitherto unknown regions. Although the southern slopes of the Altai Range have already been the object of investigation of various students, such as Pallas, Ledebur, Humboldt, and others, the eastern part of the region, the vast districts between the River Tom and the Government of Yenisei, have been almost a *terra incognita*. Adrianow's expedition started from the town of Kustnetsk, crossed the River Lebed, examined Lake Teletsk, touched Chulshman, Jan, and Agalan, crossed the Shapshal Pass, advanced to the River Kemchik, and sought for and found the sources of the Yenisei. They travelled through the region through which the river flows to the town of Yeniseisk, where the expedition came to an end. Throughout the journey Russians were found only around the sources of the Yenisei and on the River Usg. The population of the Altais is composed of sectaries who emigrated thither during the last century; their existence was wholly unknown until 1868, when they were by chance discovered by a Russian officer who was surveying there. Adrianow met similar colonies at Tobut on Koko-nor. These were founded in 1800. The colonists are described as savage and predatory. Besides these the traveller visited the so-called Black Tartars, on the rivers Koudoma and Luida—a tribe which has only once before been visited and described. They are regarded as descendants of the great Finnish and Turanian tribes, but hardly anything in an anthropological sense is known about them. The travellers also brought back a considerable number of pictures of monuments and works in stone, which exist among the Sajans and in Mongolia. Those of monuments to the dead are very interesting; some of them are merely