THURSDAY, JULY 7, 1887.

BOOK NO. 1060

PROFESSOR TYNDALL AND THE SCIENTIFIC MOVEMENT.

THE complimentary banquet to Prof. Tyndall, to which reference has more than once been made in these columns, is described in detail elsewhere. We must not, however, allow an event of so much interest, and which is, we believe, unique in the history of science in this country, to pass without comment.

Many notable gatherings have taken place in Willis's Rooms, but we question if English science has ever been more completely represented than at the "Tyndall Dinner." The President of the Royal Society was in the chair. The seven Vice-Chairmen were Presidents of the most important scientific Societies. The tables were crowded with men whose names are known wherever nature is studied.

No every-day motive would suffice to bring together such an assembly, and it is not every day that we have an opportunity of doing honour to a life-work such as that of Prof. Tyndall. Others will rank beside or above him as investigators, but in the promotion of the great scientific movement of the last fifty years he has played a part second to none. The English people are a determined but somewhat slow-witted race, and it has been no easy task to convince them that a new era—that of science—was dawning. They have been content to pride themselves on industrial successes due for the most part to isolated efforts of genius, which was hampered by unnecessary difficulties, and which cannot be produced at will. They were long in seeing, they do not yet fully see, that our industrial position can only be maintained if armies of well-equipped followers are ready to seize the ground which the leaders win.

There is, however, a still harder lesson to learn. The industrial application of a scientific principle—vitally important to the well-being of the people as that application may be—requires nevertheless a lower form of intellectual energy than the discovery of the principle itself. The triumphs of applied science, of the physician, the engineer, the telegraphist, are readily "understanded of the people." The research laboratory, on the other hand, is open to few. The flash of genius which has wrung a fresh secret from Nature can only be fully appreciated by those who are intellectually competent to understand the difficulty and the success. And yet, if a widespread knowledge of science was to be, as it is, an essential condition of national well-being, it was absolutely necessary that the people should know something of, and be in some sort in sympathy with, the methods and conditions of scientific thought.

In supplying this need, Prof. Tyndall's greatest work has been done. Uniting scientific eminence of no ordinary kind with extraordinary gifts of exposition, he has, by his lectures and his books, brought the democracy into touch with scientific research. In the lens of lecture-rooms experiments devised by him is proving that a living science is a nobler instrument of education than a dead language. In hundreds of libraries his nervous English is convincing men of the value of a career like Faraday's, and teaching them to appreciate, if they cannot always in detail follow, the methods by which they were won. He has done for many a man to compel those who only in so far as it is immediate the seed which is sown in the most abundant harvest desire for knowledge is the necessity could never have brought to the birth.

Such has been Prof. Tyndall's work; and yet we venture to think that among those who met in Willis's Rooms a deeper feeling was aroused than admiration for an eminent worker and a useful career.

Many of the greatest masters both of the moral and intellectual life have sought the attainment of their highest ideals in a more or less complete withdrawal from society, and it may well be that some natures can best achieve in seclusion the concentration which a supreme effort demands.

But although the scientific movement of to-day may receive its highest inspirations from men who, like Darwin and Joule, have worked in self-imposed retirement, its distinguishing characteristic is that it is sweeping along with it all classes and all opinions. It is a new habit of thought in the light of which the foundations of our educational, industrial, and political systems are being reconsidered. It is a new and deliberate attempt to put into practice the belief that "the sovereignty of man lieth hid in knowledge, wherein many things are reserved which kings with their treasures cannot buy, nor with their force command; their spials and intelligencers can give no news of them; their seamen and discoverers cannot sail where they grow."

Thus it has come to pass that science has gathered round it a crowd of workers, engaged in very various tasks, but all of whom would be ready to admit that the cardinal principle of the movement in which they take part is the investigation of truth for truth's sake alone. They may be professors or manufacturers, soldiers or physicians. If only they are imbued with the desire to penetrate a little further into the mysteries which surround us, if only they are willing and able to add something to the sum of human knowledge, they are scientific men.

In part this army is organized. There is in England no Academy of Literature. The Academy of Arts, it is admitted, needs reform. The principal scientific Societies, however, with the Royal Society at their head, perform the duties of an Academy of Science to the general satisfaction. No human institution is perfect, but it may be fairly said that they set in their Transactions a high standard of scientific work, and that their judgment, whether of men or of investigations, is seldom challenged.

In spite of this advantage, neither the outside world nor scientific men themselves have as yet sufficiently realized that these Societies constitute a great guild of that learning which is the most powerful and the most characteristic influence of our age.

On an occasion such as the Tyndall Dinner this realization is quickened. The curious magnetic influence of numbers is felt. Minor differences disappear in the

knowledge that all are workers in the same cause. Men become more vividly conscious that though students of Nature are excluded from the State recognition which is extended to the Church, to medicine, and to the law, they too are members of a great profession. They realize that, though State rewards are given only to those who have applied their knowledge to some directly useful end, in a gathering of the profession of science the true leaders are those who have wrested the deepest secrets from Nature, careless whether they could be turned to gold or no.

A meeting held in great numbers and for a common purpose may have an influence which many an apparently more useful testimonial would lack. Prof. Tyndall has done service in the cause of science which merited the unique compliment he received. He would, we believe, be the first to rejoice if in the future the Tyndall Dinner was remembered not only as a tribute to his own work, but as marking the beginning of a period in which the ranks of science were drawn closer together, and in which the further organization of the investigation of Nature claimed and received the attention which its importance demands.

THE GEOLOGY OF ENGLAND AND WALES.

The Geology of England and Wales. With Notes on the Physical Features of the Country. By Horace B. Woodward, F.G.S. Second Edition. (London: Philip and Son, 1887.)

THE student of physical geology has at least two large English text-books, interesting, full, accurate, judicial, and written by masters of the science; but he who would build on this foundation a knowledge of historical and palæontological geology is in a harder case, and finds either a meagre outline containing little but a few meaningless names of formations and fossil lists, or else an ill-digested and formless mass of matter, derived from everywhere, but leading nowhere. Perhaps the time has not yet come when stratigraphy can be treated from the stand-point of inorganic evolution, so that fact may be joined to his fact and an organized whole result.

While, however, we wait for one who shall give us geology in the form of the inorganic and organic evolution of the globe, we must not omit to notice the labour of those whose "work is to record the facts from which the pleasanter deductions may be made." Mr. Woodward has done wisely in republishing by subscription and in an enlarged form his admirable book on the geology of England and Wales—a veritable mine of facts, well indexed and admirably supplied with references for the advanced reader, forming a base-line for further study and research, but complete in itself for the more elementary student and rendered interesting by the author's fresh style, by his capital and apt illustrations, and by his wonderful faculty of seizing upon the individuality of the rock group he is describing and skilfully tracing its variations from place to place. This new edition is improved by a larger and better map, undertaken by Mr. Goodchild, by more free use of sections, illustrations, and fossil lists, and by the employment of local names with tables of correlation.

The author works his way upwards from the lowest rocks, but combines a geographical with a chronological arrangement, and varies his method from system to system in order to adapt it better to the rocks under consideration. Just occasionally one meets with a slip in method, as in the case of the Rhætic rocks, where for no apparent reason he has reversed his usual order and treated the White Lias first. Where the mass of facts is unusually great and somewhat barren of interest, the author has introduced little helps and alleviations for which the student will be truly grateful,—the character of the hero of a system sketched in one graphic touch, the origin of the name of a system or a fossil, or the discussion of the origin of some bed of palæontological or economic value (*vide* pp. 24, 47, 84, 266, 670).

It seems hard to criticise any points of detail in such well-intentioned and well-executed work, but the indication of a few lines for improvement will perhaps show better than anything else how little the author has left for others to suggest. First, with regard to the map. This is clearly engraved, and coloured with light but well contrasted tints; every name on it suggests some fact interesting from a geologist's point of view, and the effect of the whole is pleasing. There is no special colour for the Permian (not an unmixed advantage), and, oddly enough, the Yorkshire coal-field is left uncoloured; the boundary is engraved, however, and the student can easily fill in the colour for himself. A point has been gained in using a distinctive colour for beds below the Bala, but one lost in not using still another for the lowest Cambrians. The igneous rock colours should have been used less sparingly, and surely the Arenigs and Snowdon deserve a volcanic tint as much as the Borrowdales and the Cheviot rocks. We miss, too, the north of England dikes and the Whinsill.

The book opens with an introduction containing a little history, a little cosmogony, and a few definitions. The latter are hardly needed, and might have made room for the accounts of the geology of different lines of rail-way, which found a place in the first edition but have been crowded out of this. A few words on the Palæozoic group are followed by an account of the Archæan system, in which too little is said of the new class of work amongst these rocks instituted by Prof. Lapworth, while Prof. Bonney's papers on the Bangor area are almost passed over. The table on page 52 hardly makes it quite clear that the Harlech group of St. David's is divided into the Caerfai and Solva groups, of which the former constitutes the *Annelidian* of Lapworth, and the latter, together with the Menevian beds, the *Paradoxidian*. On page 58 we find the time-worn section across that part of the Longmynd which teaches nothing of the succession of the Longmynd rocks; this and several other sections should have been orientated. On page 60 the Hollybush sandstone is omitted from the table of Shropshire Cambrians, and awkwardly placed on page 65, while the Shineton shales are correlated with the Dolgelly beds and Malvern black shales in the table, though afterwards correctly placed with the Dictyonema shales and Lower Tremadoc. A deceptive appearance of unconformity in sections on page 89 might easily have been removed, even if present in the original woodcut. It is good to see Mr. Lewis's name brought up with

Murchison's on page 103, in connexion with the Aymestry limestone.

The author mentions, but does not definitely accept, Prof. Hull's correlation of the Devonian rocks. Throughout the work, and particularly in the Carboniferous section, great care has been taken to show and where possible give the origin of the economic value of the rocks. A little more stress should have been laid on the relations of the Coal-measures to the underlying rocks, and one might notice here the absence of the Dudley, Sedgeley, and other inliers from the South Staffordshire coal-field on the map. An important feature consists in the description of Palæozoic rocks from all the deep borings (a list of these forms the first appendix); and a good section to express the present state of knowledge on the deep-seated geology of the London Basin is given on page 202.

The Permian and Lias form a single system, the Poikilitic, which is included in the Mesozoic, the author being guided by the widespread discordance between it and the older rocks. It is not quite easy to understand all the tables (pp. 286, 470), but these only echo the difficulties which exist in the rocks themselves. It would have been as well if the Yorkshire Cornbrash had found a place after the Upper Estuarine on page 321. A good opportunity was missed of discussing the anomalous beds of Faringdon and Blackdown, particularly in relation to Mr. Starkie Gardner's recent papers on kindred questions; and we should have liked to see the grit phases in the Jurassic clays of the eastern counties more accurately defined. A section might have been introduced to show the thinning of the Gault and growth of the Cambridge nodule beds; and Mr. Sollas's work on flints ought not to have been omitted.

The Upper Eocene beds are classed as Oligocene, but the Brockenhurst bed is put in its true place in the Headon.

There are some very suggestive remarks on the connexion between health and geology, between villages and springs and consequently the outcrop of porous rocks, and on the effects of percolation of spring and sea water through rocks. The section on igneous rocks is of necessity somewhat vague and unsystematic from its brevity, but room has been found to treat the volcanic rocks historically; the Nuneaton diorites are intrusive in pre-Carboniferous rocks only. There are concluding chapters on metalliferous deposits, and on scenery and geology, the latter containing a useful list of hills, valleys, plains, and forests.

A little more space might with advantage have been pent in indicating with greater fulness what is known of the physical geography of the different periods, and epochs of earth movements, their dates, directions, and effects should have been more fully dealt with in the last chapter. A capital synopsis of the animal kingdom is furnished in an appendix by Mr. Edwin T. Newton; and grand index, occupying 45 pages of three columns each, d giving the dates of the birth and death of authors erred to, closes the volume, which is an excellent mary of the present state of our knowledge of British logy. The author has worked conscientiously and , and that we have been able to suggest so few addi- s clearly shows that his labour has not been in vain.

W. W. W.

A TREATISE ON GEOMETRICAL OPTICS.

A Treatise on Geometrical Optics. By R. S. Heath, M.A., D.Sc., Fellow of Trinity College, Cambridge, Professor of Mathematics in the Mason College, Birmingham. Demy 8vo, pp. xvii. 356. (Cambridge: University Press, 1887.)

THIS treatise is based on the conception of a beam of light as consisting of a system of rays, which obey the laws of reflexion and refraction. The transformations of such a system and the construction and properties of optical instruments are deduced, so far as the latter are capable of explanation from this point of view.

In confining himself to geometrical optics in this sense, the author follows the mode of division of the science which has been usually adopted in text-books in this country, through the succession of Cambridge treatises by Coddington, Griffin, and Parkinson, and Lloyd's "Treatise on Light and Vision." The subject then splits up naturally into the theory of reflexion and refraction of systems of rays, which is in fact a department of geometry; and the more special discussion of the nature of optical instruments and the forms and positions to be given to their refracting surfaces to diminish spherical and chromatic aberration, which allies itself with the technical science of optical construction.

The book begins with a short chapter on the nature and properties of light, in which the theory of illumination is worked out as a consequence of the experimental fact that self-luminous surfaces appear equally bright in all directions and at all distances. The second and third chapters contain the statement, in geometrical and analytical form, of the laws of reflexion and refraction, and the investigation of conjugate foci for direct pencils.

In Chapter IV. the subject of refraction through lenses and systems of lenses is treated, use being made of the symmetrical analysis, by means of the convergents of continued fractions, to determine the principal points of a system whose refracting surfaces are specified. Free use is also made of the cardinal points of the system in the semi-geometrical manner introduced by Möbius. The following chapter is devoted to an account of the general analytical investigation by means of which Gauss placed the whole theory on an independent basis. The notion of the equivalent lens is here introduced to some practical purpose, for the investigations of this and the preceding chapter enable the author to specify the exact character of the equivalence that can be secured by a single lens or a single refracting surface: viz. that if the lens or surface occupied the position of one of the principal planes of the system, it would refract any beam incident along its axis into the same configuration as it actually possesses when it emerges through the other principal plane of the instrument; so that, neglecting aberrations, the equivalence holds in every sense except as regards the displacement along the axis, and is therefore complete for most practical purposes.

The theory of caustics is treated, chiefly by analytical methods; and the existence of wave-surfaces, which cut the system of rays at right angles in an isotropic medium, is established geometrically.

Chapter VII. is devoted to the rescussion of the spherical aberration of direct pencils, which is perhaps

one of the most difficult parts of the subject to present in an elegant manner, on account of the non-symmetrical character of the necessary approximations. The treatment here given seems to leave nothing to be desired.

Chapter VIII. begins with an exposition of the properties of a general system of rays: this with the cardinal result that the rays are all bi-tangents to a focal surface is ascribed to Kummer. They had, however, been previously given by Hamilton in his memoir on "Systems of Rays," in the discussion of ray-systems in a crystalline medium where the wave-surface no longer cuts the rays at right angles; and he in turn refers back to the same papers of Malus which contain the theorem of orthogonality in isotropic media.

The theory of the characteristic function is next applied to the solution of the general problem of the refraction of a narrow beam at a surface of double curvature; and to the analytical determination of the relation between the forms of such beams before and after passing through a general optical instrument whose internal structure is not specified. In these discussions the author has closely followed a series of papers by Clerk Maxwell which appeared about fifteen years ago in the Proceedings of the London Mathematical Society, and which presumably were to find a place in a book on optics then contemplated by their lamented author. It does not seem to have been much noticed in this country that the same formulæ for oblique refraction were developed a long time ago by Sturm and others, in a direct geometrical manner, from Malus's theorem; but the conciseness and precision which arise from defining a beam by means of its characteristic function give them an enhanced importance in optical theory. Their application is here given to some cases which we do not remember having seen published before: thus the modification impressed on a beam by refraction centrically through a single thin lens is expressed by means of very simple formulæ, from which several properties of considerable elegance and some practical value might be directly drawn.

The theory of dispersion and achromatism is treated in the ordinary way. In the chapter on vision are introduced discussions, chiefly from Helmholtz, of the mechanism of accommodation and the principles of binocular vision. Then follows a clear and valuable chapter on telescopes and microscopes, a chapter on miscellaneous optical instruments, and a brief account of atmospheric refraction, mirage, rainbows, and halos.

It may seem ungracious to expect more where so much is given, but we could have wished that the theory of refraction through general systems had been treated more from an historical standpoint. A difficulty often felt in this part of the subject arises from the way in which the geometrical and analytical methods of different writers are liable to be intermixed. The book was probably in the press before a recent note by Lord Rayleigh had brought again into prominence the large share taken by the English opticians of last century, notably Cotes and Smith, in the development of the general theory of this branch of the subject.

The list of treatises and memoirs might be improved by consulting the bibliographies given by Helmholtz and Verdet.

It is a misfortune incident on the scheme of the book that it is seldom able to say the last word in relation to the more delicate arrangements of telescopes and microscopes, where diffraction plays an important part. This becomes very patent, for example, in the account of immersion objectives. The theory of diffraction as applied to optical construction is for the most part purely geometrical, and it would much increase the value and interest of books on geometrical optics if that theory were explicitly included, and the subject introduced by the consideration of light as wave-motion, instead of the artificial conception of the reflexion and refraction of rays.

As is usual in English text-books, selections of problems have been added at the ends of the chapters. In this case, Cambridge examination-papers of recent years have been largely drawn upon for questions, with the result that some are included which are not of much value as illustrations of the subject, though they may be very useful as tests of mathematical power. Indeed it seems open to question whether the practice of adding large collections of examples is not now overdone in this country; it certainly in some cases tends to unfit the books which contain them for the use of students who do not possess the advantage of tuition, or some guidance in selecting the few that will be of value for them.

The treatise is, on the whole, a most welcome addition to our optical text-books. Much of its contents, though fundamental and elementary, has only hitherto been accessible in English through Mr. Pendlebury's treatise on "Systems of Lenses"; and there is more that now appears in a text-book for the first time. The printing and general appearance of the book reflect great credit on all concerned with it. J. LARMOR.

OUR BOOK SHELF.

Shores and Alps of Alaska. By H. W. Seton Karr, F.R.G.S. (London: Sampson Low, 1887).

THIS is a very interesting account of a journey of exploration in a country which, as the author says, is probably destined soon to become better known. The most important part of the book is that which relates to the attempt made by Mr. Seton Karr and his companions upon Mount St. Elias. When this attempt was made, the combined "alpinism" of the climbers was "insignificant." Nevertheless, they achieved considerable success, and the writer has been able to present a vivid and striking record of their observations. The height of Mount St. Elias was differently estimated by the old navigators, and Mr. Seton Karr points out that it is the only mountain the real height of which has exceeded the first estimates made of it. The latest determination taken from Yakatat and from the United States Coast Survey schooner *Yukon*, gives 19,500 and possibly 20,000 feet. From its massive shape the mountain does not convey the impression of being quite so high as this, although "its whole altitude is presented to the eye, from its sharp summit down to the ocean at its foot." Of the scenery of which Mount St. Elias is the most prominent feature, Mr. Seton Karr writes most enthusiastically. He even goes so far as to say that "without a doubt the scenery easily has Yakatat is the most wonderful of its kind in the whole world." Seen early in the morning, when the air is markably transparent, the mountains seem "too ethe to have any actual existence." The observer feels "they cannot be anything except some unholy illu that must dissolve and disperse when the sun rises."

Upper Cloud Movements in the Equatorial Regions of the Atlantic.

I AM sorry that the observations of so good an observer as Capt. D. W. Barker should not agree with my own, but I certainly never confounded what he calls high low-level clouds with the true high clouds.

When clouds are being propagated in a different direction from that in which they are being blown—as sometimes happens—it is exceedingly difficult to ascertain the real direction; but that would not account for the discrepancy between our observations.

My own researches were specially directed to the doldrums, and the history of the Krakatão dust entirely confirms my observations; but in some low latitudes—as in Cuba—the highest clouds are usually from about south-west. This, however, does not affect the doldrum districts. RALPH ABERCROMBY.

21 Chapel Street.

Fish Dying.

IN a large pool in this county, well stocked with fish, especially trout and roach, a considerable number of the roach have been found dead every day during the last week. They are in fair condition, and show no evidence of poison or of parasitic disease. There is a certain amount of current through the centre of the pool, but the ingress of water has been, of course, much reduced by the drought. The pool, however, covers many acres, and there are twenty feet of water in the deepest parts. Can any of your readers suggest a cause for the death of the roach, and a remedy? No other species appears to have suffered. F. T. MOTT.

Birstal Hill, Leicester, July 4.

THE DINNER TO PROFESSOR TYNDALL.

THE dinner to Prof. Tyndall, as we stated last week, was going on at Willis's Rooms on Wednesday evening as we went to press. It was attended by as large and distinguished a company as ever assembled to do honour to a man of science. The chair was taken by Prof. Stokes, President of the Royal Society, who had acted as Chairman of the Organizing Committee. Among those who had consented to serve on the Committee were the Marquis of Salisbury, the Duke of Devonshire, the Duke of Argyll, the Right Hon. J. Inglis, the Earl of Rosse, Earl Granville, Sir F. Abel, Prof. Adams, and many others holding high positions in connexion with scientific and learned Societies, and Mr. J. Norman Lockyer and Mr. A. W. Rücker had acted as honorary secretaries to the Committee. Among those who attended the dinner were the Earl of Derby, Earl Bathurst, the Earl of Lytton, Sir F. Leighton, Lord Rayleigh, Lord Thurlow, Sir J. Lubbock, M.P., Sir W. Bowman, Sir F. Bramwell, Sir I. Lowthian Bell, M.P., Sir J. Lister, Sir H. Roscoe, M.P., Sir G. Richards, Lord A. Russell, Sir F. Pollock, Sir Lyon Playfair, M.P., Sir Prescott Hewett, Prof. J. C. Adams, Colonel Donnelly, Sir J. Hooker, Prof. Asa Gray, Prof. Flower, Dr. A. Geikie, Dr. Hirst, Mr. W. Crookes (President of the Chemical Society), Mr. G. B. Bruce (President of the Institution of Civil Engineers), Mr. D. Adamson (President of the Iron and Steel Institute), Dr. J. Evans (President of the Society of Antiquaries), Prof. B. Stewart (President of the Physical Society), Prof. Judd (President of the Geological Society), General Strachey (President of the Royal Geographical Society), Sir J. Fayrer, Sir H. Wilde, Sir H. Doulton, Sir J. Caird, Sir P. Magnus, the President of the Alpine Club, Profs. Frankland, Debus, Tilden, Ray Lankester, Liversedge, G. Darwin, Dewar, M. Foster, Carey Foster, Odling, Gamgee, W. G. Adams, Clifton, Humphry, and Dallinger, Messrs. Warren de la Rue, Gill, Kempe, J. Hopkinson, H. Pollock, E. Wood, Brudenell Carter, Romanes, Pengelly, Preece, Ellis, Vernon Harcourt, R. H. Scott, and others.

At the close of the dinner Mr. Norman Lockyer, at the request of the Chairman, read a list of absentees, from most of whom had been received letters expressing strong sympathy with the object of the banquet, and admiration of the career of Prof. Tyndall. Among the writers were the Marquis of Salisbury, Mr. Goschen, Mr. W. H. Smith, Lord Cranbrook, the Marquis of Ripon, the Earl of Rosse, Lord Monk Bretton, Profs. Max Müller, J. R. Seeley, T. H. Huxley, Sir F. Abel, and about thirty others identified with science and literature.

The first toast was "The Queen," and

The Chairman in proposing it said that the recent celebration of the Jubilee diminished the necessity for saying many words in commendation of the toast. All hearts were affected by the Queen's letter, in which she so touchingly acknowledged the manner in which she had been received. Those who were present at the scene in the Abbey were touched by the exhibition of family devotion and affection which took place at the conclusion of the service, when the Royal Family saluted her who was at the same time Sovereign and mother, and received from her the kiss of affection. And as on that occasion the Royal Family was united with the Sovereign, so on the present occasion, in drinking the health of Her Majesty, they would mentally include the health of the Prince and Princess of Wales and the rest of the Royal Family.

The toast was drunk with all the honours.

The Chairman in proposing the toast of the evening said:—My Lords and Gentlemen,—I now come to the toast of the evening, "The Health of Dr. Tyndall," and may he long enjoy the leisure which he has so well earned. A social gathering like the present is not an occasion on which it is desirable to enter into detail as to the scientific labours of a man, however eminent. Yet the circumstances of the present meeting seem to demand that I should say a few words on some of Dr. Tyndall's researches. Some of his earliest scientific work related to diamagnetism and magnecrystallic action, and in part of this he was associated with the well known German physicist Knoblauch. But I cannot dwell on these now. And I will even dismiss with this brief mention his researches on the properties of ice and his application of them to the theory of glaciers and the observations which he made in common with his friend and colleague Prof. Huxley, whose necessary absence from among us to-night we so much regret. If I be not trespassing too much on the patience of those who listen to me, I would wish to say a little more on that elaborate series of researches, forming no less than six separate papers in the Philosophical Transactions, in which Dr. Tyndall investigated the relation of simple and compound gases and of vapours to radiant heat, especially radiant heat from sources at a moderate temperature. According to his researches, while the main constituents of the earth's atmosphere, nitrogen and oxygen, are practically diathermous, at least with regard to radiations which can traverse rock-salt, as we know that by far the greater part of those that we have to deal with can, such is far from being the case with other gases equally transparent with regard to light. Dr. Tyndall found that as a rule the more complex the composition of a gas the greater is its defect of diathermancy. To confine ourselves to the two gases which occur in the atmosphere mixed with its main constituents—I allude of course to carbonic acid and to water in the gaseous state of vapour—he found that both, especially the latter, which likewise is present in by far the larger quantity, are very distinctly defective in diathermancy, and he concluded that the main part of the absorption of solar heat in passing through the atmosphere, absorption as distinguished from scattering, is due to the watery vapour which it contains. From this result he drew important inferences as to atmospheric temperature and climatological conditions. Dr. Tyndall's researches on the relation of gases to radiant heat came naturally before me

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.*]

Relation of Coal-Dust to Explosions in Mines.

THE suggestion in my former letter on this subject (vol. xxxiv. p. 595) that "keeping the ventilating air-current saturated with aqueous vapour" might prove the most effective way of rendering the dust in coal-mines innocuous, has, I am glad to see, been since shown to be practicable, in a South Wales colliery. Since the above date, I have considerably extended my research, with results that confirm the conviction therein expressed that many of the most disastrous colliery explosions during the last seven years in this northern district have been practically *dust explosions*, and therefore preventable; that the rough method of watering the floors only, or floors and sides, of the mines is delusive, since it leaves the most dangerous dust undisturbed, the upper and flocculent dust; and last, that probably the reasons why dust in dry pits does not explode more frequently are now within grasp. To this latter conclusion, with your permission, I will now briefly address myself. That every firing of a shot that is accompanied by flame in a dry and dusty pit does not produce an explosion is well known; that *sometimes* such firing of a shot does is unhappily also well known. That the local presence of gas, even in small amount, is sometimes the reason of this is universally acknowledged. That the amount and condition of the dust present (even in the practical absence of gas) is at other times the reason is now believed by many. Setting aside the *amount* of dust, which every one will allow must be an essential factor, and also the varying energy which the shot, blown out or not, develops, let us look at the other conditions. The temperature and hygroscopic state of the air-current is one most important factor, and consequently the concomitant temperature and hygroscopic state of the dust traversed by such current. Beyond this, the *degree of fineness* and the *constituents* of the dust will have much to say in the matter. The finer the particles the more readily will they ignite, and the more completely will they place their substance under the influences present. Thus ordinary screen coal-dust will not ignite when a common match is lighted and applied to it, but it will when finely pounded in a mortar. Now the dust resting on the baulks and upper portions generally of the ways will invariably so light and burn when dry, although the constituents vary greatly in different pits and in different seams of the same pit.

What are the ordinary *constituents* of coal-dust? Two, perhaps three, important substances, and others unimportant: important, as being inflammable in varying degrees; unimportant, either from their uninflammability or from their excessively small amount. The three important are mother of coal, or *dant; coal;* and certain coloured bodies, probably *spores*. The unimportant are shale or other stone dust, iron pyrites, lime flakes, and incidentals, as animal and vegetable matters, and the results of the wear and tear of the haulage and winning apparatus, &c. Dismiss these last, as only one needs any attention, the shale; and that special, not general.

Dant lights most readily; the red end of a used match is often sufficient to fire it, and then it burns itself out whether resting on wood or stone. Burned in a retort, it loses little weight, and the fumes it gives off will not ignite. Now, this dant is largely present in upper and flocculent dust, reaching in some specimens even 70 or 80 per cent. Dant clearly therefore is not itself dangerously explosive, yet is admirably fitted to act the part that tinder used to do, when it handed on the spark from the flint and steel to the old-fashioned brimstone match.

Coal forms a considerable part of all upper and flocculent dust, and constitutes the great mass of the bottom dust along intake haulage roads. Coal-dust (got as free from dant as possible) when pounded very fine ignites with some difficulty, burns at first somewhat fiercely and with considerable smoke, but generally goes out leaving a portion of the heap unburned. Placed on an iron plate and burned by heating the plate, it threw off scintillations, its fumes readily took fire, and forty grains of dust were reduced to one grain of ash. In a retort it gave off first much smoke which would not light; soon, however, the smoke lessened, when its fumes lit and burned with a long bright flame. Such coal-dust is manifestly capable of producing an explosion. Under favourable conditions it can produce a considerable amount of ordinary illuminating coal-gas, whose presence would convert the air-current into an explosive mixture. Therefore, adopting the former simile, as the dant is the *tinder*, so this coal is the *sulphur match*, as the shot flame or other initial cause is the *spark* struck from the flint and steel.

Spores.—Nearly all dusts (and I have examined many) have shown under the microscope few or many orange, brown, or reddish flakes, very often triangular in shape and with concoidal fractures. I have not yet examined thin sections of these coals, but the fragments present much the appearance presented by the spores in the well-known spore coals of the Bradford "Better Bed," and Leicestershire "Moira." If these coloured bodies originate in Lycopodian and other microspores or macrospores, they may play an important part, for the resinous nature of the microspores of the *Selaginella selaginoides*, &c., of our northern hills is so well known that they were formerly used in theatres to produce artificial lightning. As my experiments and inquiries in this direction are yet incomplete, I will only suggest that their presence may account for some dusts being so much more dangerous (as the German experiments have conclusively shown) than others, and add the hope that these words may lead others to pursue this inquiry.

ARTHUR WATTS.

Bede College, Durham, May 26.

Science for Artists.

OF the various optical errors in this year's pictures, certainly that in the elegant scene (624) of the Queen's Accession, in the morning small hours of June 20, 1837, is largest and most hopeless. Neither a source of light at 93,000,000 miles, nor one at 93 inches, could cast the bar-shadows. It is impossible to say whether they are meant to be aërial in the dust or mist, or cast on the walls and wainscot. But for either they are equally preternatural, though not by diverging perspectively. If cast on the solids they would, instead of being straight, be crooking in and out over the mouldings. But if they are in aërial mist or dust, the error is in supposing the same eye can see more than one of such shadows at a time. The eye requires to be very nearly in the plane of the shadow seen, so that, of those cast by parallel things, as window-bars, only one could be seen by any single eye, and only as continuing the line of the bar itself. The bar and its mist-shadow could never meet at an angle, as they all do in this picture. Another error (now common) is in there being no more penumbra than if the sun were a star, or a small electric arc-light.

EDWD. L. GARBETT.

Weight, Mass, and Force.

WITH reference to the extract, as to the language employed in which Prof. Greenhill invites my criticism, I have no doubt that to an engineer it would convey perfectly definite and intelligible information, and that one who has mastered the fundamental notions of dynamics as a science would be able to divine its meaning, but Prof. Greenhill would hardly maintain that the language is scientifically accurate, and that, however sufficient as a shorthand for the trained engineer addressing engineers, it is not full of pitfalls for the tyro.

There is no need to object to the statement that "the *weight* is 137,000 pounds," though it is just as easy to say, "the *mass* is 137,000 pounds." But that "the boiler carries 160 pounds of steam," I find, means that the pressure of the steam is 160 pounds (weight) *per square inch*, while "a 96-feet grade" means "a gradient of 96 feet *per mile*." Surely, except as a recognized shorthand for experts, the suppression of the words in italics is unjustifiable and liable to lead into error.

It is more important, however, to observe that (as in a great majority of the cases an engineer has to deal with) the question here discussed is essentially a *statical* one. The motion of the train considered is uniform (30 miles per hour), and the variations in pressure in the cylinders, &c., are avoided by taking the "mean effective pressure," so that there are no *accelerations* to be considered, and only, in fact, a balancing of forces. The question of *mass* therefore, (a purely *kinetic* notion), can hardly arise, and there is no room for confusion between mass and weight.

R. B. HAYWARD.

during my long tenure of office as one of the Secretaries of the Royal Society; and for my own part I may say that it seemed to me all along that the results were established on so firm a basis, and the conclusions regarding the invisible radiations were so perfectly analogous to what we know to be true regarding the visible ones, where the investigation is comparatively easy, that the work bore on it the stamp of truth. The conclusions were not, however, accepted without opposition. In the late Prof. Magnus Dr. Tyndall met a foeman worthy of his steel; a foeman, however, only in the sense of an intellectual athlete; for socially I doubt not they were the firmest friends, and their friendship was even cemented by the fact that they were both alike seeking after truth in a similar subject. But truth only gains by opposition: its defenders are led to engage in fresh researches, which end in strengthening its foundations. I think that the validity of Dr. Tyndall's results is now generally admitted. If some hesitation is still felt, it arises mainly, I think, from misconception; from imagining that assertions which were meant to apply only to heat-rays of such refrangibilities as to be absorbed by water were meant to be affirmed of the invisible radiations generally which lie beyond the extreme red. The time reminds me that I must only very briefly refer to another investigation in which Dr. Tyndall has more recently been engaged, and of which the interest is biological while the means of investigation are physical; I allude, of course, to the question of abiogenesis. Here, again, Dr. Tyndall was working on contested ground, and the objections of opponents stimulated him to fresh inquiries, which resulted in the continual strengthening of his negative conclusions. In the course of his work he was led, for instance, to the discovery of the great difference which exists between the germs of microscopic creatures and the creatures themselves, in relation to their power of resisting the destructive influence of a high temperature. This discovery not only detected a source of error in some experiments which had seemed to favour the hypothesis of abiogenesis, but threw important light on the conditions which must be fulfilled in order to secure complete sterility. But original research is not the only way in which a man can advance the cause of science. All-important though it is, it nevertheless often happens that an original investigation is too abstruse to be followed by more than a few experts; nor is it by any means necessarily the case that an eminent investigator is equally successful in expounding to others, especially to a mixed audience, the results at which he himself or other investigators may have arrived. The general diffusion of science depends largely on the clearness with which its leading principles and results are expounded, whether by lectures or by treatises, in which, while they are scientifically sound, popularity of style and general readableness are not sacrificed to the dry exactness of scientific detail. Most of us have had opportunities, whether at the Royal Institution, with which the name of Tyndall has so long been connected, or elsewhere, of being impressed with the singularly lucid style and graphic expression with which he expounded to his audience the salient points of the scientific subject which he brought before them. Nor was it only in clearness of verbal exposition that he excelled; the manipulative skill with which his original investigations were carried on served him in good stead in his more popular expositions; and by the aid of that "domestic sun," which even the murky atmosphere of a London winter could not obscure, he was enabled in very many cases to exhibit to the audience the actual results of experiments which had first been carried out in the quiet of the laboratory. Nor is it our own countrymen alone who have had the benefit of Dr. Tyndall's lucidity of exposition. Our friends across the ocean have flocked to hear and have appreciated the lectures which he has there delivered as a free gift to Transatlantic science. But oral lectures, after all—the lectures at least of one individual—can only reach a fraction of the community; nor do they admit of that pause for thought which the learner requires in endeavouring to make himself master of a new subject. But the same qualities of mind which enable a man to be a clear and interesting lecturer fit him also to be the author of eminently readable books; and for the general diffusion of science which is taking place we owe much to the writings of Dr. Tyndall. My lords and gentlemen, I fear that I have trespassed too long upon your time, and I will therefore now conclude by asking you once more to drink to the health of Dr. Tyndall. (The toast was drunk with great enthusiasm, the company rising.)

Professor Tyndall, on rising to respond, was received with loud cheers, the company rising. He said:—Mr. President, my Lords, and Gentlemen,—When the project of a dinner was first mentioned to me by a very old and steadfast friend of mine, who, to my regret and his, is not here to-night, had any dream, or vision, of the assembly now before me risen on my mind's eye, I should have declined the risk of standing in my present position; for I should have doubted, as I still continue to doubt, my ability to rise to the level of the occasion. Gratitude, however, is possible to all men; and I would offer you, Sir, my grateful thanks for the manner in which you have proposed this toast; I would thank with equal warmth an assembly which, in intellectual measure, is, probably, as distinguished as any of the same size ever addressed by man, for the way in which they have received it; and I would extend my thanks to my friends of the Department of Science and Art, for their spontaneous kindness to an old colleague, who for many years lent his humble aid to the Department in diffusing sound scientific knowledge among the masses of the people. My own scientific education began late. It had, of necessity, to be postponed until after I had reached the age of seven or eight and twenty. Notwithstanding this drawback, in learning, teaching, and working in the laboratory, I have been permitted to enjoy a spell of thirty-nine years. In 1850, during a flying visit from Germany to England, I stood, for the first time, in the bright presence of Faraday. In February 1853, I gave my first Friday evening lecture in the Royal Institution; and three months afterwards, on the motion of Faraday, the old Chair of Natural Philosophy, which had been filled at the beginning of the century by Thomas Young, was restored, and to it I was elected. It causes me genuine pleasure to think that I shall be succeeded in that Chair by so true and so eminent a man of science as Lord Rayleigh.

It is not my intention to overburden you with egotism to-night; but, casting an earnest glance back upon the past, a few words seem due from me to the memory of one or two of the group of good men, no longer with us, with whom I was so intimately associated. Regarding Faraday I will confine myself to stating that years have not altered my estimate of the beauty and the nobleness of his character. He was the prince of experimental philosophers; but he was more than this—in every fibre of his mind he was a gentleman. It is, however, of two of our honorary secretaries that I wish now to speak; premising that, for the first seven years of my life in the Royal Institution, the post of honorary secretary was held by a cultivated and very worthy gentleman, the Rev. John Barlow. From 1860 to 1873—that is, for a stretch of thirteen memorable years—I had the happiness of working hand in hand with Dr. Bence Jones. Never in my experience have I met a man more entirely and unselfishly devoted to the furtherance of scientific work. I hardly like to mention the following incident, because it furnishes but a scanty measure of his devotion. On one occasion I was in need of funds to carry out some experiments of a delicate and

costly character. Bence Jones came to me, and after some hesitation—for he knew that money was likely to raise a difficulty between us—he said, with earnestness: "Dear Tyndall, behave as my friend; do me the favour and the honour of devoting this to your investigation. There is more, if you need it, where that came from." He handed me a cheque for £100. Had I asked for £1000, he would have given it to me, and the world, as far as he was concerned, would have been none the wiser. Bence Jones was a strong man, and liked to have his own way. At first, as was natural, we sometimes surged against each other; but these little oppositions were rapidly adjusted, and for many years before his death the tie of brother to brother was not truer or tenderer than that which united myself and Bence Jones. On my return from the United States I found him dying. In fact, the knowledge of his condition caused me to take leave, earlier than I otherwise should have done, of a people that I had learnt to trust and love. Soon after my return I saw him lowered into the grave.

The death of Bence Jones, whose steadfast loyalty to the Institution he loved so well, showed itself to the last, was a sore calamity to be met. At that time one man only seemed fitted to supply his place. That man was the beloved and lamented William Spottiswoode. To him I appealed to stand by the Institution at a critical hour of its fortunes. He had his own mathematical work on hand, and he was too well acquainted with the duties of our honorary secretaryship to accept them lightly. After much reflection, he wrote me a letter regretfully but distinctly declining the office. But he reflected a second time. He knew that his refusal would cause me pain, and his affection for me prevailed. When, therefore, the letter of refusal—for he sent it to me—came, it was accompanied by a second letter, cancelling the refusal and accepting the post. With William Spottiswoode I had the happiness of working in close companionship for six years. The diligence, wisdom, and success with which he discharged his onerous duties—the princely hospitality which shed a glow upon the office while he held it—are well remembered. Of the dignity with which he afterwards filled the high position now occupied by the illustrious man who presides here this evening it is needless to speak. Him also we have seen lowered to his rest, amid the grief of friends assembled to do honour to his memory. Such were the men who served the Royal Institution in the past; and their example has been worthily followed by other men of eminence, still happily amongst us. Never was an institution better served than the Royal Institution, and not by its honorary secretaries alone. With singleness of purpose and purity of aim, its successive Presidents, Boards of Managers, and honorary treasurers have unswervingly promoted the noble work of investigation and discovery. May they never lower the flag which, for well-nigh a century, they have kept victoriously unfurled.

The year after my appointment I was called upon to deliver, in conjunction with Dr. Whewell, Faraday, Sir James Paget, and some other eminent men, one of a series of lectures on scientific education. I then referred with serious emphasis to the workers in our coal-mines, and to the terrible perils of their occupation. I pointed to the intellectual Samsons toiling with closed eyes in the mills and forges of Manchester and Birmingham, and I said: "Give these toilers sight by the teachings of science, and you diminish the causes of calamity, multiply the chances of discovery, and widen the prospect of national advancement." Thus early, you will see, I was alive to the importance of technical education; and I am no less alive to it now. You will not, therefore, misunderstand me when I say that to keep technical education from withering, and to preserve the applications of science from decay, the roots of both of them must be well embedded in the soil of original investigation. And here let it be emphatically added, that in such investigation practical results may enter as incidents, but must never usurp the place of aims. The true son of science will pursue his inquiries irrespective of practical considerations. He will ever regard the acquisition and expansion of natural knowledge—the unravelling of the complex web of nature by the disciplined intellect of man—as his noblest end, and not as a means to any other end. And what has been the upshot of science thus pursued? Why, that the investigator has over and over again tapped springs of practical power which otherwise he would never have reached. Illustrations are here manifold. I might point to the industries which affiliate themselves with Faraday's discovery of benzol, and with his discovery of the laws of electrolysis. But I need not go further than the fact that in this our day a noble and powerful profession has been called into existence by his discovery of magneto-electricity. The electric lamps which mildly illuminate our rooms, the foci which flood with light of solar brilliancy our railway-stations and public halls, can all be traced back to an ancestral spark so small as to be barely visible. With impatient ardour Faraday refused to pause in his quest of principles to intensify his spark. That work he deliberately left to others, confidently predicting that it would be accomplished. And, prompted by motives both natural and laudable, but which had never the slightest influence on Faraday, others have developed his spark into the splendours which now shine in our midst.

It would be a handsome Jubilee present, if it were a possible one, to roll up the career of Faraday into portable form, and to offer it to the Queen as the achievement of one of Her Majesty's most devoted subjects during her own reign. Faraday's series of great discoveries, however, began in 1831, which throws his work five or six years too far back. During the rest of his fruitful life he was a loyal son of the Victorian epoch. But, passing beyond the limitations of the individual, what is science, as a whole, able to offer, on the golden wedding of the Queen with her people? A present of the principle of gravitation—a handing over to Her Majesty of the bit and bridle whereby the compelling intellect of Newton brought the solar system under the yoke of physical laws—would surely be a handsome offering. I mention this case of known and conspicuous grandeur, in order to fix the value of another generalization which the science of her reign *can* proudly offer to the Queen. Quite fit to take rank with the principle of Gravitation—more momentous if that be possible—is that law of Conservation which combines the energies of the material universe into an organic whole; that law which enables the eye of science to follow the flying shuttles of the universal power, as it weaves what the Earth Spirit in "Faust" calls "the living garment of God." This, then, is the largest flower of the garland which the science of the last fifty years is able to offer to the Queen.

The second generalization is like unto the first in point of importance, though very unlike as regards its reception by the world. For whereas the principle of Conservation, with all its far-reaching, and, from some points of view, tremendous implications, slid quietly into acceptance, its successor evoked the thunder-peals which it is said always accompany the marriage of thought and fact. For a long time the scent of danger was in the air. But the evil odour has passed away; the air is fresher than before; it fills our lungs and purifies our blood, and science, in its Jubilee offering to the Queen, is able to add to the law of Conservation the principle of Evolution.

In connexion with these victories of the scientific intellect, I have mentioned neither persons nor nationalities, holding, as Davy expressed it, when the Copley Medal was awarded to Arago, that "science, like Nature, to which it belongs, is neither limited by time nor space. It belongs to the world, and is of no country and no age."

Still, it will not be counted Chauvinism if I say that in the establishment of these two great generalizations Her Majesty's subjects have quitted themselves like men. With regard to a third generalization, neither England nor Germany has been idle. Omitting the name of many a noble worker in both countries, the antiseptic system of surgery assuredly counts for something in the civilized world. And yet it is but a branch of a larger generalization, of momentous import, which in our day has been extended and consolidated to an amazing degree by a Gallic investigator. To some, however, any flower culled in this garden will be without odour. Let me therefore add a sweet-scented violet under the name of spectrum analysis which, besides revealing new elements in matter, enables the human worker to stretch forth his hand to sun and stars, to bring samples of them, as it were, into his laboratory, and to tell us, with certainty, whereof they are composed. Surely all these, and other discoveries of high importance, taken and bound together, form an intellectual wreath, not unworthy of Her Majesty's acceptance in her Jubilee year.

A short time ago an illustrious party leader summed up the political progress of the Queen's reign. What I have said will, I trust, show that the intellectual world is not entirely compounded of party politics—that there is a band of workers scattered over the earth whose arena is the laboratory rather than the platform, and who noiselessly produce results as likely to endure, and as likely to influence for good the future of humanity, as the more clamorous performances of the politician.

One word more. On the continent of Europe, kings had been the nursing fathers, and queens the nursing mothers, of science; while Republican Governments were not a whit behind in the liberality of their subventions to scientific education. In England we had nothing of this kind, and to establish an equivalent state of things we had to appeal, not to the Government, but to the people. They have been roused by making the most recondite discoveries of science the property of the community at large. And as a result of this stirring of the national pulse—this development of self-reliance—we see schools, colleges, and universities now rising in our midst, which promise by and by to rival those of Germany in number and importance.

It is time that I should cease. But before doing so, I would ask—as they do in the House of Commons—permission to say a word in personal explanation. I have climbed some difficult mountains in my time, and after strenuous effort for a dozen hours or more, upon ice, rock, and snow, I have not unfrequently reached the top. I question whether there is a joy on earth more exhilarating than that of the mountaineer, who, having achieved his object, is able to afford himself, upon the summit, a foaming bumper of champagne. But, my lords and gentlemen, the hardest climb, by far, that I have ever accomplished, was that from the banks of the Barrow to the banks of the Thames—from the modest Irish roof under which I was born to Willis's Rooms. Here I have reached my mountain-top, and you—God bless you!—have given me a bumper which no scientific climber ever before enjoyed.

Sir Frederick Pollock, in proposing the toast of "Literature and Art," said that on most occasions similar to the present one this toast was a triple one, and included the three sisters—Science, Literature, and Art. But this evening they were assembled together to do homage to science, in the person of one of its most distinguished votaries, and for the time the room in which they had met became a temple of science. In such a temple the principal figure, standing upon the pedestal appropriated to the presiding goddess, must be that of Science, and to her due rites had been already rendered. But for the sisters Literature and Art room must be found also in the sacred edifice; they too must have their altars and their shrines. He pointed out that the highest powers of the imagination were required by the man of science, as well as by the poet and the painter, and instanced the prediction by Fresnel of the bright spot in the centre of the shadow of a disk; and the suggestion made to Goethe of his theory of the development of the vertebrate skeleton, by his accidental observation of the scattered fragments of the deer's skull lying in his path. He adduced the names of Aristotle, Bacon, and other great men who had connected literature with science; and instanced Leonardo da Vinci, and Sir Christopher Wren, one of the founders of the Royal Society, as linking together science and art. He accordingly had great pleasure in submitting for acceptance "Literature and Art," coupling with it the name of Lord Lytton, who was not only a distinguished representative of modern literature, but had also a distinct hereditary claim to represent that of the last generation; and Sir Frederick Leighton, the distinguished President of the Royal Academy.

The Earl of Lytton,—In returning thanks for "Literature" upon an occasion when we are all met to honour science in the person of one of its most illustrious adepts, I cannot but forcibly remember that we are living in an age when inquiry is more active and more widespread than conviction, and it is natural that in minds of the highest order under these conditions even the imaginative faculty should be more powerfully attracted to scientific research than to purely literary production. But inquiry, I think, would be very sterile if conviction in some form or another were not the ultimate fruit of it, and I think that for a period of really vigorous, creative, imaginative art we must look forward in the course of scientific research to some such general re-settlement of ideas upon the basis of a common conviction—which is not now, perhaps, altogether attainable—as may enable art, instead of representing, as it does now, merely the mental attitude of the individual poet or the individual painter, once more to become the universally spontaneous and universally recognized imaginative expression of ideas and emotions which are common to a whole generation or a whole community. If that is the case, if science is ultimately to render this great service to literature and art, surely in the meanwhile we cannot but gratefully appreciate the literary labours of those men of science who in our own and in other countries are promoting or have promoted this result, not only as original discoverers but also as popular and powerful interpreters of scientific fact, and who in this latter capacity have already enriched contemporary literature with writings of rare literary value. If, instead of returning thanks for literature, I were permitted to return thanks on behalf of literature to those writers who have powerfully influenced my own generation, not only by thoughts which stimulate and instruct the intellect, but also by words which stir and elevate the heart, then assuredly I should ask leave to mention some distinguished names which occupy in the field of literature a position only second to the high rank they hold in the hierarchy of science; and foremost among those names I should not hesitate to mention with a special personal gratitude the name of the illustrious man who is the honoured guest of this great assembly to-night. I cannot say it is as a student of science that I myself have studied the writings of Prof. Tyndall, but this I can say, and most truly, that those writings have been to me, from a very early period of my life, companions so cherished that I learnt to look upon their writer as a dear personal friend and benefactor long before it was my privilege to be admitted to his personal intimacy. I believe that scientific research has succeeded in establishing on a physiological basis certain evidences of intelligence even among oysters; and certainly there is, I think, one form of intelligence which is conspicuously displayed by the

oyster which might perhaps be cultivated with advantage by after-dinner speakers in my position. The oyster knows when to shut up. Admonished by that very interesting and suggestive fact in natural history, what little else I have to say upon behalf of literature I shall confine to the expression of a hope that the well-deserved relaxation from his more systematic scientific labours in connexion with the Royal Institution may enable my valued and honoured friend Prof. Tyndall to enjoy an increased leisure for the continued cultivation of that department of literature which has already been so richly adorned by his admirable writings.

Sir F. Leighton, who was to have responded for "Art," had been obliged to leave before this stage of the proceedings in order to receive Royal visitors at the Academy.

Sir Lyon Playfair, M.P., proposed the next toast, "The Public Services in Relation to Science." He said that undoubtedly the public services were intimately connected with science and were profoundly affected by its progress, but, unfortunately, the truth was only beginning to be recognized in this country. In the United States scientific men were attached to all public offices, but in this country the attachment was of the loosest possible character. Nevertheless, science had undoubtedly affected our public services in the most profound way. The telegraph had altered the whole system of commerce and also the methods and the powers of government. There was to be a great naval review next month; it would be interesting to imagine Elizabeth's thirty small ships, which conquered the Armada, sailing through two miles of modern ironclads. The largest piece of ordnance used in the Crimean War cost less than a single shot fired from the huge guns of our ironclads. But it was in peace rather than in war that science rejoiced in aiding government. A strong feeling was arising that we must improve our intellectual position as a nation, and this at last was being recognized by the Government. A material index of progressive civilization had always been desired. Liebig contended that the best index of civilization was the quantity of soap consumed. When the Queen ascended the throne we consumed per head 7¾ pounds of soap, and now we use 10 pounds per head. The consumption of paper was a more reliable index. At the commencement of the Queen's reign the consumption was 1¼ pound of paper yearly; now it was 12 pounds; while in the United States it was 10 pounds, in Germany 9 pounds, in France 8 pounds, and in Italy 4 pounds. But the main question was whether we were developing the national intellect at the same rate as other nations. Our general intelligence is still high, but our trained scientific intelligence is low. Our secondary education in all matters relating to science was far behind that of the United States, Germany, and France. Neither the Government nor the people governed could go on in simple faith on our practical aptitudes by relying on a blind and vain empiricism, like a tree severed from its roots.

The Earl of Derby,—My Lords and Gentlemen: You have asked me to return thanks on behalf of the public services in connexion with science, and Sir L. Playfair, in relation to that toast, has referred to the increased consumption of soap in this country. I have attended a good many public dinners, and I must say that the expenditure of what is vulgarly called soft soap has been great this evening. I am sincerely grateful to him for the quantity of that article which it has pleased him to expend upon me. But really the toast is one which hardly any man is competent to do justice to, and certainly not one who like myself has no connexion with science, except a sincere admiration and respect for its professors, and whose connexion with the public service has only been that of a Parliamentary chief. Under our system the Parliamentary head of a department is mainly concerned to keep it in harmony with the House of Commons and with the public. He has to warn the permanent officials that something that is done, or something that is left undone, or proposed to be left undone, is what public opinion will resent; and, on the other hand, he has to tell outsiders that the things they ask him and press him to do are things unwise or impossible from an administrative point of view. That is useful; it is certainly laborious, and it is often a difficult function; but it does not involve much more scientific knowledge than is implied in driving a cab through a crowded street. It does require some knowledge of men, but that is a department of study to which, as yet, no scientific formula has been found to apply. Sir L. Playfair told us, and I was sorry to hear it, of the loose connexions which exist between science and the Government. I can only say that I am entirely ignorant of any such immoral transactions. But if the departments were better represented here and if they could speak for themselves, I am sure that they would not be backward in acknowledging their obligations to science. The Treasury would tell you that those useful though sometimes ungraceful coins in which our dinner is paid for would not circulate through Europe as they do if they had not been subjected to a careful and complicated process, requiring scientific knowledge. The Excise might tell you, if they chose, of the frauds that might be perpetrated upon the revenue and the public if it were not for the careful and scientific examination of all taxable articles. The Post Office would find no difficulty in acknowledging its obligations to Watt and to Stephenson—for where would the postal revenue be without railways?—and in later days to investigators whose researches made the telegraph possible. But the fighting departments, or the spending departments, which is their more common name in Downing Street, would have the most to return thanks for. They would point to the modern ironclad, the most elaborate, the most complete, and the most costly, of all contrivances in which the art of construction has been utilized for purposes of destruction. They would tell you how the chemist, metallurgist, the engineer, the electrician, the mathematician, have all contributed their share to that extraordinary result of science and skill. The War Office would follow the Admiralty. They would not say, as Frederick of Prussia did, that Providence is on the side of the biggest battalions, but they might possibly say that Providence was generally on the side of the army which could bring into the field the most scientifically effective weapon in the hands of the most carefully-trained soldier. If I were to turn to the line of business with which I had once something to do, I might ask any diplomatist or any statesman to explain to you how largely the position of Egypt, and, with that, the diplomacy of Europe, has been affected by that little scratch which the genius of M. de Lesseps drew across the Egyptian sands; and if, as is quite possible, the coal-carrying power of steamers and their speed and their economy are largely increased—I do not speak of those wilder predictions according to which steam is to be superseded as the motor power by something more efficient—suppose I say the large increase of the coal-carrying power of steamers, and the results to which I have referred may be again reversed; and again, at least in war time, the route to India may lie through the South African seas. If I speak of the colonies, everyone conversant with that department would admit that if we had had the ocean telegraph in existence twenty-five years ago half our little wars beyond the seas would never have taken place, and those that have taken place would have been disposed of in half the time. I know that these things are common-place, but I cannot help that. If I could tell you what the next great discovery was going to be, that would not be common-place. But, unfortunately, that is not in my power; and if it were I do not think I should be in a hurry about it, because I have observed that those who are the first to announce a discovery are generally rewarded by having a remarkably unpleasant time. But however great may be the gains which we

have derived from the applications of science, they are nothing as compared with those which will and do accrue to us from the acceptance of scientific habits of thought. That is coming already, and it will come more in a not remote future. We have many things in this age and country of which we cannot boast, but we may boast that in science England has done something more than hold her own. The great name of Darwin will survive, it may be, the British Empire itself, and with him will be remembered some others also, whom to single out might perhaps be invidious. But we may be sure of this, that among their names will be included the name of our distinguished guest of to night. It is a common complaint that politicians have done nothing for science. In that I do not agree. They have done the best they could for it—they have let it alone; they have not corrupted it by their intrigues, nor vulgarized it by their squabbles; and they being what they are, and science being what it is, that is probably the best service they could have rendered it.

Lord Rayleigh proposed "The Health of the Chairman."

Prof. Stokes briefly responded, and the company, which numbered nearly two hundred, separated.

THE ELEVEN-YEAR PERIODICAL FLUCTUATION OF THE CARNATIC RAINFALL.

MORE than fourteen years ago, in the pages of NATURE, Mr. Norman Lockyer first drew attention to an apparent periodical variation of the rainfall registered at the Madras Observatory; which seemed to be such that it reached a maximum and a minimum alternately, at about the same epochs as the corresponding phases of the sunspot frequency. The idea, once started, was followed up by others, among whom perhaps the best known is Dr. (now Sir) W. W. Hunter, whose pamphlet on the subject, without laying claim to any originality as regards its subject matter, attracted very general attention by the charm of its style, and also by its attempt to identify the periodical occurrence of famines in Southern India with the epochs of minimum rainfall shown by the Madras registers.

When, however, the data on which these speculations were based came to be critically examined, the general verdict of men of science was that the conclusions were "not proven." This was certainly my own opinion; and General R. Strachey, in a lecture delivered before the Royal Institution in 1877, and, at greater length, in a paper communicated to the Royal Society in May of the same year, showed that any attempt to educe a true cyclical variation from the recorded figures, ended in a negative result. Admitting that when the annual quantities were tabulated in eleven-year cycles, the means of the homologous terms seemed to indicate a period of maximum between the third and seventh years, and of a minimum between the eighth and second years, he found that, when the mean difference of the individual years from the supposed periodical means was compared with the mean difference of the former from the arithmetical mean of the whole series, the results differed but little.

It was further shown by myself that the supposed connexion between the periodicity of the Madras (Observatory) rainfall and that of famines in Southern India was by no means so intimate as might appear at first sight. The famines in question had occurred sometimes in one part of the peninsula, sometimes in another, by no means always in the country around Madras; but no other station in the peninsula (of those then available for the inquiry) showed even such an approach to a periodical variation of the rainfall as did the Madras Observatory.

At this stage matters have since remained, with the exception that, in 1879, an apparent periodical fluctuation of a very different character was brought to notice by Messrs. Hill and Archibald in the winter rainfall of Northern India. This, which has an interest of its own, I shall not further discuss at present.

In the course of a general investigation of the rainfall of India, the first part of which only has been as yet published ("Indian Meteorological Memoirs," vol. iii. part 1), I have lately had occasion to reconsider these old questions, and to re-examine them by the light of the accumulated data of the last twenty-two years. For convenience of discussion, I have divided India and Burmah into twenty-four rainfall provinces, one of which is the Carnatic.

This consists of the plain below the Eastern Ghâts, occupying the south-east of the peninsula, and extending from Cape Comorin to the mouths of the Kistna. Its area may be taken as 72,000 square miles. The town of Madras is situated nearly midway on the sea-coast of this province, and is a fairly representative station; but, in addition to the rainfall registers of the Madras Observatory, I have those of thirty-nine other stations, pretty equally distributed through the province; most of them extending back to 1864. The Carnatic is distinguished by one important peculiarity in the season of its chief rainfall. During the spring months, it receives a certain amount of rain, in common with the southern and eastern provinces of India generally; but while the heavy summer rains are falling in Central and Northern India, and also on the west coast of the peninsula, the Carnatic is but little affected by them. In its southern districts, indeed, the rainfall of June and July is less than that of May; and it is not until the rains are over in North-Western India, viz. in October and November, that this province receives the chief and heaviest rainfall of the year. Hence the vicissitudes of the rainfall of the summer months, which are all important in Central and Northern India, are relatively less important in the Carnatic, even if they affect that province in the same manner as Northern India—and this is far from being always the case—and as a final result the annual fluctuation of the Carnatic rainfall often differs widely from that of other provinces in the peninsula.

The mean annual rainfall of the Carnatic may be taken in round figures at 35 inches, which is about 7 inches less than the general average of the whole of India. The following table gives the annual variation from this average for the twenty-two years 1864–85, which results when the annual total fall of each individual station is compared with its local average, and the mean of all the differences taken for each year.

Annual mean rainfall variation of the Carnatic rainfall.

	Inches.		Inches.
1864	− 5·0	1875	− 5·2
1865	− 5·0	1876	−13·2
1866	− 4·0	1877	+ 8·3
1867	− 9·4	1878	0
1868	− 4·6	1879	+ 2·3
1869	− 0·3	1880	+ 7·0
1870	+ 1·8	1881	− 2·1
1871	+ 5·5	1882	+ 4·4
1872	+11·5	1883	+ 5·2
1873	+ 0·1	1884	+11·6
1874	+ 7·3	1885	− 1·1

During the first thirteen years (with the exception of 1873) the fluctuation, here shown, is remarkably distinct and regular. The rainfall reached a minimum in 1867, then rose steadily to a maximum in 1872, and after a drop in 1873, and partial recovery in the following year, fell rapidly to a second minimum in 1876. From 1877 to 1881 it oscillated considerably, but thereafter rose again steadily to a second maximum in 1884, dropping again in 1885 to something below the average. Thus we have, apparently, two complete cycles in the twenty-two years; the first remarkably regular, the second less so, but with the periodical fluctuation still dominant.

In order to ascertain with somewhat greater precision

the probable character of this periodical fluctuation in an eleven-year cycle, the coefficients of the first two periodical terms of the harmonic formula have been computed, taking 1864 as the initial epoch. These coefficients are—

$u' = 5{\cdot}340$ inches. $u'' = 2{\cdot}873$ inches.
$U' = 206° 29'$ $U'' = 247° 15'$

and the values of the eleven annual phases of the cycle thus found are—

	Inches.
1864 and 1875	− 5·1
1865 ,, 1876	− 6·7
1866 ,, 1877	− 4·4
1867 ,, 1878	− 1·5
1868 ,, 1879	− 0·6
1869 ,, 1880	− 0·7
1870 ,, 1881	+ 0·8
1871 ,, 1882	+ 4·4
1872 ,, 1883	+ 7·3
1873 ,, 1884	+ 5·9
1874 ,, 1885	+ 0·5

Taking the differences of these values from the recorded rainfall of each of the twenty-two years, the mean deviation of the latter in any one year from its periodical value is found to be—

$$\pm\ 3{\cdot}5 \text{ inches},$$

which is only one-fourth of the range of the periodical variation as above determined; and the probable error ϵ, of the periodical value, as found by the formula—

$$\epsilon = 0{\cdot}6745 \sqrt{\frac{\Sigma\,(v^2)}{n(n-1)}},$$

is ± 0·70 inch.

On the other hand, the mean deviation of a single year from the general average is

$$\pm\ 5{\cdot}2 \text{ inches},$$

and the probable error of that average ± 0·94 inch.

What, then, is the numerical probability of the cyclical variation, thus determined, being a true periodical fluctuation, representing a regularly recurrent phenomenon? As a general problem this cannot be solved, because we do not know all the variations to which the rainfall may conceivably be subject. But we can compare the relative probability of this particular variation being the result of a periodic law, and of its being a mere fortuitous series of variations from a constant average. That it is the most probable variation, having the assumed period of eleven years (with the exception of such as might be computed from a larger number of periodic terms), is assured by the method of its computation, which is based on that of least squares; and one may assume that this relative probability for a single year is represented by the inverse ratio of the probable errors of the two means above determined, viz.—

$$\frac{0{\cdot}94}{0{\cdot}70}.$$

This ratio of probability increases in geometrical progression, as the number of years during which it is found to hold good increases in arithmetical progression[1]; and, for twenty-two years, becomes—

$$\left(\frac{0{\cdot}94}{0{\cdot}70}\right)^{22} = 655 : 1.$$

This ratio, although by no means amounting to demonstration of the exact validity of this particular cycle, affords at least a very high probability that the apparent undecennial fluctuation is no chance phenomenon.

Apart from the approximate identity of its period, the oscillation of the rainfall, thus disclosed, is very different in character from that of the sunspot curve. The periodical minima of both rainfall cycles preceded those of the corresponding sunspot cycles by two years; the actual year of minimum rainfall coincided with that of sunspot minimum in the first cycle, and preceded it by two years in the second. The periodical maximum of the first cycle followed the sunspot maximum by two years, that of the second cycle coincided with the corresponding phase of sunspots, which, in this case, was retarded by two years. The actual rainfall maximum occurred two years later than the sunspot maximum in the first cycle, and one year later in the second.

Hence, as far as the evidence of two cycles goes, the minimum of the rainfall tends to precede the minimum of the sunspots, the maximum of the former to follow that of the latter; and it is noteworthy, as I shall afterwards show, that the droughts which, during the last century, have visited with more or less intensity certain portions of the Indian peninsula, have, on an average, preceded years of sunspot minimum by about one year.

In the other provinces of tropical India, an eleven-year cycle is hardly, if at all, to be detected; a conclusion fully in accord with that which I drew, in 1877, from an examination of the rainfall registers of Bangalore, Mysore, Bombay, Nagpur, &c. The more pronounced phases of the Carnatic cycle are indeed reproduced as a rule, more or less distinctly, as seasons of high or low rainfall respectively, in most parts of the peninsula; but some of the intermediate years are characterized by vicissitudes as great, and even greater than these, destroying the appearance of anything like regular fluctuation.

The Carnatic minimum of 1867, which was the culmination of five years' (1864-68) deficient rainfall, was represented also in Mysore and Bellary, in Malabar and the Deccan; but, in the last two of these provinces, 1866 had a still lower rainfall: and in Berar and Khandesh, while the deficiency of 1866 was (relatively to the average) greater than in any of the more southern provinces, that of 1867 was above the average. In the Konkan again, there was no very great deficiency before 1871, and this was shared more or less by the whole of the peninsula, excepting only the Carnatic and Malabar, which had an excess of 16 and 13 per cent. respectively.

The Carnatic maximum of 1872 was reproduced in Orissa and the Northern Circars—that is to say, in all the eastern provinces of the peninsula—and also in Berar and Khandesh; but in other parts of the peninsula the rainfall of this year differed but little from the average. 1874, however, was a year of excessive rainfall in all the western and southern provinces of the peninsula.

The great drought of 1876 (the second Carnatic minimum) extended with even greater intensity to Mysore, Bellary, Hyderabad, and the Deccan districts of Bombay, and affected more or less the whole of the peninsula, and, in addition, a great part of extra-tropical India. But in the Konkan and Malabar the deficiency was only 18 per cent. of an average fall. In the Konkan the deficiency of the following year was much greater; and in the northern provinces of Bombay, as well as in the greater part of North-Western India, the summer rainfall of 1877 failed almost completely; whereas in the Carnatic the rainfall of that year was remarkably copious.

[1] The probability of throwing any given series of numbers of a single die, in any prescribed order, repeatedly for n throws, is obviously the same as that of throwing a single given number n times in succession, viz. $\left(\frac{1}{6}\right)^n$; and the probability of throwing, in like manner, one out of a given series of dyads or triads, the dyads or triads varying in any prescribed order is $\left(\frac{2}{6}\right)^n$ or $\left(\frac{3}{6}\right)^n$. The relative probability of the dyad to the triad series is $\left(\frac{2}{3}\right)^n$; and generally the relative probability of a phenomenon, the law of variation of which is unknown, varying n times in succession, between limits ± β and ± $(\beta + x)$ respectively, is—

$$\left(\frac{\beta}{\beta + x}\right)^n.$$

Similar reasoning holds good when β and $\beta + x$ are the measures of the *mean* variation; and also when, as in the case before us, they represent the probable errors of alternative averages. Finally, the relative improbability of the more limited range, as a chance result—in other words, the probability of the limitation being the result of a regulating cause—is expressed by the inverse ratio.

The following year, 1878, was one of remarkably copious rainfall in nearly all parts of the peninsula, with the exception of the Carnatic, where the rainfall did not exceed the average. In Hyderabad it was greater than that of any other year since regular registers have been kept; and, on the general average of the peninsula (excluding the Carnatic), it is approached only by that of 1874 and 1882.

Finally, the Carnatic maximum of 1884 coincided with a small excess in Hyderabad and with a larger excess in the north-west of the peninsula (the Central Provinces, Berar, Khandesh, the Konkan, and Guzerat); but this was due to independent conditions. In Mysore, Bellary, Malabar, the Deccan, the Northern Circars, and Orissa, the rainfall of the year was more or less deficient, especially in Mysore, where the fall was only three-fourths of the average.

It may, then, be considered as demonstrated that the apparently periodical variation of the Carnatic rainfall is by no means representative of a similar variation in that of Southern India generally; and I might here conclude the discussion, were it not that the independent evidence of a certain apparent regularity in the recurrence of droughts and dearths seems to require a few words of notice.

At page 21 of the Report of the Indian Famine Commissioners is given a list of all the serious droughts, and consequent seasons of dearth, that have affected India during the last century. Selecting those that have chiefly affected some part of the peninsula, we have the following:—

Droughts.	Intervals.
1782	
1791	9 years.
1802	11 ,,
1806	4 ,,
1812	6 ,,
1823	11 ,,
1832	9 ,,
1844	12 ,,
1853	9 ,,
1865	11 ,,
1876	11 ,,

Omitting that of 1806, which divided the ordinary interval into two, the mean interval is 10·36 years, and the deviation from this mean in no case amounts to two years. According to Wolf's table, the years of minimum sunspots and their intervals were:—

Sunspot Minima.	Intervals.
1784	
1798	14 years.
1810	12 ,,
1823	13 ,,
1833	10 ,,
1843	10 ,,
1856	13 ,,
1867	11 ,,
1878	11 ,,

the mean interval being 11·18 years. The coincidence of these mean intervals is hardly so close as might be anticipated were there any real physical interdependence between recurrent phases of the sun's condition, and the recurrence of the droughts. And a comparison of the dates in detail brings to light further discrepancies. Thus the years of drought vary in their relations to the nearest years of minimum sunspots as follows:—

One, midway between two sunspots minima; seven years distant from each;
One, four years earlier;
One, three years earlier;
Three, two years earlier;
One, one year earlier;
One, coincident;
One, one year later;
One, two years later;
One, four years later.

Omitting the first (that of 1791), which occurred four years after a year of maximum sunspots, and midway between two minima, in an unusually prolonged cycle, the years of drought, on a general average, anticipated the sunspot minima by somewhat less than a year, instead of following the minima, as might have been expected on the hypothesis of the former standing to the quiescent condition of the sun in the relation of effect to cause.

I should not, however, hastily conclude from these facts that there is no relation between the recurrence of drought in Southern India, and the periodical variation of the solar photosphere; but merely that the interdependence of the two classes of phenomena, if real, is far from being simple and direct, and also that other and, as far as we know, non-periodic causes, concur largely in producing drought. If we accept the conclusions, drawn in the first part of this note, as to the highly probable periodicity of the Carnatic rainfall, one must admit that there is, in that province, a recurrent tendency to drought at eleven-year intervals, though it does not always culminate in drought of disastrous intensity; and this epoch anticipates by about two years that of the sunspot minimum. This tendency is evidently much weaker in other parts of the peninsula; and in Northern India there is some indication of a tendency to the recurrence of drought about the time of maximum sunspots, as in 1803, 1837, 1838, and 1860—all years of disastrous drought in Northern India; and the experience of late years has demonstrated that these droughts generally extend to the northern provinces of the peninsula.

HENRY F. BLANFORD.

NOTES.

WE print elsewhere a report of the speeches delivered by Mr. Goschen and by some members of the influential deputation who waited upon him last Thursday to press the claims of University Colleges. The deputation had certainly no reason to complain of the manner in which they were received. Mr. Goschen, speaking as Chancellor of the Exchequer, was of course obliged to adopt a cautious tone; but it was plain enough that those who addressed him represented a cause with which he had strong personal sympathy. His promise that the Government would give the matter "its most serious attention," means, we may hope, that the principle of State aid for University Colleges has been practically accepted.

ON Monday the foundation-stone of the Imperial Institute was laid by the Queen. No representative of science, as such, was invited to be present at the ceremony, and NATURE did not receive a Press ticket. Evidently science is to have little to do with the New Institute.

THE Prussian Society for the Promotion of Industry has recently offered a prize of about £150 for the most exhaustive critical comparison of all kinds of existing bronze, tombac, and brass alloys, used or recommended for machinery, giving their chief properties with regard to resistance, ductility, friction at different temperatures, malleability, electrical conductivity, behaviour with acids, hydrogen and carbon sulphides, chlorine, and other strongly corrosive substances met with in practice. The same Society also offers a gold medal and £250 for the best work on light and heat radiation of burning gases. The time limit in the former case is the end of 1887; in the latter, the end of 1888.

The National Association for the Promotion of Technical Education has now been formed. A meeting of persons interested in the movement was held on the 1st inst. at the rooms of the Society of Arts, Adelphi. Lord Hartington presided, and among those present were Lord Rosebery, Mr. John Morley, Sir Lyon Playfair, Sir John Lubbock, and representatives from Colleges, technical schools, trade-unions, School Boards, national Societies, and Chambers of Commerce.

About 40 members of Parliament were also present. Lord Hartington, in opening the proceedings, said their object was not so much to stimulate public interest in this great question as to consider from a practical point of view the channels into which such interest ought to be directed. He had been struck by the facts relating to technical education at home and abroad which had been presented in very voluminous form to the public in the reports of our Consuls. We had in this country attained to a great industrial and technical supremacy in the world. We had attained this position partly by the possession of great resources in coal and iron and other industrial materials, partly from the character, energy, and industry of our people, and partly—and here he might be trenching upon controversial grounds—from the fact of our having adopted a sound commercial policy. At the same time, concurrently with our attainment of this supremacy, wonderful scientific discoveries had been made, and more and more science was being applied to the industrial occupations of the world. Other nations had been quick to perceive this, and were striving to make their position equal to ours by developing at immense cost to the State and public funds that scientific instruction which would enable their manufactories and workmen to compete successfully with ours. If we were passive in the matter—if we were indolent—it was conceivable not only that foreign nations would rival us, but they might also succeed in passing us, with consequences which it would be difficult to contemplate. If we were satisfied to go on as we were, if we were content to rely in the future as we had done in the past on those advantages which had given us our present position, and if we did not think it necessary to organize more completely our system of technical instruction than at present, that decision should be the result of deliberate and well-formed consideration and not the result of apathy or indolence. Sir Lyon Playfair, moved that the Association be formed, that Lord Hartington be invited to become President, and the following gentlemen Vice-Presidents:—Lord Granville, Lord Ripon, Lord Rosebery, Lord Spencer, the Bishop of London, Mr. Broadhurst, Prof. Huxley, Sir John Lubbock, Mr. Mundella, Sir Lyon Playfair, Sir B. Samuelson, Prof. Stuart, Dr. Sullivan, Sir R. Temple, and Prof. Tyndall. Mr. John Morley, in seconding the motion, said the time for further inquiry had gone past, and the time had arrived when they could no longer with wisdom, or even with safety, delay the movement they that day commenced. The resolution was carried unanimously. Sir J. Lubbock moved the appointment of an executive Committee, which was carried; as was a motion, made by Mr. Mundella and seconded by Lord Rosebery, that those present be invited to join the Council. A discussion ensued on the proposed objects of the Association, after which Sir B. Samuelson moved, and Mr. Howell seconded, a resolution inviting the assistance of large towns and the chief industrial centres. The motion was duly carried, and votes of thanks closed the proceedings.

In his statement on Monday about the progress of business in the House of Commons, Mr. W. H. Smith said: "There is a measure for promoting technical education, which we have every reason to believe will be accepted unanimously by the House—at all events, we hope that a very slight discussion will be sufficient to pass that measure into law."

On July 25, 1837, the first practical essay in telegraph working was made by Messrs. Cooke and Wheatstone between Euston and Camden Town. In the material order of things few more magnificent triumphs have ever been achieved, and it has very properly been decided that the fiftieth anniversary of the occasion shall be celebrated. Some time ago an influential Committee was formed to take the matter into consideration, and the other day there was a well-attended meeting of the members at the offices of the Society of Telegraph-Engineers and Electricians. Mr. Preece, F.R.S., was appointed chairman. In his opening speech he said they had met to make arrangements for a dinner which was to be given in celebration of the jubilee of the telegraph. It was the success of the essay made by Messrs. Cooke and Wheatstone that led to the association of Robert Stephenson, George Parker Bidder, Brunel, and other well-known men in those days, with the telegraph, and from that little beginning they had seen how the telegraphs had spread all over the face of the earth. In England, where the first step was taken, they had succeeded in keeping well in the van, and it was only fitting that such an important event, probably the greatest event that had happened during the long reign of Her Majesty, should be celebrated, and that those who had been instrumental in bringing telegraphy to its present great position should meet together and talk over old times. It so happened that there were several reasons why the celebration should not take place on July 25, which was really the proper day. In the first place, Mondays were busy days with legislators in their House over the way, and it would be extremely difficult to get many of those whom they hoped to attract if the proposed dinner took place on a Monday; again, it was quite impossible on a Government night, like Monday, to get the Postmaster-General, who, it was thought, should take the chair, to attend; and, further, on July 23 there was to be a great naval review, and a great many who would wish to attend the dinner would not be able to get back until late on Monday afternoon. For those reasons it would be difficult to hold the dinner on the 25th, and Wednesday, the 27th, had been suggested as meeting everybody's convenience. He therefore moved that a dinner be held on July 27 to celebrate the jubilee of the telegraph. Mr. Willoughby Smith seconded the motion, which was unanimously agreed to. Discussion followed respecting matters of detail, and an Executive Committee was elected, consisting of Messrs. W. H. Preece, E. Graves, G. H. B. Patey, C. E. Spagnoletti, A. Siemens, and A. Stroh, with Messrs. H. Alabaster and C. H. W. Biggs as Honorary Secretaries, and Mr. F. H. Webb, Secretary of the Society of Telegraph-Engineers and Electricians, as Acting Secretary. The guarantee fund was at once opened, and names were soon down for upwards of £100. The meeting was adjourned until Tuesday, the 12th inst., when the Executive Committee will report as to the progress of the arrangements.

A number of vacation courses in Natural Science will shortly be started in Edinburgh. During the present summer there will be two courses—one on Practical Botany, conducted at the Royal Botanic Gardens of Edinburgh by Mr. G. F. Scott Elliot, an assistant to the Professor of Botany; another on Practical Zoology, conducted at the Scottish Marine Station, Granton, Edinburgh, by Mr. J. Arthur Thomson, Lecturer on Zoology in the School of Medicine, with the co-operation of Mr. J. T. Cunningham, the Superintendent of the Station. These courses ought to be of great service to teachers and others occupied during the University terms, for whom they are primarily intended.

Writing to us from Tashkend on June 12, M. Wilkins says that the city of Vernoje was completely ruined by the earthquake of June 9. More than 800 bodies had been excavated. "The disaster," he says, "is beyond description. Mud and water are said to flow abundantly from the disturbed mountains to the scene of the catastrophe, and many crevasses are noticed in the ground. The exact time of the tremendous shock is given as 4h. 35m. local time. At 4h. 18m. (Tashkend local time) on the same morning, we felt here a flat wave which set in motion suspended objects. Taking into account the difference of longitude between Tashkend and Vernoje and the consequent difference of time, it appears that the wave travelled in a straight line more than 400 miles in the short time of 13·5 minutes, crossing

on the way, in a diagonal direction, the whole western half of the Thian Shan range."

On June 30 a Philippine Exhibition was opened at Madrid by Queen Christina. The most important exhibits are specimens of the natural products, vegetable and mineral, of the Philippine Islands. Some forty natives, male and female, with their native houses and arms, are present. The late King of Spain started the idea of a permanent Colonial Museum, to contain the mercantile products of the Spanish colonies. The opening of this Exhibition is regarded as the first step towards the realization of his scheme.

Another instalment of his valuable work on high temperature dissociations has just been given forth from the laboratory of Prof. Victor Meyer, at Göttingen. The molecular condition of phosphorus, arsenic, and antimony at the highest accessible temperatures has been the subject of this recent work, and the following are the experimental results obtained. As is well known, the experiments of Deville and Troost brought to light the fact that as high as 1040° in the case of phosphorus, and 860° in the case of arsenic, the observed densities are such as can only be explained on the supposition that the molecules of these elements consist of four atoms. J. Mensching and Victor Meyer now show that as the temperature is gradually raised to a red heat the molecular weights begin to diminish—that is, the four-atom molecules commence to break down—and at a white heat so large a number are dissociated that the values obtained for the vapour-densities approximate to those required on the supposition that the molecules each contain but two atoms. Hence, at a white heat the vapour-densities of phosphorus and arsenic are normal, and the molecules consist of the usual two atoms. In the case of antimony, no thoroughly trustworthy work has hitherto been published as to its molecular state, but it has been generally supposed to consist also of four-atom molecules. Mensching and Meyer, however, find that it behaves quite unlike phosphorus and arsenic, inasmuch as immediately on volatilization its density is found to correspond to a molecule of but three atoms, and although dissociation continues to the limit of terrestrially procurable temperatures, yet when this is attained, the level of the normal state is not reached, and more definite results must perforce be deferred until, by some ingenious device, temperatures far higher are obtainable.

The Royal Meteorological Institute of the Netherlands at Utrecht has recently published its *Jaarboek* for 1886, containing observations taken three times daily at ten places, and daily rainfall values at eighty stations. These volumes, which have now been regularly issued for thirty-eight years, form one of the most complete series of meteorological observations in Europe, and they also contain valuable discussions on the climatology of distant parts. The volume now in question contains observations taken at San Salvador (on the Congo) for 1885, and at Djedda (Arabia Felix), Paramaribo (Dutch Guiana), and Culebra (Panama Canal) for 1886. The Director of this Institution (Dr. Buys Ballot) first enunciated the law that now bears his name, showing the universal relation of the direction of the wind to barometric pressure, which has been so instrumental in popularising weather knowledge. This Office also deals largely with maritime meteorology, and has published a long series of papers on this subject entitled "Uitkomsten van wetenschap en ervaring," as well as wind charts for the various oceans.

An unusual number of foreign men of science will be present at the Manchester meeting of the British Association for the Advancement of Science. The following is the first list of foreigners who have accepted invitations to attend the meeting:—*Section A* (*Physics and Mathematics*): Cleveland Abbe, Meteorological Office, Washington; Von Hefner Altneck, Berlin; A. Cornu, Ecole Polytechnique, Paris; A. Crova, Montpellier; J. R. Eastman, U.S. Naval Observatory; W. Foerster, Director of the Berlin Observatory; W. de Fonvielle, Paris; A. Horseman, Heidelberg; F. Kohlrausch, Professor of Physics, Würzburg; A. Kundt, Professor of Physics, Strassburg; William Libbey, Princeton College, N.J.; G. Lippmann, Paris; R. Lipschitz, Professor of Mathematics, Bonn; Malcolm McNeill, Princeton College, N.J.; O. E. Meyer, Breslau; G. Quincke, Professor of Physics, Heidelberg; Schering, Director of the Observatory, Göttingen; Ernst Schroeder, Karlsruhe; J. Violle, Ecole Normale, Paris; E. Warburg, Professor of Physics, Freiburg; H. Wild, St. Petersburg; A. C. Young, Princeton College, N.J. *Section B* (*Chemistry*): A. Bernthsen, Heidelberg; J. W. Brühl, Freiburg; Caro, Mannheim; Le Chatelier, Paris; F. W. Clarke, Washington; De Clermont, Paris; F. B. Fittica, Marburg; R. Fittig, Strassburg; Hempel, Dresden; Reinhardt Hoffman, Biebrich; A. Ladenburg, Kiel; J. W. Langley, University of Michigan; A. Lieben, Vienna; C. Lieberman, Berlin; Oscar Liebreich, Berlin; Lunge, Zurich; J. W. Mallet, University of Virginia; C. A. Martius, Berlin; Mendelejeff, St. Petersburg; Menschutkin, St. Petersburg; Lothar Meyer, Tübingen; Noelting, Muhlhausen; Pauli, Höckst; Silva, Paris; G. Wiedemann, Leipzig; Otto Witt, Berlin; J. Wislicenus, Leipzig. *Section C* (*Geology*): E. Cohen, Greifswald; H. von Dechen, Bonn; Anton Fritsch, Prague; Alfred Nehring, Berlin; Abbé Renard, Bruxelles; F. Zirkel, Leipzig. *Section D* (*Biology*): A. de Bary, Strassburg; Von Boddaert, Cutsem; C. W. Braune, Leipzig; A. Chauveau, Paris; F. Cohn, Breslau; C. Dervalque, Liége; C. Gegenbauer, Heidelberg; Asa Gray, Harvard College, Cambridge, U.S.; W. His, Leipzig; A. Habrecht, Utrecht; Ch. Julin, Liége; F. Kühne, Heidelberg; Count von Solms Lanbach, Göttingen; Lortet, Lyon; Marey, Paris; C. S. Minot, Harvard College; G. S. Morse, Salem, Mass.; P. Preyer, Jena; Pringsheim, Berlin; J. von Sachs, Würzburg; De Saporta, Aix; A. Weismann, Freiburg; R. Wiedersheim, Freiburg. *Section E* (*Geography*): Comodore Jansen, The Hague; M. Lindemann, Bremen; M. Venukoff, Paris. *Section F* (*Economic Science*): Carl Greven, Leyden; Dana Horton; Judge Mackay; A. de Marcoartu, Madrid; Carl Menger, Vienna; *Section G* (*Engineering*): Thos. Egleston, Washington; J. B. Francis, Past President of the American Society of Civil Engineers; A. Gobert, Bruxelles; Quinette de Rochemont, Havre; R. H. Thurston, Sibley College, Cornell University. *Section H* (*Anthropology*): Dr. O. Finsch; Marquis de Nadaillac, Paris.

Messrs. Marcus Ward and Co. will publish, early this autumn, a work, in two volumes, on the Canary Islands. The writer, Mrs. Olivia M. Stone, author of "Norway in June," visited with her husband all the islands of the group—a feat which had never before been accomplished by English people. Illustrations from photographs taken during the tour, and eight maps made from the author's personal observations, will accompany the letterpress.

In a letter printed by us last week, describing a meteor which was seen in West Sussex by daylight, the meteor is said to have "disappeared near the *meridian* of Antares." For "meridian" read "position."

The additions to the Zoological Society's Gardens during the past week include three Blotched Genets (*Genetta tigrina*) from South Africa, presented by Gen. J. J. Bisset; an Ocelot (*Felis pardalis*) from South America, presented by the Earl of Dudley; a Barn Owl (*Strix flammea*), British, presented by Mr. —— Wickham; a White-tailed Sea Eagle (*Haliaetus albicilla*), European, presented by Mr. G. J. Mayer; a Ceylonese Jungle Fowl (*Gallus stanleyi*) from Ceylon, presented by Mr. Hugh Neville; six Corn-Crakes (*Crex pratensis*), British, presented by Mr. G. J. B. Willows; a Magpie (*Pica rustica*) from France, presented by Mr. Walter H. Ince; a Yellow-fronted Amazon

(*Chrysotis ochrocephala*) from Guiana, deposited; six Chinchillas (*Chinchilla lanigera*) from Chili, a Burrowing Owl (*Speotyto cunicularia*) from Buenos Ayres, two Hoopoes (*Upupa epops*), British, a Gould's Monitor (*Varanus gouldi*) from Australia, purchased; two Mule Deer (*Cariacus macrotis*), a Yellow-footed Rock Kangaroo (*Petrogale xanthopus*) born in the Gardens; two Blood-breasted Pigeons (*Phlogœnas cruentata*) bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

RELATIVE POSITIONS OF THE PRINCIPAL STARS IN THE PLEIADES.—We have received vol. i., part 1, of the Transactions of the Astronomical Observatory of Yale University, containing an important paper by Dr. W. L. Elkin, giving the results of his researches with the Yale heliometer on the relative positions of sixty-nine stars situated in the above-mentioned group. The work consists, in reality, of two independent triangulations: one resting on measurements of the distance of each star in the group from each of four stars situated near its outer limits, so that nearly the entire group is inclosed symmetrically by the quadrilateral formed by them; the other resting on measurements of distance and position-angle from Alcyone, the central star of the group. These two independent determinations are in very satisfactory agreement, and Dr. Elkin has thus furnished a most accurate catalogue, for the epoch 1885, of the relative positions in R.A. and declination of these sixty-nine stars. For comparison of his results with the Königsberg places for 1840, Dr. Elkin has adopted the corrections to the latter resulting from Prof. Auwers's researches, and brought up the newly reduced places to 1885, exhibiting the comparison in the form of apparent displacements in R.A. and in declination, the place of Alcyone being made identical in both series. For the six largest cases of relative displacement there is a remarkable community both of direction and amount of apparent motion, and it is remarkable that this general drift is very similar to the reversed absolute motion of Alcyone as given by Newcomb. For two of the stars, Bessel's Nos. 14 and 35, the coincidence is, in Dr. Elkin's opinion, sufficiently close to warrant the deduction that these two stars at least do not belong to, but form only optical members of, the group. It is possible, if not probable, that the other four should also be placed in the same category. The general character of the internal motions of the group appears, however, to be extremely minute, and Dr. Elkin thinks that the hopes of obtaining any clue to the internal mechanism of this cluster seem not likely to be realized in the immediate future. Dr. Elkin also compares his results with the micrometrical measures of M. Wolf at Paris and of Prof. Pritchard at Oxford, and arrives at the conclusion that "the use of the filar micrometer for such large distances as those under consideration is likely to be accompanied with considerable casual error, and, unless great care is taken, with large systematic error. The conclusions of Messrs. Wolf and Pritchard as to the relative motions in the group have thus been unfortunately vitiated, and must be replaced by those formulated" in Dr. Elkin's most able paper.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 JULY 10-16.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on July 10

Sun rises, 3h. 57m.; souths, 12h. 5m. 1·7s.; sets, 20h. 13m.; decl. on meridian, 22° 16′ N.: Sidereal Time at Sunset, 15h. 27m.

Moon (at Last Quarter on July 13) rises, 22h. 32m.*; souths, 3h. 51m.; sets, 9h. 19m.; decl. on meridian, 8° 32′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Decl. on meridian. ° ′
Mercury	6 14	13 40	21 6	15 33 N.
Venus	8 10	15 10	22 10	10 57 N.
Mars	2 21	10 42	19 3	23 59 N.
Jupiter	13 8	18 26	23 44	9 4 S.
Saturn	4 34	12 35	20 36	21 16 N.

* Indicates that the rising is that of the preceding evening.

July.	h.	
12	2	Mercury at greatest distance from the Sun.
13	20	Venus at greatest elongation from the Sun, 46° east.
14	14	Mercury stationary.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·3	81 16 N.	July 12,	22 52	*m*
o Ceti	2 13·6	3 29 S.	,, 11,		*m*
Algol	3 0·8	40 31 N.	,, 12,	21 12	*m*
S Leonis	11 5·0	6 4 N.	,, 13,		*M*
W Virginis	13 20·2	2 48 S.	,, 11,	3 0	*M*
δ Libræ	14 54·9	8 4 S.	,, 15,	23 42	*m*
β Lyræ	18 45·9	33 14 N.	,, 12,	22 0	*M*
R Lyræ	18 51·9	43 48 N.	,, 16,		*M*
δ Cephei	22 25·0	57 50 N.	,, 15,	1 0	*M*
R Pegasi	23 1·0	9 56 N.	,, 13,		*M*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A. °	Decl. °	
From Herculis	271	21 N.	Very slow.
Ophiuchus	280	14 S.	Very slow.
Near π Pegasi	329	36 N.	Swift. Red streaks.
From Andromeda	352	38 N.	Swift.

REPORT OF THE COMMITTEE OF INQUIRY INTO M. PASTEUR'S TREATMENT OF HYDROPHOBIA.

THE following is the text of this important Report to the President of the Local Government Board:—

SIR,—In accordance with the instructions contained in a letter dated April 12, 1886, from your predecessor, the Right Honourable Joseph Chamberlain, M.P., appointing us to be a Committee to inquire into M. Pasteur's treatment of hydrophobia, we beg leave to present to you the following Report.

In order to answer the several questions involved in the inquiry, we found it necessary that some of the members of the Committee should, together with Mr. Victor Horsley, the Secretary, visit Paris, so as to obtain information from M. Pasteur himself, and observe his method of treatment, and investigate a considerable number of the cases of persons inoculated by him; and, further, that a careful series of experiments should be made by Mr. Horsley on the effects of such inoculation on the lower animals. The detailed facts of these observations and experiments are placed in the Appendix to this Report; a summary of them, and the conclusions which we believe may be drawn from them, are given in the next following pages.

The experiments by Mr. Horsley entirely confirm M. Pasteur's discovery of a method by which animals may be protected from the infection of rabies. The general facts proved by them may be thus stated:

If a dog, or rabbit, or other animal be bitten by a rabid dog and die of rabies, a substance can be obtained from its spinal cord which, being inoculated into a healthy dog or other animal, will produce rabies similar to that which would have followed directly from the bite of a rabid animal, or differing only in that the period of incubation between the inoculation and the appearance of the characteristic symptoms of rabies may be altered.

The rabies thus transmitted by inoculation may, by similar inoculations, be transmitted through a succession of rabbits with marked increase of intensity.

But the virus in the spinal cords of rabbits that have thus died of inoculated rabies may be gradually so weakened or attenuated, by drying the cords, in the manner devised by M. Pasteur and related in the Appendix that, after a certain number of days' drying, it may be injected into healthy rabbits or other animals without any danger of producing rabies.

And by using, on each successive day, the virus from a spinal cord dried during a period shorter than that used on the previous day, an animal may be made almost certainly secure against rabies, whether from the bite of a rabid dog or other animal, or from any method of subcutaneous inoculation.

The protection from rabies thus secured is proved by the fact that, if some animals thus protected and others not thus protected be bitten by the same rabid dog, none of the first set will die of

rabies, and, with rare exceptions, all of the second set will so die.

It may, hence, be deemed certain that M. Pasteur has discovered a method of protection from rabies comparable with that which vaccination affords against infection from small-pox. It would be difficult to over-estimate the importance of the discovery, whether for its practical utility or for its application in general pathology. It shows a new method of inoculation, or, as M. Pasteur sometimes calls it, of vaccination, the like of which it may become possible to employ for protection of both men and domestic animals against others of the most intense kinds of virus.

The duration of the immunity from rabies which is conferred by inoculation is not yet determined; but during the two years that have passed since it was first proved there have been no indications of its being limited.

The evidence that an animal may thus, by progressive inoculations, be protected from rabies suggested to M. Pasteur that if any animal or any person, though unprotected, were bitten by a rabid dog, the fatal influence of the virus might be prevented[1] by a timely series of similar progressive inoculations. He has accordingly, in the institution established by him in Paris, thus inoculated a very large number of persons believed to have been bitten by rabid animals; and we have endeavoured to ascertain with what amount of success he has done so.

The question might be answered with numerical accuracy if it were possible to ascertain the relative numbers of cases of hydrophobia occurring among persons of whom, after being similarly bitten by really rabid animals, some were and some were not inoculated. But an accurate numerical estimate of this kind is not possible. For

(1) It is often difficult, and sometimes impossible, to ascertain whether the animals by which people were bitten, and which were believed to be rabid, were really so. They may have escaped, or may have been killed at once, or may have been observed by none but persons quite incompetent to judge of their condition.

(2) The probability of hydrophobia occurring in persons bitten by dogs that were certainly rabid depends very much on the number and character of the bites; whether they are on the face or hands or other naked parts; or, if they have been inflicted on parts covered with clothes, their effects may depend on the texture of the clothes, and the extent to which they are torn; and, in all cases, the amount of bleeding from the wounds may affect the probability of absorption of virus.

(3) In all cases, the probability of infection from bites may be affected by speedy cauterizing or excision of the wounded parts, or by various washings or other methods of treatment.

(4) The bites of different species of animals, and even of different dogs, are, probably, for various reasons, unequally dangerous. Last year, at Deptford, five children were bitten by one dog and all died; in other cases, a dog is said to have bitten twenty persons, of whom only one died. And it is certain that the bites of rabid wolves, and probable that those of rabid cats, are far more dangerous than those of rabid dogs.

The amount of uncertainty due to these and other causes may be expressed by the fact that the percentage of deaths among persons who have been bitten by dogs believed to have been rabid, and who have not been inoculated or otherwise treated, has been, in some groups of cases, estimated at the rate of only 5 per cent., in others at 60 per cent., and in others at various intermediate rates. The mortality from the bites of rabid wolves, also, has been, in different instances, estimated at from 30 to 95 per cent.

To ascertain, as far as possible, the influence of these sources of fallacy in cases inoculated by M. Pasteur, the members of the Committee who went to Paris requested him to enable them to investigate, by personal inquiry, the cases of some of those who had been treated by him. He at once, and very courteously, assented, and the names of 90 persons were taken from his note-books. No selection was made, except that the names were taken from his earliest cases, in which the periods since inoculation were longest, and from those of persons living within reach in Paris, Lyons, and St. Etienne.

[1] The terms referring to "preventive" treatment will be used for that designed to prevent the occurrence of the disease in one already infected; those referring to "protective" treatment for that designed to protect a man or an animal from the risk of becoming infected. And it may be well to state that, though the usual custom is followed of employing the name of "hydrophobia" for the disease in men, and of "rabies" for that in animals, they are really the same disease.

The notes made on the spot concerning all these cases are given in the Appendix, and they include, as far as was possible, the evidence whether the dogs deemed rabid were really so, the situation and kind of bites, the immediate treatment of them, the statements of medical practitioners and veterinary surgeons to whom any useful facts were known.[1]

Among the 90 cases there were 24 in which the patients were bitten on naked parts by undoubtedly rabid dogs, and the wounds were not cauterized or treated in any way likely to have prevented the action of the virus; there were 31 in which there was no clear evidence that the dog was rabid; others in which the bite, though inflicted by undoubtedly rabid animals, having been through clothes, may thus have been rendered harmless. Among these, therefore, it is probable that, even if they had not been inoculated, few would have died. Still, the results observed in the total of the 90 cases may justly be compared with those observed in large numbers of cases similar to these as regards the uncertainties of infection, but not inoculated. The estimates published as to the mortalities in such unassorted cases are, as we have said, widely various. We believe that among the 90 persons, including the 24 bitten on naked parts, not less than eight would have died if they had not been inoculated. At the time of the inquiry, in April and May 1886, which was at least eighteen weeks since the treatment of the bites, not one had shown any signs of hydrophobia, nor has any one of them since died of that disease.

Thus, the personal investigation of M. Pasteur's cases by members of the Committee was, so far as it went, entirely satisfactory, and convinced them of the perfect accuracy of his records.

After the first few months in which M. Pasteur practised his treatment, he was occasionally obliged, in order to quiet fears, to inoculate persons who believed that they had been bitten by rabid animals, but could give no satisfactory evidence of it. It might, therefore, be deemed unjust to estimate the total value of his treatment in the whole of his cases as being more than is represented by the difference between the rate of mortality observed in them and the lowest rate observed in any large number of cases not inoculated. This lowest rate may be taken at 5 per cent. Between October 1885 and the end of December 1886, M. Pasteur inoculated 2682 persons, including 127 who went from this country. Of the whole number, at the rate of 5 per cent., at least 130 should have died. At the end of 1886, the number of deaths stated by M. Vulpian, speaking for M. Pasteur, was 31, including 7 bitten by wolves, in three of whom the symptoms of hydrophobia appeared while they were under treatment, and before the series of inoculations were complete. Since 1886 two more of those inoculated in that year have died of hydrophobia.

The number of deaths assigned by those who have sought to prove the inutility of M. Pasteur's treatment is, as nearly as we can ascertain, 40 out of the 2682; and in this number are included the seven deaths from bites by wolves, and probably not less than four in which it is doubtful whether the deaths were due to hydrophobia or to some other disease. Making fair allowance for uncertainties and for questions which cannot now be settled, we believe it sure that, excluding the deaths after bites by rabid wolves, the proportion of deaths in the 2634 persons bitten by other animals was between 1 and 1·2 per cent., a proportion far lower than the lowest estimated among those not submitted to M. Pasteur's treatment, and showing, even on this lowest estimate, the saving of not less than 100 lives.

The evidence of the utility of M. Pasteur's method, indicated by these numbers, is confirmed by the results obtained in certain groups of his cases.

Of 233 persons bitten by animals in which rabies was proved, either by inoculation from their spinal cords, or by the occurrence of rabies in other animals or in persons bitten by them, only 4 died. Without inoculation it would have been expected that at least 40 would have died.

Among 186 bitten on the head or face by animals in which rabies was proved by experimental inoculations, or was observed by veterinary surgeons, only 9 died, instead of at least 40.

And of 48 bitten by rabid wolves only 9 died; while, without the preventive treatment, the mortality, according to the most probable estimates yet made, would have been nearly 30.

[1] The Committee are much indebted to M. Arloing, Director of the Veterinary School at Lyons; M. Savary, Veterinary Surgeon at Brie-Comte-Robert; and M. Charlois, Veterinary Surgeon at St. Etienne, for assistance in their inquiries.

Between the end of last December and the end of March, M. Pasteur inoculated 509 persons bitten by animals proved to have been rabid, either by inoculation with their spinal cords, or by the deaths of some of those bitten by them, or as certified by veterinary surgeons. Only 2 have died, and one of these was bitten by a wolf a month before inoculation, and died after only three days' treatment. If we omit half of the cases as being too recent, the other 250 have had a mortality of less than 1 per cent., instead of 20 or 30 per cent.

It has been objected that the number treated by M. Pasteur, which, from October 1885 to the end of 1886, included 1929 French and Algerians, was much greater than could reasonably be supposed to have been bitten by rabid animals. But there had hitherto been no careful registration of such cases, and the numbers that have occurred in the present year are not less than in the same part of last year, when the alarm about hydrophobia was greatest.

From the evidence of all these facts, we think it certain that the inoculations practised by M. Pasteur on persons bitten by rabid animals have prevented the occurrence of hydrophobia in a large proportion of those who, if they had not been so inoculated, would have died of that disease. And we believe that the value of his discovery will be found much greater than can be estimated by its present utility, for it shows that it may become possible to avert by inoculation, even after infection, other diseases besides hydrophobia. Some have, indeed, thought it possible to avert small-pox by vaccinating those very recently exposed to its infection; but the evidence of this is, at the best, inconclusive; and M. Pasteur's may justly be deemed the first proved method of overtaking and suppressing by inoculation a process of specific infection. His researches have also added very largely to the knowledge of the pathology of hydrophobia, and have supplied what is of the highest practical value, namely, a sure means of determining whether an animal, which has died under suspicion of rabies, was really affected with that disease or not.

The question has been raised whether M. Pasteur's treatment can be submitted to without danger to health or life; and, in answering it, it is necessary to refer to two different methods of inoculation which he has practised, and which are fully described in the Appendix.

In the first, which may be called the ordinary method, and which has been employed in the very large majority of cases, the preventive material obtained from the spinal cords of rabbits that have died of rabies derived, originally, from rabid dogs is injected under the skin, once a day for ten days, in gradually increasing strengths.

In the second or intensive method (*méthode intensive*) which M. Pasteur adopted for the treatment of cases deemed especially urgent, on account either of the number and position of the bites or of the long time since their infliction, the injections, gradually increasing in strength, were usually made three times on each of the first three days, then once daily for a week, and then in different degrees of frequency for some days more. The highest strength of the injections used in this method was greater than the highest used in the ordinary method, and was such as, if used at first and without the previous injections of less strength, would certainly produce rabies.

By the first or ordinary method, there is no evidence or probability that anyone has been in danger of dying, or has in any degree suffered in health even for any short time. But after the intensive method deaths have occurred under conditions which have suggested that they were due to the inoculations rather than to the infection from the rabid animal.

There is ample reason to believe that in many of the most urgent cases the intensive method was more efficacious than the ordinary method would have been. Thus, M. Pasteur mentions that, of 19 Russians bitten by rabid wolves, 3 treated by the ordinary method died, and the remaining 16, treated by the intensive method, survived; and he contrasts the cases of 6 children, severely bitten on the face, who died after the ordinary treatment, with those of 10 similarly bitten children who were treated by the intensive method, and of whom none died; and M. Vulpian reports that, of 186 persons badly bitten by animals that were most probably rabid, 50 treated by the intensive method survived, and of the remaining 136 treated by the ordinary method 9 died.

The rate of mortality after the intensive method was not greater than that after the ordinary method; for among 624 patients thus treated only 6 died, or, counting one doubtful case, 7. But that which excited suspicion was the manner of death in some of them; and this manner was observed in a man named Goffi, sent from England. On September 4 last, he was severely bitten at the Brown Institution by a rabid cat, to which, in spite of repeated warnings, he exposed his naked hand. Twelve wounds were inflicted. They were at once treated with pure carbolic acid, and, six hours later, he was put under the influence of chloroform at St. Thomas's Hospital, the wounded portions of skin were freely excised, and the wounds thus made were treated with carbolic acid. On the same evening he was sent to Paris, and on the following morning M. Pasteur commenced the intensive treatment, and it was continued during twenty-four days. During all this time the man was repeatedly intoxicated.[1] He once fell into the Seine; and while crossing the Channel on his return home he was severely chilled.

On October 10 he returned to his work, and appeared to be in his usual health; but he became unwell, with pain in the abdomen, like colic, and with pain in the back. On the 18th he had partial motor paralysis in the lower limbs, and on the 19th complete motor paralysis of these limbs and of the trunk, and partial motor paralysis of the upper limbs and face. He was taken to St. Thomas's Hospital, where he died on the 20th.

To the last he was free from all the usual symptoms of hydrophobia, and the progress of his disease and the manner of his death were so similar to those of what is described as acute ascending paralysis, or Landry's paralysis, that a verdict to this effect was given at a coroner's inquest. But the certainty that his death was due to the virus of rabies was proved by experiments by Mr. Horsley. A portion of his spinal cord was taken to provide material for inoculations, and rabbits and a dog inoculated with it died with characteristic signs of paralytic rabies, such as usually occurs in rabbits.

In most of the other cases of death after treatment by the intensive method, the symptoms have been nearly the same as those just related; but in none of them has the same test of death from hydrophobia been applied. The likeness of the symptoms to those of the form of rabies called dumb or paralytic, usually observed in rabbits, has suggested, as we have said, that the deaths were due not to the virus of the rabid dog or cat, but to that injected from the spinal cord of the rabbit. But this is far from certain. In the case of Goffi, especially, the incubation period was such as would have followed the bite of the cat, not the inoculation of highest intensity; and the incubation period in the rabbits and dog inoculated from his spinal cord were such as have been observed after similar inoculations with virus derived, not only from rabbits inoculated in series by M. Pasteur, but from a dog, a cat, and a wolf that died of ordinary rabies. It may well have been, therefore, that the intensive inoculations in him and in the other persons who died after them were not themselves destructive, but that they failed to prevent the rabies which was due to the bites. They may also have modified the form in which the rabies manifested itself; giving it the characters of the paralytic rabies usual in rabbits, instead of the convulsive or violent form usually, but not always,[2] observed in man after bites of cats or dogs.

The question is likely to remain undecided; for to avoid the possible, however improbable, risk of his intensive treatment, M. Pasteur has greatly modified it, and even in this modified form employs it in none but the most urgent cases.

The consideration of the whole subject has naturally raised the question whether rabies and hydrophobia can be prevented in this country.

If the protection by inoculation should prove permanent, the disease might be suppressed by thus inoculating all dogs; but it is not probable that such inoculation would be voluntarily adopted by all owners of dogs, or could be enforced on them.

Police regulations would suffice if they could be rigidly enforced. But to make them effective it would be necessary: (1) that they should order the destruction, under certain conditions, of all dogs having no owners and wandering in either town or country; (2) that the keeping of useless dogs should be discouraged by taxation or other means; (3) that the bringing of

[1] Other cases, as well as this, have led M. Pasteur to believe that the risk of death from hydrophobia is much increased by habits of drunkenness.

[2] Cases of paralytic hydrophobia have been observed, though rarely, in men bitten by rabid animals, and not treated by inoculation. It may, indeed, be suspected that at least some of the cases of "acute ascending paralysis" may have been cases of this form of hydrophobia, although, in the complete absence of the usual violent symptoms, no suspicion of the source of the disease was entertained.

dogs from countries in which rabies is prevalent should be forbidden or subject to quarantine; (4) that, in districts or countries in which rabies is prevalent, the use of muzzles should be compulsory, and dogs out of doors, if not muzzled or led, should be taken by the police as "suspected." An exception might be made for sheep-dogs and others while actually engaged in the purposes for which they are kept.

There are examples sufficient to prove that, by these or similar regulations, rabies, and consequently hydrophobia, would be in this country "stamped out," or reduced to an amount very far less than has hitherto been known.

If it be not thus reduced it may be deemed certain that a large number of persons will every year require treatment by the method of M. Pasteur. The average annual number of deaths from hydrophobia, during the ten years ending 1885, was, in all England, 43; in London alone, 8·5. If, as in the estimates used for judging the utility of that method of treatment, these numbers are taken as representing only 5 per cent. of the persons bitten, the preventive treatment will be required for 860 persons in all England; for 170 in London alone. For it will not be possible to say which among the whole number bitten are not in danger of hydrophobia, and the methods of prevention by cautery, excision, or other treatment, cannot be depended on.

We have the honour to be,

Sir,

Your obedient Servants,

(Signed) JAMES PAGET, Chairman,
T. LAUDER BRUNTON,
GEORGE FLEMING,
JOSEPH LISTER,
RICHARD QUAIN,
HENRY E. ROSCOE,
J. BURDON SANDERSON.

VICTOR HORSLEY, Secretary, June 1887.

The Report is followed by appendices, two of which we reprint:—

Abstract Report of Mr. Horsley's Experiments.

The first object of the experiments was to test M. Pasteur's method of transmitting rabies by inoculation, and to compare its effects with those of rabies due to the bites of dogs found rabid in the streets.[1]

Through the kindness of M. Pasteur, two rabbits inoculated by him were placed at the disposal of the Committee on May 5, 1886, and were conveyed within 24 hours safely to the Brown Institution, where the experiments were carried out by Mr. Horsley.

In these two rabbits the first symptoms of rabies appeared on May 11 and 12, and the disease followed exactly the course described by M. Pasteur.

At first the animals appeared dull, but continued to take food readily until symptoms of paralysis appeared. The first of these symptoms was commencing paralysis of motion of the hind-legs, not accompanied by any loss of sensibility. The paralysis soon extended to the muscles of the fore-legs, and later to those of the head, and the animals died comatose.

After post-mortem examination, portions of the spinal cord of each of these rabbits were crushed, according to M. Pasteur's method, in sterilized broth, and the liquid so obtained was injected beneath the dura mater into four rabbits and the same number of dogs, all being first rendered insensible with chloroform or ether.[2]

Of the four rabbits so inoculated, the first two showed the first symptoms seven days after the inoculation; the third and fourth on the sixth day. The symptoms as well as the incubation period exhibited by these rabbits were exactly the same as were observed in those brought from M. Pasteur's laboratory. Careful notes and photographs were taken in the case of all the animals, in order that the constant and specific nature of the disease might be demonstrated by observations during life and after death. It was also observed that during the incubation period the temperature of the body remained normal, that is, about 39°·4 C. With the first definite symptom the temperature rose to about 40°·4 C., which is the temperature usually observed during the first day of the obvious illness. By the next day it began to fall, and on the third day after the appearance of the first symptom it averaged 37°·5 C. On the last day it was always below normal, and on one occasion fell before death to 24° C. The animals did not appear to suffer any pain whatever in the course of the disease. They were free from the spasms which, in the earlier stages of the malady in man, form so painful a feature of the disease, and indeed the disease in them resembled throughout that rapidly fatal, but painless, disease of man known as acute ascending paralysis.

The post-mortem appearances in the rabbits were remarkably uniform. As a rule nothing abnormal, save congestion, presented itself either in the brain, spinal cord, heart, blood-vessels, or serous membranes. The larynx, pharynx, and, more especially, the epiglottis, and the root of the tongue, were frequently intensely congested. The lungs showed almost invariably capillary congestion; and sometimes small patches resembling broncho-pneumonia were observed. The mucous membrane of the stomach was very markedly congested, and there were at its cardiac extremity numerous hæmorrhages.[1] The constancy of these appearances was most remarkable, and corresponded in every particular with those subsequently observed in rabbits which had died of rabies from the bite of rabid dogs.

Of the four dogs inoculated, the first showed on the eighth day after inoculation an alteration in the voice and commencing excitement; on the following day the excitement became excessive, and the bark was quite characteristic; on the eleventh day the dog was aggressive, notwithstanding slight paralysis of the legs; on the twelfth day the paralysis had increased, and on the next day there was complete paralysis and coma, and death occurred on the fifth day after the onset of the symptoms.

The second dog showed the first symptom on the ninth day after inoculation, when it was very dull and partially paralyzed; its bark was characteristic. Next day the paralysis was almost complete, and on the twelfth day the animal died. This was therefore a case of the rapid paralytic form; whilst in the first dog the disease was of the ordinary furious form of rabies terminating in paralysis.

The third dog showed the first symptom on the ninth day after inoculation, and from that time became gradually paralyzed, and died on the sixteenth day.

The fourth dog showed the first symptom in from eight to nine days after inoculation, and during the first day was extremely aggressive; on the two following days the characteristic bark was observed; and on the twelfth day there was paralysis of the hind-legs; it died on the thirteenth day. Thus the furious form and the paralytic or dumb form of rabies were represented in equal numbers, whereas, in the usual mode of infection by biting, the former is more prevalent.

The post-mortem appearances were as follows:—The brain and central nervous system were in some of the dogs the seat of considerable congestion; in others these organs appeared normal. The serous membranes were perfectly normal; the larynx especially, and sometimes the pharynx, were congested; the lungs always congested, especially in the lower lobes; the heart normal; the blood usually fluid, occasionally with post-mortem clots; the stomach was always found to contain foreign bodies, such as straw; and its mucous membrane was congested, frequently showing numerous hæmorrhages; the small intestine was always empty, and the large glandular organs showed venous congestion.

For the purpose of exact comparison of the disease just described with that produced when rabies is communicated to the rabbit in the ordinary way, some rabbits previously narcotized with ether were caused to be bitten by rabid dogs of the streets, or were inoculated by trephining with material obtained from the spinal cord of dogs or other animals which had died of rabies, and in one instance from that of a man who had died with hydrophobia.

Four series of experiments of observations in which rabbits were bitten by rabid dogs from the streets were made. In one of them the dog by which the rabbit was bitten exhibited the dumb form, in others the furious form, of the disease. In each series excepting the first a large proportion of the rabbits died; the symptoms presenting themselves in these cases were identical with those observed in the rabbits inoculated from M. Pasteur's virus, but the duration of the symptoms was usually longer. As

[1] This expression is adopted from that usual in France, "*rage des rues*."

[2] All the experiments performed in this inquiry were thus made painless.

[1] In some, signs of post-mortem digestion were found.

has been stated, rabbits inoculated by M. Pasteur's virus rarely show symptoms during more than three days before death, whereas the rabbits bitten by rabid dogs from the streets often live for a week after the appearance of the first symptoms.

The post-mortem appearances in the rabbits dying after having been bitten by rabid dogs of the streets were the same as those already described in rabbits inoculated with the virus from M. Pasteur's rabbits.

In the case of rabbits inoculated by trephining with the virus from animals dying of rabies of the streets, the incubation period was from 14 to 21 days. In all cases the symptoms were similar to those produced by M. Pasteur's virus, and those of rabbits bitten by rabid dogs from the streets; but in the prolongation of the disease approached more closely in character to the latter.

The results of these experiments confirm several of the chief observations made by M. Pasteur; especially—

(1) That the virus of rabies may certainly be obtained from the spinal cords of rabbits and other animals that have died of that disease.

(2) That, thus obtained, the virus may be transmitted by inoculation through a succession of animals, without any essential alteration in the nature, though there may be some modifications of the form, of the disease produced by it.

(3) That, in transmission through rabbits, the disease is rendered more intense; both the period of incubation, and the duration of life after the appearance of symptoms of infection, being shortened.

(4) That, in different cases, the disease may be manifested either in the form called dumb or paralytic rabies which is usual in rabbits; or in the furious form usual in dogs; or in forms intermediate between, or combining, both of these, but that in all it is true rabies.

(5) That the period of incubation and the intensity of the symptoms may vary according to the method in which the virus is introduced, the age and strength of the animal, and some other circumstances; but, however variable in its intensity, the essential characters of the disease are still maintained.

The certainty that the virus of rabies can thus be transmitted without essential change made it desirable, in the next place, to ascertain whether, as M. Pasteur states, it can be so attenuated that it may be inoculated without risk to life, and whether animals thus inoculated are thus made safe from rabies. The methods for this protective inoculation which M. Pasteur has employed are described.

To test them, six dogs were "protected" by injecting subcutaneously the emulsions of spinal cords of rabbits which had died of rabies; beginning with that of a cord which had been dried for 14 days, and, on each following day, using that of a cord which had been dried for one day less, till at last that from a fresh cord was used.

None of these dogs suffered from the injections; and when they were completed, the six dogs thus "protected," and two others unprotected, and some rabbits unprotected, were made insensible with ether, and were then bitten by rabid dogs, or by a rabid cat, on an exposed part.

A "protected" dog, No. 1, was bitten on July 8, 1886, by a dog which was paralytically rabid. It remains perfectly well.

An "unprotected" dog, No. 1, was bitten a few minutes afterwards by the same rabid dog, and died paralytically rabid.

A "protected" dog, No. 2, was bitten on November 6, 1886, by a dog which was furiously rabid; it remains well. At the same time, four "unprotected" rabbits were bitten by the same rabid dog, and of these two died of rabies in the usual form (*i.e.* 50 per cent. of animals bitten).

The same results followed with the "protected" dog, No. 3, and the "unprotected" rabbits, bitten at the same time. The dog still lives, the rabbits died of rabies.

The "protected" dogs, Nos. 4 and 5, were bitten on January 20, 1887, by a furiously rabid dog; and on the same day the "unprotected" dog, No. 2, and three "unprotected" rabbits were bitten by the same dog. The "protected" dogs remain well; the "unprotected" dog and two rabbits died with rabies (*i.e.* 75 per cent. of the animals bitten.)

The "protected" dog, No. 6, was bitten on three different occasions by a furiously rabid cat on September 7, 1886; by a furiously rabid dog on October 7, 1886; and by another furious rabid dog on November 6, 1886. It died ten weeks after being bitten for the third time, but not of rabies. It had been suffering with diffuse eczema during the whole of the time that it was under observation, and it died of this. At the post-mortem examination, no indication of rabies was found; and two rabbits, inoculated by trephining with the crushed spinal cord, showed no sign of rabies, either during life or, when they were killed several months afterwards, in any appearance after death. It was thus made certain that the dog was not rabid.

Thus, all the experiments performed by Mr. Horsley have confirmed those of M. Pasteur, and the experiments last described have shown that animals may be protected from rabies by inoculations with material derived from spinal cords prepared after M. Pasteur's method. The protection may be deemed somewhat similar to that given by the inoculation for anthrax, or by vaccination for small-pox, though the theory of the method of inoculation devised by M. Pasteur is very different from that upon which vaccination for small-pox and inoculation for anthrax is based. The further step, the prevention of rabies or hydrophobia in animals or in persons into whom the virus has already been introduced by bites or otherwise, is considered in the body of the Report.

In the course of his experiments, Mr. Horsley observed many interesting facts concerning the modification of the action of the virus according to the method of its inoculation, and the condition of the animal inoculated; but he found nothing to justify a belief that any animal not inoculated is insusceptible of rabies, or that the disease ever arises spontaneously.[1]

Coincidently with these experiments, some were made by Mr. Dowdeswell for the purpose of ascertaining whether any drugs can protect an animal from rabies. Their result is recorded in a paper read before the Royal Society, and may be summed up in the statement that rabies can neither be prevented nor influenced in its course, unless it be for the worse, by any of the drugs that were employed, including allyl alcohol, atropine, benzoate of soda, chloral, cocaine, curare, iodine (dissolved in iodide of potassium), mercuric perchloride, quinine, salol, strychnine, urethane.

M. Pasteur's Methods of Preventive Inoculation.[2]

M. Pasteur believes that the virus of rabies is a living micro-organism, and that, like some others, it produces in the tissues it invades an excretory substance by which, when present in sufficient quantity, its own development and increase are checked, as are those of the yeast ferment by the alcohol produced in the vinous fermentation. In accordance with this theory, he thinks that the spinal cords of animals that have died of rabies contain both the virus and this excretory substance which, practically, may be deemed its antidote. He believes therefore that by injections of an emulsion from such spinal cords into the systems of animals bitten or inoculated with the virus of rabies, the antidote may be able, during the period of incubation, to arrest and prevent the fatal influence of the virus. But, in order to avoid the possibility of injecting a still potent virus, M. Pasteur holds that the virus in the spinal cord must be weakened by drying the cord in a pure and dry atmosphere at a temperature of 20° C.; in which drying the efficiency of the antidote may be reduced to a much less extent than the potency of the virus. By such drying this potency may be so reduced that an emulsion of the dried spinal cord may be injected without any risk of producing rabies: and this risk is in no measure increased by the daily injections of emulsions from cords dried during a gradually less number of days, and which, though more virulent than those first used, still contain a larger proportion of the antidote than of the virus.

In accordance with this theory, the method of the preventive injections first used by M. Pasteur was adjusted in the following manner:—

Days of Inoculation.	1st.	2nd.	3rd.	4th.	5th.	6th.	7th.	8th.	9th.	10th.
Days during which the spinal cord had been dried	14	13	12	11	10	9	8	7	6	5

In consequence of some deaths among those who had been thus treated, M. Pasteur deemed it necessary, in cases of very severe bites and of persons bitten long before the treatment

[1] The minuter facts connected with all these experiments will soon be communicated to one of the scientific Societies.

[2] As derived from the observations made by the Committee, and from a full description supplied by Prof. Dr. Grancher, April 11, 1887.

could be commenced, to increase the intensity of the treatment by more speedily increasing the strength of the injections, by more frequent repetitions of them, and by using on certain days spinal cords dried during only three, two, and one days. Thus in September and October 1886 he adopted the following formula :—

Days of Inoculation.	1st.	2nd.	3rd.	4th.	5th.
Days' drying of the cords	14, 13, 12	11, 10, 9	8, 7	6, 5	4, 3

Days of Inoculation.	6th.	7th.	8th.	9th.	10th.	11th.
Days' drying of the cords	2	1	6, 5	4, 3	2	1

In very severe and perilous cases this course was repeated even three or four times. It was distinguished as the *méthode intensive*, and among such severe cases it was followed by a marked diminution of mortality. But when it appeared possible that it might be dangerous, M. Pasteur changed it for that which he now uses, and which may be thus represented :—

Days of Inoculation.	1st.	2nd.	3rd.	4th.	5th.	6th.	7th.
Days' drying of the cords	14, 13	12, 11	11, 10	10, 10	9, 9	9	8

Days of Inoculation.	8th.	9th.	10th.	11th.	12th.	13th.	14th.	15th
Days' drying of the cords	8	8	7	7	7	6	6	5

The material for injection is prepared by crushing portions of the dried spinal cord, and diffusing them in sterilized broth free from all risk of putrefaction, decomposition, or any change due to the presence of other micro-organisms; and the injection is made with syringes through fine tubular needles into the subcutaneous tissue.

For transmissions of rabies through rabbits, in order to obtain the spinal cords required for its prevention in other animals, injections of virus of highest intensity are made through minute holes in the skull into the space under the dura mater or fibrous covering of the brain.

The materials for the protective inoculations are prepared in the same manner as those for the preventive, from spinal cords dried from ten days to one day.

UNIVERSITY COLLEGES AND THE STATE.

ON Thursday last, June 30, a deputation consisting of members of Parliament and others had an interview with the Chancellor of the Exchequer, who was accompanied by Mr. Jackson, M.P., to urge that Government assistance should be extended to local university colleges situated in various parts of the country. Among those present were Sir John Lubbock, M.P., Mr. Mundella, M.P., Mr. J. Chamberlain, M.P., Sir Lyon Playfair, M.P., Mr. Bryce, M.P., Mr. Arnold Morley, M.P., Mr. Jesse Collings, M.P., Mr. R. Chamberlain, M.P., Sir U. Kay-Shuttleworth, M.P., Mr. Theodore Fry, M.P., Mr. Burt, M.P., Sir Henry Roscoe, M.P., Sir A. K. Rollit, M.P., Prof. Stuart, M.P., Sir Bernhard Samuelson, M.P., Mr. Howard Vincent, M.P., Sir W. H. Houldsworth, M.P., Dr. Percival, and Sir Philip Magnus.

Sir John Lubbock, as the representative of the University of London, introduced the deputation. Their request was that a Parliamentary grant should be made to English colleges, as was already made to those in Ireland, Scotland, and Wales. The colleges on behalf of which they appeared were doing excellent work, but were greatly hampered for want of funds. The claims of these colleges were not based alone on their services to learning and study; they were calculated to contribute largely to the material prosperity of the country. We now imported £150,000,000 worth of food annually, and our population increased at the rate of about 350,000 a year. How were so many to be fed, and how could a revival and return of trade be promoted? Our rivalry with foreign nations was now not on the battlefield but in the manufactory and the workshop; and it was none the less severe because it was a competition rather than a contest. The need of the assistance for which they asked was very pressing. Without going into details as to particular colleges, he observed that the more recent institutions were generally spending more than their income, and even the oldest and the richest were sadly crippled for want of funds. It was found practically impossible to increase the subscriptions, and local authorities, as a rule, had no power to supplement their funds. As to raising the fees so as to make the colleges self-supporting, that might be possible but would be very undesirable. He only wished the fees could be abolished altogether, for those receiving education at the colleges benefited not only themselves but the whole nation. As to the expenditure on education, it was in the opinion of some people very large, but it was small in comparison with other items. Our ignorance cost us very much more than our education. Moreover, the principle for which they contended had been conceded in regard to Scotland, Ireland, and Wales. The grants to Irish colleges amounted to £25,000, to Scotland £16,000, and to Wales £12,000. The University of Glasgow had a special grant of £150,000 for building. None of the English colleges had such aid. Their request simply was that in this matter of education England should be treated in the same way as Ireland, Scotland, and Wales.

Mr. J. Chamberlain said that he attended as the representative of Mason College, Birmingham. Their case was the same in principle as that of all the other colleges. They urged that State-aided education had been accepted in principle in England and in all other countries, but in England alone we had not followed out the principle to its logical conclusion. We had stopped at the lower grade, and in this respect had made a great mistake. If it was of national importance that every one should have placed within his reach the instruments of education, it was of equal importance that they should be stimulated and encouraged to make use of these facilities. An attempt had been made in some halting fashion to redress the inequality in which this country was placed. The Charity Commissioners had recently been diverting funds which were, to some extent at all events, intended for the benefit of the poorer classes of the population to the purposes of higher secondary education. That practice was open to very serious objection, because it was robbing Peter to pay Paul; and also because under that system nothing whatever was done for the colleges represented by the deputation, which were carrying on and extending the education given in the primary and secondary schools. The enormous development of primary and secondary education had created a demand for higher education. Proof of that was to be found in the fact that, although the institutions now represented were nearly all of them the creation of the present generation, they had had, in spite of deficiency of means, the most remarkable success; and the daily increasing number of their students showed that they were established to meet a real want. The pressure of commercial competition came almost exclusively from those nations in which technical instruction and higher education had been developed and stimulated by the action of the State. The demand now made upon the Government was really a very moderate one, and the sum asked for was never likely to assume any very large amount. He believed the grants for primary education amounted to something between £2,000,000 and £3,000,000 a year, and that the additional grant now asked for would only amount to something like £50,000.

Mr. Goschen,—Will the deputation be able to supply me with a scheme for the distribution of the £50,000 or with the principle?

Mr. Chamberlain replied that, in his opinion, the grants should be made conditional upon further local aid. In that way the Treasury would be able to distinguish the colleges which were entitled to share in the grant.

Mr. Goschen,—Conditional upon further local aid?

Mr. Chamberlain,—Proportioned, in the first place, to the number of students, and, in the second place, conditional upon the amount of local aid.

Mr. Mundella,—Not in all cases further local aid.

Mr. Chamberlain agreed with Mr. Mundella that in some

cases large local contributions were being made, and those cases should be taken into account. In conclusion, he urged upon the Government the consideration of a nationality that was sometimes apparently forgotten, and in the name of the 25 millions of English population he asked that they should receive a recognition in the matter of education proportionate to that given to Ireland, Scotland, and Wales.

Mr. Mundella pointed out that there were precedents for what they were asking in the grants given in Scotland, Wales, and elsewhere, and, in his opinion, Sir John Lubbock had rather underrated the benefit which schools in Scotland derived from the system. The high schools in Scotland were aided by grants out of the rates, and all middle-class education was largely supplemented by grants. In every country in Europe which really rivalled England a first-class technical education was within the reach of the humblest classes. In France very much the same education as given in our colleges could be obtained free and at the expense of the State. There was at present no power to aid the colleges in England, but he felt sure the Education Department would have no difficulty in framing a scheme for the purpose; and he ventured to hope the Chancellor of the Exchequer would assist them, and that grants should be allowed to these institutions in proportion to the efficiency of each college.

Sir Lyon Playfair observed that as the Government was going to introduce a Bill for giving to School Boards and other authorities power of rating for higher and technical education, he thought it would be well to extend the operation of the Bill by giving power to the same bodies to rate for higher colleges. The authorities were quite ready to be rated, and only wanted the necessary powers. The experience of commercial nations throughout the world was that the competition of industries was a competition of intellect. The difference between the policy of this and other countries was that while in other countries the State recognized the fruits of education and acted upon their perception of them, we left the first steps to the efforts of intelligent men in various localities. These men had now done their part, and, in consequence of the action of the English Government in the past, he thought it was the duty of the Government to come to the rescue of this small and highly intelligent body of men who had got up these colleges, and give to them that permanence which they were not likely to have without some small support from the public funds.

Sir Bernhard Samuelson said that in the course of the investigations of the Technical Education Commission he had the opportunity of visiting nearly all the colleges now appealing for aid, and, as far as his judgment enabled him to form an opinion, he must say that, considering the means at their disposal, these colleges were doing a thoroughly good work, and a work which deserved the encouragement of the community.

Mr. Thomas Burt, M.P., was not quite clear that this was the best way of spending money educationally, but he was quite sure it was a very good one. He could testify to the value conferred upon the miners and the industrial classes generally of the North by the College of Science in Newcastle. That institution had been greatly crippled in its resources. There was among the miners a widespread desire for improved education, and the College of Science and the University Extension lectures had not only given positive instruction of a valuable kind, but had conferred still greater advantages by creating and stimulating a desire on the part of the industrial classes for improved education. If the Government could see their way to help this and kindred institutions, a very great benefit would be conferred on the industrial classes of the country.

After a few words from Prof. Tilden (who differed from other speakers as to charging local rates), Dr. Perceval, and Sir George Young,

Mr. Goschen, in reply, said:—When Mr. Tilden sat down just now I was thinking on the whole that it was rather advisable for the deputation that the list of speakers was very nearly exhausted, because the differences of opinion began to be manifest. Mr. Tilden objected to powers being given to corporations or boroughs to rate themselves, whereas one of the objects of the deputation, or, at all events, one of the suggestions made to me during the course of this deputation, was that we should be sure to give powers to localities to rate themselves for these purposes. I do not know what the view would be of the deputation upon the subject, but I suspect that the bulk of the deputation is in favour of power being given, which of course would be optional, for large towns to rate themselves for this purpose. Mr. Tilden argued that it would be unfair that a college which drew students from other quarters should be supported by local rates; but I am inclined to think that that is a dangerous argument to use, because you might find whole masses of the population—the agricultural population, for instance, which would derive comparatively little advantage from these colleges—who might say that they would not wish to be taxed towards national funds which were to be applied to the big towns for the support of their colleges. So that I think it is rather a dangerous argument, and I further think the towns derive a very considerable advantage from having these large institutions, and that they ought not to look too narrowly to the area over which those rates would be charged. Then, Mr. Burt, I think, made a remark which to my ears was rather significant—that though this was a good way of spending money, he was not sure that this was the best way of spending money upon education. The interpretation I put upon that remark was that probably a further demand would be made upon the national purse for educational assistance in other directions which, in Mr. Burt's opinion, would be the best. No one is more aware, gentlemen, than you, who have studied this subject so deeply, that there are many directions in which, and many points of view from which, this educational question has to be considered. I am glad to have received this most important deputation. I might almost call it in some respects a formidable deputation; but I know that this is not the only direction in which pressure would be put upon the Treasury with regard to education. You represent here what I understand to be the higher forms of technical, scientific, and I think you may also say of literary instruction. There is the elementary education, and the expense attendant upon that; then there is the Science and Art Department, which in some respects is apart from the elementary education; and there remains a field for which I am sure pressure will be put upon the Treasury, which is that technical education which lies between the elementary education and that higher form of education which I understand these colleges give. I make these remarks to show that it would be impossible, I think, for the Government—though, of course, upon that it would be rather for the Education Department than for the Chancellor of the Exchequer to speak—to look upon this matter simply from a partial point of view. We must review the whole of the demands which are likely to be made upon the Government for educational purposes. It is certain that the comparative test which has been applied by this deputation is a very dangerous one to the finances of the State. The grant to certain colleges is certain to lead to a grant to other colleges; and if some of the gentlemen present had heard how the Scotch gentlemen argued that their case was practically peculiar, and that the assistance given to the Scotch Universities could not be possibly made applicable to the English colleges, they would see the scope of the remarks which I have made. I am far from saying that, while there is this serious pressure likely to be put upon the Government, this is not a question which must be carefully and deliberately faced, and looked upon in all its bearings. You have come to me to-day, I presume, in order to remove what we may call any financial scruples with regard to your proposals; but, of course, it would mainly rest with the Education Department to work out any system if these colleges are to be granted assistance upon the scale which you suggest, and so you will not expect me to give you any declaration of policy to-day. But I presume that you are anxious that the arguments you have used should sink into my mind, in order to remove any opposition I might make financially to proposals that would be made by other departments and through other channels. Now, from the financial point of view, I gather that you assent to certain propositions, and the main of those propositions is that assistance should only be given by the State where there has been a distinct local effort in support. I admit that we cannot argue any more that the State should not aid education up to a certain line. That line seems to be by public opinion raised every day, and while formerly it was only the very poor, now it is a higher class; and so from class to class it appears to me the demand for State education is rising very rapidly, perhaps I might say very formidably. But I am glad to take note of the fact that, anxious as you are, representing as you do immense educational efforts in various parts of the country, you do not wish in any way to stop that magnificent flow of private contributions towards education which has been the glory of this country in many ways. It would be deplorable if by the action of the State you were to arrest that action;

you are rather anxious that that action should be stimulated by moderate contributions from the public funds and from rates. As to contributions from rates, I think that as the municipal institutions of our country are more and more reformed and developed, and the more power is given to them in the course of that process of decentralization which is now accepted by almost all politicians, the more power you may give to these localities to act with a certain freedom in the way of assisting such institutions as they think are calculated to advance their interests in every way. There is one point on which I should like to say something from a personal point of view. When I made a remark that your colleges were partly literary and partly technical, I did not wish to convey the impression that I think colleges for general culture deserve less recognition than colleges for technical education. I think that they must to a very great extent stand or fall together, and that it would be an error—though I am aware there may be others here who take a different view—if technical education were too much to displace that general education and development of the intellect which surely must always be one of the great objects of education in every form. I do not know whether the sum which was first mentioned, I think, by Sir John Lubbock has been arrived at by any general agreement, college by college, or whether it is a mere general guess. But I am sure it would be necessary, as a preliminary examination of the case which you have put before me, that there should be some standard suggested, either of numbers or of local contribution, and also of work, before the matter could be taken into serious practical consideration; because not only are there these twelve colleges, but I fancy that, as soon as any arrangement had been made in favour of them, we should find another list of colleges, not precisely on the same footing, but which were sufficiently strong to make a kind of claim on that comparative system which is constantly increasing the national expenditure. I think those of the deputation who are members of Parliament will acknowledge that it would be perfectly impossible to deal with the matter in the supplementary estimate this year, even if we assented to it, without much further examination, for it is really the Education Department which must examine this matter. I have not had the opportunity of consulting my colleagues on the magnitude of the sum which you suggest, or on the general principle. All I can say to-day is that I am glad to receive the suggestions which you have made; that I recognize, of course, the great importance of further developing technical and scientific education; but I cannot pledge myself to any particular sum or to any particular mode of carrying out your wishes. The deputation may rely on the Government giving the matter its most serious attention, and we shall be most willing to receive suggestions from such men as Sir Lyon Playfair, Sir John Lubbock, Mr. Mundella, and the other gentlemen who take so deep an interest in education, to see what practical shape can be given to the wishes of the deputation.

Mr. Mundella observed that the condition of some of the colleges was such that it was desirable that the intentions of the Government should be known at the earliest possible moment.

Sir John Lubbock moved, and the Mayor of Sheffield seconded, "That the thanks of the deputation be given to Mr. Goschen for his courtesy."

The deputation then withdrew.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, June 16.—"Dispersion Equivalents," Part I. By J. H. Gladstone, Ph.D., F.R.S.

The object of this paper was to bring to the notice, especially of chemists, the subject of dispersion equivalents; a property of bodies similar to the refraction equivalents which are now generally recognized. In the paper of Gladstone and Dale in the Phil. Trans. for 1863 they had adopted the difference between the refractive indices for the solar lines A and H as the measure of dispersion. This, divided by the density, gave the specific dispersion. In 1866 they multiplied this by the atomic weight, and termed the product the dispersion equivalent. The subject has scarcely been touched since that time either by English or Continental observers.

The author holds that the following conclusions are warranted by the accumulated data:—

(1) That dispersion, like refraction, is primarily a question of the atomic constitution of the body: the general rule being that the dispersion equivalent of a compound is the sum of the dispersion equivalents of its constituents.

(2) That the dispersion of a compound, like its refraction, is modified by profound differences of constitution; such as, changes of atomicity.

(3) That the dispersion frequently reveals differences of constitution at present unrecognized by chemists, and not expressed by our formulæ.

The dispersion equivalents of a few elements may be determined by direct observation, such as phosphorus, sulphur, selenium, &c.; but more important results have been obtained from organic substances by comparing the dispersion equivalents of different liquid or dissolved compounds of carbon.

From a consideration of the data afforded by Continental observers, or obtained by the author himself, the following table has been drawn up. The dispersion figures must be taken merely as approximate.

Substance.	Atomic weight	Refraction equivalent A	Dispersion equiv. H—A.
Phosphorus	31	18·3	3·0
Sulphur, double bond	32	16·0	2·6
,, single bonds	,,	14·0	1·2
Hydrogen	1	1·3	0·04
Carbon	12	5·0	0·26
,,	,,	6·1 ?	0·51
,,	,,	6·1	0·66
Oxygen, double bond	16	3·4	0·18
,, single bonds	,,	2·8	0·10
Chlorine	35·5	9·9	0·50
Bromine	80	15·3	1·22
Iodine	127	24·5	3·65
Nitrogen	14	4·1	0·10
CH_2	14	7·6	0·34
NO_2	46	11·8	0·82

An examination of many salts of potassium and sodium in aqueous solutions led to the conclusion that there was always a difference of about 0·09 in their dispersion equivalents; but it was not so easy to determine the actual dispersive value of the metals in question, the determinations of potassium from different salts varying from 0·40 to 0·59.

The main conclusion is that the specific dispersive energy of a compound body is a physical property analogous to, but distinct from, its specific refractive energy; and that the phenomena of dispersion are capable in like manner of throwing light upon chemical structure.

Royal Microscopical Society, June 8.—The Rev. Dr. Dallinger, F.R.S., President, in the chair.—Dr. E. M. Crookshank exhibited a series of cultivations of micro organisms, and called attention to the somewhat unusual circumstance of being able to show such a typical series all growing at the same time. One of the specimens shown was a chromogenic *Spirillum*, which had developed its colour in the depths of the gelatine, contrary to the general rule. He also showed a micro-organism which had been said to cause the swine fever—or, rather, swine erysipelas—in Germany. It was to be noted that in Germany there had been many cases of swine disease, and that a different organism had been found associated with it there from the one found here, and recognized as the cause of Dr. Klein's swine fever. So far as he (Dr. Crookshank) had been able to make out, they were not identical, the German form being an extremely minute Bacillus forming only a cloudy appearance and seeming to be similar to mouse septicæmia. He thought there was good ground for regarding the two diseases as distinct from each other, the German form being swine erysipelas as distinct from swine fever. He also exhibited an example of a Bacillus obtained from putrid fish, which caused the remarkable phosphorescence frequently noticed when fish was decaying.—Mr. Freeman exhibited a number of series of sections of the anatomy of spiders, worms, &c., made by Mr. Underhill at Oxford.—Mr. Eve called attention to some specimens of *Actinomyces* from the jaw of an ox, and described the effect of the disease upon the animal.—Prof. Rupert Jones and Mr. C. D. Sherborne's paper "On the Foraminifera, with especial reference to their varia-

bility of form, illustrated by the Cristellarians," was read.—Mr. G. Massee gave a *résumé* of his paper on the genus Lycoperdon, illustrating the subject by drawings on the blackboard.—Prof. Bell said that the Fellows of the Society would remember that in the course of last winter he described what he had observed in some diseased grouse which had been sent to him for examination. Within the last few weeks, the disease, whatever it might be, had been killing grouse in considerable numbers on the moors in the south-west of Scotland. He had received some of these grouse, and examined them very carefully to see if he could discover any cause of death. In the case of the first, though there were tapeworms, there was no evidence that they were the cause of death; in the second case the birds had died from inflammation of the intestines, the cause of which was not quite clear; and in the third case they died of *Strongylus*. It would therefore appear that what was called "grouse disease" must be either more than one disease, or it must be a disease which could kill its victims in different stages. He was himself disposed to think that there was more than one cause of disease, but up to that time there was no diagnostic sign internally to show conclusively what those causes were.—Mr. Grenfell's paper on "New species of *Scyphydia* and *Disophysis*" was read.

PARIS.

Academy of Sciences, June 27.—M. Hervé Mangon in the chair.—Remarks accompanying the presentation of a volume on the geodetic and astronomic junction of Algeria with Spain, by General Perrier. In this work, published at the joint expense of the French and Spanish Governments, a detailed account is given of the methods of observation and of the results obtained by the protracted operations which secure for the physical science of the globe the accurate measurement of an arc of the meridian of over 27° comprised between the Shetland Islands and Laghwat in Algeria.—Remarks accompanying the presentation of the first volume of a course of infinitesimal analysis intended for the use of persons who study this science with a view to its mechanical and physical applications, by M. Boussinesq. This work, the first volume of which deals with the differential calculus, is addressed more especially to those physicists, naturalists, engineers, and others, who are little accustomed to the treatment of algebraic formulas, but who, for their special purposes, feel the want of understanding in its essence and chief results the calculus of the infinitely little, or of continuous functions.—Memoir on submarine sound-signals, by M. Brillouin. The two chief results already obtained are transmission of sound to a distance of thirty-five kilometres, and the neutralization of all violent surface disturbances, such as thunderstorms or hurricanes. A summary description is given of the apparatus, together with a general statement of the circumstances under which these signals might be used with advantage.—Observations of Barnard's Comet (May 12, 1887), made at the Bordeaux Observatory with the 0·38 m. equatorial, by MM. G. Rayet, Flamme, and F. Courty.—Observations of a planet sighted at the Observatory of Marseilles, by M. Borrelly. The observations of this body, which is of the twelfth magnitude, extend from June 9 to June 19 inclusive.—On linear differential equations of the third order, by M. Paul Painlevé. In supplement to the paper published in the *Comptes rendus* of May 31, the author here deals with the linear and homogeneous equation of the third order—

$$y''' + ay'' + by' + cy = 0;$$

and he arrives at the general conclusion that, given a linear and homogeneous equation of the third order, it may always be ascertained, by a limited number of purely algebraic operations, whether its integral be algebraic, or the equation may be reduced to a quadrature.—Determination of the quantity of bisulphate of potassa in a diluted liquor, by M. E. Bouty. The author here deals with the difficulty of determining this quantity, which arises from the fact that in diluted solutions the bisulphate of potassa is always accompanied by sulphuric acid and sulphate of potassa. He shows that the bisulphate is stable especially in hot and concentrated liquors, and that the proportion of this salt increases with the excess of one or other of the reacting bodies.—On the ammoniacal vanadates, by M. A. Ditte. The vanadates here treated are those of methylamine, of ethylamine, ammoniaco-magnesian vanadate, and the double ammoniacal vanadates. The general study of these vanadates, prepared by the dry and wet processes, shows that all these compound bodies are reducible to a few well-defined types and simple formulas, such as: $3VO_5,MO$, $2VO_5,MO$, $3VO_5,2MO$, for the acids; VO_5,MO for the neutral vanadates; $VO_5,2MO$, $VO_5,3MO$, $VO_5,4MO$, for the basic salts, apart from the water, the quantity of which varies according to the circumstances in which the crystallization is effected.—Solubility of uric acid in water, by MM. Ch. Blarez and G. Denigès. For the determination of this point the authors have applied the process of analysis by chameleon indicated in their previous note.—On the hydrochlorate of ferric chloride, by M. Paul Sabatier. M. Engel having recently announced that he had succeeded in preparing this substance, which had been anticipated but not isolated by M. Sabatier, the author remarks that so early as 1881 he had obtained and fixed the composition of the hydrochlorate of ferric chloride (*Bulletin de la Société Chimique*, second series, p. 197, 1881).—On the identity of dambose and inosite, by M. Maquenne. A careful study of a remarkably pure specimen of dambonite prepared according to M. Girard's indications from the caoutchouc of the Gaboon, shows that dambose is identical in every respect with the inosite already described by the author. Hence dambonite should be considered as the dimethylene of inosite, and the term dambose should be replaced in chemical nomenclature by that of inosite, which has the right of priority and is in other respects more convenient.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Fungi, Mycetozoa, and Bacteria: A. de Bary; translated by Garnsley and Bayley (Clarendon Press).—Physiology of Plants: J. von Sachs; translated by H. Marshall Ward (Clarendon Press).—La Exposicion Nacional de Venezuela en 1883, tomo i., Texto: A. Ernst (Carácas).—Transactions and Proceedings of the Royal Society of Victoria, vol. xxiii. (Williams and Norgate).—Transactions of the Astronomical Observatory of Yale University, vol. i. part 1 (New Haven).—Report on the Mining Industries of the United States: R. Pumpelly (Washington).—Proceedings of the American Association for the Promotion of Science, August 1885 (Salem).—Bird-Life in England: E. L. Arnold (Chatto and Windus).—Text-book of Gunnery, new edition: Major G. Mackinlay.—The Owens College Course of Practical Organic Chemistry: J. B. Cohen (Macmillan).—Hay Fever and Paroxysmal Sneezing, 4th edition: Morell Mackenzie (Churchill).—Technical School and College Building: E. C. Robins (Whittaker).—Melting and Boiling Point Tables, vol. ii.: T. Carnelly (Harrison).—Smithsonian Report, 1885, part 1 (Washington).—A Contribution to the Study of Well-Waters (Harrison).—Peabody Institute, Baltimore, 26th Annual Report.—Electric Light Primer: C. S. Levey (New York).—Beiblätter zu den Annalen der Physik und Chemie, 1887, No. 6 (Barth, Leipzig).—Beiträge zur Biologie und Pflanzen, v. Band, 1 Heft (Breslau).—American Journal of Mathematics, vol. ix. No. 4.

CONTENTS.

	PAGE
Professor Tyndall and the Scientific Movement	217
The Geology of England and Wales	218
A Treatise on Geometrical Optics. By J. Larmor	219
Our Book Shelf :—	
Karr: "Shores and Alps of Alaska"	220
Letters to the Editor :—	
Relation of Coal-Dust to Explosions in Mines.—Arthur Watts	221
Science for Artists.—Edwd. L. Garbett	221
Weight, Mass, and Force.—R. B. Hayward, F.R.S.	221
Upper Cloud Movements in the Equatorial Regions of the Atlantic.—Hon. Ralph Abercromby	222
Fish Dying.—F. T. Mott	222
The Dinner to Professor Tyndall	222
The Eleven-Year Periodical Fluctuation of the Carnatic Rainfall. By Henry F. Blanford, F.R.S.	227
Notes	229
Our Astronomical Column :—	
Relative Positions of the Principal Stars in the Pleiades	232
Astronomical Phenomena for the Week 1887 July 10-16	232
Report of the Committee of Inquiry into M. Pasteur's Treatment of Hydrophobia	232
University Colleges and the State	237
Societies and Academies	239
Books, Pamphlets, and Serials Received	240

THURSDAY, JULY 21, 1887.

THE MINING INDUSTRY OF NEW ZEALAND.

Report on the Mining Industry of New Zealand. (Papers laid before Parliament, Session 1886.) 8vo, pp. 323. (Wellington, New Zealand, 1887.)

The Hand-book of New Zealand Mines. With Maps and Illustrations. 8vo, pp. 519. (Wellington, New Zealand, 1887.)

THESE volumes, which cover the same ground, and to some extent reproduce the same information, are in great part the result of a personal investigation of the mining districts of our great antipodean colony, made by the Hon. Mr. W. J. M. Larnach, C.M.G., the Minister of Mines. From the Report, which is about six months older than the Hand-book, we learn that the latter has been compiled by the officers of the Mining Department, under the direction of the Minister, in order to furnish systematic information as to the area of mining claims, and as to other particulars concerning the working of mines, which has not hitherto been available. This result has been fairly well attained in the volume before us, which is a valuable summary, arranged topographically, of the condition of the mines actually at work, the description of each district being preceded by an historical sketch of the early explorations. Among these, that describing the progress of discovery on the west coast of the Middle Island is especially interesting, as it goes back as far as 1836, when an early settler, named Toms, "on one occasion was caught and thrown down by a large seal, receiving a severe bite on the thigh, but he escaped death by dealing it some hard blows with his fist on the nose." Other and more serious difficulties were encountered from the opposition of the native inhabitants, whose interests were finally purchased by Sir George Grey and the successive Governors, subject to certain reserves, which at the present time produce an income of about £4000 per annum, and as there are only about a hundred natives on the west coast, they are comfortably fed, housed, and clad, peaceable and sober, and generally respected by their European neighbours. From this part of the colony gold was exported of the value of nearly £12,000,000 sterling between 1864 and 1873, and the yield, though diminished, still continues, with the prospect that the product of alluvial rocks will be more than eclipsed by that of the quartz reefs, some of which have been proved to be extraordinarily rich. The total produce of gold in New Zealand between 1853 and the end of 1885, according to the Report, is 10,789,560 ounces, valued at £42,327,907 sterling, and the Hand-book gives the area of country proved to be auriferous in the three islands as about 21,000 square miles.

The product next in importance to gold, although perhaps it is scarcely to be classed as a mineral, is kauri gum, which is produced at the rate of about 6000 tons annually from deposits in the North Island, which have already yielded upwards of £3,500,000 sterling to the wealth of the colony. The prosperity of Auckland has been largely aided by its kauri gum fields, and the valuable kauri tree, which is only found in the northern forests of the North Island.

The coal of New Zealand seems to be largely of the character of lignite, though some portion is of a more highly carbonaceous character. The output at present is little in excess of 500,000 tons, which suffices for about three-quarters of the consumption of the colony. Several other minerals have been produced in small quantities, but their aggregate value is insignificant when compared with that of the three staples noticed above.

In going over the detailed accounts of the different gold-mines, given in both volumes, we cannot but be struck by the great diversity of the character of the deposits, and this, as might be expected, has led to several interesting modifications in the method of working. Among the more remarkable of these are, the use of a steam dredger for working auriferous alluvial gravels in the channel of the Molyneux River, and a method of lifting similar materials by a water-jet aspirator applied at Gabriel's gully in the Tuapeka district. These are described at some length, but the descriptions and illustrations are not as full and precise as they might be, considering the interest of the subjects. Another novelty is the use of electricity on the large scale for driving a stamping mill at the Phenix Mine, in Otago. The current produced by a pair of turbines of about 100 horse-power and two Brush dynamos is transmitted to a distance of about two miles to the crushing battery, which contains thirty heads of stamps and is driven by a Victoria electromotor and a Leffel turbine conjointly. This is probably the largest application of electric power to mining purposes that has yet been made.

Mining in New Zealand appears to receive greater support from the State than is customary in most other countries, as not only are large sums devoted to the opening up of roads and pack trails through the country, but contributions are made towards the construction of water races and channels for tailings, and subsidies are paid towards prospecting in different localities. These grants are made contingently upon much larger sums being furnished by local or individual effort, and, according to the testimony of the Reports, have been of great value in encouraging discoverers.

A point of interest in connexion with the economics of New Zealand mining is the general establishment of local schools of mines, or, as they are called in some localities, chemistry clubs, in the different mining centres. These are organized apparently on a system somewhat similar to that of the science classes of the Science and Art Department, the instruction being given to the members by means of a staff of seven teachers under the charge of Prof. J. G. Black, of the University of Otago, who travels through the different districts giving lectures and laboratory demonstrations, for periods varying from two to five months at each, according to its size and importance. The course of instruction includes mineral chemistry and assaying, mineralogy and metallurgy, and provision is being made for the addition of the subjects of mining engineering and surveying. The results expected from the scheme are set forth in full, from which it appears that miners will be able to assay ores and metals of every kind, be able to assay their own bullion, and become generally familiar with the metal-

lurgy of the precious metals. Such results will probably not be realized in their entirety, neither is it desirable that they should be, as the presence of a well-educated specialist, an assayer or smelter, for example, may often be of more permanent value to a district than the necessarily superficial knowledge of subjects not immediately connected with their own occupation that the local miners are likely to acquire under the scheme; but there can be no doubt that great good will result from giving them an intelligent interest in mineralogy, and the observation of the phenomena brought under their notice when at their own particular work.

The Hand-book concludes with a description of the principal forest trees of New Zealand, taken from Dr. Hector's "Hand-book of New Zealand." It has also several maps, supplied by Dr. Hector and Mr. Gordon, of the Mines Department. The greater part of the material has been collected by Mr. Patrick Galvin, of Wellington.

We are sorry to see that in the final paragraphs of the preface, Mr. Larnach appeals to the honourable gentleman who may succeed him to improve the work in a second edition; from which we infer that the author has fallen a victim to a Ministerial crisis. If it be so, we have to thank him for what he has done, but if not, we hope that he may have the opportunity of extending and improving the work which he has so worthily begun, instead of leaving it to his successor. H. B.

A CENTURY OF ELECTRICITY.

A Century of Electricity. By T. C. Mendenhall. (London: Macmillan and Co., 1887.)

IN this readable little work, Prof. Mendenhall has striven to depict the origin and growth of many of the modern electric appliances—the telegraph, the dynamo, the telephone, and the electric lamp. He opens with a felicitous quotation from Benjamin Franklin describing with characteristic humour a proposal to hold an electrical party of pleasure on the banks of the Skuylkil, when the healths of all the famous electricians in England, Holland, France, and Germany are to be "drank" in electrified bumpers, under the discharge of guns from the electrical battery. This is followed by a very interesting account of the early development of the experimental science, and in particular of the work of Gilbert and of Franklin. It is satisfactory to note that for once Gilbert's just fame as the creator of the double science of electricity and magnetism is recognized, and his pre-Baconian use and development of the experimental and deductive methods of philosophizing acknowledged. The discoveries of Galvani, Volta, Oersted, and Ampère are set forth in a style which, while losing nothing in accuracy of description, is enlivened by pleasant biographical touches. Speaking of the week during which Ampère wrought out to such brilliant conclusions the train of ideas suggested by Oersted's discovery of the electric deflexion of the magnet, Prof. Mendenhall observes: "It is safe to say that the science has at no other time advanced with such tremendous strides as during that memorable week." The work of Sturgeon in inventing, and of Henry in perfecting, the electro-magnet is duly noted; but we miss, in connexion with electro-magnetic subjects, the name of Prof. Cumming, who did so much to expand and define the growing science.

The vexed question, Who invented the electric telegraph? is here reached, and is very carefully handled. Prof. Mendenhall's frank impartiality in touching on this and sundry other delicate topics of contested priority is worthy of praise. *A propos* of the part taken by Henry in the invention of the electric telegraph, the author gives a sketch of Henry's arrangement of a bell for receiving electric signals, with a polarized lever to strike the bell, as it was exhibited in Albany in 1832. The most technical part of the work is that dealing with duplex and multiplex telegraphy, which is very fully treated, though here we miss the name of La Cour, who preceded Delany in the synchronous distribution of currents. Sir William Thomson's labours in submarine telegraphy, and those of Gaston Planté on accumulators, are emphasized, but not unduly. Respecting the telephone, after noting the early work of Page and the similarity between Reis's telephone transmitter and those used to-day, the author turns to the work of Elisha Gray and Graham Bell in the following terms:—"By a curious coincidence Mr. Gray deposited his specifications and drawings for a speaking-telephone in the United States Patent Office, in the form of a caveat, on February 14, 1876; and on the same day Mr. Bell filed his application for a patent, the latter being received a few hours earlier than the former. The coincidence becomes more interesting when it is remembered that it was also on February 14, 1867, that Wheatstone and Siemens simultaneously presented to the Royal Society their independent discovery of the important fact that dynamo-electric machines could be constructed and operated without the use of permanent magnets." The double coincidence of dates is certainly curious; but the significance of it is marred when we remember, first, that both Wheatstone and Siemens must yield priority of date to Varley, who patented the same discovery on December 24, 1866; and, secondly, that the apparatus described by Bell in the patent application of February 14, 1876, was one in which a separate instrument was employed for every pitch, "each instrument being capable of transmitting or receiving but a single note," and therefore did not describe a speaking-telephone at all. Bell's patent for "the transmission by the same means of articulate speech" was only applied for some ten months later. Due credit is given to Hughes for his well-known research on the microphone, to Edison for his button of lamp-black, and to Dolbear for the invention of the electrostatic receiver. The chapter on the electric light is all too short, and might with advantage be expanded. Faraday's splendid discovery of magneto-electric induction, leading to the invention of the dynamo, is admirably recounted, and the important part played by modern American constructors of powerful machines is modestly narrated. A similar remark will apply to the paragraphs upon electric motors, a department of electro-technics which America is likely to make peculiarly her own.

When we reflect that the rapid introduction into British industries of the gas-engine is slow compared with the tremendous rate at which electric motors are being everywhere brought into use in the States, we think that Prof. Mendenhall has under-rated rather than over-rated the importance of this item in his account of the

developments of the century. Strangely enough, there is in the whole work no mention of that most widely-spread of all electric inventions, the domestic electric bell, nor of its almost forgotten inventor, John Mirand. Prof. Mendenhall has added to the interest of his sketch by supplying a number of illustrative cuts of objects of historic interest, such as Faraday's first magneto-electric machine, and his first transformer or induction-coil. We should have welcomed some account of the great theorists, Coulomb, Laplace, and Weber, who, with Sir William Thomson and Clerk Maxwell, have, by their calculations and mathematical developments, played so leading a part in the progress of the century; but the author would probably have found it impracticable with the plan of his sketch to deal with the labours of these intellectual giants. In his less ambitious aim of popularizing the experimental development of the subject he has succeeded admirably.

OUR BOOK SHELF.

The Fungus Hunter's Guide and Field Memorandum Book, with Analytical Keys to the Orders and Genera, Illustrated, and Notes of Important Species. By W. Delisle Hay, F.R.G.S. (London: Swan Sonnenschein, Lowrey, and Co., 1887.)

A FIELD guide and mentor is a welcome companion for the practical botanist, provided it is so compiled as to meet all the requirements of field work, otherwise it is merely "a delusion and a snare." This little volume, unfortunately, belongs to the "otherwise," for it is insufficient, antiquated, and misguiding: insufficient, because it includes only a few species under each genus or sub-genus, and these have been selected without manifest reason; antiquated, because, although dated 1887, it is based upon the state of this branch of science in 1871, and might have been published at that date, for all internal evidence to the contrary; and misguiding, because the errors of 1871 are not corrected, the illustrative figures are entirely without names of the species intended to be represented, and more important or essential species are excluded than many of those included in the lists.

Under each genus or sub-genus in the volume a list is given of "common or notable species,"—each with its scientific name (but without the authority for the specific name, which any botanist would regard as essential); an imaginary popular name, which is useless because imaginary and not real; a short description, rarely sufficient; and letters indicating esculent or poisonous qualities. As only one or two species are given under a genus or sub-genus which has a dozen or more other British representatives, it should have been stated distinctly that there are so many more species which are not named, any of which the collector might meet with in his rambles. Unfortunately the selection of the species favoured with a place has been made with very little judgment. Some are included which are so rare that they have only been found once or twice in this country, whilst others are excluded which are almost sure to be met with in any moderately successful ramble. The fact is patent that the "Hand-book" issued sixteen years ago is accepted as the authorized record for to-day, whereas it is absolutely out of date, and all the great advances made during the intervening period are studiously ignored. The volume is interleaved with ruled paper for notes and memoranda, and we venture to affirm that this is the only useful and unexceptionable portion of the work. The purchaser must judge whether it would not have been more economical to secure a blank memorandum book, since the numerous figures are valueless without names, and the analytical keys ought to have been more accurate and better constructed. M. C. C.

My Hundred Swiss Flowers: with a Short Account of Swiss Ferns. By Mary A. Pratten. (London: W. H Allen and Co., 1887.)

THIS is a very unpretending book, and should be of considerable service to beginners in botany who may wish to carry on botanical studies among the Alps during the month of July or early in August. The writer has selected those Swiss flowers which seem to her "most remarkable, most characteristic of the country, or most commonly seen," and she is, of course, right in thinking that a great many of them will be new to such as make a first visit to the Alps. Her descriptions are clear and sufficiently full, and the illustrations are very good.

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 Carnatic Rainfall.

MR. H. BLANFORD'S authority is so deservedly high, that I have had some hesitation in writing to controvert the conclusions he has adopted in the paper published in NATURE of July 7 (p. 227), entitled "The Eleven-Year Periodical Fluctuation of the Carnatic Rainfall"; and to state my reasons for thinking that there is no real validity in the arguments he uses in favour of "the very high probability that the apparent undecennial fluctuation is no chance phenomenon."

Mr. Blanford brings forward a series of figures which show the mean annual variation of the rainfall during twenty-two years, at a number of stations in that part of Southern India locally known as the Carnatic, from the mean annual rainfall for the Carnatic generally. From these figures he has inferred the appearance of two complete cycles of eleven years, with a dominant periodical fluctuation.

To test the character of this apparent periodicity he obtains from these figures the two first terms of an harmonic expression that shall represent the observed facts for an assumed eleven-year period; and he finds the mean difference between the observed values and those calculated from the adopted harmonic expression to be $\pm 3{\cdot}5$ inches, from which the mean probable error of any of the calculated periodical values is found to be $\pm 0{\cdot}70$ inch.

Now it is apparent that such a series of calculated values has no physical signification whatever. The greater or less degree of difference between the observed and calculated quantities only indicates how far the sums of the terms of the harmonic series employed coincide with the series of observed quantities which the calculated series was designed to represent. It is also obvious that by a sufficiently extended series of terms the calculated quantities might be brought to agree, within any desired degree of approximation, with those observed. No conclusion whatever, therefore, can be based on the amount of the differences above alluded to, so far as any question of periodicity is concerned, and the so-called "probable error" is merely an arithmetical result of the particular form of calculation adopted.

Mr. Blanford goes on to remark that the mean difference between the observed series of values of the annual variation of rainfall and the mean of the whole of them, is $\pm 5{\cdot}2$ inches, with a probable error of the general average of $\pm 0{\cdot}94$ inch.

And here again I am unable to see that any weight can be attached to these figures in connexion with the main point at issue. The mean variation of the series of observed values, from the mean of all of them, will of course be greater than the mean variation of those observed values from a series deliberately calculated so as to correspond with them, such as that obtained by aid of the harmonic series. The introduction of the expression "probable error" of the general average is also

likely to be misleading. This too only represents an arithmetical result, and signifies that as in the series of twenty-two observations there is an average departure of ±5·2 inches from the mean of all the measurements, the probability is that this mean will be within ± 0·94 inch of the truth, so far as those measurements are to be trusted.

For these reasons I am quite unable to follow the arguments by which it is sought to connect the amounts of these two "probable errors," or to see how they can in any way indicate "the relative probability of this particular variation being the result of a periodic law, and of its being a mere fortuitous series of variations from a constant average."

Neither does there appear to be any justification for assuming that the relative probability of the truth of two hypotheses is represented by the inverse ratios of the probable errors of results derived from them. Still less is there any ground for saying that because the particular series of quantities under discussion relates to a period of twenty-two years, the relative probability just alluded to is thereby increased to the twenty-second power of that ratio, or from about 1½ to 1, to 655 to 1. It is no doubt true that if the probability of an event occurring once be represented by the fraction $\frac{1}{x}$, the probability of its recurring n times in succession will be represented by $\left(\frac{1}{x}\right)^n$; but I fail to see how this affects the question at issue.

RICHARD STRACHEY.

July 11, 1887.

Is Cold the Cause of Anticyclones?

IN a review of Loomis's papers in this volume of NATURE, p. 2, occur the following sentences:—"While all, or nearly all, of the high pressure of anticyclones may be accounted for by the very low temperatures which overspread the same region at the same time along with the resulting upper currents concentrating upon them from adjoining cyclonic regions, it is quite different with the low pressures of cyclones. In the case of cyclones the problem is complicated by the strong winds, the copious precipitation, and the ascending currents, which affect the results in ways which no physicist has yet been able to explain."

This induction of Loomis's, that anticyclones are largely the result of cold, which the reviewer here repeats, is in entire opposition to the deductive views of Ferrel, and I think the discrepancy is to be found in the method used by Loomis in drawing his inductions. In order to investigate the cause of anticyclones, Loomis selected only decided areas of high pressure, and as a consequence his data were almost entirely confined to the winter months, when the temperature accompanying anticyclones is always low. If, however, he had selected more moderate anticyclones, he would have found that in summer anticyclones in the United States are sometimes accompanied by intense heat (90° F. or more). This is especially so in periods of drought. Under these conditions the approach of a cyclone with rain brings a most refreshing cooling. Furthermore, Hahn, attacking the problem by a different method, has obtained results apparently directly opposed to this induction of Loomis. Hahn made a careful study and comparison of the observations obtained last autumn and winter on the Sonnblick and at adjacent mountain and valley stations (see *Meteorologische Zeitschrift*, February and April 1887). One of the most marked results found was that at heights exceeding 1000 metres above sea-level there was an increase of temperature during anticyclones, while a decreased temperature was only found in valleys and near the general level of the earth's surface. Hahn's average results show that the temperature on the Sonnblick, height 3090 metres, rose from an average of − 16°·4 C. at the average barometric pressure of 509·1 mm. to − 7°·7 C. at the barometric pressure of 529·3 mm.; while at the same time the average temperature at Schafberg, height 1776 metres, rose from − 9°·0 C. to + 1°·4 C.; but on the contrary at Zell-a-See, height 754 metres, the average temperature fell from − 5°·9 C. to − 8°·9 C. These results, which show that the larger portion of the atmosphere is warmed instead of cooled within the area of an anticyclone, seems entirely destructive of Loomis's hypothesis that the cooling of the air near the earth's surface is the chief cause of the anticyclone. Hahn's results, however, indicate that the cooling of the air near the earth's surface does increase the pressure somewhat. Thus in October 1886 the barometric minimum occurred at all the stations, both mountain and valley, on the 17th; while the barometric maximum occurred at all of the stations on the 30th. When the difference in pressure between the mountain stations at the time of barometric minimum was compared with the difference in pressure between the same stations at the time of barometric maximum, it was found almost exactly the same; but the difference in pressure between the valley stations and the mountain stations was about 5 mm. greater at the time of maximum pressure than at the time of minimum pressure. Hahn refers this greater range of pressure at the valley stations to the decreased temperature at valley stations during anticyclones, but this still leaves a range of pressure of nearly 20 mm., which the decreased temperature entirely fails to explain. These results of Hahn are in entire accord with the results obtained at Blue Hill Observatory (640 feet above sea), as compared with Mount Washington and with stations near sea-level, which indicate that the cooling in anticyclones is almost entirely confined to within a few hundred feet of the earth's surface (see *Science*, vol. viii. pp. 233 and 281).

In the light of these facts, it seems more reasonable to assume that the warmth found on mountains and the cold in valleys, accompanying anticyclones, are the result, rather than the cause, of anticyclones. Such researches as those of Langley on the solar heat, and of Hahn on the distribution of temperature pressure, &c., in different planes of the atmosphere, indicate that the effect of the sun's heat on the atmosphere is far more complex than some of our text-books on meteorology would have us believe; and instead of the cause of anticyclones being as simple as the reviewer of Loomis's article states, it seems probable that we shall understand the phenomenon of the anticyclone only when we master the problem of the cyclone.

H. HELM CLAYTON.

Blue Hill Observatory, Boston, Mass., U.S., June 28.

Physiological Selection.

I AM perhaps in a position to contribute something practical to the discussion upon Mr. G. J. Romanes's proposal of physiological selection as an improvement upon Darwin's natural selection.

I failed to meet with Mr. Romanes's paper in the Journal of the Linnean Society, and I confess that I did not gain a very clear idea of what he meant by physiological selection, until I read his article in the *Nineteenth Century* for January. His main difficulty appears to be the intercrossing with parent stock, which he thinks would prevent the survival of any varieties naturally selected to become species. Now, my Australian bush experience of the habits of animals and birds satisfies me that this difficulty is mainly, if not entirely, imaginary, and that Nature amply provides against the supposed intercrossing. Any person who has observed the habits of semi-domesticated stock, such as horses and cattle, can scarcely fail to know that migration is a general practice of one sex, and a frequent one of the other. The old always hunt the young males and strangers entirely away to form herds and families of their own, and thus the supposed intercrossing is by one sex at least effectually obviated. But it is also frequently the case that young fillies and heifers, at the same season, take to wandering, for less evident reasons, far from their accustomed haunts, scores of miles, after which they will stop, and attach themselves to another herd and locality as tenaciously as their parents remain in theirs. This of course further tends to prevent intercrossing with parent stocks.

I cannot but think that Mr. Romanes's anxiety to find a solution of his difficulty has led him into serious mistakes, which vitiate his treatment of the subject. For instance, he says (page 59, *Nineteenth Century* for January), "The hypothesis of physiological selection sets out with an attempted proof of the inadequacy of natural selection, *considered as a theory of the origin of species*." I was out walking yesterday when I read this, and I wrote in the margin, "The theory of natural selection is one, not of the origin of species at all, but of the *preservation* of *particular varieties*." On reaching home, I referred to the "Origin of Species" (4th edition, 1866), and was certainly pleased to find that I had adopted Darwin's precise words repeated in several places (see pages 71, 91, 123, &c.). At page 91 he says:—"Some writers have misapprehended or objected to the term natural selection. Some have imagined that natural selection induces variability; whereas it implies only the *preservation* of *such varieties as occur*, and are beneficial under

the conditions of life." Was not this a prophet? Yea, I say unto you and more than a prophet! Of course if the conditions of life are unfavourable, the incipient variations cannot become species. But surely it is obvious that in *variation* is the real *origin* of species. Variations must occur before the selection of some of them in preference to others. To consider the theory of natural selection as a theory of the origin of species, is therefore clearly an error. In his "Origin of Species" Darwin certainly expounded variation, and I might have ventured to think that as the book deals more largely with the subsequent selection of a few varieties to survive as species at the expense of many extinguished, a more exact title for it would have been "The Evolution of Species." But what says the great master? See page 71:—"Owing to this struggle for life, any *variation*, however slight . . . will tend to the *preservation* of *that individual*, and will generally be inherited by its offspring. . . . I have called this principle by which each slight *variation*, if useful, is *preserved*, by the term natural selection, in order to mark its relation to man's power of selection." And who will not recognize the wisdom of his selection of the term? It has been before observed that the "*Ascent* of Man" would seem a more accurate title than the "*Descent* of Man." But I have no doubt that his reasons for preferring the latter were equally cogent.

But Mr. Romanes proceeds:—"This proof is drawn from three distinct heads of evidence. (1) The inutility to species of a large proportional number of their specific characters. (2) The general fact of sterility between allied species, which admittedly cannot be explained by natural selection, and therefore has hitherto never been explained. (3) The swamping influence of even useful variations of free intercrossing with the parent form." I have advanced, I think, ample reasons why No. 3 may be regarded as imaginary, and which therefore reduce the value of No. 2 to a minimum. No. 1 depends entirely upon the definition of "*utility*." Has this word any real significance outside human interests and considerations? The idea of utility, if extended to Nature's operations, may, it seems to me, apply to the interests of any other variation than the one whose specific characters are in question, which may therefore be, without compunction or regret, sacrificed to the most fit, as we know that innumerable species have been extinguished in the interest of those that supplanted them. But utility to Nature may be the extinction of one variation and the preservation of another. As Mr. Romanes's whole paper is built upon what I have already quoted from it, I need scarcely follow it any further. With your permission, however, I have another remark to make.

Mr. Romanes seems to me to have been much exercised by the consideration of the intercrossing with parent forms, and, not knowing of the simple solution given above, to have cleverly invented his physiological selection to escape from the dilemma. Of course Nature is not clever, but simple in its operations. I was always much impressed with what appeared to me a greater difficulty, which might be thought to have a clearer title to be called "physiological selection." I allude to a general tendency in the (human at least) sexes to prefer a mate with opposite characteristics, with the apparent result of insuring mediocrity in the progeny. Thus, as a general rule, the tall prefer the short; the dark, the fair; the wise, the silly; &c., and *vice versâ*. Variation is, on the other hand, apparently insured to a large extent by the differences between parents, but still it would seem that the tendency should, *cæteris paribus*, be inevitably towards a mean in the progeny. The general migration, however, as above indicated, of young males and females, gives plainly ample opportunity for the preservation of viable variations, besides others which experience and care will doubtless discover.

Melbourne, April 11. H. K. RUSDEN.

Weight, Mass, and Force.

APPLICATIONS of the data previously given, in the extract from the American journal, to the dynamical principles of varied motion are easily provided for Mr. Hayward. Take the following: "Determine the weight of the greatest train the Strong locomotive can take up a 96-feet grade from rest at one station to stop at the next station a mile off in four minutes, taking the brake power as a resistance of 400 lbs. to a ton."

The main points at issue, however, are whether the language of the engineer, and in fact the usage of our own and other languages, is scientifically correct or incorrect in its use of the words *weight* and *weighing*; and whether the mathematician is to be allowed to restrict the word weight to the subsidiary sense of force of attraction by the earth.

It is of great importance that this question of dynamical terminology should be thoroughly thrashed out now, before Mr. Hayward's Committee on Dynamics, of the Association for the Improvement of Geometrical Teaching, prepare their final report on the subject. A. G. GREENHILL.

Woolwich, July 11.

The Sky-coloured Clouds.

ON the evenings of June 14, 18, and 19 there was a feeble reappearance in Sark of the sky-coloured clouds, as I may call them in default of a better name, which were so brilliant in the twilights of the last two summers. Though the display this month has been comparatively faint, it has been unmistakably of the same character. I have seen nothing of these clouds since the 19th in travelling in the Channel Islands and through France.

Geneva, June 29. T. W. BACKHOUSE.

P.S.—*Chamounix, July* 13.—I have seen one more display—a brilliant one seen from this neighbourhood on the 6th inst.—T. W. B.

The Migrations of Pre-Glacial Man.

THE question raised by "Glaciator" has been treated by me in a paper entitled "The Faunas of the Ffynnon Beuno Caves and of the Norfolk Forest Bed" in the *Geological Magazine* for March 1887. I there stated that, "Although man probably reached this country from the east, it seems to me equally clear that he must also have arrived here with the reindeer from some northern source during the advance of glacial conditions." Though the Norfolk Forest Bed fauna contains abundant remains of deer and of other animals suitable as food for man, it is curious that so far no implements or other traces of man have been found there. The Forest Bed contains in the main the fauna of an eastern area, as the river on the banks of which the animals roamed flowed from the south-east. If pre-glacial man arrived in this country from the east or south, we should therefore expect to find evidences of this in the Forest Bed. On the other hand, wherever the remains of northern animals, such as the reindeer, mammoth, and rhinoceros, occur in any abundance, there we almost invariably find traces of man. Now that we know that man arrived in this country before the climax of the Ice age, as proved by the explorations carried on for several years at the Ffynnon Beuno Caves (amply confirmed also by this year's researches), it seems but natural to infer that man arrived in this country with the northern animals as they were compelled to migrate southwards by the gradually advancing glacial conditions, and that he kept mainly with the reindeer near the edge of the advancing ice. HENRY HICKS.

ABSTRACT OF THE RESULTS OF THE INVESTIGATION OF THE CHARLESTON EARTHQUAKE.[1]

I.

THE amount of information now in possession of the United States Geological Survey, relating to the Charleston earthquake, is probably larger than any of similar nature ever before collected relating to any one earthquake. The number of localities reported exceeds 1600. The sources of information are as follow: (1) we are deeply indebted to the U.S. Signal Service for furnishing us the reports of their observers; and (2) equally so to the Lighthouse Board, which has obtained and forwarded to us the reports of keepers of all lighthouses from Massachusetts to Louisiana, and upon the great lakes; (3) to the Western Union Telegraph Company, which instructed its Division superintendents to collate and transmit many valuable reports; (4) to the associated Press, which has given us access to the full despatches (with transcripts thereof) which were sent over the wires

[1] Paper read before the National Academy of Sciences at Washington, on April 19, 1887, by C. E. Dutton, U.S.A., and Everett Hayden, U.S.N., U.S. Geological Survey.

centering at Washington during the week following the earthquake; (5) to geologists and weather bureaus of several States, who have kindly exerted themselves in this matter and collected much important information; (6) to a considerable number of scientific gentlemen who have distributed for us our circular letters of inquiry in special districts,—notably, Profs. W. M. Davis, C. G. Rockwood, J.P. Lesley, T. C. Mendenhall, and Messrs. W. R. Barnes, of Kentucky, and Earle Sloan, of South Carolina; (7) to a large number of postmasters in the Eastern, Central, and Southern States; and, finally, to hundreds of miscellaneous correspondents throughout the country.

In collecting this information, a printed list of questions was prepared. This practice has been resorted to in Europe and in Japan with considerable success, and the questions which have been devised for distribution in those countries have been prepared with great skill by some of the ablest investigators of earthquakes. Prof. C. G. Rockwood, of Princeton, has also been in the habit of distributing formal questions of this character in this country whenever apprised by the newspapers of a notable shock. Availing ourselves of his advice and assistance, questions prepared by him were printed and widely distributed. They were much fewer and more simple than those employed in Europe, because European investigators depend almost wholly upon the educated classes to answer them, while in this country the uneducated but intelligent and practical classes of the people must be the main reliance. These questions were designed to elicit information: (1) as to whether the earthquake was felt, (2) the time of its occurrence, (3) how long it continued, (4) whether accompanied by sounds, (5) the number of shocks, (6) general characteristics which would serve as a measure of its intensity and indicate the kind and direction of motion.

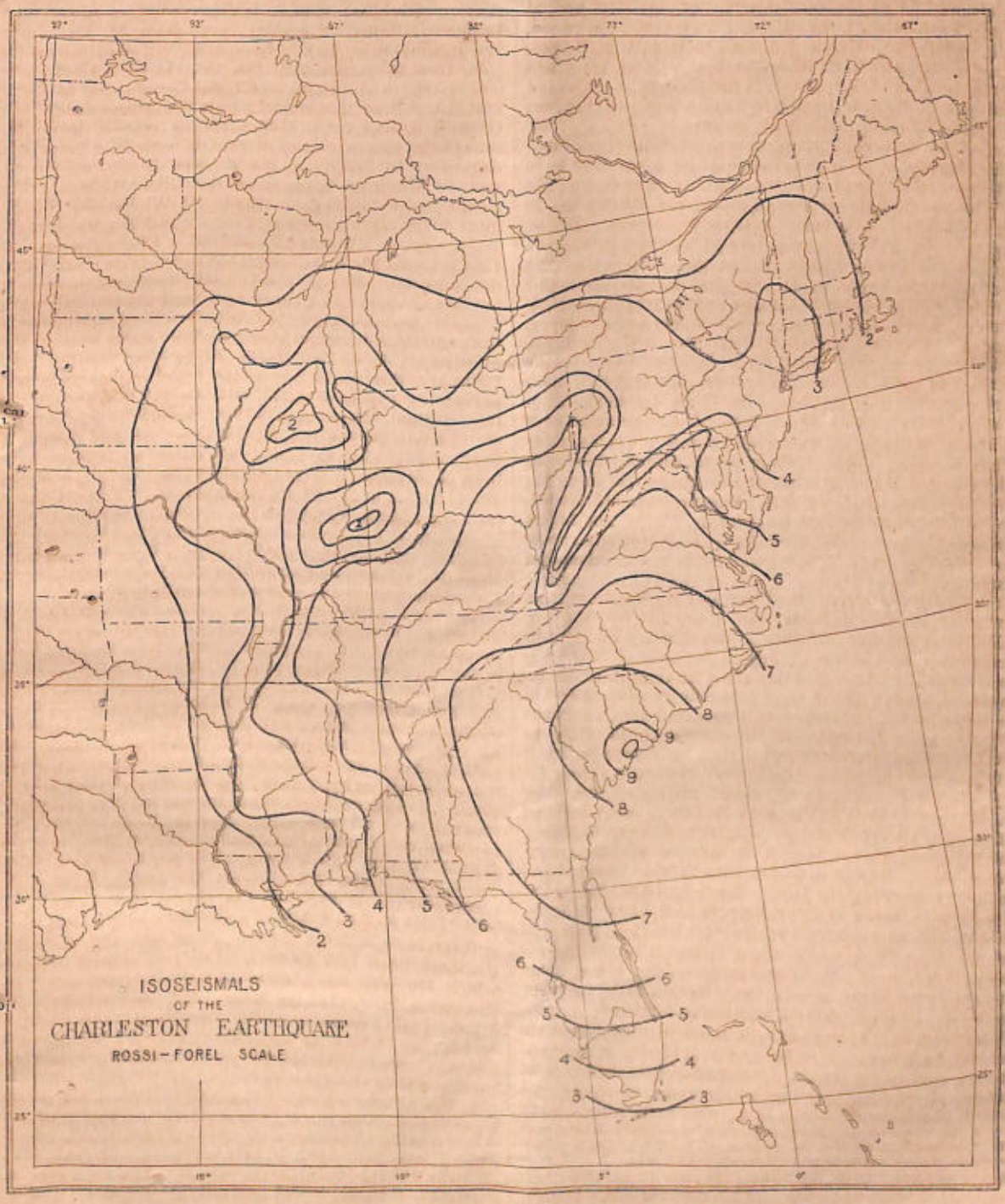
It is to be observed that the only information to be hoped for which can have even a roughly approximate accuracy is the time of transit of the shock. The degree of approximation in the time data actually obtained will be adverted to later. Special effort was made to obtain information as to the relative intensity of the shocks in all parts of the country. At the very outset a serious difficulty presents itself. In the estimates of intensities there is no absolute measure. What is really desired is some reliable indication which shall serve as a measure of the amount of energy in any given portion of the wave of disturbance as it passes each locality. The means of reaching even a provisional judgment are very indirect, and qualified by a considerable amount of uncertainty. To estimate the force of a shock, we have no better means than by examining its effects upon buildings, upon the soil, upon all kinds of loose objects, and upon the fears, actions, and sensations of people who feel it. In view of the precise methods which modern science brings to bear upon other lines of physical research, all this seems crude and barbarous to the last degree. But we have no other resource. Even if it were possible to obtain strictly comparative results from such facts, and decide with confidence the relative measure of intensity which should be assigned to each locality, we should have gained measures only of a series of local surface intensities and not of the real energy of the deeply-seated wave which is the proximate cause of the surface phenomena. Notwithstanding the indirect bearing of the facts upon the real quantities we seek to ascertain, and their apparently confused and distantly related character, they give better results than might have been supposed. When taken in large groups, they give some broad indications of a highly suggestive character, and though affected with great inequalities which for the time being seem to be anomalous, these anomalies are as instructive as the main facts themselves.

We have given the preliminary plotting of the intensities in the map before you. The first point to which we shall invite attention is the magnitude of the area affected by the shocks. It was sensibly felt in Boston, which is the most distant point on the Atlantic coast from which affirmative reports have been received. From Maine the answers are all negative. Most of those from New Hampshire are negative, but two or three positive ones show clearly that it was felt in sensitive spots. In Vermont, affirmative reports come from St. Johnsbury and Burlington on Lake Champlain. No positive reports come from the province of Quebec. In New York State it was felt in the vicinity of Lake George, and at Lake Placid and Blue Mountain Lake in the Adirondacks. In Ontario, it was quite noticeable in several localities, though the great majority of reports from that place are negative. In Michigan, it was noted in several places, and at Manistee Lighthouse, on Lake Michigan, the trembling was strongly marked. In Winconsin, though most of the reports are negative, it was felt quite strongly at Milwaukee, and was also noticed at Green Bay, and at La Crosse on the Mississippi, 967 miles from Charleston, the remotest point in the United States which gives a positive answer. In Central Iowa and Central Missouri, it was unmistakably felt. In Arkansas, the eastern portion of the State, from sixty to seventy-five miles west of the Mississippi, gives numerous favourable reports. In Louisiana, the reports are mostly negative, but numerous persons in New Orleans felt the shocks and recognized their nature. In Florida, it was universally felt, and the northern part of the State was severe and alarming. From the Everglade region, of course, no reports have been received, as it is uninhabited; but in some of the Florida Keys it was felt in notable force. From Cuba a few reports have come, and the most distant point in that island which was shaken was Sagua la Grande, where the vibration was very decided. Lastly, a report comes from Bermuda, 1000 miles distant from Charleston, which leaves little doubt that the tremors were sensible there.

The area within which the motion was sufficient to attract the attention of the unexpectant observer would be somewhat more than circumscribed by a circle of 1000 miles' radius, and the area of markedly sensible shaking would, including the oceanic area, be somewhere between two and a half and three million square miles. In this estimate, however, only well-defined seismic movement of notable force is considered. There are reasons for believing that by proper instrumental observation the movement could have been detected over a much greater area. In the first place it is to be noted that the peripheral portions of the observed area lie in districts which are rather thinly populated, sometimes, also, in districts which from the nature of the ground do not disclose forcibly the passing shock. Furthermore, the passing wave in the outer portions of the area was almost everywhere of an undulatory character and of great wave-length, and while still retaining a large amount of energy, did not often dissipate itself into those smaller and shorter tremors which are very much more likely to attract attention, though really possessing very much less energy. Six hundred miles from the origin the long swaying motion was felt, and was often sufficient to produce sea-sickness, yet was unaccompanied by sound or by the tremulous motion due to short waves.

It will be observed upon the map that there are several large tracts which show a comparatively feeble intensity, while completely surrounding them is the general area of greater intensity. The most conspicuous of these areas of silence is the Appalachian region. The facts here are extremely interesting and suggestive. It has been generally supposed that a mountain-range serves as a barrier to the propagation of earthquakes—not from any known relation of cause and effect, but merely as the result of observation. In Japan it is universal testimony that the central range of the island marks the dividing line between earthquake and no earthquake. The shocks so frequent there are seldom or never felt beyond the mountains. A

similar conclusion has been drawn from South American earthquakes, and also from those which have visited Southern Italy. As soon as the data in the earlier stages of the inquiry began to indicate insulated areas of minimum action, they were completely investigated, and every effort has been made to secure full data from them. The result has been to show satisfactorily that such was the case. The Appalachian belt south of Middle Pennsylvania disclosed a few spots where the shaking was considerable, but in the main it was but slightly affected



until we reach the extreme southern portion of this range, where the shocks begin to be somewhat vigorous, even in the mountains. West and north-west of the range, however, the force of the undulations resumes even more than its normal vigour. In Eastern Kentucky and South-Eastern Ohio, the force of the shocks was very considerable, causing general alarm. Chimneys and bricks were shaken down, and the oscillation of the houses was strongly felt. In South-Eastern Ohio, nearly every theatre, lodge, and prayer-meeting, was broken up in confusion. It does not

appear that the Appalachians offered any sensible barrier to the progress of the deeper waves, but it does appear that they affected in a conspicuous degree the manner in which the energy of the waves was dissipated at the surface. Another minimum area was found in Southern Indiana and Illinois, and also in Southern Alabama and Mississippi. There is a curious circumstance connected with the minimum area in Indiana and Illinois. On February 6 last, an earthquake of notable force occurred in just this locality. Circulars were sent out at once, and on plotting the isoseismals they showed a singular coincidence in almost exactly filling the vacancy or defects of intensity of the Charleston earthquake. At present there is nothing to indicate whether this coincidence is accidental or whether there is some hidden relation.

Where the waves passed into the newer delta region of the lower Mississippi, the surface intensity of the shocks rapidly declined. This is indicated in the map by the compression of the isoseismals in those localities. We incline to the opinion that this sudden diminution of the intensity is due to the dissipation of the energy of the waves in a very great thickness of feebly elastic, imperfectly consolidated, superficial deposits. It is a matter of common observation in all great earthquakes that the passage of the principal shocks from rigid and firm rocks into gravels, sands, and clays is, under certain circumstances, attended with a local increase in the amplitudes of the oscillations and in the apparent local intensity and destructiveness, and the reason for it is intelligible. But where such looser materials are of very great thickness and great horizontal extent the reverse should be expected. For when a wave passes from a solid and highly elastic medium into a less solid and imperfectly elastic one, the amplitude may be suddenly increased at the instant of entering; but so rapid is the extinction, that, if the new medium be very extensive, the impulse is soon dissipated.

Many reports throughout the Central States indicate localities of silence which are not expressed upon the map. The reason for omitting them is that it has been impracticable to secure a sufficient density of observation (*i.e.* a sufficient number of reports per unit area) to enable us to mark out and define these smaller areas with very great precision. To do this for the whole country would require some tens of thousands of observations and the expenditure of tens of thousands of dollars to systematize and discuss the data. A map shaded to show the varying intensity by varying the depth of the shading would have a mottled appearance, in which the mottling would be most pronounced in the areas of a little below the mean intensity, say between the isoseismals 3 and 5. This fact is of great importance in the interpretation of the isoseismals, for the omission to consider it results in giving to the middle isoseismals too high a value. In any isoseismal zone, what we should like to ascertain is the mean intensity of the whole area included within that zone. As a matter of fact, the data we possess consist more largely of maximum than of minimum or average intensities, and therefore tend to considerably augment the mean derived intensity above the true mean. This will become apparent by an inspection of the map where the zones of 5, 6, and 7 intensity are disproportionately broad, while those of 3 and 4 are disproportionately narrow. We have not attempted to allow for this source of error, though fully aware of it, because we had no means of determining what allowance to make. We have drawn the lines wholly upon the face of the returns, and the investigators who may attempt to utilize our results must grapple with the corrections as best they may.

Throughout the States of North Carolina, South Carolina, Georgia, and North-Eastern Florida, and in general anywhere within about 250 miles of the centre, the energy of the shocks was very great. At Columbia, Augusta, Raleigh, Atlanta, and Savannah, the consternation of all the people was universal. The negroes and many of the poor whites were for a week or two not exactly demoralized, but intensely moralized, giving themselves to religious exercises of a highly emotional character, the stronger and deeper natures among them being impressed with a feeling of awe, the weaker natures with a feeling of terror. And this was general throughout the large region just specified. In all of the large towns within 200 miles of Charleston more or less damage was suffered by houses and other structures. Walls were cracked to such an extent as to necessitate important repairs; dams were broken, chimneys were overthrown, plastering shaken from ceilings, lamps overturned, water thrown out of tanks, cars set in motion on side tracks, animals filled with terror, fowls shaken from their roosts, loose objects thrown from mantels, chairs and beds moved horizontally upon the floor, pictures banged against the walls, trees visibly swayed and their leaves agitated and rustled as if by a wind. These occurrences were general, and were more strongly marked until they became terrifying and disastrous as the centre of the disturbance was approached. At Augusta, 110 miles distant from the epicentrum, the damage to buildings was considerable; and at the arsenal in that place the commanding officer's residence was so badly cracked and shattered as to necessitate practical reconstruction. In Columbia, 100 miles distant, the shock was very injurious to buildings and appalling to the people, but no substantial structures were actually shaken down. In Atlanta, 250 miles distant, there was no worse injury than falling chimneys and some slight cracks in the walls, but the houses were instantly abandoned in great alarm and confusion by their occupants, and many preferred passing the night in the streets to re-entering their dwellings. At Asheville, N.C., 230 miles distant, and at Raleigh, 215 miles distant, the shocks were quite as vigorous as at Atlanta.

Coming nearer the seismic centre we find the intensity increasing on all sides as we approach it. The region immediately about the epicentrum in a great earthquake always discloses phenomena strikingly different from those at a distance from it, and the differences are not merely in degree but also in kind. The phenomena characteristic of the epicentral area cease with something like abruptness as we radiate away from the epicentrum. The central phenomena are those produced by shocks in which the principal component of the motion of the earth is vertical. Proceeding outwards, these predominating vertical motions pass, by a very rapid transition, into movements of which the horizontal component is the greater, and in which the undulatory motion becomes pronounced. The epicentrum, and the zone immediately surrounding it, is the portion of the disturbed tract which merits the closest attention, for it is here that we may find the greatest amount of information concerning the origin and nature of the earthquake. To appreciate this we will venture to offer some theoretical considerations.

Allusion has already been made to the indefinite character of the data used for estimating the intensity of the shock. There is no unit of intensity which is at present available. In selecting certain effects of an earthquake to characterize varying degrees of intensity, the most that can be hoped for is a means for discriminating whether the relative energy of a shock is greater or less in one locality than in another. But how much greater and how much less—in conformity with what law—is a problem which remains to be solved. An earthquake impulse, however, is a form of energy transmitted as an elastic wave through the deeply-seated rocks, and its propagation and varying intensity are subject to the laws of wave-motion There must be, therefore, some typical law governing the rate at which such a wave diminishes the intensity of its effects as it moves onward. To anticipate the objection that this typical law would apply only to a

medium which is perfectly elastic, homogeneous, and isotropic, while the rocks are far from being so, we reply that we have investigated the objection, and are satisfied that while it has some validity, the effect of these inequalities is not great enough to seriously impair the applicability of the law, nor to vitiate greatly the results to be deduced from it. The analysis we offer is a novel one. We attach considerable importance to it, and the consequences which flow from it are somewhat remarkable.

(*To be continued.*)

EXPERIMENTS ON THE SENSE OF SMELL IN DOGS.[1]

I ONCE tried an experiment with a terrier of my own, which shows, better than anything that I have ever read, the almost supernatural capabilities of smell in dogs. On a Bank holiday, when the Broad Walk in Regent's Park was swarming with people of all kinds, walking in all directions, I took my terrier (which I knew had a splendid nose, and could track me for miles) along the walk, and, when his attention was diverted by a strange dog, I suddenly made a number of zigzags across the Broad Walk, then stood on a seat, and watched the terrier. Finding I had not continued in the direction I was going when he left me, he went to the place where he had last seen me, and there, picking up my scent, tracked my footsteps over all the zigzags I had made, until he found me. Now, in order to do this, he had to distinguish my trail from at least a hundred others quite as fresh, and many thousands of others not so fresh, crossing it at all angles.[2]

The object of the experiments about to be described was that of ascertaining whether a dog, when thus distinguishing his master's trail, is guided by some distinctive smell attaching to his master's shoes, to any distinctive smell of his master's feet, or to both these differences combined.

I have a setter-bitch, over which I have shot for eight years. Having a very good nose, she can track me over immense distances, and her devotion to me being very exclusive, she constituted an admirable subject for my experiments.

These consisted in allowing the bitch to be taken out of the kennel by someone to whom she was indifferent, who then led her to a pre-arranged spot from which the tracking was to begin. Of course this spot was always to leeward of the kennel, and the person who was to be tracked always walked so as to keep more or less to leeward of the starting-point. The district—park-lands surrounding a house—was an open one, presenting, however, numerous trees, shrubberies, walls, &c., behind which I could hide at a distance from the starting-point, and so observe the animal during the whole course of each experiment. Sundry other precautions, which I need not wait to mention, were taken in order to insure that the bitch should have to depend on her sense of smell alone, and the following are the experiments which were tried :—

(1) I walked the grass-lands for about a mile in my ordinary shooting-boots. The instant she came to the starting-point, the bitch broke away at her full speed, and, faithfully following my track, overtook me in a few minutes.

(2) I set a man who was a stranger about the place to walk the park. Although repeatedly put upon his trail by my servant, the bitch showed no disposition to follow it.

(3) I had the bitch taken into the gun-room, where she saw me ready to start for shooting. I then left the gun-room and went to another part of the house, while my gamekeeper left the house by the back door, walked a certain distance to leeward in the direction of some partridge-ground, and then concealed himself. The bitch, who was now howling to follow me, was led to the back door by another servant. Quickly finding the trail of the gamekeeper, she tracked it for a few yards; but, finding that I had not been with him, she left his trail, and hunted about in all directions for mine, which, of course, was nowhere to be found.

(4) I collected all the men about the place, and directed them to walk close behind one another in Indian file, each man taking care to place his feet in the footprints of his predecessor. In this procession, numbering twelve in all, I took the lead, while the gamekeeper brought up the rear. When we had walked two hundred yards, I turned to the right, followed by five of the men; and at the point where I had turned to the right, the seventh man turned to the left, followed by all the remainder. The two parties thus formed, after having walked in opposite directions for a considerable distance, concealed themselves, and the bitch was put upon the common track of the whole party before the point of divergence. Following this common track with rapidity, she at first overshot the point of divergence; but, quickly recovering it, without any hesitation chose the track which turned to the right. Yet in this case my footprints in the common track were overlaid by eleven others, and in the track to the right by five others. Moreover, as it was the gamekeeper who brought up the rear, and as in the absence of my trail she would always follow his, the fact of his scent being, so to speak, uppermost in the series, was shown in no way to disconcert the animal when following another familiar scent lowermost in the series.

(5) I requested the stranger before mentioned to wear my shooting-boots, and in them to walk the park to leeward of the kennel. When the bitch was led to this trail, she followed it with the eagerness wherewith she always followed mine.

(6) I wore this stranger's boots, and walked the park as he had done. On being taken to this trail, the bitch could not be induced to follow it.

(7) The stranger walked the park in bare feet; the bitch would not follow the trail.

(8) I walked the park in bare feet: the bitch followed my trail; but in quite a different manner from that which she displayed when following the trail of my shooting-boots. She was so much less eager, and therefore so much less rapid, that her manner was suggestive of great uncertainty whether or not she was on my track.

(9) I walked the park in new shooting-boots, which had never been worn by anyone. The bitch wholly refused to take this trail.

(10) I walked the park in my old shooting-boots, but having one layer of brown paper glued to their soles and sides. The bitch was led along my track, but paid no attention to it till she came to a place where, as I had previously observed, a small portion of the brown paper first became worn away at one of my heels. Here she immediately recognized my trail, and speedily followed it up, although the surface of shoe-leather which touched the ground was not more than a few square millimetres.

(11) I walked in my stocking-soles, trying first with new cotton socks. The bitch lazily followed the trail a short distance and then gave it up. I next tried woollen socks which I had worn all day, but the result was the same, and therefore quite different from that yielded by my shooting-boots, while more resembling that which was yielded by my bare feet.

(12) I began to walk in my ordinary shooting-boots, and when I had gone fifty yards, I kicked them off and carried them with me, while I continued to walk another three hundred yards in my stocking-soles; then I took off

[1] Paper read by Mr. George J. Romanes, before the Linnean Society, on December 16, 1886. Reprinted from the Linnean Society's Journal—Zoology, vol. xx.

[2] "Mental Evolution in Animals," pp. 92-93; where also see for additional remarks of a general kind on the sense of smell in different animals.

my stockings, and walked another three hundred yards on my bare feet. On being taken to the beginning of this trail, or where I had started in my shooting-boots, the bitch as usual set off upon it at full speed, nor did she abate this speed throughout the whole distance. In other words, having been once started upon the familiar scent of my shooting-boots, she seemed to entertain no doubt that the scent of the stocking-soles and of the bare feet belonged to me; although she did not clearly recognize them as belonging to me when they were not continuations of a track made by my shooting-boots (10 and 11).

(13) I requested a gentleman who was calling at the house, and whom the bitch had never before seen, to accompany me in a conveyance along one of the carriage-drives. At a distance of several hundred yards from the house I alighted in my shooting-boots, walked fifty yards beside the carriage, again entered it while my friend alighted and walked two hundred yards still further along the drive. The bitch ran the whole 250 yards at her full speed, without making any pause at the place where the scent changed. This experiment was subsequently repeated with other strangers, and with the same result.

(14) I walked in my ordinary shooting-boots, having previously soaked them in oil of aniseed. Although the odour of the aniseed was so strong that an hour afterwards the path which I had followed was correctly traced by a friend, this odour did not appear to disconcert the bitch in following my trail, for she ran me down as quickly as usual. It was noticed, however, by the friend who took her to the trail that she did not set off upon it as instantaneously as usual. She began by examining the first three or four footsteps with care, and only then started off at full speed.

(15) Lastly, I tried some experiments on the power which this bitch might display of recognizing my individual odour as emanating from my whole person. In a large potato-field behind the house, a number of labourers had been engaged for eight or ten hours in digging up and carrying away potatoes all the way along half-a-dozen adjacent "drills." Consequently, there was here a strip of bared land in the field about twenty yards wide, and a quarter of a mile long, which had been thoroughly well trampled over by many strange feet. Down this strip of land I walked in a zigzag course from end to end. On reaching the bottom I turned out of the field, and again walked up a part of the way towards the house, but on the other side of a stone wall which bounded the field. This stone wall was breast high, and was situated nearly a hundred yards to windward of my previous course through the potatoes. The bitch, on being led out of the house, was put upon my trail at the top of the field, and at high speed picked out my trail among all the others, following roughly the various zigzags which I had taken. But the moment she gained the "wind's eye" of the place where I was standing behind the wall, she turned abruptly at a right angle, threw up her head, and came as straight as an arrow to the spot where I was watching her. Yet while watching her I had allowed only my eyes to come above the wall, so that she proved herself able to distinguish instantly the odour of the top of my head (without hat) at a distance of two hundred yards, although at the time she was surrounded by a number of over-heated labourers.

(16) On another day, when it was perfectly calm, I tried the experiment of standing in a deep dry ditch, with only the top of my uncovered head above the level of the surrounding fields. When she was led within two hundred yards of the place, she instantly perceived my odour, and ran in a straight line to where I had then ducked my head, so that she should receive no assistance from her sense of sight. This experiment shows that, in the absence of wind, the odour of my head (and no doubt, in a lesser degree, that of my body) had diffused itself through the air in all directions, and in an amount sufficient to enable the setter to recognize it as my odour at a distance of two hundred yards.

From the above experiments I conclude that this bitch distinguishes my trail from that of all others by the peculiar smell of my boots (1 to 6), and not by the peculiar smell of my feet (8 to 11). No doubt the smell which she recognizes as belonging distinctively to my trail is communicated to the boots by the exudations from my feet; but these exudations require to be combined with shoe-leather before they are recognized by her. Probably, however, if I had always been accustomed to shoot without boots or stockings, she would have learnt to associate with me a trail made by my bare feet. The experiments further show that although a few square millimetres of the surface of one boot is amply sufficient to make a trail which the animal can recognize as mine, the scent is not able to penetrate a single layer of brown paper (10). Furthermore, it would appear that in following a trail this bitch is ready at any moment to be guided by inference as well as perception, but that the act of inference is instantaneous (12 and 13 as compared with 2, 8, and 11). Lastly, the experiments show that not only the feet (as these affect the boots), but likewise the whole body of a man exhales a peculiar or individual odour which a dog can recognize as that of his master amid a crowd of other persons (15); that the individual quality of this odour can be recognized at great distances to windward (15), or in calm weather, at great distances in any direction (16); and that it does not admit of being overcome by the strong smell of aniseed (14), or by that of many other footprints (4).

FOSSIL WOOD FROM THE WESTERN TERRITORIES OF CANADA.[1]

SILICIFIED wood occurs in the country west of Manitoba in the Upper Cretaceous beds, in the Laramie and in the Miocene of the Cypress Hills, and has found its way into the drift. The numerous specimens in our collections, picked up on the plains, are thus of little palæontological value, as their sources are uncertain, and it has become desirable to obtain specimens found *in situ*. A small collection of this kind was made by Dr. G. M. Dawson in the course of the Boundary Survey, and was described in the Report on the 49th Parallel, in 1875. In 1880, Schrœter, in an appendix to Heer's paper on the plants of Mackenzie River, described a few species from the Laramie of that district. More recently, numerous specimens have been collected from beds of known geological age by Dr. G. M. Dawson, Mr. J. B. Tyrrell, and Mr. T. C. Weston, of the Geological Survey, and slices have been prepared by the latter. They include species from the Belly River and Fort Pierre groups, which are Upper Cretaceous; from the Lower Laramie, apparently a transition group between the Cretaceous and Eocene; and from the Upper Laramie, which is probably Lower Eocene, though at one time regarded as Miocene. These woods are mostly coniferous, but there are also angiospermous exogens of several kinds. In describing them in detail, they are not named as species, but merely referred to the modern genera which they most closely resemble. We thus find in the Belly River series two types of *Sequoia* corresponding to the wood of the two modern species, and woods of the types of *Taxus Salisburia* or *Ginkgo*, *Thuja*, and possibly *Abies*, along with exogens referable conjecturally to the genera *Betula*, *Populus*, *Carya*, *Ulmus*, and *Platanus*. In the Laramie we have a similar assemblage of conifers and exogens, with forms referable to *Pinus* and *Abies*, and to *Juglans* and *Acer* among the exogens.

[1] Abstract of a Paper by Sir William Dawson, read before the Royal Society of Canada, May 1887.

Some fruits and other fragments from the Belly River series appear to indicate the presence of a species of *Podocarpus*. Appended to the descriptions of the woods are notices of new species and localities in connexion with the Laramie flora, and remarks on the grand coniferous fruits of the period, as connected with the formation of coal and lignite. The concluding remarks are given in full, as of interest in connexion with the British Eocene flora :—

Concluding Remarks.—While studying the specimens described in this paper, I received the volume of the Palæontographical Society for 1885, containing the conclusion of Mr. Starkie Gardner's description of the Eocene Coniferæ of England. The work which he has been able to do in disentangling the nomenclature of these plants, and fixing their geological age, is of the greatest value, and shows how liable the palæobotanist is to fall into error in determining species from imperfect specimens. Our American species no doubt require some revision in this respect.

I have also, while writing out the above notes for publication, received the paper of the same author on the Eocene beds of Ardtun in Mull, and am fully confirmed thereby in the opinion derived from the papers of the Duke of Argyll and the late Prof. E. Forbes (Journ. Geol. Soc. of London, vol. vii.), that the Mull beds very closely correspond in age with our Laramie. The *Filicites hebridica* of Forbes is our *Onoclea sensibilis*. The species of *Gingko*, *Taxus*, *Sequoia*, and *Glyptostrobus* correspond, and we have now probably found a *Podocarpus* as noted above. The *Platanites hebridicus* is very near to our great *Platanus nobilis*. *Corylus Macquarii* is common to both formations, as well as *Populus arctica* and *P. Richardsoni*, while many of the other exogens are generically the same, and very closely allied. These Ardtun beds are regarded by Mr. Gardner as Lower Eocene, or a little older than the Gelinden series of Saporta, and nearly of the same age with the so-called Miocene of Atanekerdluk in Greenland. I have ever since 1875 maintained the Lower Eocene age of our Laramie, and of the Fort Union group of the North-Western United States, and the identity of their flora with that of Mackenzie River and Greenland, and it is very satisfactory to find that Mr. Gardner has independently arrived at similar conclusions with respect to the Eocene of Great Britain.

An important consequence arising from this is that the period of warm climate which enabled a temperate flora to exist in Greenland was that of the later Cretaceous and early Eocene, rather than, as usually stated, the Miocene. It is also a question admitting of discussion, whether the Eocene flora of latitudes so different as those of Greenland, Mackenzie River, North-West Canada, and the Western States, were strictly contemporaneous, or successive within a long geological period in which climatal changes were gradually proceeding. The latter statement must apply at least to the beginning and close of the period; but the plants themselves have something to say in favour of contemporaneity. The flora of the Laramie is not a tropical but a temperate flora, showing no doubt that a much more equable climate prevailed in the more northern parts of America than at present. But this equability of climate implies the possibility of a great geographical range on the part of plants. Thus it is quite possible, and indeed highly probable, that in the Laramie age a somewhat uniform flora extended from the Arctic seas through the great central plateau of America far to the south, and in like manner along the western coast of Europe. It is also to be observed that, as Gardner points out, there are some differences indicating a diversity of climate between Greenland and England, and even between Scotland and Ireland and the south of England; and we have similar differences, though not strongly marked, between the Laramie of Northern Canada and that of the United States. When all our beds of this age, from the Arctic Sea to the 49th parallel, have been ransacked for plants, and when the palæobotanists of the United States shall have succeeded in unravelling the confusion which now exists between their Laramie and the Middle Tertiary, the geologist of the future will be able to restore with much certainty the distribution of the vast forests which in the early Eocene covered the now bare plains of interior America. Further, since the break which in Western Europe separates the flora of the Cretaceous from that of the Eocene does not exist in America, it will then be possible to trace the succession of plants all the way from the Mesozoic Flora of the Queen Charlotte Islands and the Kootanie series, described in previous papers in these Transactions, up to the close of the Eocene; and to determine, for America at least, the manner and conditions under which the angiospermous flora of the later Cretaceous succeeded to the pines and cycads which characterized the beginning of the Cretaceous period.

THE LIVERPOOL MARINE BIOLOGY STATION ON PUFFIN ISLAND.

THE Liverpool Marine Biology Committee was formed in the spring of 1885 for the purpose of working up thoroughly the fauna and flora of that large rectangular area of the Irish Sea which lies around Liverpool Bay, and is bounded by the Isle of Man and the coasts of Anglesey, North Wales, Cheshire, and Lancashire. During the last three seasons the members of the Committee have conducted a large number of dredging, tow-netting, and other investigating expeditions in various parts of the Liverpool Marine Biology Committee district, and, as a first result of their labours, they published, in the summer of 1886, a "First Report upon the Fauna of Liverpool Bay and the Neighbouring Seas." It became evident at an early stage in these investigations that, as the sand-banks and channels in the immediate neighbourhood of the estuary of the Mersey are comparatively barren, it would be



FIG. 1.—Puffin Island from the north.

necessary, in order to carry on the work of the Committee satisfactorily, to establish a small marine laboratory somewhere on the coast of North Wales or Anglesey. Such a station, close to the region where there is a rich and varied fauna, and yet within easy reach of Liverpool, would enable the members of the Committee, and other biologists who were working with them, to pay frequent and regular visits to the best ground for the purpose of collecting specimens; and also to carry on observations on the habits of the animals, and to investigate their structures and life-histories. The Liverpool Marine Biology Committee have been aided in their work by small grants this year and last year from the Government Grant Committee of the Royal Society, and have received most important and generous assistance, by the loan of steamers for the dredging expeditions and in other ways, from some of the Liverpool ship-owners—amongst others, from the present Mayor, Sir James Poole, from Mr.

George Holt, and from the Liverpool Salvage Association—and now they owe the attainment of their desire for a marine laboratory to the kindness of Sir Richard Williams Bulkeley, Bart., of Beaumaris, in allowing them to make use, for scientific purposes, of the former Dock Board Telegraph Station on Puffin Island (Fig. 1).

Puffin Island, or Priestholme, is a small uninhabited island close to the north-east corner of Anglesey, and lying with its longer axis north-east and south-west. It is composed mainly of beds of limestone, and has precipitous sides, which have been worn into caves, crevices, and innumerable pools. The best landing-place is on the end nearest to Anglesey, where there is a beach of shingle. The shores all round the island support an abundant fauna, and some of the best dredging-grounds in the Liverpool Bay district lie close to Puffin Island, and a little further to the west along the coast of Anglesey. A glance at the accompanying chart will show the diversity in the depth of water off the north and east ends of the island (Fig. 2).

The house which the Liverpool Marine Biology Committee have now taken possession of as a centre for their further operations was built by the Liverpool Dock Board, and used as a signalling station, but has been uninhabited for some years. It contains four good rooms, besides lofts and out-houses, and a long observatory

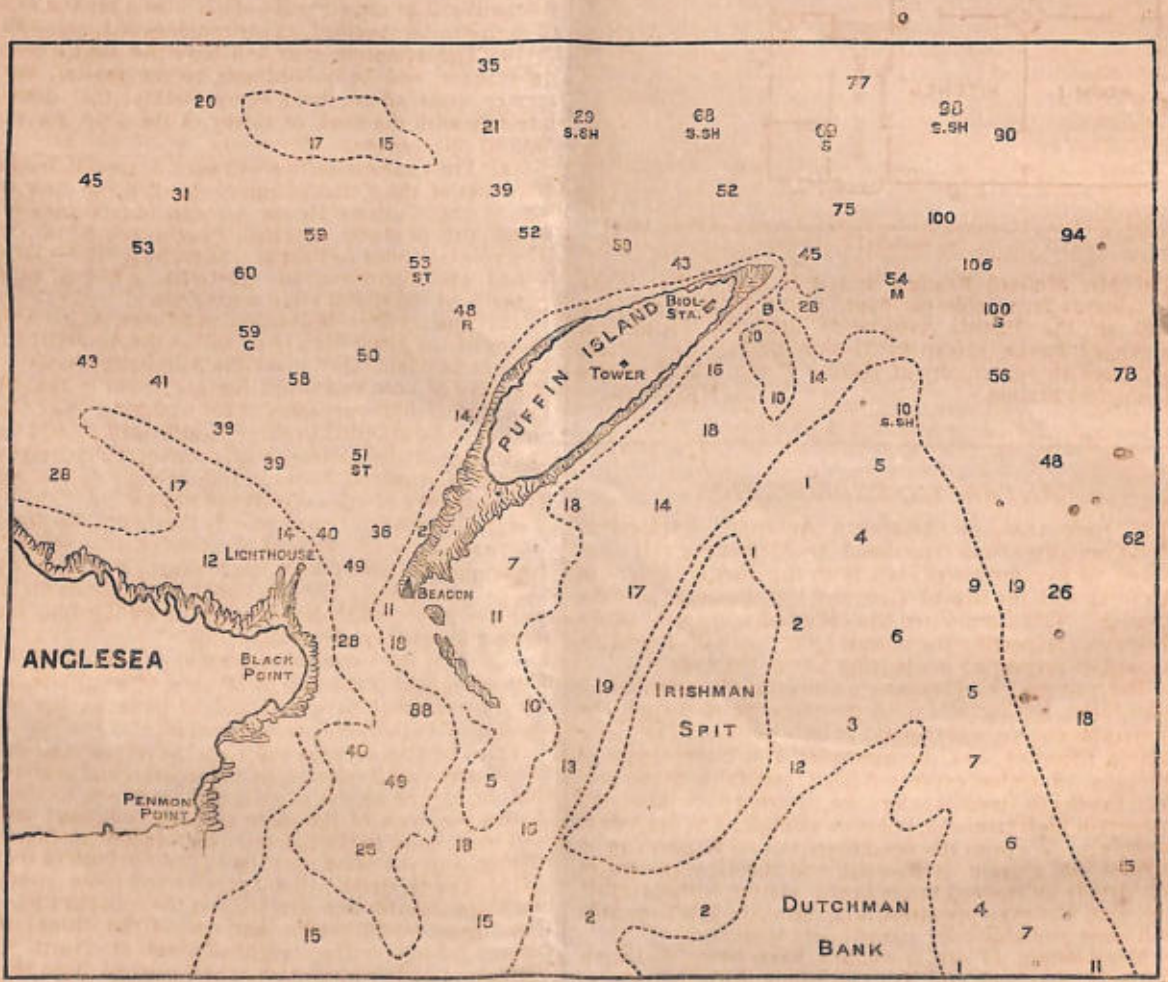


FIG. 2.

running towards the sea (north-east), and lighted by a series of seven windows round the outer end (Fig. 3). This observatory will make a well-lighted, convenient laboratory, while the other four rooms serve as kitchen and sleeping rooms for the naturalists and the keeper of the station.

At the end of May the new doors and windows, shutters, tables, and other fittings, which had been prepared in Liverpool, were ready for transference to the station, and a number of the Liverpool Marine Biology Committee, along with some workmen, were taken down to Puffin Island by the ss. *Hyæna*, which had been lent for the purpose by the Liverpool Salvage Association. The house was rapidly made weather-tight and put in working order, and is now under the charge of a keeper and his assistant. Tanks will soon be erected, and some of the shore-pools are being converted into natural aquaria. A small sailing-boat has been obtained, by which dredging and tow-netting in the neighbourhood of the island can be carried on, and by means of which communication can be kept up with the Liverpool steamers at Beaumaris and the railway at Bangor.

Since the establishment of the station some of the members of the Liverpool Marine Biology Committee have already had half a dozen expeditions to the island, and the following naturalists have commenced

work on their respective groups of animals: Mr. I. C. Thompson, on the Copepoda; Mr. J. Lomas, on the Polyzoa; and Prof. Herdman, on the Tunicata. Various other scientific men have come as visitors to see the station, including: Prof. Lodge, F.R.S., Prof. Hele Shaw, Mr. Reginald Phillips, of Bangor, Mr. I. Roberts,

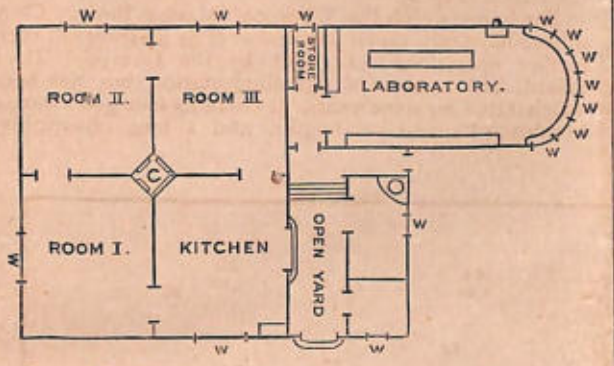


FIG. 3.—Plan of Liverpool Marine Biological Station on Puffin Island. w, windows; c, chimneys.

and Mr. Mellard Reade; and it is hoped that if the weather is favourable on Sept. 3, the biologists taking part in the British Association dredging expedition, arranged by the Liverpool Marine Biology Committee, will have an opportunity of visiting Puffin Island and its Biological Station.

W. A. HERDMAN.

ANTARCTIC EXPLORATION.

IN June 1886, an Australian Antarctic Exploration Committee was appointed at Melbourne. It consisted of five members each from the Royal Society of Victoria and the Royal Geographical Society, Victoria Branch. This Committee has collected a quantity of information respecting the islands lying south of Tasmania as well as respecting lands lying nearer the Pole.

The prospect of obtaining a Government grant for an expedition having scientific purposes only, though the preferable course, was thought to be hopeless. The Committee therefore has recommended the Government of Victoria (which had expressed itself favourably to the project) to offer to steam-whalers, for carrying a scientific staff to certain high latitudes, bonuses, graduated to degrees of southing. We print the conditions, but no tenders can be invited till a grant is assured, and the Government of Victoria is indisposed to act in the matter without other colonies, whose co-operation is doubtful, though Tasmania will most probably offer a small contribution.

Many offers of steam-whalers have been sent from England, Scotland, and Norway, where the owners seem anxious to dispose of their ships and gear. Most important and valuable information and advice have been received from Capt. Gray, of Peterhead.

The following are the recommendations of the Antarctic Committee to the Honourable the Premier of Victoria:—

(1) The Antarctic Committee begs respectfully to recommend to the Honourable the Premier the propriety of stimulating Antarctic research by the offer of bonuses.

(2) That a sum of £10,000 be placed upon the Estimates, to provide for the amount of the bonuses, and for the expenses of the equipment and of the staff.

(3) The amount of the bonuses to be paid to the shipowners for the hereinafter mentioned services is to be decided by tender, and the same, together with the cost of equipment and the staff, not to exceed the sum of £10,000.

(4) That the Government invite tenders from shipowners willing to perform the services required.

(5) That the tenders be sent to the Treasury direct, or through the Agent-General, not later than June 1.

(6) That tenderers must provide two fortified steamships, each of not less than 175 tons register, 60 horse-power nominal, and A1 at Lloyd's, or of an equivalent class.

(7) That tenderers must supply full descriptions of the ships and their equipments.

(8) That the master and chief mate of both ships shall have held similar positions in Arctic steam-ships.

(9) That the tenderer shall provide, free of charge, cabin accommodation in each ship for two gentlemen, who will sail as the scientific staff; also a separate cabin, of a size to be specified, as instrument-room and office.

(10) The scientific staff will have the status of cabin passengers, and be subordinate to the master, but the master must afford them every facility, that does not interfere with the work or safety of the ship, for noting natural phenomena.

(11) The chartered ships will earn a special bonus (to come out of the £10,000 appropriated) upon their entering at the Custom House a cargo of 100 tons of oil, being the produce of fish caught south of 60° S. The special bonus to be paid as follows, viz.:—To ships owned and registered in Australia, £1,000; to ships owned and registered elsewhere, £800.

(12) The services desired are as follows, viz.:—A flying survey of any coast-lines lying within the Antarctic Circle, and not now laid down upon the Admiralty charts. The discovery of new waterways leading towards the South Pole, and of harbours suitable for wintering in. Opportunities to be afforded to the scientific staff to add to our knowledge of the meteorology, oceanography, terrestrial magnetism, natural history, and geology of the region. The discovery of commercial products.

(13) The tenderer must specify the bonus he demands for passing 70° S. with either one or two ships; also the bonus he demands for each degree attained beyond 70° S. by one ship; also the bonus he demands for every occasion upon which he succeeds in establishing on the shore a temporary observing camp.

(14) That the Government should pay for only one such station for each 120 miles of latitude or longitude, unless the master shall have established more at the written request of both members of the staff.

(15) The staff to have the right to refuse to accept the site of any camp selected by the master, and such refusal shall be logged by the master, and read over to the staff in the presence of the mate and the surgeon; and the staff shall hand to the master their objections thereto in writing, and the same must be signed by both of them.

(16) The tenderer will not receive any more bonus for two ships than for one after passing the 70th parallel. The Committee would prefer that one of the ships should remain fishing in the neighbourhood of North Cape, Victoria Land, whilst the other pushed into higher latitudes. In case of accident to the latter, the former would serve as a depot and relief for the shipwrecked crew to fall back upon.

(17) Should the master of either ship despatch an exploring party from his vessel, the contractor will be entitled to a bonus for each sixty miles of latitude or longitude traversed by such party, but the tenderer must specify what sum he will require for each sixty miles so traversed.

(18) That the ships should proceed direct to the bight situated on the meridian of 180°, with a view of one of them getting beyond Ross's furthest, and especially of observing the conditions of the volcanoes at the head of the bight.

(19) The contractor will be liable to no penalty should he fail to reach to any latitude tendered for.

(20) The contractor will have the right to employ his ships in whaling or sealing, and in loading guano or other cargo.

(21) Should the masters be unable to get right or sperm whales to enable them to compete for the bonus offered under the 12th proviso, they will nevertheless be entitled to the bonus should they return with a cargo of any merchantable commodity obtained within the Antarctic Circle, and having a value equivalent to that of 100 tons of whale oil.

(22) Both ships must be in Port Phillip Bay and ready to start on October 15.

(23) That in case of any difficulty arising in England between the Agent-General and the contractor, it shall be referred to the British Antarctic Committee for decision.

THE CAPTIVE KITE-BALLOON.

IT has always been an objection to the extensive use of captive balloons for scientific or military purposes, that a wind of moderate strength suffices not merely to depress them considerably from the vertical, but to cause them to jerk, rotate, and oscillate vertically and horizontally in such a manner as to render them either partially ineffective or totally useless.

During the recent military manœuvres at Dover, it was stated that the captive balloon under the charge of Major Templer was not allowed to ascend beyond the shelter of the surrounding downs, owing to the strong wind then prevailing. It was thus *hors de combat* as far as the enemy was concerned, and this seems to be a common experience of military balloonists.

The jerking, as a balloon after a freshening of the wind suddenly reaches the end of its tether, is, I am told by an experienced member of the Balloon Corps, very trying to the nerves, while the rotation on its axis is a serious obstacle to steady observation.

The depression of a captive balloon in a wind of any sensible strength is also more than most persons would imagine, and as the velocity of the wind generally increases with the height (very rapidly for the first few hundred feet), while the buoyancy of the balloon, owing to several causes, diminishes, this condition becomes more pronounced at the higher levels.

The depression is obviously due to the fact that a captive balloon, as at present employed, can only be secured at its *base*, and thus the normal component of the wind is resolved in a downward direction, pressing the balloon towards the earth. If the fastening could be made two-thirds of the way up its side, this normal component could be resolved in an upward direction, and utilised so as to add to the elevating power of the balloon. The fragile nature of the balloon fabric, however, renders it impossible to do this except by interposing a kite-surface between it and the wind.

All the preceding defects are remedied and several positive advantages are gained by attaching a balloon to a kite in the manner indicated in the accompanying diagram.

(1) The addition of the kite with the fastening at the side instead of the base counteracts the depression produced by the wind, and not only raises its own weight, but even in a light anticyclonic breeze elevates the whole apparatus to a higher level than that which could be attained by the balloon alone.

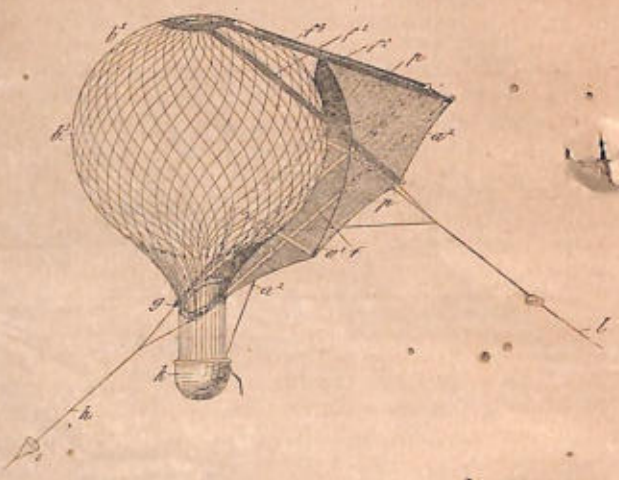
Thus, in an experiment here on Friday, June 10, in the presence of Mr. Eric S. Bruce and others, with a very light wind,[1] the balloon of 113 cubic feet capacity and with 1200 feet of wire out attained *alone* a mean vertical height of 693 feet, while when attached to a kite of 9 feet by 7 feet and the same length of wire it kept steadily at 789 feet. The lifting power in the second case was also greatly increased, as shown by the following comparison of the angles of the kite and wire in the two cases[1]:—

	Angle of Balloon.	Wire near the ground.
Balloon alone	38° ...	18°
Balloon with kite	41½° ...	35°

The addition of the kite raised 1¼ lbs. more than the balloon could have done alone, with a good deal to spare. It increased the height by 96 feet and diminished the sag by 13½°.

(2) With the tail (made of self-regulating cones) it completely counteracts the jerky, rotatory, and oscillatory movement of the balloon, by keeping the wire taut and exerting a constant pull on the balloon at its lower extremity.

(3) With the addition of the *top hood*, an essential



Archibald's Captive Kite-Balloon. a^1, octagonal kite, with frame of four pieces of bamboo; b^1, spherical balloon; f, covering of kite (preferably silk); p, extra or top hood; $f^1 f^2$, &c., bands connecting kite and hood with top of balloon; g, ring connecting lower end of kite with the converging net cords of balloon; h, tail of cones (i); l^1, earth-line connected with kite, one branch passing through a pulley to the car (k).

feature of the combination, the kite shields the balloon fabric from the destructive action of the wind.

(4) The combination can be flown on a much larger percentage of days than the balloon alone.

(5) In a large balloon with car attached the occupant can alter his altitude and azimuth by pulling the lower or side attachments of the kite, and thus extend his area of observation.

(6) With the kite, and except in the rare case of a dead calm, a much smaller balloon is needed to raise a given weight.[2]

(7) The use of wire (a suggestion which I owe to Sir William Thomson) greatly increases the strength, and lessens the weight, of the earth-line.

I arrived at the idea of uniting the two apparatuses while conducting my kite anemometrical observations in 1884, owing to my desire to prevent my kites from coming down suddenly when the wind dropped. I found the balloonists equally desirous of some means for shielding their balloons from damage and keeping them *up* in a *wind*. The kite-balloon satisfies both requirements, and will, I trust, be of use both to scientific as well as military observation.

E. DOUGLAS ARCHIBALD.

Tunbridge Wells, June 25.

[1] I have since ascertained that during the trial the mean velocity at Greenwich [211 feet above the sea with a good exposure for the wind (N.E.)] was 12 miles per hour. The present locality was in a valley 260 feet above sea-level, surrounded by hills rising to 500 feet above the sea.

[1] The lifting power of the balloon with hydrogen was about 5 lbs., the wire weighed about 4 lbs. and the kite 2¼ lbs.

[2] The kite portion is portable and easily detachable in the event of a calm.

NOTES.

A BILL dealing with the question of technical education was submitted to the House of Commons on Tuesday, and read a first time. We print elsewhere the speech delivered by Sir W. Hart Dyke in introducing the measure.

THE Report for 1886 of the Science and Art Department has just been issued.

THE summer meeting of the Institution of Mechanical Engineers will be held in Edinburgh, on Tuesday morning, August 2, and Wednesday morning, August 3, in the University. The chair will be taken at half-past nine o'clock by the President, Mr. Edward H. Carbutt, in the Natural History Lecture Theatre. The following papers have been offered for reading and discussion, not necessarily in the order here given:—On the structure and progress of the Forth Bridge, by Mr. E. Malcolm Wood, of London; notes on the machinery employed at the Forth Bridge works, by Mr. William Arrol, of Glasgow; on the paraffin oil industry in Scotland, by Mr. St. John V. Day, Honorary Local Secretary; description of the electric light on the Isle of May, by Mr. David A. Stevenson, of Edinburgh; description of the new Tay Viaduct, by Mr. Fletcher F. S. Kelsey, Resident Engineer; on electro-magnetic machine-tools, by Mr. Frederick John Rowan, of Glasgow; on the dredging of the lower estuary of the Clyde, by Mr. Charles A. Stevenson, of Edinburgh; on the position and prospects of electricity as applied to engineering, by Mr. William Geipel, of Edinburgh. Various excursions are being arranged, and it is desired that members who propose to be present, and to accept the several invitations, should let their intention be known without delay.

THE summer meetings of the Institution of Naval Architects will be held at Newcastle-on-Tyne on July 26 and 28, and at Sunderland on July 27. The following papers will be read at Newcastle: on the application of hydraulic pressure to naval gunnery, by the Right Hon. Lord Armstrong, F.R.S. Vice-President, and Mr. J. Vavasseur, Associate; recent developments in marine engineering, by Mr. Frank C. Marshall, Member of Council; Tyne improvements, by Mr. P. J. Messent, Engineer to River Tyne Commissioners. At Sunderland the following papers will be read: on some recent experiments with basic steel, by Mr. W. H. White, Director of Naval Construction, Vice-President; on the present position occupied by basic steel for ship-building, by Mr. B. Martell, Chief Surveyor to Lloyd's Register of British and Foreign Shipping, Vice-President. There will be excursions to places of scientific interest in the neighbourhoods.

MR. THOMAS HUDSON BEARE has been unanimously appointed by the Governors of the Heriot-Watt College, Edinburgh, Professor of Mechanics and Engineering in that institution. Mr. Beare came over to this country from Australia in 1880, having gained the South Australian Scholarship at the University of Adelaide. He then became a student at University College, London, to which he afterwards returned about three years since to be one of the principal demonstrators under Prof. Kennedy in the Engineering Laboratory.

THE Geographical Society of St. Petersburg has decided to send an Expedition to Turkestan for the scientific investigation of the earthquake at Werny. Prof. Muschketoff, the head of the Expedition, will be accompanied by five other men of science, including the St. Petersburg geologist, M. W. S. Dmitrewski.

THE seventh Bulletin of Miscellaneous Information, issued from the Royal Gardens, Kew, contains a careful and interesting paper on annatto, a colouring substance obtained from the seeds of *Bixa Orellana*. This colouring substance has long been known and used for various purposes. It is, however, liable to so many fluctuations, and the prices generally are so low, that it has never received serious attention in British colonies, and hence few, if any, plantations have been exclusively devoted in such colonies to the annatto plant. The annatto of commerce is practically a forest product obtained from wild or semi-wild plants, and the supply has only kept pace with the demand. Of late years a slight revival has taken place in the use of annatto, especially in America, and inquiries have in consequence been made for information as regards culture and preparation. This information the writer of the paper in the Bulletin supplies, and his notes will be of great service to all who may wish to become growers of annatto.

ON May 9 the Governor of Jamaica addressed to the Governors of Barbados, the Leeward Islands, the Windward Islands, and British Honduras, a letter relating to the scheme for the establishment of botanical stations in some West India Islands in connexion with the Botanical Department in Jamaica. From this letter, which is printed in the seventh Kew Bulletin of Miscellaneous Information, we are glad to learn that the Government of Jamaica is prepared to adopt the proposed scheme from August 1, or from any subsequent date.

BEFORE the end of the year the great Tweeddale collection and library will, it is hoped, be safely housed in the Natural History Museum. This princely donation to the national collection is the gift of Capt. R. G. Wardlaw Ramsay, to whom it was bequeathed by his uncle, the late Marquess of Tweeddale. With the exception of the Hume Collection it is the finest series of Indian birds in existence, and is especially rich in species from the Philippine Archipelago, where Mr. Alfred Everett collected for some years for Lord Tweeddale. Capt. Ramsay's collections from the Karen Hills, in Burmah, are also most important, this being one of the few localities unworked by Mr. Hume's collectors.

THE American Museum of Natural History, New York, has just acquired the ornithological library of Mr. D. G. Elliot, a well-known American naturalist. This library consists of about 1000 volumes, and is one of the most important in America. Mr. Elliot has at the same time presented his collection of humming-birds to the above Museum. It is, according to the *Auk*, "represented by about 2000 specimens, and includes some fifty or more types. Its importance is further enhanced from its having formed the basis of Mr. Elliot's recent monograph of the family. It doubtless ranks as second in the world in point of completeness, or next to that of the British Museum." The latter collection, however, must now contain at least 10,000 skins, irrespective of the Gouldian series of mounted specimens. Another important addition to the American Museum is that of the large ornithological collection of Mr. G. N. Lawrence, which contains some 300 types. This has been purchased, and is one of the chief of the private collections in America.

THE special groups, illustrating the nesting habits of British birds, which have proved so attractive in the Natural History Museum at South Kensington, have now been introduced into the galleries of the American Museum of Natural History, and twelve cases of American birds have already been mounted. The cost of these effective, but expensive, groups will be defrayed by Mrs. Robert E. Stuart, and the Museum has secured the services of Mrs. Mogridge, who executed the artificial flower-work for the British Museum. Mrs. Mogridge is without a rival in this branch of decorative art.

THE expedition made by Mr. John Whitehead to the great mountain of Kina Balu, in Northern Borneo, has turned out more successful than could have been foretold. In addition to

the splendid new Broadbill, described by Mr. Bowdler Sharpe as *Calyptomena whiteheadi* at the last meeting of the Zoological Society, there are nearly twenty other new species, including some very remarkable forms of *Arachnothera*, *Chloropsis*, *Cryptolopha*, and an apparently new genus of *Campophagidæ*. These will all be described in the October *Ibis*, by Mr. Sharpe.

DEALING with the question as to the influence of small birds in assisting the extinction of *Aporia crataegi*, Mr. A. G. Butler, in the current number of the *Entomologist's Monthly Magazine*, says he has collected in Kent for at least thirty years, and during the whole of that time he has never seen any bird but a sparrow attempt to catch a butterfly. Nor has he ever known a small bird to eat a large caterpillar if it could get one that could be more easily swallowed. "Of our indigenous species," he says, "the robin and the great tit certainly select green caterpillars in preference to others, and, when feeding their young, I have watched both these birds with their mouths full of the green pests of the gooseberry and currant. From observation of cage-birds I should say that the finches certainly show a similar preference, the green larvæ of *Mamestra* being chosen before the brown, though all are greedily devoured."

A BOLD attempt has recently been made to penetrate the darkness surrounding the subject of the inter-molecular arrangement of atoms, and to raise our ideas of the constitution of chemical compounds beyond what is expressed by the orthodox chemical formulæ. It has long been felt that the chemical formula of a substance as expressed in one plane on paper, although invaluable as far as it goes, must of necessity be a very misleading one, inasmuch as it in no way indicates the probable position of the various atoms in space. This insufficiency has been especially felt in the case of substances like tartaric acid, where we have several distinct isomers acting quite differently upon polarized light, and frequently forming right- and left-handed hemihedral crystals, although expressed by the same constitutional formula. Since the year 1874, when Van t' Hoff and Le Bel published their celebrated theory of the "asymmetric carbon atom," the idea has been gaining ground that this kind of isomerism must be due to different spacial arrangement, and Van t' Hoff gave impetus to the theory by showing that the existence of four isomeric tartaric acids could be explained by imagining the four radical-groups to be variously placed at the four corners of a regular tetrahedron, of which an asymmetric carbon atom occupied the centre. During the last few days a comprehensive paper has been issued by Prof. J. Wislicenus, on the "Spacial Arrangement of Atoms in Organic Molecules, and its Determination in Geometrically Isomeric Compounds," further expanding Dr. Van t' Hoff's somewhat sceptically received ideas, proceeding to build up the spacial constitution of a large number of unsaturated organic compounds, and giving nearly 200 figures, of which the regular tetrahedron representing CH_4 is the base. Prof. Wislicenus practically demonstrates that the cases of so-called abnormal isomerism may be completely cleared up, and that existing experimental data are generally sufficient to enable spacial constitution to be determined. This remarkable paper will doubtless give rise to much discussion, and appears likely to lead to results which will mark a genuine advance in chemical philosophy.

IN a paper to the Berlin Academy, Herr Liebreich lately called attention to what he calls the "dead space" in chemical reactions: a space, *i.e.*, in which the reaction going on in other parts of a uniformly mixed liquid does not occur, or occurs late, or in less degree. It may be very well observed, *e.g.*, in decomposition of chloral hydrate by sodium carbonate (yielding chloroform) in a test-tube. A layer of 1 to 3 millimetres' depth under the surface remains clear, and separate by a convex surface from the "reaction space," where the solution is turbid from droplets of chloroform. Even after twenty-four hours' rest of the mixture the two spaces can be distinguished; and after mixture by shaking again, the surface of separation is reproduced in a few minutes. In horizontal capillary tubes the dead space appears on both sides, and, if the liquid columns introduced are short, no reaction occurs, as the dead spaces unite. Thus is explained the absence of reaction in the case of liquid absorbed in vessels by glass pearls; there is dead space everywhere. Contact with air seems essential to the formation of dead space. Thus if a vessel closed at one end, and holding the liquids above named, be inverted, so that there is no air space above the mixture, the reaction occurs uniformly throughout. But if the upper end be closed with a fine animal membrane, the dead space appears as in the former case; and if the lower end be also closed with fine membrane the dead space appears there too. Herr Liebreich is studying the phenomena further.

SOME instructive experiments on atmospheric electricity are described by Herr Nahrwold in *Wiedemann's Annalen*, No. 7; one being a suitable lecture experiment, showing the action of electricity from points on finely divided matter in the air. He thinks it established that such a stream of electricity does not electrify the air itself statically (indeed that air and other gases probably cannot be statically electrified), but only dust particles in it. Further, a glowing platinum wire sends out particles which diffuse in air that has been electrically freed from dust, making a fresh charge possible. Here, too, the electricity streaming from such wire does not statically electrify the air, but the charges which are observed as atmospheric electricity belong to fine non-gaseous particles given out by the wire, or already present in the air. An experiment is also adduced to show that at ordinary temperature negative electricity of high potential streams more readily from solid conductors into atmospheric air than positive.

ELECTRICITY in the house has some important bearings on hygiene. One of these M. Sambuc has recently called attention to (*Revue d'Hygiène*), in the liberation of hydrogen, where strong batteries are used in which zinc is dissolved by sulphuric acid. Besides the danger of shattering of the vessels, the hydrogen spreading in the air may form an explosive mixture; and it may have a cooling effect through its great conductivity for heat. It also deadens the voice and alters its timbre. Further, if, as may be, the hydrogen is charged with sulphur, arsenic, phosphorus, carbon, or silicium, there are other and greater dangers. A chemist is known to have died from breathing a little arsenietted hydrogen. These facts are not cited against the use of the electric light, but to induce proper care in those who use it.

THE twenty-sixth volume of the magnetical and meteorological observations made at the Government Observatory, Bombay, containing the results for the year 1885, has just been published, under the superintendence of Mr. C. Chambers, F.R.S. Continuous registrations are obtained by means of self-recording instruments (although not published), and eye-observations are taken five times a day, as a check upon the automatic records. The following is a summary of the principal meteorological results:—The mean barometric pressure for the year was 29·826 inches, the difference of the greatest and least mean daily pressure amounting to 0·581 inch. The mean annual temperature was 79°·2, and the greatest daily mean was 87°·3 on June 6. The absolute maximum was 91°·8 in June (being slightly *lower* than the maximum in the shade at Greenwich on the 4th inst.), and the minimum 62°·1 in February, giving a range of 29°·7. The rainfall measured by a gauge 4½ feet above the ground was 67·91 inches; rain fell on 113 days, and mostly occurred between June and September; the greatest fall was 10·29 inches on August 15.

The Observatory does not appear to possess a sunshine-recorder. Observations for a few selected hours for Bombay and five other stations for the years 1885–86 have already been published separately by the Indian Meteorological Office.

METEOROLOGICAL observations have been regularly made at the Khedivial Observatory at Cairo (Abassieh) during the past five years, and have been published in various forms. The publication has now assumed a more definite shape, under the title, *Résumé Mensuel*, and is issued by the Ministry of Public Instruction. The observations are taken every three hours during the day and night. Yearly summaries are not given, but we find from the monthly values that the mean shade temperature for the year 1886 was 69°·6. The absolute maximum in the shade was 113°·4 in June, and the minimum, 36°·7, in December; giving a yearly range of 76°·7. The thermometers are placed much too high, being about 33 feet above the ground, instead of about 4 feet. The amount of rainfall is not regularly published.

THE Jahrbuch of the *Magdeburgische Zeitung* for the year 1885 (Magdeburg, 1887, 88 pp. 4to) contains, in addition to the usual observations and reproductions of the continuous registrations of barometer and sunshine-recorder, a table showing the extremes of temperature on the surface of the earth observed by means of five maximum and five minimum thermometers, one pair lying flat and the other four pairs being inclined about 45° under the four principal points of the compass, between May 1885 and April 1886; but there is no discussion of the results. There is also an interesting appendix relating to the choice of hours that will give the nearest approach to the mean daily temperature. The author has used the continuous records for Berne, Vienna, Magdeburg, Pawlowsk (near St. Petersburg), and Upsala for a year, and has found the following to be the mean values of the corrections to be applied to the various yearly means:—For 8h., 2h., 8h., 0°·040; for 7h., 2h., 9h., −0°·092; for 6h., 2h., 10h., 0°·104; and for max. and min. 0°·084. The best combination according to this investigation is therefore 8h., 2h., 8h., whereas in this country 9h. a.m. and 9h. p.m. are found to give a good mean. The combination of max. and min. also gives a fairly approximate value for mean latitudes. The author has also investigated the epoch of the maximum and minimum temperature for the same places, and shows how the highest daily temperature occurs later as the summer advances, being at about 3h. p.m. in June and July and between 12h. and 1h. in January and December; and further, that the lowest temperature does not always take place at about sunrise, as is generally supposed, but only during summer, while in winter the minimum is near midnight. The present Director of the Observatory is A. Grützmacher; the former Director, Dr. Assmann, having been appointed to the Meteorological Office at Berlin.

MR. A. L. ROTCH has published the results of the observations made at the Blue Hill Meteorological Observatory, Norfolk County, Massachusetts, U.S., in the year 1886 (NATURE, vol. xxxv. p. 472). This Observatory, which was established by Mr. Rotch in 1885, is now one of the best-equipped stations in the United States, and the current expenses amount to about 2500 dollars a year. An auxiliary station has also been established at the foot of the hill, 440 feet below the Observatory, and some curious variations of temperature and precipitation have been noted between the two stations, but enough data have not yet been accumulated for publication. Among the special instruments in use may be specified a Campbell-Stokes bright-sunshine recorder, which is believed to be the only one in the United States; a Jordan sunshine-recorder, which registers both bright and faint sunshine photographically; and a mirror for the measurement of the azimuth and altitude of clouds, but these results are not yet ready for publication. The mean temperature for the year was 45°·6. The absolute maximum in the shade was 91°·0 in July, and the minimum −15°·0 in January, giving a yearly range of 106°. The greatest daily range was 38°·2 on December 25, and the least 1°·7 in February. The total rainfall and melted snow was 46·99 inches, measured on 132 days; the greatest monthly fall being 8·29 inches in February, and the least 1·52 inch in June. The work is accompanied by tracings from the self-recording instruments, selected to illustrate certain phenomena during the year, with explanatory text, a practice which is both inexpensive and very instructive. The hourly tabulations of atmospheric pressure and wind velocity have been published *in extenso*.

ON February 5 last there was a shower of ashes, lasting from 7 a.m. to 11 a.m., at Finschhafen in Kaiser Wilhelm's Land. It covered the surrounding district with a layer of pale grey volcanic ashes. As the condition of the winds at the time was abnormal, it is impossible to say in what locality the volcanic eruption took place. Dr. Schrader reports that on February 2 a bright red halo, as if produced by smoke at a great elevation, was noticed around the sun; a few evenings before, similar halos had been noticed around the moon. Samples of the ashes have been sent to Dr. Neumayer, of Hamburg, for analysis.

SOME time ago, Mr. F. W. Putnam, the American archæologist, wrote a letter to the newspapers, pointing out that the Serpent Mound in Adams County, Ohio, had lately been much damaged by "wash-outs," and begging that steps might be taken for its preservation. Thereupon three Boston ladies took the matter in hand. The money they asked for was soon obtained, and now the ground upon which the mound is situated has been bought, and handed over to the guardianship of the Trustees of the Peabody Museum of American Archæology and Ethnology. Mr. Putnam, through whom the purchase was effected, proposes to spend the approaching autumn in the neighbourhood of the mound, restoring it where it has been injured, transforming wheat-fields into grass lawns, making paths and fences, and planting trees. "So long as the place is respected and guarded by all who visit it," he says in a letter to a Cincinnati newspaper, "the park will be free to all, but should any vandalism be committed, an arrangement would at once be made to put a keeper at the place, and possibly entrance fees would have to be charged in order to pay the expenses."

PROF. A. H. KEANE's translation of "The Necropolis of Ancon, in Peru," a German contribution to our knowledge of the culture and industries of the empire of the Incas, presenting the results of excavations made on the spot by W. Reiss and A. Stübel, has been issued in fourteen parts by Messrs. A. Asher and Co. during the years 1880–87. The work is now ready in three volumes, which contain, besides a comprehensive text, 141 coloured plates in folio. A separate volume, complete in itself, but at the same time forming a supplement to the present work, is in course of preparation. It will contain treatises by Herren W. Reiss, A. Stübel, L. Wittmack, R. Virchow, and A. Nehring.

WE have received Part I. of the Annual Report of the Board of Regents of the Smithsonian Institution, showing the operations, expenditures, and condition of the Institution, to July 1885. In addition to the Secretary's Report, there is a general appendix containing some valuable scientific papers. In one set of these papers an account is given, by eminent writers, of the progress made during the year 1884 in astronomy, geography, physics, chemistry, and other sciences. Other papers deal with various problems in anthropology.

THE Clarendon Press is publishing a fourth edition of "Exercises in Practical Chemistry," by Mr. A. G. Vernon Harcourt,

F.R.S., and Mr. H. G. Madan. The first volume, containing elementary exercises, has been issued. In the preface to this new edition, Mr. Madan, who has undertaken the task of revision, explains that he has made some verbal alterations, introduced additional experiments and exercises, and somewhat altered the course of analysis of a single substance. In many cases the preparation of useful compounds of the radicle is more fully dealt with than in former editions.

THE "Flora of West Yorkshire," a volume of about 800 pages, by Mr. Frederick Arnold Lees, will be ready in August. It will be published by the Yorkshire Naturalists' Union, by subscription, and will form an extra volume of the Botanical Series of the Transactions of the Union. The work is divided into four sections—(1) Climatology; (2) Lithology; (3) the Botanical Bibliography of the Riding; (4) the Flora proper. With regard to the fourth section, it is claimed that "such a complete flora for any district in the world has never before been published, more than 3000 species being dealt with."

AN interesting volume relating to the "Grand Concours International des Sciences et de l'Industrie," which is to be held at Brussels in the year 1888, has just been issued. It consists of reports drawn up by the Committees which have been appointed to make preparations for the Exhibition. Each of these reports includes a letter addressed to producers, a general and detailed classification of objects, a list of sub-committees, and a series of desiderata in the department to which the report relates. If the "Grand Concours International" corresponds to the scheme which the Committees have worked out, it will be one of the most complete and suggestive Exhibitions that have yet been held.

ON August 7 the University of Göttingen will celebrate the 150th anniversary of its foundation.

THE annual *conversazione* given by the students of the Finsbury Technical College was held on Friday the 15th inst., and was remarkably successful. The College was tastefully decorated with flowers and flags, and a large fountain, illuminated by powerful coloured arc and incandescent lamps, played during the evening. All the rooms were thrown open to visitors, and exhibitions of chemical, electrical, and mechanical apparatus and manufactures were arranged in the laboratories. Over fifty of the leading scientific firms lent exhibits, and one electrical firm sent over £500 worth of apparatus. In the workshops specimens of the work of the students during the session were shown. Two concerts, both attended by crowded audiences, were given; and Prof. Ayrton lectured on "Church Bells," and Prof. Meldola on "Spectrum Analysis." Over four hundred visitors were present, including many distinguished men of science and commerce; and the students are to be congratulated on having provided a very pleasant entertainment for their friends.

THE additions to the Zoological Society's Gardens during the past week include a Pig-tailed Monkey (*Macacus nemestrinus*) from Java, presented by Mrs. Lewis; a Tiger (*Felis tigris* ♂) from India, presented by Mr. Sandford Kilby; a Turtle-Dove (*Turtur communis*), British, presented by Mr. R. Humphries; a Bonnet Monkey (*Macacus sinicus* ♀) from India, two Booted Eagles (*Nisaetus pennatus*) from Spain, a Golden-crowned Conure (*Conurus aureus*) from Brazil, two Alligators (*Alligator mississippiensis*) from the Mississippi, two Common Toads (*Bufo vulgaris*) from North Africa, deposited; a Ruffed Lemur (*Lemur varius*) from Madagascar, an Elate Hornbill (*Ceratogymna elata*) from West Africa, two Common Boas (*Boa constrictor*) from South America, purchased; a Squirrel-like Phalanger (*Belideus sciureus*) born in the Gardens; two Diuca Finches (*Diuca grisea*), an Auriculated Dove (*Zenaida auriculata*) bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

THE NICE OBSERVATORY.—M. Faye has published in the *Comptes rendus*, tome cv. No. 1, a note on the work of the Nice Observatory, from which the following particulars are extracted:—As soon as a small meridian circle by Gautier had been erected at the new Observatory, M. Perrotin, the Director determined the difference of longitude telegraphically from Paris and from Milan. These operations gave for the difference: Paris–Milan, 27m. 25·325s., whilst a direct determination previously made by MM. Perrier and Celoria gave 27m. 25·313s. The value 43° 43′ 16″·9 has been provisionally adopted for the latitude. With the equatorial of 0·38 m. aperture M. Perrotin has undertaken an extensive series of double-star measures, which have already proved of great excellence and value. It is proposed to continue these measures on a more extended scale with the large telescope of 0·76 m. aperture. A large number of observations of comets and of minor planets have been made by M. Perrotin and by M. Charlois, his assistant. The latter has also quite recently discovered a new asteroid (No. 267). M. Faye goes on to speak of the spectroscopic researches carried out at Nice by the late M. Thollon, particularly those connected with the investigation of the telluric lines in the solar spectrum. As our readers will remember, M. Thollon showed that in the regions B and α of the solar spectrum some of the telluric lines are due, not to an element varying with the temperature, such as aqueous vapour, but to a constituent of the atmosphere, such as oxygen, the influence of which varies with the altitude of the Sun only. M. Egoroff afterwards confirmed this by showing that the lines referred to are due to the oxygen present in our atmosphere.

The instrumental equipment of the Nice Observatory is now all but complete, and M. Faye speaks with enthusiasm of the career of usefulness before it—favoured as it is with a splendid climate, and, thanks to the munificence of M. Bischoffsheim, with instruments which suffice to place it in the front rank of modern Observatories.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 JULY 24–30.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on July 24

Sun rises, 4h. 14m.; souths, 12h. 6m. 14·3s.; sets, 19h. 59m.; decl. on meridian, 19° 54′ N.: Sidereal Time at Sunset, 16h. 8m.

Moon (at First Quarter on July 27) rises, 8h. 38m.; souths, 15h. 26m.; sets, 22h. 0m.; decl. on meridian, 5° 53′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Decl. on meridian. ° ′
Mercury ...	5 20	12 33	19 46	13 24 N.
Venus ...	8 33	15 1	21 29	4 47 N.
Mars ...	2 8	10 28	18 48	23 53 N.
Jupiter...	12 20	17 35	22 50	9 29 S.
Saturn...	3 48	11 48	19 48	20 57 N.

Occultation of Star by the Moon (visible at Greenwich).

July.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. ° °
25 ...	B.A.C. 4277 ...	6 ...	20 20 ...	20 45 ...	34 350

July.
24 ... 4 ... Venus in conjunction with and 3° 8′ south of the Moon.
27 ... 1 ... Jupiter in conjunction with and 3° 59′ south of the Moon.
29 ... 5 ... Mercury in inferior conjunction with the Sun.

Meteor-Showers.

The Aquarids, R.A. 340°, Decl. 13° S., near δ Aquarii, form the principal meteor-shower at this season of the year; the meteors from this radiant are slow, in marked contrast to those from Perseus, radiant at R.A. 32°, Decl. 55° N., at the same time, which are swift.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·3	81 16 N.	July 27,	21 51	*m*
Algol	3 0·8	40 31 N.	,, 30,	2 5	*m*
δ Libræ	14 54·9	8 4 S.	,, 29,	22 50	*m*
S Ophiuchi	16 27·8	16 55 S.	,, 30,		*M*
U Ophiuchi	17 10·8	1 20 N.	,, 26,	3 16	*m*
			26,	23 24	*m*
U Sagittarii	18 25·2	19 12 S.	,, 25,	0 0	*M*
β Lyræ	18 45·9	33 14 N.	,, 25,	8 30	*M*
T Sagittarii	19 9·7	17 10 S.	,, 28,		*M*
S Vulpeculæ ...	19 43·8	27 0 N.	,, 25,		*M*
η Aquilæ	19 46·7	0 43 N.	,, 25,	22 0	*M*
R Vulpeculæ ...	20 59·4	23 22 N.	,, 28,		*M*

M signifies maximum; *m* minimum.

GEOGRAPHICAL NOTES.

THE new supplementary part of *Petermann's Mitteilungen* (No. 87) is devoted to Dr. R. von Lendenfeld's explorations in the Australian Alps in 1885–86. The region explored by Dr. Lendenfeld covers the greater part of the mountain districts of Victoria and New South Wales, and already in NATURE and elsewhere he has given some details concerning the geological and glacial results of his work. In the present memoir he gives a sketch of the Australian Alps in general, their geology, physiography, meteorology, flora, and fauna; he indicates the general physiognomy of the mountain system, its leading ranges, its valleys, and its river systems. He then devotes separate sections to the Kosciusko group and the Bugong group, and to a discussion of the Australian Ice period. There can be no doubt, Dr. Lendenfeld maintains, that at one time the Australian highlands were deeply glaciated, and that during the Tertiary the climate of the country must have been far richer in moisture than it is at the present day.

IN the new number (vii.) of *Petermann's Mitteilungen* Dr. Gerhard Rohlfs describes in a letter to Dr. Schweinfurth the results of his recent exploration of the limestone plateau which borders each side of the great Wadi Arabah, in Central Egypt. General Tillo brings together elaborate data bearing on the variation of the mean sea-level above or below a normal zero in the various seas of Europe; and Nikolaus Latken contributes a short paper on mining in East Siberia for 1874–85. There is an excellent map of the Khuriseb Valley, extending south-east from Walfisch Bay, West Africa, by Dr. Stapff, which, with the accompanying paper, gives a very full idea of the geology of the region.

A NUMEROUS and carefully-equipped Expedition is being sent out this summer by the Finnish Society of Botany and Zoology for the exploration of the interior of the Kola Peninsula. Another Expedition, organized by the St. Petersburg Society of Naturalists, set out last month to Petropaulovsk, to explore the White Sea and the Mediterranean coast.

UNDER Prof. O. Doering, the Government of the Argentine Province Cordoba is establishing a network of meteorological stations which will begin work in January 1888. It is intended to form and equip 40 stations of the first order, 15 of the second, 10 of the third, and 10 of the fourth order. The instruments are being obtained from Berlin.

DR. L. BRACKEBUSCH, Professor of Geology and Mineralogy in the University of Cordoba, has recently returned from a five-months' excursion in the Cordilleras, bringing with him rich collections of minerals, and a mass of geological, geographical, and hypsometrical data.

THE Venezuelan Government has, it is stated, organized an Expedition for the geological and anthropological exploration of the territory on the Upper Orinoco and the Amazons.

AT a recent meeting of the Geographical Society of the Pacific, Prof. Davidson stated that his study of the ocean currents had brought him to the conclusion that a branch of the Japanese warm current, the Kuro Siwo, does pass into the Arctic Ocean through Behring Strait; and he promised to lay before the Society, at a future meeting, some information on the subject.

ACCORDING to the last mail from Zanzibar Lieut. Wissmann has arrived at the Kavala mission station on Lake Tanganyika. The explorer left Luluaburg on the Saukuru in November last, to traverse the unknown country in which are the sources of the Lulongo, the Chuapa, and the Lomami. He then meant to reach Lake Tanganyika by Nyangwé.

To the last part of the *Verhandlungen* of the Vienna Geographical Society (Nos. 5 and 6 of Band xxx.) Herr W. Putick contributes a valuable paper on the subterranean district of Inner Carniola, the curious region known as the Karst.

THE TECHNICAL EDUCATION BILL.

THE following is the speech delivered by Sir W. Hart Dyke on Monday in introducing the Technical Education Bill into the House of Commons:—

"In the observations that I am about to make I shall be as concise as possible, because I know that members are waiting to deal with other important matter. I feel that I am guilty of something like cruelty in introducing at this period of the session, after all we have gone through and with the labours still before us, any further legislation, but I plead in extenuation the fact that this is no new topic. It is one which has for some time past stirred up among the artisan classes considerable interest. Voluntary efforts have for some time past been made in this country in regard to technical instruction, and if I am asked why it is that we are going to endeavour to supplement by legislation what has been done the answer is that it is because we believe in the reality of this movement. For some years, not only among our artisan classes, but among our large employers of labour in industrial centres, it has been recognized that, though the commercial depression cannot be traceable to the lack of technical and commercial education in this country, yet that some part of it is due to the fact that Continental nations have had great advantages over us in regard to technical training for their youths, and that this has given them considerable commercial advantages over us. I am encouraged to hope that these proposals will meet with some acceptance from the House. If they enable the best material which is now turned out by our schools to continue longer in their school life and to start into some new educational groove for the benefit of themselves and of the industrial localities in which they live, and for the benefit also of the community at large, I think I may venture to urge that the time of the House will not be wasted in discussing these proposals. It is perfectly true that it may be urged that as I have not long held my present office I am rather rash in introducing this subject, and still more so considering that a Royal Commission has been sitting for some time and dealing with this great educational question. But I think that the House will agree with me that this is somewhat outside the scope of the Commission which is now sitting. There was a Royal Commission on Technical Education which reported in 1884. That Commission let in a flood of light on the question of technical instruction, and I should like for one instant to refer to their special recommendation as regards this country. As the House is aware, that Commission extended its labours to Continental countries, and conducted an exhaustive inquiry in connexion with this subject. The Commission pointed out that there was a considerable difference in respect of our treatment of the educational question and its treatment in countries abroad. They also pointed out that with the exception of France there was no European country of the first rank that has an educational Budget so large in amount as our own. They say that all our existing educational institutions will not alone accomplish the object aimed at, and that the localities must rely more than they have done hitherto upon their own special exertions. I may quote further from the Report of the Commission in reference to the advisability of introducing technical instruction into our schools. The Commissioners state that in Manchester, Sheffield, Birmingham, and other great centres, a considerable step has already been made in this direction, and they ask this pertinent question: "If we introduce needlework into girls' schools, why should not grants be made for manual instruction in boys' schools?" The Commissioners also recommended that rudimentary drawing should be continued through-

out the standards. These are some of the recommendations with which the present Bill proposes to deal. I think I am not taking too sanguine a view of this scheme when I say that it will carry out all those proposals. The object of the Bill is to enable local authorities to provide for the establishment of technical schools or to assist in providing them, and also to give local authorities power to supplement existing teaching in elementary schools by technical instruction, whether by day or evening classes. There will also be a proposal in the Bill with regard to the ratepayers, to whom a power of vetoing any proposal under the Bill will be given. We propose that the Bill should be administered by the Science and Art Department—that is to say, that it should be administered subject to the directorate of that Department. We also propose that the Bill should have the limitation that no scholar should come under its operation until he has reached the sixth standard. The authorities for administering the powers conferred by the Bill will be School Boards where they exist, and where they do not exist town councils. I should just like to refer to Clause 4 of the Bill. To make the Bill acceptable to the ratepayers you must show that it is a cheap Bill and that consideration has been shown for them. Clause 4 is what I may call the operative clause of the Bill, and it enables local authorities to provide technical schools. Of course that would involve expense in building; but there is a sub-section of Clause 4 which enables the local authority to combine with any other local authorities. This will enable a system of combination to be adopted which will prove a great saving to the ratepayers; and, further, the next sub-section provides that the local authority may contribute towards the maintenance or provision of any technical school which has been established by any other local authority. It is further intended to include a provision that local authorities shall be empowered to rate for the purpose of supplementing any existing institution. These, I think, the House will admit are provisions which will enable this Bill to be worked cheaply. A further sub-section gives the local authority power to make any arrangements it may deem necessary or expedient for supplementing the technical instruction at present given in the schools. This provision I consider one of the most valuable in the Bill. It will enable technical instruction to be at once given without putting the ratepayers to any expense in building. I should like to refer for a moment to the limitation to the sixth standard. Though this will necessarily exclude many children, I think all interested in education will admit that the Bill should apply to the pick of our scholars, and that a good educational foundation should be required. With regard to the question of agricultural instruction, I am free to admit that the Bill, as drawn, can extend that instruction only to a very small extent, but I believe that the measure is capable of very considerable development, and that under certain of its sub-sections agricultural instruction may be afforded to a satisfactory extent. It is proposed to insert a provision in the Bill that where any local authority passes a resolution to establish a technical school a certain proportion of the ratepayers may demand a poll, but I am here at once met with a difficulty with regard to the metropolis—namely, that it is very wrong to propose to bring into existence the enormous voting power within the metropolis for such a purpose by this Bill. It may be asked how I propose to protect the ratepayers of the metropolis in regard to this matter. I have a proposal on the subject which has been drawn up by the Vice-Chairman of the London School Board, the hon. member for Worcestershire, and which has received the approval of the Chairman of that Board. I believe, from all I can gather, that that proposal will be popular with the present London School Board, and that it has been accepted by the hon. members for the metropolis who have been consulted in this matter, on the understanding that this policy alone will be carried out by the London School Board until the next election. I believe, also, that this proposal will be approved by the ratepayers of the metropolis. I have been asked by the hon. member for Worcestershire whether the scheme would not involve an extra charge for building, and I have been able to assure him that it will not do so. Therefore, Sir, I am prepared to admit that the members for the metropolis must be considered in this matter. Of course, if they think that further security to the ratepayers will be necessary it will be possible to insert an addition to this clause in the Bill to the effect that no action with reference to this Bill shall take place until after the next School Board election. I believe it would be a mistake to do anything of the kind; I believe that the interest of the ratepayers and of the great mass of working men in the metropolis may be safely trusted in the manner proposed by the Bill. Then, Sir, I may be asked this question, which, I think, is a very pertinent one. It is true that a vast amount has already been expended in the cause of technical instruction, and I may be asked: "If you once establish the principle of rating, will you not check the principle of voluntary effort?" I believe we shall do nothing of the kind, for several reasons. I believe that this will be an essentially popular measure among the working classes. I believe it will be impossible to check voluntary effort in a cause such as this where you supplement it by rates, for I believe that those who are spending money voluntarily are doing it in a cause which they know to be a very vast one, and that for all sums of money whenever spent in this cause more than compound interest will be repaid as the result. There are numerous instances in regard to this matter. It was only the other day I noticed that in Lambeth the Public Libraries Act was adopted. That is a case in point. What was the result there of adopting the principle of rating? I noticed that at the concluding meeting held, when arrangements were to be made for this new library under the system of rating, the hon. member for Barrow-in-Furness, who was in the chair, announced that a friend of his had not only given the ground but was going to build the whole library at his own cost. Numerous instances of the same type have come under my notice. Therefore I do not think we ought to dread that the establishment of voting power will check in any degree voluntary effort. I have only one other point to deal with—the administration of this Bill. We propose that this Bill shall be administered by the Science and Art Department at South Kensington. I have been anxious that the Bill should be so administered, for there we have a Department whose educational capacity has been thoroughly well tested. I have heard some hon. members attack the results of South Kensington as rather expensive. I am anxious that the House should be in a position to judge of the actual yearly expenditure at South Kensington, not only as regards administrative expenditure, but as regards its results. By the leave of the House, I shall therefore lay upon the table a document that will show in a concise form the actual expenditure at South Kensington for five years, both as regards administration and as regards results. Hon. members will then see how vast an increase there has been as regards payment by results and how small has been the increase of administrative expenditure. I should like to read to the House what that Department is now doing with regard to science and art. During 1886-87 there were 1936 schools or separate institutions in which instruction was given in one or more branches of science. There were 6976 classes in different branches of science, and the number of individual students under instruction was 100,419. At the May examination 127,900 papers were sent up for examination to South Kensington. I should like also to give some more instances to show the vast strides made in chemical instruction. In chemistry, 21,085 papers were worked at examinations. To show the advance made of late years in the facilities afforded for instruction in science of a thoroughly practical and experimental character, it may be mentioned that thirty years ago there were only one or two places where students could obtain laboratory instruction in chemistry, and that at very high fees. The Royal College of Chemistry, established in 1845, was one of these. Another was soon after started in Craig's Court. Now there are 234 chemical laboratories in connexion with the Science and Art Department in which students can obtain laboratory instruction at very low fees. There were 4257 separate benches at the last examination which afforded accommodation for 16,155 candidates. In the last session there were in operation 234 schools of art and 626 art classes, with 71,132 students in them; 50,000 were examined in May last, and the number of papers worked was 75,678. I should not for one moment have attempted to put the administration of this Bill under the Department of Science and Art at South Kensington if I had any doubt of its ability to work it with efficiency and economy. I thank the House for the attention with which it has heard me. I need not go again through the details. Happily this cannot be regarded as a party question. It is one which interests members on each side of the House, and although I do not submit the Bill as covering all the ground of technical instruction, I do believe that it is a measure which will do an enormous amount of good to our industrial population. In conclusion I would urge that, if it were only for the two provisions alone with

regard to continuation classes and to evening classes, this Bill is worthy of the serious consideration of the House. I hope that hon. members will not at this time of the session overload the Bill with amendments.

SCIENTIFIC SERIALS.

Bulletin de la Société des Naturalistes de Moscou, 1886, No. 3.—On two great comets (41 and 42) of 1886, by Th. Bredichin (in French).—On the *Agromyza lateralis* and its metamorphoses, by Prof. Lindeman (in German).—On the iron-bearing mud of Lipetzk, by E. Kislakovsky. It appears much like that of Franzensbad in Germany, and especially that of Ciechocinek in Poland.—On the Ammonites of the group *Oleostephanus versicolor* (Trautschold), by Mary Pavlow (in French, with two plates). Studying a rich collection of *Ammonites versicolor*, some of which reach 8 inches in diameter, while others have the size of a pin's head, the author considers them as belonging to the genus *Oleostephanus*, and establishes the following species, of which the last three are new: *O. versicolor*, *elatus*, *subinversus*, *inversus*, and *coronatiformis*.—On the importance of oxygen for plants, by W. Palladin (summed up in German). An elaborate research into the amount of matter destroyed in consequence of fermentation in an atmosphere devoid of oxygen, as also into the relations between the breathing of plants and their growth.—On the dynamic centra of a rotation-ellipsoid, with relation to earth, by K. Weihrauch, being a mathematical inquiry (in German) from which it results that the centres of attraction are situated in the earth nearer to the centre of figure than would be the case in an homogeneous ellipsoid of the same average density.—On the Algæ of Moscow, by A. Artari (in French), being a continuation of a former publication, and containing a list of eighty-five more species, chiefly Bacillariaceæ.—On the fauna of the lakes of the Slavyansk mineral waters, by P. Stepanoff. The fauna is mixed and contains representatives both of fresh-water and marine species, these latter being chiefly found amidst the Infusoriæ.—The annual report of the Society contains obituary notices of the late President of the Society, Dr. Renard.

No. 4.—Vascular plants of Caucasus, by M. Smirnoff. In this second paper (in French) the nebulosity of different parts of Caucasus is discussed, and data given.—Wild plants of the Government of Tambof, by D. Litvinoff, continued.—The species of *Thrips* living on corn in Middle Russia, by Prof. Lindeman (in German). The new species *Thrips secalina* and *Phlœothrips armata* are described together with former ones.—Zoological researches in the Kirghiz Steppe, by P. Nazarow, being a most valuable review of the fauna of the steppe, especially of its avi-fauna (with a map).—Speeches pronounced at the death of Dr. Renard.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, June 16.—"Experiments on the Discharge of Electricity through Gases." (Second Paper.) By Arthur Schuster, F.R.S.

In thinking over the phenomena presented to us in vacuum tubes, I always felt a difficulty owing to our ignorance of the conditions which hold at the surface of bodies, either suspended in or near the discharge, or even at the boundary of the vessel through which the discharge is passing. It is evident enough that if there is a flow of electricity on the surface of a non-conductor that flow must be tangential, but it is not so clear whether we are justified to conclude from this that there can be no normal forces at such surfaces, for it is not necessary that the flow should always take place along the lines of force.

Supposing we suspend two pieces of gold leaf, as in an electroscope, at any place in a partially exhausted vessel, and render them divergent by electrification, they should collapse as soon as the discharge begins to pass, if tangential forces only can permanently exist at their surface. This I have tested by experiment, and found to be the case.

A cylindrical glass vessel 38 centimetres high and 15 centimetres wide, was divided into two approximately equal compartments by a vertical metallic screen. There was an open space of about 5 millimetres between the screen and the sides of the vessel, a space of about 4 centimetres above, and 2·5 centimetres below the screen. One compartment contained two pieces of gold leaf, which could be charged from the outside. The other compartment contained two electrodes about 5 centimetres apart, and 2 centimetres from the screen; these distances could be varied during the experiment. The screen was always conducted to earth, and the electric fields on the two sides of the screen were therefore nearly independent of each other. When the gold leaves were electrified and divergent, and discharges from the induction-coil passed between the electrodes on the other side, no effect could be observed at atmospheric pressure: the gold leaves remained divergent.

At a pressure of about 4·3 centimetres of mercury, the effect I was looking for first appeared; when the discharge passed, the divergent leaves slowly collapsed, and as the pressure was further diminished the collapse took place more and more quickly.

We have here, then, even with the discontinuous discharge, a neutralization of all normal forces at the surface of the gold leaf.

It seemed to me to be interesting to observe more particularly the effects of the ordinary discharges we have at our command, at atmospheric pressure. I took two light balls, and suspended them so that they could be made to diverge by electrification. The electrodes (either spheres or points) of a Voss machine were placed at a distance of 3 inches from each other, and the electrified balls were placed at a distance of 9 inches from the discharge. The results are contained in the following table, in which the first two columns indicate whether the electrodes of the Voss machine were points or spheres. The third column gives the electrification of the balls, and the fourth column the results.

Negative electrode.	Positive electrode.	Balls.	Result.
Sphere	Sphere	Positive	Balls collapse slowly
,,	,,	Negative	,, remain divergent
Point	Point	Positive	,, collapse quickly
,,	,,	Negative	,, remain divergent
Sphere	,,	Positive	,, ,,
,,	,,	Negative	,, collapse slowly
Point	Sphere	Positive	,, ,, quickly
,,	,,	Negative	,, remain divergent

It will be seen that when the two electrodes are similar, whether spheres or points, the balls collapse when they are electrified positively only; but that when one electrode is a sphere and another a point, the balls collapse if their electrification is of the opposite nature to that supplied by the point.

The conclusion thus arrived at, which will be proved beyond possibility of doubt in the second part of this paper, is this: we can only have tangential forces at the surfaces of vessels inclosing a gas through which a discharge is passing, provided no current crosses the surface.

After I had convinced myself that an electrified body placed in a partial vacuum through which an electric current is going, has its electricity quickly neutralized, it was doubtful still whether this neutralization was due to an actual discharge or merely to a covering of electrified particles of an opposite sign. The question is a vital one in all cases where potentials have to be measured. For we can only measure potentials of a gas by measuring the potential of a metal in contact with it; and if an electrified body is covered by electrified particles of a different sign, there is a finite difference of potential between the metal and the gas, and we should have to inquire carefully, in each particular case, how far such a difference would affect our conclusions.

The question is settled by the principal result of this paper:

A steady current of electricity can be obtained in air from electrodes at the ordinary temperature which are at a difference of potential of one-quarter of a volt only (and probably less); provided that an independent current is maintained in the same closed vessel.

In other words, a continuous discharge throws the whole vessel into such a state that it will conduct for electromotive forces which I believe to be indefinitely small, but which the sensitiveness of the galvanometer I used has prevented me from tracing with certainty below a quarter of a volt. There cannot be therefore a finite difference of potential between a gas and a metal in contact greater than that amount.

The same vessel was used as in the previous experiment.

On one side of a screen conducted to earth, were the two main electrodes, from which the current of the large battery passed. On the other side were two auxiliary electrodes connected to the two poles of a small battery. Whenever the main current passed, the small battery was found to send a steady current which could be measured. The smallest electromotive force which was observed to send a current under these conditions was one-sixth of a Leclanché.

An electromotive force of one-sixth of a Leclanché is about one-quarter of a volt, and a current has thus been obtained in a gas from an electromotive force which could not maintain a current through water.

An electromotive force of 0·1 volt gave doubtful results, but this was probably due to the experimental difficulty of detecting the current.

In some previous experiments, which, however, were not quite free from objection on other grounds, the lowest electromotive force for which the currents could be measured was 0·2 volt.

The experimental arrangement which is the best for the qualitative investigation of the effect is not the best for quantitative measurements, and I have therefore not endeavoured to follow out to any great extent the quantitative laws of these currents produced by low electromotive forces. I may give, however, some facts which I have observed. The intensity of the current depends on a great many circumstances.

(1) It increases rapidly with the intensity of the main discharge, and also with a reduction of pressure, as far as I have tried it (that is about half a millimetre).

(2) The intensity of the current from the auxiliary battery increases less rapidly than the electromotive force.

(3) In some experiments, in which one of the electrodes of the auxiliary battery was a copper wire and the other a copper cylinder, the current was nearly always considerably stronger when the larger surface was the kathode.

(4) Anything that facilitates the diffusion of gas from the main current to the auxiliary electrodes will increase the strength of the current observed. In some experiments, in which the screen separating the two fields was made of wire gauze instead of tinfoil, the currents were stronger than those given above.

These experiments show conclusively that there is nothing peculiar in the gaseous state of a body to prevent any electromotive force, however small, to produce a current. If a finite electromotive force is required under ordinary circumstances, the fact cannot be accounted for, as Edlund and others have done, by a special surface resistance which has to be overcome by a finite difference of potential at the surface.

I think the facts are very well accounted for by the theory which I have proposed in my last paper. If the two atoms of a gas making up the molecule are charged with opposite electricities, but are held together in addition by molecular forces, a finite force is required to overcome the latter. But as soon as that force is overcome and the atoms themselves are set free to diffuse and constitute a current, these atoms will be able to follow any electromotive force which we may apply. If, then, we have auxiliary electrodes, these electrodes will establish their electric field, which we can never screen off completely from any other part of the vessel except by closed surfaces. The atoms, with their positive and negative charges, will diffuse across to the auxiliary electrodes and give off their electricity to them. No finite difference of potential is required in the auxiliary electrodes, because, even if there is work done in making an atom interchange its positive for negative electricity, that work is undone again at the other pole, where atoms of a similar kind interchange negative for positive electricity.

I should like, in conclusion, to point out an important application of these results. I have last year obtained by calculation results which seem to show that the principal cause of the diurnal variation of terrestrial magnetism is to be looked for in the upper regions of the atmosphere. Prof. Balfour Stewart at various times suggested that the air-currents in these regions may, owing to the lines of force of terrestrial magnetism, have electric currents circulating in them.

The difficulty against this supposition always seemed to me to lie in the fact that the electromotive forces required to start a current were larger than those which could possibly exist in the atmosphere. But as there are very likely continuous electric disturbances going on, such as we observe in auroræ and thunderstorms, the regions within which these discharges take place would act as conductors for any additional electromotive force however small, so that any regular motion, such as tidal motions, could very well produce periodic effects affecting our magnetic needles.

If these original discharges increase in importance, then, according to the results obtained in this paper, the currents due to the smaller periodic causes would increase also, and they may increase in a very rapid ratio. We know that the electric discharges in the upper regions of the atmosphere are considerably stronger at times of many sunspots, and this may account for the fact that at those times the amplitude of the daily oscillation of the magnetic needle is considerably increased.

I have had considerable assistance in these experiments from my assistant, Mr. Stanton, to whom my best thanks are due.

Geological Society, June 23.—Prof. J. W. Judd, F.R.S., President, in the chair.—The following communications were read:—On nepheline rocks in Brazil, with special reference to the association of phonolite and foyaite, by Mr. Orville A. Derby. The author refers to the phonolites and associated basalts of Fernando Noronha, a deep-sea island off the north-eastern shoulder of the continent of South America. Nepheline rocks of a somewhat different character are abundantly developed on the mainland, and under conditions favourable for throwing light on the relations existing between the granitic type, foyaite, and the other members of the group. There are some mountains near Rio de Janeiro composed of these rocks, as is also the peak of Itatiaia, 3000 metres high, the loftiest mountain of eastern South America. A cursory examination of some of these localities having shown an apparent relation between foyaite, phonolite, trachyte, and certain types of basalt, Mr. Derby determined to visit the Caldas region, where a railway under construction gave unusual facilities for examining the series. A fine development of foyaite, phonolite, and tuff was found, associated with several types that have not yet been met with in the other localities. The existence of a leucite basalt was recognized. The bulk of the paper was devoted to a detailed description of these railway-sections, and the following deductions are drawn:—(1) The substantial identity, as regards mode of occurrence and geological age, of the Caldas phonolites and foyaites. (2) The connexion of the latter through the phonolites with a typical volcanic series coataining both deep-seated and aërial types of deposits. (3) The equal, if not greater, antiquity of the leucite rocks as compared with the nepheline rocks, whether felsitic, as phonolite, or granitic, as foyaite. (4) The probable Palæozoic age of the whole eruptive series. The President said it was seldom that a paper containing such important facts was presented to the Society. It was reserved to Mr. Derby to have proved that plutonic rocks containing nepheline (foyaite) passed into volcanic masses which were true phonolites. This Mr. Derby had clearly established by observations in the field. He had also shown that leucite existed in rocks of Palæozoic age, thus rendering untenable the last stronghold of those who insisted on making geological age a primary factor in petrographical classification. He alluded also to the value of the independent determinations of Prof. Rosenbusch. Mr. Bauerman said he had been over portions of the ground with the author, and was glad to add his testimony to the value of the paper. He spoke of the importance, in a geological sense, of these generalizations. It was remarkable how highly crystalline masses of rock pass over into a sort of phonolite. These were associated with Palæozoic masses, which were pre-Permian, or at least pre-Triassic. He alluded to the difficulty of investigating Fernando Noronha, and also to the difficulties attendant upon the investigation of rocks in Brazil, which were subject to such an enormous amount of local alteration. Prof. Bonney also expressed his sense of the value of the paper. He alluded to the comparative rarity of nepheline and leucite rocks, and to the confusion in the nomenclature. He was reminded of the nepheline rocks near Montreal, where dolerite was broken through by nepheline syenite, associated with tephrites and phonolites. Although there might be a doubt here, these rocks were most probably of Silurian age; but the evidence in Brazil was still clearer as to the Palæozoic age, and he believed that, in the case of some other masses, the evidence had satisfied the Canadian geologists. He alluded also to the nepheline rocks in the Katzen-Buckel, where there was a similar passage from coarse-grained to fine-grained. Dr. Hatch said that in this case leucite was clearly shown to be of Palæozoic age, and regarded the paper as a step towards the better classification of this group of rocks. Prof. Seeley asked for evidence as to the identification of the leucite. The President thought there was no possibility of a mistake in this respect. As regards the rocks of the

Katzen-Buckel, none were truly holocrystalline, and hence they could not be compared with foyaite or elæolite-syenite.—Notes on the metamorphic rocks of South Devon, by Miss Catherine A. Raisin, B.Sc. Communicated by Prof. T. G. Bonney, F.R.S.—On the ancient beach and boulders near Braunton and Croyde in North Devon, by Prof. T. McKenny Hughes.—Notes on the formation of coal-seams, as suggested by evidence collected chiefly in the Leicestershire and South Derbyshire coal-field, by Mr. W. S. Gresley.—Note on some Dinosaurian remains in the collection of A. Leeds, Esq.; Part I. *Ornithopsis leedsii*, Part II. *Omosaurus*, sp., by Mr. J. W. Hulke, F.R.S.—Notes on some Polyzoa from the Lias, by Mr. Edwin A. Walford.—On the superficial geology of the southern portion of the Wealden area, by Mr. J. Vincent Elsden. Communicated by the President.—Report on palæobotanical investigations of the Tertiary flora of Australia, by Dr. Constantin Baron von Ettingshausen.—On some new features in *Pelanechinus corallinus*, by Mr. T. T. Groom. Communicated by Prof. T. McKenny Hughes.—On boulders found in seams of coal, by Mr. John Spencer.

EDINBURGH.

Royal Society, June 6.—Mr. J. Murray, Vice-President, in the chair.—Mr. J. B. Readman read a paper on a furnace capable of melting nickel and cobalt.—Mr. R. Kidston communicated the last part of his paper on the fossil flora of the Radstock series of the Somerset and Bristol coal-fields.—Prof. Grainger Stewart read a paper on investigations into the discharge of albumen from the kidneys of healthy people.—Dr. H. R. Mill communicated the result of his investigations on the salinity and temperature of the Firths of Inverness, Cromarty, and Dornoch, and of the North Sea.—Prof. Ewart discussed the existence of Bacteria in the lymph, &c., of fish and other vertebrates.

June 20.—Sheriff Forbes Irvine, Vice-President, in the chair.—Prof. Geikie communicated a paper by Prof. Frederico Sacco on the origin of the great Alpine lakes.—Dr. E. Sang read a paper on the minute vibrations of a uniform chain hung by one end, and on the functions arising in the course of the inquiry.—Dr. A. W. Hare read a note on the biological tests employed in estimating the purity of water.—Prof. Tait submitted a paper by Mr. A. H. Anglin on alternants which are constant multiples of the difference-product of the variables.

PARIS.

Academy of Sciences, July 11.—M. Janssen in the chair.—Presentation of the minutes of the International Astronomical Congress for the execution of the photographic chart of the heavens, by M. Mouchez. It was stated that although the Congress held in Paris last April concluded its labours before the end of the same month, the publication of its proceedings has been delayed till now, owing to the necessity of sending the proofs for revision to the members scattered over various parts of the world. Two main resolutions were arrived at, the first regarding the adoption of the photographic process, and of a uniform class of instruments, so as to secure the greatest possible degree of homogeneity in the results. The instrument unanimously adopted was that of Gautier, already in use for two years in the Paris Observatory. The second resolution regarded the period and extent of work to be carried out at the various international stations. It was decided that there should be two series of stellar photographs, the first comprising stars to the 11th magnitude approximately, the second to include all down to the 14th magnitude, or about 15,000,000 altogether. A permanent Bureau was also appointed, for the purpose of executing the decisions of the Congress and maintaining constant relations between the members and the Observatories taking part in the work of stellar photography. A special bulletin may also perhaps be issued from time to time, to report generally on the progress of this great astronomic undertaking.—Heat of formation of hydrotelluric acid, by MM. Berthelot and Ch. Fabre. Four determinations effected by the agency of the perchloride of iron in solution, give a mean of 29·12 calories.—On the presence of microscopic crystals of albite in various limestone rocks of the Western Alps, by M. Ch. Lory. The genesis of these crystals appears to have been generally favoured in the Western Alps by the conditions under which the Triassic formations have been developed. They occur somewhat exceptionally in association with the Middle Lias at Villette, and about the head of the long fjord of the Miocene sea, which flowed from the Maritime Alps to a point a little north of Saint-Jean-de-Maurienne. Hence the formation of these microscopic crystals appears to be connected with the special character of the deposits, and to be independent of the more or less intense local mechanical actions which affected the various stratified rocks at the time of the Alpine dislocations.—Presentation of M. Godefroy Malloizel's volume containing a complete list of M. Chevreul's writings issued between the years 1806-86, by M. de Quatrefages. The cost of this publication has been met by the balance of the sum subscribed by the youth of France to strike a medal in honour of M. Chevreul on his hundredth anniversary. Besides the titles and dates of everything issued by M. Chevreul during the last eighty years, careful tables of contents are appended to all memoirs and scientific papers of any considerable length. An introduction is added by M. Desnoyers, Librarian of the Museum, and a fine portrait of the illustrious *doyen* of the *savants* of the whole world, by M. Champollion.—On antipyrine as a substitute for morphine in subcutaneous injections, by M. Germain Sée. The continued experience of the author since his first communication on antipyrine as an anæsthetic (April 18, 1887), shows its decided superiority over morphine in all cases of rheumatic, hepatic, and cardiac affections. It is administered in the same way, but is more easily prepared, more efficacious, and entirely free from the dangerous consequences too often attending the use of morphine.—On a simple dynamic method of determining the degree of isotropy of an elastic solid body, by M. E. Mercadier. According to Saint Venant, in all true solid isotropes $\frac{\lambda}{\mu} = 1$, where λ and μ are two characteristic quantities of a solid body, by means of which may be expressed all the coefficients relative to its elasticity (Lamé). Hence, if this relation can be measured for different bodies, their degree of isotropy may be determined by the difference between the value of such relation and unity. M. Mercadier here supplies a simple method for making this determination based on the theory of the vibrations of circular plaques, the laws of which have recently been verified by him. He shows, for instance, that for glass $\lambda = \mu$; that is to say, that it is an isotropous body. This is an extremely simple confirmation, by a *dynamic* process, of the result of the beautiful experiments made by M. Cornu on glass by a *static* method.—On the alums formed by selenic acid, by M. Charles Fabre. Continuing the studies of Wohlwil, Wöhler, and Petterson, the author here describes a series of selenic alums with alumina or sesquioxide of chromium base, which he has succeeded in preparing. They comprise the alums of alumina corresponding to the general formula $Al_2O_3 . 3SeO_3 + MO . SeO_3 + 24HO$, and the alums of chromium corresponding to the general formula $Cr_2O_3 . 3SeO_3 + MO . SeO_3 + 24HO$.—Researches on the reactions of the vanadates considered from the stand-point of chemical analysis, by M. Ad. Carnot. In this paper the author completes the study of the reactions produced between the vanadates and the chief metallic salts under the ordinary conditions of analysis. Amongst the salts here treated are those of cobalt, nickel, zinc, cadmium, copper, mercury, lead, and bismuth.

BERLIN.

Physiological Society, June 17.—Prof. Du Bois-Reymond, President, in the chair.—Prof. Ewald spoke on the behaviour of salol (salicylate of phenol) in the stomach, a question which he has investigated in order to obtain information as to the movements of the stomach in relation to the time in which the contents of this organ are sent on into the intestine. Prof. Nencki had stated that salol is not acted upon by gastric juice, but is split up into salicylic acid and phenol by the action of pancreatic juice. Prof. Ewald's experiments confirmed the statement that salol undergoes no change in the stomach; thus, after administering salol, this substance could be detected in portions of the contents of the stomach examined at intervals of from one-half to three hours after it had been taken. Pancreatic juice was found to be similarly inert on salol, but on the other hand it was decomposed by most alkaline fluids. When injected into the intestine through a fistula, salol could readily be detected after half an hour, as salicyluric acid, in the urine. Since, therefore, salol undergoes no change in the stomach, but is readily decomposed in the intestine, and appears in half an hour as salicyluric acid in the urine, it was found to be extremely well suited to the purposes of the proposed experiments. When salol is given to healthy men whose gastric apparatus is in

a normal condition, whether on an empty stomach, or with food, or at different stages of gastric digestion, salicyluric acid was found in their urine on an average three-quarters of an hour after it had been taken. From the data given above, the salol must have remained one-quarter of an hour in the stomach. In the case of patients suffering from gastric dilatation, the salol remained much longer in the stomach. The time which elapses between the administration of salol and the appearance of salicyluric acid in the urine may hence be used as an important means of diagnosing cases of slight gastric dilatation. After prolonged electrical stimulation of the abdominal muscles, the passage of salol into the intestine was quickened.—Prof. Zuntz criticised a theory of the excretion of carbonic acid in the pulmonary alveoli which has been put forward by von Fleischl, according to which the shock given to the blood by the contraction of the heart is to be regarded as the chief cause of the diffusion of the carbonic acid through the alveolar walls. The speaker refuted this theory as being both unproved and unnecessary.—Dr. Goldschneider communicated the results of his experiments on the reaction-time of the perception of temperature. It has been known for a long time that cold is more quickly perceived than heat. As a starting-point, the speaker had first carried out some direct measurements. He sought out portions of the surface of the body which were equally sensitive to heat and cold; these parts were then stimulated as far as possible with equal intensity, and the results were as follows, taken as a mean of about two thousand separate measurements:—The reaction-time for cold as a stimulus is for the face 13·5, for the arm 18, for the abdomen 22, and for the knee 25 hundredths of a second. When an equally strong heat stimulus was applied, the numbers obtained were 19, 27, 62, and 79 hundredths of a second. The ratio of the reaction-times was found to be about the same when the stimuli were applied to such a nerve as the trigeminal which goes straight to the brain and to a spinal nerve. The experiments on thermal stimulation were made by bringing a metallic button in contact with the skin and recording electrically the moment of contact; the resulting sensation was indicated by a Beiss key. The degree of heat and cold employed as a stimulus was selected so as to differ by equal amounts from the temperature of the skin. From the results of the experiments, Dr. Goldschneider deduced some theoretical conclusions as to the nature of our sensations of heat and cold.

Physical Society, June 27.—Prof. von Helmholtz, President, in the chair.—Prof. von Bezold demonstrated the currents, which he has very fully investigated, which occur in a fluid as the result of varying temperatures or the rotation of the vessel in which the fluid is contained. These currents were made visible, as they occur in a large mass of water, by means of a few drops of hectograph ink, which at first spreads itself out in radiating lines over the surface, then sinks in the form of threads and columns, and, following the direction of the currents in the fluid, presents an extremely interesting appearance of rotatory formations.—Dr. Richarz has closely studied what takes place in an electrolyzing cell during the decomposition of water in the immediate neighbourhood of the electrodes during the passage of the currents of electrolytic convection. As is well known, an electromotive force of 1·5 Daniell is necessary in order that the current may pass electrolytically and the water be decomposed; if the electromotive force is less than the above, the water is not decomposed, but at the same time it can be shown that the electricity does traverse the fluid. According to Von Helmholtz's views on electrolysis, when the electromotive force is small, currents of electrolytic convection pass through the fluid, which are kept up by the occlusion of the positively charged hydrogen atoms at the kathode and by the neutral oxygen in solution. Starting from the work of Moritz Traube, who has proved the formation of hydrogen peroxide at the kathode in the electrolyzing cell, Dr. Richarz has been able to prove, both qualitatively and quantitatively, the formation of hydrogen peroxide at the kathode during the passage of convection currents. This formation of the peroxide takes place, according to the views of the speaker, by the union of two atoms of the occluded hydrogen with the neutral molecule of the dissolved oxygen, which has given up its positive charge to the kathode. As the result of this separation of the occluded hydrogen, fresh portions of hydrogen can be occluded by the metal of the electrode, and in this way a renewal of the electric current can take place.—Prof. Neesen described a vapour-calorimeter, consisting of a glass vessel into the centre of which projects a glass tube, closed at the lower end, for the reception of the substance under investigation. This tube is surrounded with a mass of lamp-wick, which is saturated with ether, and dips into a small quantity of liquid ether in the bottom of the outer vessel. Another glass vessel, exactly similar to the above, is joined to it by means of a capillary U-tube, in which a small drop of ether serves as an index. When a warm substance is dropped into the calorimeter, an amount of ether is converted into vapour proportional to the heat given up, and the volume of this vapour, as measured by the displacement of the ether index, gives the heat yielded by the substance. Prof. Neesen is still engaged in testing and improving the calorimeter, and only made the above preliminary communication as this was the last meeting of the Society before the summer vacation.—Dr. Grunmach exhibited a double quartz plate, which was not made of a right- and left-handed quartz plate, but was cut from a twin crystal, in which the fusion of the two crystals is so perfect that every slice cut from this twin crystal may be used as a double plate in the polarizing apparatus.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Walks in the Ardennes: J. W. Richards (Low).—Welsh Question and Druidism, Third Edition: Griffith (R. Banks).—Annual Report of the Department of Revenue, Settlement, and Agriculture, for 1885-86 (Madras).—Journal of the Royal Statistical Society, June (Stanford).—Journal of Physiology, vol. viii. No. 2 (Cambridge).—The Indian Forester, vols. ii., iv., vi., vii., viii. (Calcutta).—Annalen der Physik und Chemie, 1887, No. 8 (Leipzig).—Museum d'Histoire Naturelle des Pays-Bas, tome ix. Catalogue Ostéologique des Mammifères: F. A. Jentink (Brill, Leide).

CONTENTS.

	PAGE
The Mining Industry of New Zealand	265
A Century of Electricity	266
Our Book Shelf:—	
Pratten: "My Hundred Swiss Flowers"	266
Hay: "The Fungus Hunter's Guide and Field Memorandum Book"	267
Letters to the Editor:—	
The Carnatic Rainfall.—General Richard Strachey, F.R.S.	267
Is Cold the Cause of Anticyclones?—H. Helm Clayton	268
Physiological Selection.—H. K. Rusden	268
Weight, Mass, and Force.—Prof. A. G. Greenhill	269
The Sky-coloured Clouds.—T. W. Backhouse	269
The Migrations of Pre-Glacial Man.—Dr. Henry Hicks, F.R.S.	269
Abstract of the Results of the Investigation of the Charleston Earthquake, I. By C. E. Dutton, U.S.A., and Everett Hayden, U.S.N., U.S. Geological Survey. (*With a Map*)	269
Experiments on the Sense of Smell in Dogs. By Dr. George J. Romanes, F.R.S.	273
Fossil Wood from the Western Territories of Canada. By Sir J. William Dawson, C.M.G., F.R.S.	274
The Liverpool Marine Biology Station on Puffin Island. By Prof. W. A. Herdman. (*Illustrated*)	275
Antarctic Exploration	277
The Captive Kite-Balloon. By E. Douglas Archibald. (*Illustrated*)	278
Notes	279
Our Astronomical Column:—	
The Nice Observatory	282
Astronomical Phenomena for the Week 1887 July 24-30	282
Geographical Notes	283
The Technical Education Bill	283
Scientific Serials	285
Societies and Academies	285
Books, Pamphlets, and Serials Received	288

THURSDAY, AUGUST 4, 1887.

THE JUBILEE OF THE ELECTRIC TELEGRAPH.

IT is something to have lived to take part in an epoch-making event. Many monarchs have celebrated their jubilees: printing, steam, gas, have passed through this period in silence and disregard, but the first practical application of electricity has commemorated the fiftieth anniversary of its birth with an *éclat* and success that reflect the highest credit on the managers of the banquet who brought together such a distinguished gathering on July 27.

It is remarkable that of all who were present not one took part at the birth of the electric telegraph. The pioneers are gone, and their memory was silently toasted. Of those associated with Cooke, William Watkins, who carried out his early experiments, and who put up the first overhead wires between Paddington and Slough, alone remains, but the inexorable duties of the law compelled his attendance on a special jury at Exeter on the day when he ought to have been present at the Holborn Restaurant. No collaborateur of Wheatstone in his early work exists. John Greener, who had charge of the telegraph on Bidder's celebrated rope railway between Fenchurch Street and Blackwall in 1842, was there, and many like Henry Weaver and J. R. France can date their telegraphic career from the incorporation of the first telegraph company in 1846.

One of the most interesting features of the meeting was the gathering around Mr. Edwin Clark of his old lieutenants. Edwin Clark's reforms in the early days of telegraphy (1850-54) still bear fruit. The footprints on the sands of telegraphic time are nowhere so deeply impressed as on the ground traversed by Clark. His mode of insulation, his underground work, his instruments, his test-boxes, still remain a type of English telegraphy everywhere. His work was well carried out by his brother and successor, Latimer Clark, and it is continued even in the present day by his pupil, Preece.

The success of telegraphy in this country is due essentially to the superposition of scientific method on to the rude rules of practice. The rule-of-thumb principles of the early engineers were inoperative in telegraphy, for the exact laws of Ohm, Ampère, and Coulomb, the experimental skill of Faraday, Joule, and Grove, the mathematical genius of Helmholtz, Thomson, and Maxwell, have kept our electricians in the straight path, and prevented them from wandering in the wilds of guesswork and in the labyrinth of tentative troubles. It is impossible to say how much this influence has been reflective. The science of electricity has been indebted as much to practice as practice has been indebted to science. Submarine telegraphy chronicles no failure. The first Atlantic cable raised the curtain. The conditions were evident. Thomson stepped in, and all was light.

To telegraphy "all the world's a stage." The inventor has no nationality. Alongside of Wheatstone we find Morse and Siemens, Meyer, Hughes, and Edison, La Cour, Varley, Leclanché, and Minotto. This polyglottism is seen in the nomenclature of the units of measurement, ohm, farad, ampere, and coulomb, the only universal system of measurement, excepting that of time, extant.

Telegraphy, without which railway traffic would be impossible, has followed the growth of railways, and it has revolutionized the procedure of commerce. Hence the great commercial nations, England and the United States, show the greatest development of its progress.

One regretted to hear so little said about the great commercial spirits who set the ball a-rolling. John Pender, Cyrus Field, Tom Crampton, deserve all that was said of them, but where were Ricardo and Scudamore in England, Orton and Vanderbilt in America?

The story as told by the Postmaster-General reads like a romance of fairyland. The first five-needle instrument of Cooke and Wheatstone required five wires to transmit at most five words a minute: now five wires can transmit 2500 words in the same time.

We can pride ourselves in England on being in advance of all other nations not only in the development of the business of telegraphy, but also in the invention and perfection of apparatus. It is something to have in ten years increased the capacity of the wires for the transmission of messages *tenfold*, and to have done that without patent, or any reward but the consciousness of having done well. Government officials are unfortunately placed in this respect. It is improper to patent an invention developed in the discharge of duty, while they are singularly liable to be assailed by the daily Press for their supposed shortcomings. The work they do is only known by their own writings, when they are allowed to write; and even then they are subject to unfair and dubious criticism. The Press takes no trouble to find out what is done. The feeling is, "What good can come out of Nazareth?" Yet the introduction into the Post Office system of high-speed repeaters and of shunted condensers marks two epochs as successful, eventful, and meritorious as the introduction of duplex, of quadruplex, or of multiplex working. We were told that the rate of working between London and Dublin had gone up from 50 to 462 words a minute. *One* cable will do the work of *ten*. What has been the reward? We venture to say, *nothing*; and that the Lords of the Treasury are profoundly ignorant of the good work that is being done in the service over which they preside—work which they are just as likely to reward with a kick as with a half-penny.

The jubilee is now over, and we have every reason to feel proud that Mr. Raikes, the present Postmaster-General, Sir Lyon Playfair and Mr. Shaw-Lefevre, his predecessors, had such excellent tales to tell, and so gracefully assisted at so successful a gathering.

THE CLASSIFICATION OF ALGÆ.

Till Algernes Systematik. Nya bidrag af J. G. Agardh. (Femte afdelningen.) Transactions of the University of Lund, Tom. XXIII., 4to, pp. 180, 5 plates.

THE indefatigable Dr. Agardh has recently issued the fifth instalment of his work on the systematic classification of Algæ. Although it bears a Swedish title, the work is in Latin. The subject treated is the interesting group of the Siphoneæ.

Dr. Agardh mentions that but few observations have

been made upon the fruit of the Siphoneæ; but the little that is known on this subject proves that great differences exist in the fructification of these Algæ. Thus the organs of reproduction in Vaucheria differ from those of Botrydium, while those of the latter vary from those of Bryopsis, Codium, Dasycladus, and Acetabularia. Of many other genera observations are deficient or in various respects uncertain and inconclusive. For interesting general remarks on the fructification of the Siphoneæ the reader is referred to page 10, and for special details to the observations on each genus and on the fruit of such of the species as are best known.

Setting aside the true characteristics of the fruit, it becomes a question by what characters of the structure the Siphoneæ are to be distinguished from the Conferveæ and Ulvaceæ, and their affinities determined. In Dr. Agardh's opinion these characters are to be found in the filaments or utricles of the frond, being tubular, and not, as in the Confervaceæ and Ulvaceæ, consisting of subdivided cells. Good examples of the former are afforded by Caulerpa and Valonia, whose fronds, roots, stems, branches, and ramuli, though distinct, consist of a single cell. These remarks are followed by observations on the comparative structure in different genera and their affinities with each other.

With the exception of Vaucheria and Botrydium, says Dr. Agardh, the Siphoneæ inhabit the sea. This is unquestionably the case as regards Botrydium, but it may be asked whether it be quite true as to Vaucheria, several of the British species of which are recorded by Dr. Nordstedt ("Remarks on British Submarine Vaucheriæ," Lund, 1886) as growing at the lowest tide-marks, and one species (*V. piloboloides*, "probably in quite salt water." The greater number of the Siphoneæ are natives of the warmer seas, and are especially abundant on the shores of rocky islands of which the principal constituent is lime. They spread their fibrous roots among the sandy *débris*, and are thus useful in holding together the particles of sand.

Some of the Siphoneæ have creeping stems, as have the Caulerpeæ. These plants, by extending the network of their creeping stems and roots over the sand, seem to exercise on the coast, within tide-marks, the same functions as the Maram (*Psamma arenaria*). This plant grows on the coast of Norfolk, and is found so useful in holding together the particles of sand, and thus aiding in the formation of land and preventing the inroads of the sea, that strict regulations are in force to prevent its destruction. In the same manner as the Maram spreads over the dry sands, the Caulerpeæ extend on the seashore within tide-marks, and are thus uncovered at low water. When the tide is out, they resemble green meadows. The utility of these plants in protecting the land was, a few years ago, unexpectedly proved in the neighbourhood of Adelaide, South Australia. A farmer suffered his sheep to stray upon the coast where the Caulerpeæ were exposed at low water. The sheep devoured the Algæ; the sea consequently broke in and established itself, and land was thus permanently lost. Many species, as Halimeda and Penicillus, have roots which are occasionally as large as small hens' eggs, formed of innumerable branched fibres which penetrate deeply into the sand. Some of the stipiform species emit flagelliform creeping "propagula," from which spring new plants; hence, observes Dr. Agardh, the Siphoneæ may be said to be social plants. Some Algæ, as Anadyomene, grow in shallow water exposed to the full influence of light, while others, like Bryopsis, prefer deep water to which light scarcely penetrates.

Many, but not all, of the Algæ belonging to the Siphoneæ have, like the Corallina family, the power of absorbing lime from the water. Young plants are generally green, but the incrustation of lime, in certain species, increases with age. In some genera it is entirely absent, as in Codium; in others it is extremely slight; while in some species of Halimeda the whole frond is frequently cased with a hard coating of lime, and looks like a gigantic frond of Corallina.

The disposition of the families of which the Siphoneæ are composed must be attended with some difficulty until the fruit is more perfectly known. In the interim Dr. Agardh proposes the arrangement adopted in the work. The group has been considerably enlarged, including as it now does the Dasycladeæ and Valoniaceæ. With regard to the former, the author observes that the Dasycladeæ are quite distinct from all the other genera, with their verticillate stems and external sporangia; hence he considers that they undoubtedly form a natural family. The fructification nevertheless varies in different genera.

There appears no doubt as to the limits of the Caulerpeæ. These are set forth in the first part of the present work ("Till Algernes Systematik"), and, although some observations are still wanting as to the fructification, the Caulerpeæ form a very distinct family.

The limits of the Valoniaceæ are very difficult to determine. In the form and size of their cells they for the most part are very near the Caulerpeæ. They are little more than ramulose prolifications; hence the ramification, although it is in some species more or less obscured, may be said to have a common character. But little is known of the fruit of these plants. In some genera the structure varies from that of the other Valoniæ, and approaches near to that of Ulva, as may be seen in Dictyosphæria and Anadyomene.

Among the remaining genera are some whose fronds consist almost entirely of compound tubes incrusted with lime. In these the normal ramification is di-trichotomous, but in Bryopsis and Codium—which, however, are not coated with lime—it may be said to be pinnate. Of the fructification of these plants few observations are recorded. In *Udotea Desfontainsii* and *Halimeda tuna*, true sporangia have been observed. Both belong to the Udoteaceæ. Whether the Spongodieæ, with their *quasi* composite fronds, and the Bryopsideæ, with their free filaments, should be separated from each other, or united into one family, may be subject for consideration.

Vaucheria and Botrydium are not treated in the present work: neither does Dr. Agardh know to which family of the Siphoneæ they should be attached.

The whole group, in which the Dasycladeæ and Valoniaceæ are now included, is thus arranged by Dr. Agardh:—

I. BRYOPSIDEÆ.

(1) Bryopsis; (2)? Derbesia.

II. SPONGODIEÆ.

(3) Codium,?? Cladothele.

III. UDOTEACEÆ.

(4) Chlorodesmis; (4*a*)? Avrainvillea; (5) Espera; (6) Penicillus; (7) Rhipocephalus; (8) Callipsygma; (9) Udotea; (9*a*)? Rhipidosiphon; (10) Halimeda.

IV. VALONIACEÆ.

(11) Valonia; (12) Siphonocladus, ? Ascothamnion, ? Trichosolen; (13) Apjohnia; (14) Struvea; (15) Chamædoris; (16) Dictyosphæria; (17) Anadyomene.

V. CAULERPEÆ.

(18) Caulerpa.

VI. DASYCLADEÆ.

(19) Dasycladus; (20) Chlorocladus; (21) Botryophora; (22) Cymopolia; (23) Neomeris; (24) Bornetella; (25) Halicoryne; (26) Polyphysa; (27) Acetabularia, ? Pleiophysa.

It will be observed that the position of Derbesia, Cladothele, Avrainvillea, Rhipidosiphon, Ascothamnion, and Trichosolen are not yet finally determined. Neither does the author yet see his way to include Chlorodictyon (*Ast. Holm.* 1870, Öfversigt No. 5, p. 427, tab. iv.) in the present arrangement. Codiolum is also excluded. The genus Balbisiana is mentioned (p. 10) once, but is not again referred to. Has *Bryopsis Balbisiana* been formed into a distinct genus under this name?

Under Avrainvillea Dr. Agardh includes the Fradelia of Chauvin, the Chloroplegma of Zanardini, and the Rhipilia of Kützing. This genus has the habit of Udotea, a cylindrical stem, a coriaceous, flabellate frond, of a very dark colour, with lacerated apex, forming irregular lobes, in which the zones of Udotea are absent. Of the fruit nothing appears to be known.

Of Rhipidosiphon there is no description, and very little seems to be actually known about this Alga. To Dr. Agardh it appears to be a young plant of Udotea.

Our knowledge of Ascothamnion (*Valonia intricata*, C. Ag.) is very limited, although the plant has been found in most of the warmer seas.

Trichosolen is a native of the Antilles, where it was found by Montagne. With Pleiophysa Dr. Agardh is acquainted only through Kützing's figure (*Tab. Phyc.*, vol. xvi. tab. 1). The habit and form of the sporidia agree with those of Halicoryne.

Penicillus Phœnix now appears as *Rhipocephalus Phœnix*. The new genus Callipsygma is founded on an Australian Alga which bears a certain resemblance to the last-mentioned plant. The former has an undivided terete stem incrusted with lime, while in the latter the stem is two-edged, without incrustation, and from the margins issue pinnate ramuli. The fruit of both genera is unknown.

Chlorocladus is between Dasycladus and Botryophora = *Dasycladus occidentalis*. These three genera are especially distinguished from each other by their fruit.

Of the whole group of the Siphoneæ three genera only have representatives on the British shores. These three genera are Bryopsis, Derbesia, and Codium. They have all a wide range. Of the nineteen species of Bryopsis, two are natives of these shores. Derbesia ranges from the Adriatic to the Faroe Isles and Norway. Dr. Agardh does not seem to be aware that *D. tenuissima* has been found on the British coast. Although Codium has so extensive a range, no species has yet been recorded from the east coast of the United States. That remarkable plant, *C. bursa*, which is found on the southern coast of Britain, the Mediterranean and Adriatic, has recently been obtained from Victoria, Australia. On the Sussex coast it may sometimes be picked up after storms. Its range in depth of water is about the same as that of *Rytiphlæa pinastroides*, with fragments of which, when hollow and torn, the frond is sometimes filled. *Sphacelaria plumula* grows on it occasionally. Dr. Agardh mentions that three or four fronds often grow together. The writer possesses a specimen from Brighton, which consists of a group of ten fronds, one of which is fixed to a piece of chalk; the others grow upon one another, a few filaments attaching the young plants to the older ones. In 1870 the Rev. E. S. Dewick was fortunate enough to pick up a specimen at Eastbourne, which, on examination, proved to be in fruit. He stated at a meeting of the Eastbourne Natural History Society (November 18, 1870) that "the Coniocystæ are produced on the outer surface of the clavate filaments, and differ from those of *C. tomentosum* only in being nearer the top of the filaments, and smaller in proportion to their size."

Codium tomentosum was reputed to be nearly cosmopolitan. Dr. Agardh, however, shows that several species have been included under this name, and that the so-called Australian forms belong to *C. Muelleri*, *C. Galeatum*, and *C. mucronatum*. In the last two species the utricles are mucronate, as represented in Plate 1, Figs. 1, 2, 3. *C. elongatum*, in which the frond, instead of being cylindrical, as in *C. tomentosum*, is compressed, is recorded by Dr. Agardh from Ireland. This fact is worthy the attention of British algologists. *C. latum*, found by M. Suringar on the coast of Japan, is not referred to in Dr. Agardh's work, neither is the plant, apparently allied to Codium, called by M. Suringar Acanthocodium (see "Alg. Jap.," p. 23). This also is a native of Japan, and probably but very little known.

Although so many points in the history of the Siphoneæ are still undetermined, this work of Dr. Agardh's will be found full of interest and instruction.

MARY P. MERRIFIELD.

AMERICAN MINING INDUSTRIES.

Report on the Mining Industries of the United States (exclusive of the precious metals.) By R. Pumpelly, 4to, pp. xxxviii.–1025. (Washington: Government Printing Office, 1886.)

THIS, the fifteenth and final volume of the Reports illustrating the results of the census of the United States taken in 1880, is in great part devoted to descriptions of the principal districts producing iron ores in the United States, the condition of the mines during the census year being studied in considerable detail, and in many cases illustrated by sketches of the workings. A very large number of samples of the ores of the different mines were collected by specially appointed agents, who visited every district and almost every mine of importance, and these were examined by a chemical staff at a special laboratory at Newport, Rhode Island. It was originally intended to make complete analyses of the greater number of the 1400 samples so collected, but the early exhaustion of the funds voted for the census necessitated an extensive

curtailment of the plan, and only the more important minerals from the older rocks were completely analyzed; while for the bulk of the remainder, the properties of the more important constituents, iron, phosphorus, and sulphur, were alone determined, and the presence of titanium and manganese noted incidentally. The total number of samples investigated was 1250, 53 being completely and 1157 partially analyzed. The description of the methods of analysis adopted, and the tabulation of the results, occupy about a hundred pages, in addition to the 500 devoted to the geology and topography of the iron ore mines and their statistics.

The section devoted to coals, occupying eighty-seven pages, is mainly statistical, and has a very valuable introduction by Dr. Frederick Prime, Jun., which is perhaps the best condensed account of the nature and distribution of American coals that has yet appeared. A third section on the Cretaceous coals and lignites of the North-West is the result of an extensive exploration of the country traversed by the Northern Pacific Railway, made by the author subsequently to the completion of the census work proper, in 1882. This work, under the title of the Northern Transcontinental Survey, was suddenly stopped after about £20,000 had been expended upon it; and in order that the results might not be lost the observations have been reduced, analyses of the coals have been made, and a systematic memoir on the whole subject has been produced, which, although not exactly in the place where we should expect to find it, is too valuable an addition to American geology not to be welcomed in spite of its incongruous surroundings. The statistics of the base metals and minor minerals, occupying the remainder of the volume, are now of comparatively little interest, as these subjects have been treated from year to year in the returns published by the United States Geological Survey, and are available up to 1885. It must, however, be remembered that it is only in census years that returns from individual establishments can be obtained, and that therefore the figures for those years may be regarded as more authoritative than those of other dates. In any case, statistics five years old are tolerably ancient history.

In conclusion, we must call attention to the author's introductory paper on the geographical and geological distribution of the iron ores of the United States. This is a masterly abstract of the main subject of the book, and will be particularly useful to those who may wish to acquire some knowledge of the basis of the American iron industry without searching through the great mass of reports and surveys in which most of the detailed information is to be found. A plate of comparative sections of the strata in the principal iron-ore producing States is especially interesting as showing how the most important ore deposits are confined to the older rocks, such as the Archæan regions of New York and New Jersey, the Huronian of Michigan and Wisconsin, and the great stratified belt of hæmatite or "fossil ore" in the Clinton group of the Upper Silurian; while the most important iron-bearing strata of this country and Western Europe, the Lias and Lower Oolitic series, are entirely absent. Although the great activity of the iron trade in 1880-81 was the cause of very energetic explorations, very few discoveries were made in the older producing districts, and it became evident that to make these it was necessary to go into new fields, and in any case the author considers that the accessible rich ores may perhaps be practically exhausted within the life of the present generation. It will then be necessary to fall back upon the leaner kinds, containing from 30 to 45 per cent. of iron, which are known to exist in vast quantities, though generally far removed from coal suited for smelting purposes.

H. B.

OUR BOOK SHELF.

Theory of Magnetic Measurements. By F. E. Nipher, Professor of Physics in Washington University. (London: Trübner and Co., 1887.)

THIS little work is intended to furnish information as to the practical details of a magnetic survey. The description of the instruments used is poor. Full details as to the necessary calculations are given. The directions for the use of the instruments involve in a few cases unnecessary precautions, while in others the method suggested appears rather rough. Thus the statement that it is advisable not to make any observations with a dip needle till ten minutes after magnetization, is not, we think, borne out by experience. On the other hand, the suggestion that the vibrations of a declination needle may be checked by the finger would be likely to mislead beginners. It would have been better to describe the method of bringing the magnet to rest by means of a small auxiliary magnet. On the whole, English students will probably find all that they want, and with more direct reference to the Kew pattern instruments, in Stewart and Gee's "Practical Physics," and are thus not likely to make much use of Mr. Nipher's work. A. W. R.

Studies in Life and Sense. By Andrew Wilson, F.R.S.E. (London: Chatto and Windus, 1887.)

PREVIOUS works of this kind by Dr. Andrew Wilson are so well known, that a very few words will suffice to introduce the present one to the notice of our readers. It consists of a re-publication of essays on biological and psychological topics, which the author has from time to time contributed to sundry magazines. Although there is little or no attempt at originality, the collection is well calculated to prove of use and interest to general readers. The style is everywhere entertaining, and the following is a list of the subjects treated:—"Human Resemblances to Lower Life," "Some Economics of Nature," "Monkeys," "Elephants," "Past and Present of the Cuttle-Fishes," "Migration of Animals," "The Problems of Distribution," "Songs without Words," "The Laws of Speech," "Body and Mind," "The Old Phrenology and the New," "The Mind's Mirror," "What Dreams are made of," "Coinages of the Brain," "The Inner Life of Plants," "An Invitation to Dinner."

Fermenti e Microbi. Saggio di Igiene Antimicrobica Italo Giglioli. (Napoli, 1887.)

THIS book may be considered as marking a new departure in the teaching of hygiene. The enormous advances that have been made of late years in the recognition of pathogenic microbes, their life-history, and the conditions affecting them one way or another, have added a large and important chapter to the study of sanitary science. It is this particular subject in all its bearings on sanitary science which is treated in the volume by Prof. Giglioli. The study of ferments, like yeasts, forms the introduction: their life-history, physiological and chemical action, are described, and, owing to the accurate knowledge that we possess of them—thanks in a great measure to the researches of M. Pasteur—they form a fit starting-point in the study of schyzomycetes, bacteria, or microbes proper.

The book treats of microbes from every aspect, morphological and physiological. The relation of microbes in general to the nutritive media, their chemical products, and the relation of these to the microbes themselves; the production of soluble ferments by them; the influence of light, heat, &c., are passed in review and treated fully.

The pathogenic organisms are next considered. Their relation to the animal body; the means by which they gain access to the animal system; the various influences commonly understood to constitute "predisposition"; the relation of pathogenic bacteria to food, air, soil, and water; the adverse influences, such as heat and light, disinfectants and antiseptics, &c., are all discussed with great lucidity and thoroughness.

There is hardly any aspect under which the study of pathogenic microbes—including the question of attenuation—presents itself, which is not discussed in this volume. The arrangement of the subject-matter is systematic, and the method of treatment does great credit to the author, inasmuch as he is, as far as possible, objective. He carefully weighs and sifts evidence, and does not disdain to make references to the literature of England and France. He has, in fact, carefully read the literature of this country on infectious diseases, and thus attests that he is not guided by that spirit of narrowness which one often meets with in modern German works.

An English translation would, we have no doubt, be a valuable addition to our own literature. E. KLEIN.

Photography of Bacteria. By Edgar M. Crookshank, M.B. (London: H. K. Lewis, 1887.)

SINCE Koch first employed photography in bacteriology ("Biol. d. Pflanzen," 1877, ii. 3) various attempts have been made in this country and on the Continent to advance the methods of photographing microscopic objects, such as Bacteria, with high magnifying powers. About fifteen years ago Dr. Woodward, of Washington, published photographic plates of histological objects taken under tolerably high magnifying power (400 and 500 diameters). These plates were brought out by the Surgeon-General's Office, Army Medical Museum of the United States: they attracted at the time a good deal of attention owing to their comparatively high excellence. That good photographs of histological and other microscopic objects are of great value in themselves, owing to their exactness, and the various advantages for purposes of publication, may be taken as requiring no further proof, and it seems equally obvious that indifferent photographs are of less value than accurate drawings.

Now, comparing Dr. Crookshank's photograms of histological and bacteriological objects, published in the present volume, the former with those of Dr. Woodward, the latter with those of Koch, there can be little doubt that no real advance has yet been made in producing photograms that are to take the place of accurate drawings. By saying this I do not mean to convey the impression that in Dr. Crookshank's volume there are not some good photographs—*vide* his Plate XVI., further his Figs. 7, 8, 30, 35, and 45, all of which are really fine in many respects—but taking photography as a whole, as applied to the representation of microscopic objects under high powers, I think that the time has not yet come when it can be said to have supplanted good and accurate drawings. In connexion with this it must certainly appear remarkable that in the numerous and important publications on Bacteria by Koch and his pupils since 1877 to the present time we do not find a single illustration represented by micro-photography. All their published illustrations are drawings.

With the new apochromatic objectives and projection eye-pieces by Zeiss better results may be looked for, and Dr. Crookshank, with his great skill in, and knowledge of, the technique, will, we have little doubt, be able to produce them.

As a clear and detailed account of practical micro-photography, Dr. Crookshank's book is of great merit, and will prove very useful and important. As the first treatise on the subject in any language it is sure to command a high place. E. KLEIN.

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 Sense of Smell in Dogs.

IT is, I think, of some interest to supplement the very striking and exact experiments of Mr. Romanes on the scent of dogs, by an account of some experiments of a like kind made with a very different kind of dog, viz. a pug bitch. She was taught to hunt for small pieces of dry biscuit in a good-sized dining-room. The dog was put out of the room and a small piece, not much bigger than a shilling, of dry Osborne biscuit, was hidden; and as long as the hiding-place was accessible to the dog she never failed to find it. Sometimes the biscuit would be placed under a heap of a dozen or more newspapers on a dinner waggon, sometimes under a footstool, or sofa-cushion, or fire-shovel, and on two or three occasions in the foot of a boot which had been just taken off, the hiding body being always carefully replaced before the dog was admitted into the room, and without exception the biscuit in a very short time was discovered. It was over and over again proved that the dog did not follow the trail of the person who had hidden the biscuit; often the dog went by a different route, and in some cases one person hid the biscuit and another opened the door.

The experiment which has now special interest is the following one. A small piece of biscuit was placed on the floor under the centre of a footstool which was one foot square and six inches high, and standing on feet which raised it one inch from the ground. The dog, from the way in which she would set about moving the stool, not a very easy thing to do, as it stood in an angle of the wall, was evidently certain that the biscuit was beneath, and as scent seemed the only means by which she could have come at this conclusion, I thought to entirely mask this scent and prevent her finding the biscuit by pouring eau-de-Cologne on the stool. I found, however, it had no such effect, the biscuit was as readily and surely found when the eau-de-Cologne was there as when absent. It seems, then, that not only well-worn boots leave behind a recognizable odour, as Mr. Romanes proved, but also that to us at least so odourless a substance as dry plain biscuit emits so much and so characteristic a smell that it immediately spreads, even through considerable obstacles, to a distance of several inches in a few seconds, for in most cases the biscuit was found in thirty to sixty seconds after it had been hidden; thus time was not allowed, one would think, for all the surroundings of the hiding-place to become saturated with the scent.

W. J. RUSSELL.

Units of Mass, Weight, and Force.

MIGHT I venture to suggest to Prof. Greenhill that it would be very interesting to mathematicians, and probably would throw great light on the above subject, if he would give us quotations from some work by a practical engineer in which the idea of *inertia* distinctly appears. Or, failing this, perhaps Prof. Greenhill could give practical instances (other than problems in gunnery) in which *mass* quite apart from weight enters into the engineer's calculations.

It seems to me that many practical engineers never have occasion to deal with acceleration, except that of circular motion, and consequently only need to consider the *weight* of stuff, and have no use for the dynamical unit of force.

Gonville and Caius College, July 23. JOHN B. LOCK.

Chemical Affinity and Solution.

In continuation of my inquiry into the relation between chemical affinity and solution (NATURE, vol. xxxiii. p. 615, and and vol. xxxiv. p. 263) I would direct attention to some remarkable facts in connexion with the heats of formation of the sulphates. Take H_2SO_4Aq, and assume that SO_3 acts on the O of water with the average energy with which the S acts on O_2, and we have the following :—

$[H_2,O]$	= 68360	$[H_2S,O_4Aq]$	= 210770
$[S,O_3]$	= 103240		
$[S,H_2]$	= 4740		
$[SO_3,O]$	= 34413		
	210753	=	210770

Now consider $BaSO_4$. We have $[Ba,S,O_4] = 338070$ and—

$[Ba,O]$	= 124240		
$[S,O_3]$	= 103240		
Difference	= 110590		
	338070	=	338070

The difference 110590 is almost exactly equal to $[Ba,S] = 109600$, so that the heat of combination of BaO with SO_3 is practically equal to [Ba,S], and the whole of the affinity of S is used up so that it has no power to act on the O of water, and hence the salt is insoluble.

Take again in the same manner $SrSO_4$, and the result is even more striking—

$[Sr,O]$	= 128440	$[Sr,S,O_4]$	= 330900
$[S,O_3]$	= 103240		
Difference	= 99220		
	330900	=	330900

Difference 99220 = [Sr,S] = 99200, and again we have an insoluble salt. This seems to me pretty strong evidence that the cause of these combinations is the affinity of S for the metal, and that the S cannot act on the water to cause solution, because of its intense affinity for the metal. Further, the heat of neutralization is the difference between the heat of solution of the oxide and SO_3 on the one hand, and the heat of [MS] on the other, thus :—

$[SrO,Aq]$	= 29340
$[SO_3,Aq]$	= 39153
Neutralization	= 30710
99203	= [Sr,S] = 99200

and so on in other cases.

Now examine $CaSO_4$, which is a sparingly soluble salt, and note the difference, we have—

$[Ca,O]$	= 130930	$[Ca,S,O_4]$	= 318370
$[S,O_3]$	= 103240		
Difference	= 84200		
	318370		318370

This difference, 84200, is not equal to [Ca,S], which is = 92000, or 7800 units more, and accordingly we find this salt slightly soluble with a heat of 4440 units, because the S is somewhat free to act on water. Further, we have the remarkable fact that $CaSO_4$ combines with $2H_2O$, and evolves in so doing 4740 units of heat, which is exactly equal to $[S,H_2]$. Evidently the whole of the affinity of S for Ca not being used up in CaO,SO_3 the S can act with its full energy on the H of the water. $MgSO_4$, which is a still more soluble salt, shows entirely analogous results, the freedom of the S to act on water being much greater than with $CaSO_4$.

Take now an example of a somewhat different nature ; consider the following :—

$[Na_2,O]$	= 99760	$[Na_2,S,O_4,10H_2O]$	= 347810
$[S,O_3]$	= 103240		
Difference	= 144810		
	347810	=	347810

The heat of $[Na_2,S]$ is only 88200 units, but the heat of solution of Na_2O is 55500, and these two make up very nearly the difference of 144810 units. Thus we have the affinity of the S entirely used up, but the affinity of the Na_2 for the oxygen of the H_2O is so great that it can combine as a crystal with ten molecules, in addition to combining with the SO_3.

If space permitted, these facts might be extended and gone into more minutely, and their complete agreement in every particular with my theory of solution pointed out.

I may add further that the amount of salt dissolved in saturated solutions which I have examined is in complete harmony with that theory, as the following example will show :—

Heat of Combination. $[M,Cl_2] - [M,O,Aq]$	Amount of Salt in Saturated Solution. MCl_2
Ca = 20560	63 grains
Sr = 26770	46 ,,
Ba = 35980	35 ,,

It is evident at once that the amount of salt in solution is almost exactly inversely as the difference of heat of $[M,Cl_2]$ and [M,O,Aq].

WM. DURHAM.

Early Perseids.

From my observations in preceding years I found the great shower of Perseids commenced on about July 25, and that the last visible traces of it were seen on August 22, after a duration of 29 days.

This year a series of very clear nights occurred on July 16, 18, 19, 20, 21, 22, 23, 27, 28, and 29, and I watched the sky attentively throughout each one, with the idea of tracing, if possible, the earlier stages of this famous shower. On the 16th there were certainly no Perseids visible, but on the 18th, at 11h. 1m., I saw a brilliant streak-leaving meteor in Andromeda, which must have belonged to this stream. On the 19th I recorded 4 Perseids (2 of which were brilliant), and the radiant-point was sharply defined at 19° + 51°. On the 20th and 21st I noticed several other Perseids, but they were too distant from their radiant, and the paths too few to indicate a good centre. On the 22nd, however, I saw 5 Perseids (one of which was as bright as Jupiter), and the radiant now appeared at 25° + 52°. On the 23rd I registered 4 Perseids, apparently from the same point of the heavens.

The few ensuing nights were cloudy, but on the 27th the sky became partly clear, and in 3 hours I counted 38 meteors, of which 5 were Perseids from a radiant at 29° + 54°. On the 28th in 3¾ hours I saw 47 meteors, though clouds were very prevalent all night. On this occasion 10 Perseids were seen from a centre at 30° + 55°, and there were 15 Aquariads from 337° − 12°. On the 29th the sky was almost uninterruptedly clear, and in 3¾ hours I recorded 52 meteors, including 10 Perseids from 31° + 54½. On the 30th, clouds prevailed.

Between July 16 and 29 I observed 287 meteors, of which 43 were Perseids. These observations prove that the display really begins a week earlier than that (July 25) given in my paper in the *Monthly Notices* of the Royal Astronomical Society, vol. xlv. p. 97. The displacement of the apparent radiant-point as there described is well confirmed by my new observations. During the interval from July 18 to August 22 this point advances from 19° + 51° to 77° + 57°.

I subjoin the observed paths of a few bright meteors seen during my recent observations :—

1887.	h.	m.	mag.	Path				Radiant.
July 19	11	43	1	358½° + 38° to 351° + 30°		streak		19° + 51°
	11	43	1	298° + 56° ,, 14° + 65½°		slow		271° + 61°
	12	25	1	350° + 62° ,, 330½° + 64°		streak		19° + 51°
	12	52	1	9° + 20° ,, 17° + 20½°		swift		333° + 12°
,, 22	10	59	½	16° + 41° ,, 16½° + 51°		streak		16° + 31°
	12	21	½	356° + 45° ,, 332° + 29°		streak		25° + 52°
	12	25	½	344° + 33° ,, 320° + 29°		streak		16° + 31°
	13	15	½	393° + 37° ,, 355½° + 10½°		slowish		271° + 48°
	13	35	1	111° + 50° seemed stationary.				
,, 27	10	40½	½	325° + 6° ,, 330° + 8°		slow		322° + 4°
	13	21	½	319½° + 16½° ,, 308° + 32°		slowish		337° − 12°
,, 29	11	28	½	66° + 72½° ,, 114° + 70°		swift, streak		21° + 57°

Many others were seen of 1st mag. A perfectly stationary meteor of the 2nd mag., and sparkling like a star, was visible on July 29 at 14h. 17m. at 337° − 12°, so that it was an Aquariad travelling directly in the line of sight.

On the 22nd I registered some brilliant meteors, of precisely the same visible type as the Perseids, from a radiant at 16° + 31° or 3° south of β Andromedæ. Many meteors have also been falling from the points 269° + 49°, 310° + 9°, 333° + 12°,

335° + 49°, and 351° + 38°. All these are swift and short, and generally devoid either of streaks or trains.

Bristol, July 31. W. F. DENNING.

P.S.—In 1885 and some other years I have seen, on about July 13, a very definite shower of bright streak-leaving meteors from the point 11° + 48°, and it is a very probable conjecture that this radiant represents the earliest manifestation of the stream of Perseids.—W. F. D.

Floating Eggs.

REFERRING to the remarks of Mr. E. E. Prince in NATURE, July 28, p. 294, on the above subject, I wish to add that the spawn found by me had "the light violet-gray tint" and crape-like appearance he describes. I am very much on the water in harbours frequented by *Lophius*, but never saw any of this spawn before.

We found it in a swirl of the tide off Bantry Bay, where the sea was over 40 fathoms deep, and in the midst of innumerable jelly-fish, which seem to congregate wherever the current is most swift. W. S. GREEN.

Carrigaline.

THE "METEOROLOGISKE INSTITUT" AT UPSALA, AND CLOUD MEASUREMENTS.

THE Meteorological Institute at Upsala has gained so much fame by the investigations on clouds which have been carried on there during the last few years, that a few notes on a recent visit to that establishment will interest many readers.

The Institute is not a Government establishment; it is entirely maintained by the University of Upsala. The *personnel* consists of Prof. Hildebrandsson, as Director; M. Ekholm and one other male assistant, besides a lady who does the telegraphic and some of the computing work.

The main building contains a commodious office, with a small library, and living apartments for the assistant. The principal instrument-room is a separate pavilion in the garden. Here is located Thiorell's meteograph, which records automatically every quarter of an hour on a slip of paper the height of the barometer, and the readings of the wet and dry thermometers. Another instrument records the direction and velocity of the wind.

This meteograph of Thiorell's is a very remarkable instrument. Every fifteen minutes an apparatus is let loose which causes three wires to descend from rest till they are stopped by reaching the level of the mercury in the different tubes. When contact is made with the surface of the mercuries, an electric current passes and stops the descent of each wire at the proper time. The downward motion of the three wires has actuated three wheels, each of which carries a series of types on its edge, to denote successive readings of its own instrument. For instance, the barometer-wheel carries successive numbers for every five-hundredth of a millimetre—760·00, 760·05, 760·1, &c.; so that when the motion is stopped the uppermost type gives in figures the actual reading of the barometer. Then a subsidiary arrangement first inks the types, then prints them on a slip of paper, and finally winds the dipping wires up to zero again.

An ingenious apparatus prevents the electricity from sparking when contact is made, so that there is no oxidation of the mercury. The mechanism is singularly beautiful, and it is quite fascinating to watch the self-acting starting, stopping, inking, and printing arrangements.

We could not but admire the exquisite order in which the whole apparatus was maintained; the sides of the various glass tubes were as clean as when they were new, and the surfaces of the mercuries were as bright as looking-glasses.

The University may well be proud that the instruments were entirely constructed in Stockholm, by the skilful mechanic Sörrenson, though the cost is necessarily high. The meteograph, with the anemograph, costs £600, but the great advantage is that no assistant is required to sit up at night, and that all the figures wanted for climatic constants are ready tabulated without any further labour.

But the Institute is most justly celebrated for the researches on the motion and heights of clouds that have been carried on of late years under the guidance of Prof. Hildebrandsson, with the assistance of Messrs. Ekholm and Hagström.

The first studies were on the motion of clouds round cyclones and anticyclones; but the results are now so well known that we need not do more than mention them here.

Latterly the far more difficult subjects of cloud heights and cloud velocities have been taken up, and as the methods employed, and the results that have been obtained are both novel and important, we will describe what we saw there.

We should remark, in the first instance, that the motion of the higher atmosphere is far better studied by clouds than by observations on mountain-tops; for on the latter the results are always more or less influenced by the local effect of the mountain in deflecting the wind, and forcing it upwards.

The instrument which they employ to measure the



FIG. 1.—N. EKHOLM MEASURING CLOUDS.

This figure shows the peculiar ocular part of the altazimuth, with the vertical and horizontal circles. It also shows the telephonic arrangement.

angles from which to deduce the height of the clouds is a peculiar form of altazimuth, that was originally designed by Prof. Mohn, of Christiania, for measuring the parallax of the aurora borealis. It resembles an astronomical altazimuth, but instead of a telescope it carries an open tube without any lenses. The portion corresponding to the object-glass is formed by thin cross-wires; and that corresponding to the eye-piece, by a plate of brass, pierced in the centre by a small circular hole an eighth of an inch in diameter. The tube of the telescope is replaced by a lattice of brass-work, so as to diminish, as far as possible, the resistance of the wind. The vertical and horizontal circles are divided decimally, and this much facilitates the reduction of the readings.

The general appearance of the instrument is well shown in the figure, which is engraved from a photograph I took of M. Ekholm while actually engaged in talking through a telephone to M. Hagström as to what portion of a cloud should be observed. The lattice-work tube, the cross-wires in place of an object-glass, and the vertical circle are very obvious, while the horizontal circle is so much end on, that it can scarcely be recognized except by the tangent screw which is seen near the lower telephone.

Two such instruments are placed at the opposite

extremities of a suitable base. The new base at Upsala has a length of 4272 feet; the old one was about half the length. The result of the change has been that the mean error of a single determination of the highest clouds has been reduced from 9 to a little more than 3 per cent. of the actual height. At the same time the difficulty of identifying a particular spot on a low cloud is considerably increased. A wire is laid between the two ends of the base, and each observer is provided with two telephones—one for speaking, the other for listening. When an observation is to be taken, the conversation goes on somewhat as follows:—First observer, who takes the lead: "Do you see a patch of cloud away down west?" "Yes." "Can you make out a well-marked point on the leading edge?" "Yes." "Well then; now." At this signal both observers put down their telephones, which have hitherto engaged both their hands, begin to count fifteen seconds, and adjust their instruments to the point of cloud agreed on. At the fifteenth second they stop, read the various arcs, and the operation is complete.

But when the angles have been measured the height has to be calculated, and also the horizontal and vertical velocities of the cloud by combining the position and height at two successive measurements at a short interval. There are already well-known trigonometrical formulæ for calculating all these elements, if all the observations are good; but at Upsala they do far more. Not only are the observations first controlled by forming an equation to express the condition that the two lines of sight from either end of the base should meet in a point, if the angles have been correctly measured, and all bad sets rejected; but the mean errors of the rectangular co-ordinates are calculated by the method of least squares.

The whole of the calculations are combined into a series of formulæ which are necessarily complicated; and even by using logarithms of addition and subtraction, and one or two subsidiary tables—such as for log. $\sin^2\frac{\theta}{2}$, specially constructed for this work—the computation of each set of observations takes about twenty minutes.

Before we describe the principal results that have been attained, it may be well to compare this with the other methods which have been used to determine the height of clouds. A great deal of time, and skill, and money, have been spent at Kew in trying to perfect the photographic method of measuring the height of clouds. Very elaborate cloud cameras, or photo-nephoscopes, have been constructed, by means of which photographs of a cloud were taken simultaneously from both ends of a suitable base. The altitude and azimuth of the centre of the plate were read off by the graduated circles which were attached to the cameras; and the angular measurements of any point of cloud on the picture were calculated by proper measurements from the known centre of the photographic plate. When all this is done, the result ought to be the same as if the altitude and azimuth of the point of the cloud had been taken directly by an ordinary angle-measuring instrument.

It might have been thought that there would be less chance of mistaking the point of the cloud to be measured, if you had the pictures from the two ends of the base to look at leisurely, than if you could only converse through a telephone with the observer at the other end of the base. But in practice it is not so. No one who has not seen such cloud-photographs can realize the difficulty of identifying any point of a low cloud when seen from two stations half a mile or a whole mile apart; and for other reasons, which we will give presently, the form of a cloud is not so well defined in a photograph as it is to the naked eye.

At Kew an extremely ingenious sort of projector has been devised, which gives graphically the required height of a cloud from two simultaneous photographs at opposite ends of the same base, but it is evident that this method is capable of none of the refinements which have been applied to the Upsala measures, and that the rate of vertical ascent or descent of a cloud could hardly be determined by this method. But there is a far greater defect in the photographic method which at present no skill can surmount.

We saw that the altazimuth employed at Upsala had no lenses. The meaning of this will be obvious to anyone who looks through an opera-glass at a faint cloud. He will probably see nothing for want of contrast, and if anything of the nature of a telescope is employed, only well-defined cloud outlines can be seen at all. The same loss of light and contrast occurs with a photographic lens; and many clouds that can be seen in the sky are invisible on the ground glass of the camera. Cirrus and cirro-stratus—the very clouds we want most to observe—are always thin and indefined as regards their form and contrast against the rest of the sky; so that this defect of the method is the more unfortunate.

But even when the image of a cloud is visible on the focusing glass, it does not follow that any image will be seen in the picture. In practice, thin high white clouds against a blue sky can rarely be taken at all, or only under exceptional circumstances of illumination. The reason seems to be that there is very little light reflected at all from a thin wisp of cirrus, and what there is must pass through an atmosphere always more or less charged with floating particles of ice or water, besides earthy dust of all kinds. The light which is scattered and diffused by all these small particles is also concentrated on the sensitive plate by the lens, and the resulting negative shows a uniform dark surface for the sky without any trace of the cloud. What image there might have been is buried in photographic fog.

In order to compare the two methods of measuring clouds, I went out one day last December at Upsala with Messrs. Ekholm and Hagström when they were measuring the height of some clouds. It was a dull afternoon, a low foggy stratus was driving rapidly across the sky at a low level, and through the general misty gloom of a northern winter day we could just make out some striated stripes of strato-cirrus—low cirro-stratus—between the openings in the lower cloud-layer. The camera and lens that I use habitually for photographing cloud-forms—not their angular height—was planted a few feet from the altazimuth which M. Ekholm was observing, and while he was measuring the necessary angles I took a picture of the clouds. As might have been expected under the circumstances, the low dark cloud came out quite well, but there was not the faintest trace of the strato-cirrus on the negative. MM. Ekholm and Hagström, however, succeeded in measuring both layers of cloud, and found that the low stratus was floating at an altitude of about 2000 feet high, while the upper strato-cirrus was driving from S. 57° W. at an altitude of 19,653 feet, with a horizontal velocity of 81, and a downward velocity of 7·2 feet per second. This is a remarkable result, and shows conclusively the superiority of the altazimuth to the photographic method of measuring the heights of clouds.

Whenever opportunity occurs, measures of clouds are taken three times a day at Upsala, and it may be well to glance at the principal results that have been obtained.

The greatest height of any cloud which has yet been satisfactorily measured is only 43,800 feet, which is rather less than has usually been supposed; but the highest velocity, 112 miles an hour with a cloud at 28,000 feet, is greater than would have been expected. It may be interesting to note that the isobars when this high velocity was reported were nearly straight, and sloping towards the north-west.

The most important result which has been obtained from all the numerous measures that have been made is

the fact that clouds are not distributed promiscuously at all heights in the air, but that they have, on the contrary, a most decided tendency to form at three definite levels. The mean summer level of these three stories of clouds at Upsala has been found to be as follows: low clouds—stratus, cumulus, cumulo-nimbus, 2000-6000 feet; middle clouds—strato-cirrus and cumulo-cirrus, 12,000-15,000 feet; high clouds—cirrus, cirro-stratus, and cirro-cumulus, 20,000-27,000 feet.

It would be premature at present to speculate on the physical significance of this fact, but we find the same definite layers of clouds in the tropics as in these high latitudes, and no future cloud-nomenclature or cloud-observations will be satisfactory which do not take the idea of these levels into account.

But the refinements of the methods employed allow the diurnal variations both of velocity and altitude to be successfully measured. The velocity observations confirm the results that have been obtained from mountain stations—that, though the general travel of the middle and higher clouds is much greater than that of the surface winds, the diurnal variation of speed at those levels is the reverse of what occurs near the ground. The greatest velocity on the earth's surface is usually about 2 p.m.; whereas the lowest rate of the upper currents is about midday.

The diurnal variation of height is remarkable, for they find at Upsala that the mean height of all varieties of clouds rises in the course of the day, and is higher between 6 and 8 in the evening than either in the early morning or at midday.

Such are the principal results that have been obtained at Upsala, and no doubt they surpass any previous work that has been done on the subject. But whenever we see good results it is worth while to pause a moment to consider the conditions under which the work has been developed, and the nature and nurture of the men by whom the research has been conducted. Scientific research is a delicate plant, that is easily nipped in the bud; but which, under certain surroundings and in a suitable moral atmosphere develops a vigorous growth.

The Meteorological Institute of Upsala is an offshoot of the Astronomical Observatory of the University; and a University, if properly directed, can develop research which promises no immediate reward in a manner that no other body can approach.

If you want any quantity of a particular kind of calculation, or to carry on the routine of any existing work in an Observatory, it is easy to go into the labour market and engage a sufficient number of accurate computers either by time or piece-work, or to find an assistant who will make observations with the regularity of clockwork.

But original research requires not only special natural aptitudes and enthusiasm to begin with, but even then will not flourish unless developed by encouragement, and the identification of the worker with his work. It is rarely, except in Universities, that men can be found for the highest original research. For there only are young students encouraged to come forward and interest themselves in any work for which they seem to have special aptitude.

Now, this is the history of the Upsala work. Prof. Hildebrandsson was attached as a young man to the meteorological department of the Astronomical Observatory, and when the study of stars and weather were separated, he obtained the second post in the new Meteorological Institute. From this his great abilities soon raised him to the directorship, which he now holds with so much credit to the University. M. Ekholm, a much younger man, has been brought up in the same manner. First as a student he showed such aptitudes for the work as to be engaged as assistant; and now, as the actual observation and reduction of the cloud-work is done by him and M. Hagström, the results are published under their names, so that they are thoroughly identified with the work.

Upsala is the centre of the intellectual life of Sweden, and there, rather than at Stockholm, could men be found to carry out original research. It redounds to the credit of the University that it has so steadily supported Prof. Hildebrandsson, and that he in his turn has utilized the social and educational system by which he is surrounded to bring up assistants who can co-operate with him in a great work that brings credit both to himself, to themselves, and to the Institute which they all represent.

RALPH ABERCROMBY.

A NEW COSMOGONY.[1]

I.

THE volume before us is composed of a series of essays, first published in the Catholic periodical, *Natur und Offenbarung*, in 1885-86. By far the greater part of it is, nevertheless, of a purely scientific character. The opening chapter alone enters upon theological considerations, which we cannot here pretend to discuss, recommending merely, in passing, the broad and wise doctrines it contains to the notice of those well-meaning persons who apprehend danger to creeds from speculations as to origins.

That of our planetary system is very actively in debate just now. The nebular hypothesis, as fashioned by Laplace, no longer fits in with all the known facts. There are so many of them that it would be surprising if it did, since the outside of its claim was to the plausible representation of possible truth. It had a part to play in the history of science, which it played with eminent success. This was to show that thought, safeguarded by right reason, might be trusted to run backward towards the beginnings of things—that, without visible discontinuity or breach of known laws, the present fair scheme of creation might have emerged from the brooding darkness of chaos, along paths not wholly inaccessible to human discursive faculties.

But now the reiterated blows of objectors may fairly be said to have shattered the symmetrical mould in which Laplace cast his ideas. What remains of it is summed up in the statement that the solar system did originate, somehow, by the condensation of a primitive nebula. The rest is irrevocably gone, and the field lies open for ingenious theorizing. It has not been wanting.

The newer cosmogonists are divided into two schools by the more or less radical tendencies of the reforms they propose. Some seek wholly to abolish, others merely to renovate, the Kant-Laplace scheme. The first class is best represented by M. Faye, the second by M. Wolf and Dr. Braun. Dr. Braun is, however, a more thorough-going renovator than M. Wolf. The edifice, as "restored" by him, shows, indeed, little trace of its original aspect. Scarcely the invisible foundations are the same; the superstructure is unrecognizable. We will endeavour to sketch its main features.

In widening the nebular hypothesis to embrace the whole sidereal world, our author demands as little as possible in the way of postulates: simply a co-extensive nebula, structureless, motionless, tenuous, its particles endowed with gravity and atomic repulsion. Such a nebula, if perfectly homogeneous, should give birth to one portentous, solitary sun. But, in point of fact, it would possess innumerable, almost imperceptible, local irregularities, which, forming so many centres of attraction, would eventually lead to the breaking-up of the nebula into a vast multitude of separate fragments.

On one of these, the destined progenitor of the solar

[1] "Ueber Cosmogonie vom Standpunkt christlicher Wissenschaft. Mit einer Theorie der Sonne." Von Carl Braun, S.J. (Münster: Aschendorff, 1887.)

system, we are asked to concentrate our attention. The manner of its development is, however, a widely different one from that traced by the great French geometer. Laplace *assumed* the needful rotation, and left the rest to work itself out spontaneously. He permitted no external interference with the tranquil processes which he discerned as progressing through the ages. Dr. Braun, on the other hand, assumes less to begin with, but invokes adventitious aid in emergencies. No single nebula thrown on its own resources sufficed, he finds, for the production of the solar system. The complicated phenomena which it presents demand a complex origin to explain them. The mass of cosmical matter in which they first began to unfold themselves was accordingly but the nucleus of what it afterwards became. It not only grew by the accretion of similar masses falling towards it from space, but acquired its gyratory movement by eccentric collisions with them. The consequences of such events elsewhere are visibly pictured to us in the spiral lines of light of certain nebulæ. The great whirlpool in the Canes Venatici, for instance, betrays and records the fall of a comet, on the gigantic primitive scale, into an embryo sun. Only thus, in our author's opinion, can the strange peculiarities of its structure be accounted for; and only thus can the first impulse to axial rotation in our own system have been given.

The visits of comets, as we now see them, feebly represent, we are told, the colossal in-rushes from interstellar regions by which the machinery of planetary production was set going and modified. But it is difficult to allow to such bodies the independent origin implied in the claim for them of such illustrative significance. Comets can no longer be set down as mere casual intruders upon the solar system. They certainly share its translatory motion, since, if they were either overtaken or encountered, they should seem to come most numerously from near the apex of the sun's path. But they approach him indifferently from all parts of the sky. A further proof of the absence of relative motion is afforded by the shape of the tracks they pursue. M. Faye has remarked that, of 364 cometary orbits calculated, not one is a decided hyperbola ("Origine du Monde," p. 146); and Laplace's view that they are hyperbolic by nature, and elliptical only through perturbations, is thus seen to be the exact reverse of the truth.

A fundamental objection to Laplace's cosmogony is that it implies a far swifter axial movement in the central bodies of our system than they actually possess. For in the theory of annular separation, the rotation of the generating mass is strictly correlated with the revolution of its offspring by the principle of the conservation of areas, which requires that a rotating homogeneous globe should spin quicker, as it contracts, in the proportion of the square of its radius. Thus, if the solar nebula, when it filled the orbit of Neptune, rotated (as on the hypothesis in question it must have done) in Neptune's period of 165 years, the period of the sun shrunken to its present dimensions, should have shortened in the ratio of the square of 2,780,000,000 (the mean distance of Neptune) to the square of 434,000 miles (the solar radius). In other words, the rotation of the sun should be accomplished in 127 seconds, in lieu of 25 days. Similarly, the terrestrial rotation-period corresponding to the lunar revolution in 27⅓ days, is no more than 10⅓ minutes! It is true that in both these estimates (the latter taken from Dr. Braun's pages), the effects of central condensation are neglected, although it must inevitably have made some progress before annulation began; but no allowance on this score, however liberal, can possibly reconcile, though it contributes to lessen, the discrepancy.

Dr. Braun adjusts the balance in this way. The solar nebula had never at any time, in his view, a uniform axial movement. He even ventures to consider the present unequal rotation of the sun as a survival of the primitive state of things to which the central deficiency of rotational momentum is due. For the entire mass was, in the beginning, set gyrating by external impacts. Movement was hence generated predominantly in its outer regions, and was only by degrees and imperfectly communicated to the nuclear parts.

The device is marked by considerable ingenuity, and is at any rate preferable to the eddying movements by which M. Faye evades the same embarrassment. It is, however, scarcely needed by Dr. Braun, since the "ring-theory" of planetary formation is almost, and logically ought to be completely, abandoned by him. Difficulties have of late been thickening round it; they reached a climax when the conviction was attained that, apart from the neutralizing effects of tidal friction, it could only result in the retrograde motion of all secondary systems. The plan of centres of condensation is accordingly substituted by our author. This has the advantage of allowing planets to begin to form anywhere in the nebula. It emancipates them from that strict conformity to the equatorial level which was an inconvenient feature of Laplace's hypothesis; and though they necessarily tended, in the course of their growth, to descend towards it, enough may perhaps remain of their primitive divergence to explain the observed slight deviations from the fundamental plane. Yet Dr. Braun's confidence in this *rationale* of the inclinations of the planetary orbits is so far from being unlimited that he holds in reserve, in case of its failure, other means for bringing about the same end.

Each planet is roughly estimated to have started on its career at about five times its present distance from the sun. In condensing, it descended towards it, sweeping up materials as it went, until finally almost the whole of the diffused gaseous stuff was concentrated in sun and planets, and the intervening spaces were void. By that time, too, tangential velocity had come to balance gravity, and the slow inward approach ceased. But the resistance met with in the earlier stages of its history by the growing and falling planet had had one result of vital importance to its future. It had imparted to it a movement of rotation. As it settled down in close spirals towards its present orbit, its velocity must everywhere have exceeded, by a small amount, the velocity in the same direction of the medium in which it moved. The density of that medium must, however, have increased towards the sun; and the embryo-planet consequently experienced a slight excess of resistance on its inner side, resulting in a direct whirling movement. Dr. Braun endeavours to show that the rotation thus set on foot must have belonged chiefly to the external layers of the planetary nebula. His motive is that of conciliating the swift circulation of satellites yet to be born from it with the comparatively sluggish spinning of the parent mass.

Tidal friction he rejects as an agent of planetary development, attributing to it barely the power to have rendered absolute an already approximate coincidence between the periods of rotation and revolution of satellites. Perhaps he might here be induced to reconsider his position. At least in the case of the lunar-terrestrial system, the evidence is overwhelming that tidal friction was largely concerned in bringing about its present condition. We may further assure him that Prof. G. H. Darwin (whom he evidently identifies with the late Charles Darwin, his father) has not committed the blunder he imputes to him of ascribing to the moon a *shorter* period of revolution than that of the earth's rotation, at the time when it began, under the reactive influence of the tidal wave, to travel slowly outward from near its surface. On the contrary, a slightly inferior angular velocity in the satellite is the assumed starting-point of all his subsequent reasoning on the subject.

For the completion of the solar system in its minor details, Dr. Braun is driven to the expedient of collisions

with some of the many nebulous fragments which continued to be drawn towards it from unfathomed depths of space. Most of these became incorporated with the sun, but a certain proportion must have been intercepted by the planets, which, in their forming state, as possessing less mass and velocity, were more sensitive to such shocks than when fully formed. Thus, the plane of the ecliptic might have been altered, we are told, 1° by the impact upon the inchoate earth of a body possessing 1/1000 its present mass. Facilities even greater were offered for changing the elements of rotation; that of the earth, when of seventy times its actual radius, might even have been stopped altogether, by collision, under specially favourable circumstances, with a mass only 1/10600 the terrestrial.

But this method of explanation is radically unsatisfactory. It suggests the *Deus ex machinâ* of an unskilled dramatist, and cannot be admitted without protest into scientific speculation. We have learned to regard cometary impacts as the last resource of the distressed cosmogonist. Such events are not impossible, but to resort to them in difficulties is to throw up the game of ordered inference. The conviction remains unalterable that the results visible to us were brought about by means less apparently fortuitous. Dr. Braun, for example, is obliged to suppose not only that, before the separation of the moon, the axis of the lunar-terrestrial nebula was deviated, by extraneous agencies of the kind indicated, to the extent of 5° from its original position of perpendicularity to the plane of the ecliptic; but that, subsequently to the separation, further shocks continued the process upon the earth alone until the inclination attained its present value of 23½°. Still less admissibly, the solar axis is assumed, after the formation of Venus, to have been tilted 5° by a number of successive impacts. A transcendent degree of improbability seems to be reached by this conjecture.

In the order of planetary production, Dr. Braun follows Laplace. Neptune is his oldest planet. And the fact that it revolves very nearly in the "invariable plane" of the solar system is confirmatory of the view that it really was the first body (instead of being the last, as M. Faye supposes) to become severed from the primitive nebula, the rotation of which is likely to have been conducted in that plane (Wolf, *Bull. Astronomique*, t. ii. p. 228). Neptune alone, owing to the distinction of his retrograde rotation, is allowed by our author to have been formed by the detachment and eventual condensation of a nebulous ring. But Prof. Kirkwood has raised an objection to this orthodox mode of genesis which applies with especial force to the remotest planet. The coalescence into a single globe of the fragments of a broken-up ring, if it happened at all (which is uncertain) would, it appears, have been an unconscionably slow process. Thus, two opposite portions of a ring of the dimensions of Neptune's orbit, could scarcely have come together in less than 150,000,000 years. It must be admitted that this is a startling demand on the time-exchequer even of the cosmos.

Uranus is regarded by Dr. Braun as what Bacon called a "limiting instance" between the annular and the nuclear methods of generation. An abortive ring gave place to a centre of condensation, the result (helped, perhaps, by some well-aimed cometary shoves) being the indecisive character of the Uranian rotation on an axis lying prone in the plane of revolution.

These, then, are the main outlines of the last and newest cosmogony. While dissenting from some of its conclusions, we readily admit that it is, in several ways, a noteworthy effort. Its appearance may be said to mark the definitive abandonment, by sound thinkers, of the annular method of planet and satellite formation. The preciseness of the conditions of that celebrated hypothesis lent it its charm, but has proved its ruin. Had they been less definite, it might have lived longer. But it gave, as it were, hostages to the future which it has not been able to redeem.

It is gradually becoming clear that, while the various members of the solar family owned unquestionably a common origin, they can scarcely be said to have had a common history. Each ran through a cycle of development particular to itself, and appointed, without doubt, to adapt it to a special purpose. The biography of the earth and moon, as narrated by Prof. Darwin, is an example. Here influences predominated which, in every other secondary system, were comparatively unfelt.

This growing persuasion of what we may call planetary individuality is reflected in Dr. Braun's vigorous and original chapters. He has honestly, and with no small ability, worked out *ab initio* the problems that they deal with, and he finds them insoluble by the uniformitarian method of treatment. The expedients by which he seeks to obtain a diversity of results by introducing a diversity of vicissitudes, strike us perhaps as arbitrary and awkward; but the admission of their necessity by an inquirer of such acuteness, and so well abreast of contemporary scientific thought, is highly instructive. We shall return later to the part of his interesting work devoted to solar theory.

A. M. CLERKE.

NOTES.

ON Tuesday Lord Hartington introduced to Sir W. Hart Dyke a deputation consisting of Sir Henry Roscoe, Sir Lyon Playfair, Mr. Rathbone, Mr. G. Howell, Mr. Cyril Flower, Sir B. Samuelson, and other gentlemen interested in education. They urged the desirability of the Technical Education Bill being passed, and of certain changes being made in the measure. Sir W. Hart Dyke replied favourably on both points. He was of opinion that the prospects of the Bill were good, both in the House of Commons and in the House of Lords.

THE list of foreign men of science who have accepted the invitation of the Local Committee to attend the Manchester meeting of the British Association is steadily increasing, and now numbers considerably over a hundred. Amongst those whose names have been received since the list we published on July 7, we note Prof. Cremona, of Rome; Dr. Neumayer, Director of the Hamburg Marine Observatory; H. A. Rowland, Baltimore; Dr. Werner Siemens, of Berlin; Prof. Horstman, Heidelberg; Prof. L. Weber, Breslau; Prof. Capellini, Bologna; Prof. Carvill Lewis, Philadelphia; Prof. O. Bütschli, Heidelberg; Prof. Carnoy, Louvain; Prof. Erb, Heidelberg; Dr. Treub, Director of the Botanic Gardens, Java; Capt. Coquilhat, Brussels; Dr. Godefroi, 's Hertogenbosch; Dr. Ludwig Wolf, Leipzig; Signor Bonghi, the late Italian Minister of Education; Signor Luzzati, Rome; Dr. E. Atkinson, Director of the Massachusetts Bureau of Statistics; and Dr. G. Boissevain, Amsterdam. The King of the Belgians has intimated his intention of nominating two representatives of the Congo Free State to attend the meeting, and of these General Strauch, Administrator-General of the Congo Free State, is expected to be one. A correspondent in Paris writes to us that the Emperor of Brazil, who has lately spent some time in the French capital, will probably attend the meeting of the Association.

MR. A. T. ATCHISON, Secretary of the British Association, is authorized to state that at the Manchester meeting space will be provided in the galleries of the Reception Room for the exhibition of specimens and instruments shown in connexion with and in illustration of papers read in the Sections. To secure admission a brief description of the specimens or instruments must be submitted to the Local Secretaries not later than August 10, together with a statement of the dimensions of the table or other fixture required. No motor power will be available. The objects must be exhibited at the risk of the owners;

and the Local Committee, while it will endeavour to meet all reasonable wishes, reserves to itself the right to exclude all exhibits that may appear to it to be for any reason unsuitable. No objects shown for advertising purposes will be admissible.

THE summer meetings of the Institution of Mechanical Engineers were held this week on Tuesday and Wednesday in the University of Edinburgh, under the presidency of Mr. E. H. Carbutt. After the meeting on Tuesday, the members inspected the Forth Bridge and Works.

ON Saturday, July 30, the statue of Paul Broca, close to the Medical School, Paris, was unveiled. Addresses were delivered by different persons connected with the Anthropological School.

A CHAIR of Sanitary Engineering has been established in the Imperial University of Japan. It is believed to be the only Chair of the kind at present in existence. The Professor appointed to fill it is Mr. W. K. Burton, lately senior Sanitary Engineer to the London Sanitary Protection Association.

THE geographical habitat of *Peripatus leuckarti*, Sänger, is said (*Archiv für Naturg.* 1871, p. 407) by Prof. Leuckart to be "New Holland." Owing to the vagueness of our knowledge of the subject, it may be of interest to state that two specimens of what appears to be this species have been found in the Queensland scrubs, near Wide Bay. These specimens have been presented to the British Museum by Mr. E. P. Ramsay, of the Australian Museum, Sydney.

THE announcement of the discovery of more than a dozen new elements appears at first sight rather sensational, and were it not that the names of Krüss and Nilson are sufficient guarantees of its authenticity the intimation would probably be received with more than a little caution. Such, however, is the startling result of the long, laborious, and exceptionally difficult researches of the Swedish chemists upon the nature of the rare earths contained in several sparsely-distributed minerals, and a detailed account of their labours will be found in the number of the *Berichte* just issued. A precise measurement of the wave-lengths of the lines and bands in the absorption-spectra of the nitrates of the rare earths contained in thorite of Brevig and Arendal, wöhlerite of Brevig, cerite of Bastnäs, fergusonite of Arendal and Ytterby, and in euxenite of Arendal and Hitterö, has resulted in the surprising observation that only a particular few of the lines supposed by former observers to belong to the nitrate of any one metal are present in the absorption-spectra of the nitrates derived from certain minerals, the other lines being absent in these, but present in the nitrate spectra of other minerals, while some that are present in the former are absent or very faint in the latter. For example, only one of the lines supposed to belong to the nitrate of holmium is present in any intensity in the spectrum of the nitrates from thorite of Brevig, while in the spectrum of nitrates derived from other minerals it is but an insignificant line among several holmium lines much more intense. The conclusion from most exhaustive spectral measurements is inevitable, that most of the so-called elements in these minerals are compound substances, the various ingredients of which are present in certain minerals and absent in others; further, the fractionation of the nitrates has led to the partial separation of a large number of the components themselves. Holmium, the metal which Soret called X, and which Lecoq de Boisbaudran separated into two components, is now shown to consist of seven distinct elements—Xα, Xβ, Xγ, Xδ, Xϵ, Xζ, and Xη; erbium of two—Erα and Erβ, which it is possible to separate by fractionation; thulium, named by Cleve in 1879, also of two—Tmα and Tmβ; didymium which was shown by von Welsbach to consist of two components, praseodym and neodym, must, in the light of these spectral differences, consist of not less than nine distinct elements, while samarium, the name given by Lecoq de Boisbaudran to Marignac's Yβ, is composed of at least two components—Smα and Smβ. Hence, in place of holmium, erbium, thulium, didymium, and samarium, we are constrained to accept the existence of more than twenty elements, in the work of completing the isolation of which Drs. Krüss and Nilson urgently invite assistance.

THE United States *Monthly Weather Review* for March and April contains interesting notes, *e.g.* (1) Average storm-tracks over the United States during March, compiled from observations for the years 1873-85. The paths pursued by these storm-centres are divided into four distinct classes, and are traced until the disappearance of the storms at various points on the Atlantic coast. (2) Rain frequency and wind rose for April, with charts constructed from all observations available from the commencement of the records until the end of 1886. The charts have been prepared for use in the current weather predictions of the service. The Reviews also contain descriptions of the storms which occurred over the North Atlantic during each month, and their approximate paths are shown on charts, together with the distribution of icebergs reported.

THE *Jahres-Bericht* of the Central Meteorological Office of the Grand Duchy of Baden, for 1886, contains the results of meteorological observations at sixteen stations of the second and third orders, and twenty-nine rain stations, of which the positions and heights above sea are given. The Central Office has taken part in two Conferences during the year—one at Munich, relative to the investigation of the frequency, direction of propagation and intensity of hailstorms; and the other at Friedrichshafen, with respect to a physical survey of the Lake of Constance. The proposals made at both Conferences are awaiting the decisions of the various Governments concerned. The Report is accompanied by a chart giving the distribution of the rainfall during the year 1886, and shows three districts with maxima exceeding 55 inches, in positions corresponding with those on the chart of the previous year. The greatest amount was 79·56 inches at Todtnauberg, and the least 30·28 inches at Diedesheim. It also contains hydrological observations at various stations on the Rhine and its larger tributaries.

DR. K. WEIHRAUCH, Director of the Observatory at Dorpat, Russia, has published the mean values of the meteorological observations at that place for the twenty years 1866–85, giving (1) the results of the individual months and years, and (2) the results for the twenty years combined. This Observatory was established in December 1885 by the late Dr. A. von Oettingen, and is one of the few stations that have persistently published wind *components*, under each of the points N. E. S. W. (in addition to the usual components N. – S., and E. – W.), whereby a better knowledge of the general distribution of the wind is obtained than when only two components are given. The highest mean monthly shade temperature, for the twenty years, is 63°·2 in July, the lowest 19°·6 in January, and the mean for the year 39°·9. The greatest mean monthly rainfall is 3·64 inches in July, the lowest 1·02 inches in March, and the total for the year 16·21 inches. The fourth volume of the actual observations, for the years 1881–85, is now being printed, the publication having been delayed hitherto for want of funds.

THE work done at the Melbourne Observatory in connexion with meteorology and terrestrial magnetism expands a little every year. The importance attaching to rainfall and water-supply renders it necessary to spread rain-gauges wherever trustworthy observers can be secured, and we learn from the latest Report of the Observatory that 272 monthly returns are now received, most of the observers being volunteers. A complete register of Victorian rainfall has been prepared, showing at a glance the annual and monthly rainfall, as well as the averages for a series of years and months. The issue of weather-maps and forecasts for Southern and Northern Victoria has been regularly continued, and this service appears to be fully appreciated.

MR. R. H. SCOTT, of the Meteorological Office, sends us some notices of earthquakes observed at North Unst and Sumburgh Lighthouses. These notices he has come across while examining Journals of Shetland Lighthouses, and it may be worth while to put them on record. The following are records from logs at North Unst Lighthouse: 1879, January 4, 5 minutes past 1 p.m. mean time, felt smart shock of earthquake, lasted about 4 seconds; 1880, July 18, 20 minutes after midnight last, a smart shock of earthquake lasted from 34 to 35 seconds, then a second shock not so strong or of so long duration,—whole rock and building oscillated; 1885, September 26, at 10 p.m., we felt the tower shake very suddenly,—men in bed as well as the man on the watch cannot account for it, unless a slight shock of an earthquake,—no heavy sea, and the wind light from north. At Sumburgh Head Lighthouse the following observation was made: 1876, November 28, at 6 p.m., a slight shock of earthquake was felt at this station. For two or three seconds the lamp-glass in the tower shook violently. As my attention was taken up with the lamp, and fearing that the glass would fall, I therefore did not observe any other movement. The wind at the time was north by east, and light, accompanied with showers, and dark clouds hanging about.

SOME time ago (vol. xxxiii. p. 491) we gave an account of the excellent papers on "Technical Education, Applied Science, Buildings, Fittings, and Sanitation," by Mr. Edward Cookworthy Robins. These papers, revised and admirably illustrated, have now been brought together in a handsome volume entitled "Technical School and College Building," and published by Messrs. Whittaker and Co. The book is dedicated to Prof. Huxley. It will be very welcome to all who are engaged, or who look forward to being engaged, in the construction of technical school buildings.

A BOOK on "Canada and Newfoundland," by Ernst von Hesse-Wartegg, is about to be published in Freiburg-im-Breisgau. The author has repeatedly visited the country he describes, and his work is the more likely to be appreciated in Germany, because the northern part of the American continent has hitherto been almost wholly neglected by German writers of books of travel.

THE first number of the American *Journal of Psychology* will appear early in October. The object of the *Journal* is to record psychological work of a scientific as distinct from a speculative character.

PROF. GEO. H. PALMER, of Harvard College, has published the results of some inquiries lately made by him as to the annual expenditure of Harvard undergraduates. Of the members of the graduating class, about one-fourth had spent from 400 to 650 dollars; another fourth, from 650 to 975 dollars; another, from 975 to 1200 dollars; and another, upwards of 1200 dollars. The lowest sum reported was 400 dollars; the highest, 4000. Addressing parents, Prof. Palmer says:—"If your son is something like an artist in economy, he may live at Harvard under 600 dollars a year. If he is able to live closely, carefully, and yet with due regard to all that he requires, he may easily accomplish it on between 600 and 800 dollars. If you wish him to live here at ease, from 800 to 1000 dollars may be well expended. I should be very confident that every dollar given him over 1200 dollars was a dollar of danger."

IT seems, from a paper by M. Jammes, who lives in Camboja, that animals, as well as human beings, are liable to become addicted to opium-poisoning. In countries where the use of opium is prevalent, many animals remain beside their master while he takes a whiff at his pipe. Breathing an air containing a good deal of opium vapour, they become quite intoxicated, and appear to relish the sensation. This has been noticed concerning cats, dogs, and monkeys. The latter, according to M. Jammes, like the sensation so much that some of them take to eating opium.

THE Foreign Department of the Chinese Government (the Tsung-li-Yamên) has just addressed a very striking memorial to the Emperor proposing the introduction of mathematics and physics into the metropolitan and provincial competitive examinations for public employment. It is suggested that this should be done in all the provinces of the Empire, the successful candidates being sent to Pekin to compete for higher grades. They are to be examined in the capital, in addition to the preceding subjects, in civil and military engineering, international law and history. Those who are successful in obtaining the highest degree will receive an honorary official status, equivalent to a Fellowship, in the Foreign Language College at Pekin, together with official appointments in the capital or the provinces. This scheme has now received the Imperial sanction, and it is difficult for those unacquainted with China to appreciate the vast change which it will produce in time. Hitherto the competitive examinations which must be passed by every intending official, have been confined to the ancient Chinese classics, exercises in prose and poetical composition, and Chinese history, and they have been of the same kind for centuries. They were the ark of the covenant, which it was sacrilege to touch; but the Chinese have now introduced mathematical science into the curriculum. It will be interesting to see how the new and old subjects will fare respectively, now that they are brought together for the first time in the long history of China.

THE first volume of a new periodical specially devoted to botany has been issued in St. Petersburg. The periodical is published in connexion with the botanical garden of the St. Petersburg University by Profs. Beketoff and Gobi, under the title of "Scripta Botanica Horti Universitatis Petropolitanæ." The first volume contains an important work by Prof. Beketoff, on the flora of Yekaterinoslav, which not only gives a list of 1032 species of flowering plants (instead of the 536 species formerly found in the province), but contains a most interesting inquiry into the flora of the steppes of South Russia, as compared with those of the Hungarian *pusztas* on the one side, and the Caspian steppes on the other. The same volume contains a note by Prof. Gobi on a new form of fungi, *Cœoma Cassandræ*, which is found in the peat-marshes of Finland as a parasite on the *Andromeda* (*Cassandra*) *calyculata*; and a paper on the vegetation of the Altai, by A. Krasnoff, containing the enumeration of plants found by the author. These plants were found on the *Artemisia* steppes, on the salt steppes, on black-earth, on meadows inundated during the spring, in forests, and on the higher Alps. While comparing the present flora of the Altai with the Pliocene flora of the same area, characterized by the predominance of the beech and other trees of the temperate region, the author points out that only feeble vestiges of the old flora survive in the present flora of the meadows inundated during the spring. The vegetation of the other sub-regions has completely changed since the Tertiary period, and continues still to change. Thus larch forests disappear with astonishing rapidity, and are succeeded by herbaceous steppes, while *Artemisia* steppes have taken the place formerly occupied by the lakes and brackish marshes which covered the bottoms of the valleys. A feature of the "Scripta Botanica," most welcome to European botanists, is that the papers in Russian are followed by short abstracts in French or German. In the bibliographical section there are analyses of botanical works published in Russia since January 1, 1886. The works analyzed under this head are: Prof. Schmalhausen's "Flora of South-West Russia" (Kieff, Volhynia, Podolia, Poltava, Tchernigoff, and neighbouring regions), published at Kieff; Prof. W. Zinger's work on the "Flora of Central Russia;" Prof. Maximowicz's "Diagnoses Plantarum Novarum Asiati-

carum," fascicule vi.; M. Kamenski's "Comparative Researches into the Development and Structure of Urticularia;" and several botanical papers published in Russian scientific periodicals.

THE additions to the Zoological Society's Gardens during the past week include two White-eared Bulbuls (*Pycnonotus leucotis*) from North-West India, presented by General W. H. Breton; a Magpie (*Pica rustica*), British, presented by Mr. H. Stacy Marks, R.A., F.Z.S.; two Turtle Doves (*Turtur communis*), British, presented by Mr. N. Brooks; a Daubenton's Curassow (*Crax daubentoni*) from Venezuela, presented by Capt. Rigaud, s.s. *Ugrne;* a Loggerhead Turtle (*Thalassochelys caouana*) from the Atlantic Ocean, presented by Mr. R. T. Ward; two Green Lizards (*Lacerta viridis*); two Marbled Newts (*Molge marmorata*), European, presented by the Rev. F. W. Haines; a Crested Pigeon (*Ozyphaps lophotes*) from Australia; a Secretary Vulture (*Serpentarius reptilivorus*) from South Africa; an Elliot's Pheasant (*Phasianus ellioti* ♂); a Temminck's Tragopan (*Ceriornis temmincki* ♂) from China; four Spotted Tinamous (*Nothura maculosa*) from Buenos Ayres; two Indian Crocodiles (*Crocodilus palustris*) from India, deposited; eight Ocellated Sand Skinks (*Seps ocellatus*) from Malta, purchased; a Bennett's Wallaby (*Halmaturus bennetti* ♂) born in the Gardens; a Common Crowned Pigeon (*Goura coronata*), a Cockateel (*Calopsitta novæ-hollandiæ*) bred in the Gardens.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 AUGUST 7–13.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on August 7

Sun rises, 4h. 35m.; souths, 12h. 5m. 33·8s.; sets, 19h. 36m.; decl. on meridian, 16° 27′ N.: Sidereal Time at Sunset, 16h. 40m.

Moon (at Last Quarter on August 11) rises, 20h. 58m.*; souths, 2h. 29m.; sets, 8h. 9m.; decl. on meridian, 6° 14′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Decl. on meridian. ° ′
Mercury ...	3 36	11 7	18 38	16 31 N.
Venus	8 44	14 42	20 40	1 14 S.
Mars	1 59	10 13	18 27	23 8 N.
Jupiter... ...	11 32	16 45	21 58	10 3 S.
Saturn... ...	3 3	11 0	18 57	20 37 N.

* Indicates that the rising is that of the preceding evening.

Occultations of Stars by the Moon (visible at Greenwich).

August.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. ° °
8 ...	B.A.C. 81 ...	6½ ...	3 53 ...	4 57 ...	88 353
8 ...	26 Ceti	6½ ...	23 57 ...	1 2† ...	105 240
9 ...	29 Ceti	6½ ...	3 24 ...	4 5 ...	172 240
13 ...	48 Tauri	6 ...	2 18 ...	3 24 ...	76 244

† Occurs on the following morning.

August. h.
8 ... 1 ... Mercury stationary.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·3	81 16 N.	Aug. 11,	20 49	*m*
R Persei	3 22·8	35 17 N.	,, 9,		*M*
S Virginis	13 27·1	6 37 S.	,, 10,		*m*
U Coronæ	15 13·6	32 4 N.	,, 9,	3 11	*m*
U Ophiuchi	17 10·8	1 20 N.	,, 11,	1 42	*m*
			,,	21 50	*m*
X Sagittarii... ...	17 40·5	27 47 S.	,, 10,	21 0	*m*
T Serpentis	18 23·3	6 14 N.	,, 12,		*M*
β Lyræ... ...	18 45·9	33 14 N.	,, 10,	23 0	*m*
S Sagittæ	19 50·9	16 20 N.	,, 8,	21 0	*M*
S Cygni	20 3·1	57 40 N.	,, 13,		*M*
δ Cephei	22 25·0	57 50 N.	,, 10,	21 0	*M*

M signifies maximum; *m* minimum.

Meteor-Showers.

The present season is generally the richest in the year for meteors, being the season of the *Perseids*, and the neighbouring showers.

	R.A.	Decl.	
Near δ Andromedæ ...	7 ...	32 N. ...	Swift; streaks.
Perseids	44 ...	56 N. ...	Swift; streaks.
From Aries	44 ...	25 N. ...	Swift; streaks.
Near β Persei	48 ...	43 N. ...	Very swift; streaks.
From Camelopardus...	96 ...	71 N. ...	Slow.
Near θ Cygni	292 ...	52 N. ...	Rather slow.

THE JUBILEE OF THE ELECTRIC TELEGRAPH.

ON December 12, 1837, William Fothergill Cooke, on behalf of himself and Charles Wheatstone, set his hand and seal to Patent No. 7390, the subject of the specification being: "Certain apparatus or mechanism which is constructed according to our said improvements for giving signals and sounding alarums in distant places by means of electric currents transmitted through metallic circuits." This, the first English patent dealing with the electric telegraph, contains the elements of a thoroughly practical apparatus, as the historical experiment of July 25, 1837, made between Euston and Camden Town, had proved. Unlike many other developments of practical science, the commencement of the epoch when electric telegraphy became in a practical success in this country can be sharply defined, and what will become an historical event, viz. the commemoration of July 27, 1887, can strictly be said to be the true jubilee of the electric telegraph.

To say that the invention of which Cooke and Wheatstone were the pioneers has done more to transform the conditions of human existence than any other except the steam-engine, and some would add "gunpowder," is but to restate an acknowledged fact. The electric telegraph has so changed the conditions of our social existence as to become indispensable to the same, and we could almost as soon do without food and clothing as dispense with the power that has annihilated distance. The evolution of the electric telegraph as a means of transmitting intelligence from a distance did not, of course, commence from the year when Her Majesty began her reign. The names of Ronald, Schilling, Watson, Sömmering, Schweigger, Weber, Lesage, and very many others, will at once occur to those who give a moment's thought to the subject, as workers in the field long before Wheatstone made his famous experiment, but few, we think, will question the assertion that electric telegraphy as a commercial success distinctly dates from the year 1837.

The commemorative dinner was held in the Venetian Hall at the Holborn Restaurant on Wednesday evening, July 27, the Right Hon. H. C. Raikes, M.P., the Postmaster-General, being in the chair. A large number of representative men were present (the company mustering about 250), though during the last ten years or so death has sadly thinned the ranks of the "old hands," of the Electric, U.K. and Magnetic Companies. Amongst the men of science and others who attended were Capt. Fonseca Varz, Mr. S. W. Silver, Dr. J. H. Gladstone, Mr. C. B. Bruce, the Marquis of Tweeddale, Mr. William Crookes, F.R.S., Mr. Edward Graves, Prof. W. G. Adams, M. Cael, Mr. Jacob Brett, Mr. H. Weaver, Mr. John Pender, Mr. C. H. B. Patey, Mr. G. Shaw-Lefevre, M.P., Lord Onslow, Prof. Stokes, Sir Lyon Playfair, M.P., Sir William Thomson, Sir Frederick Goldsmid, Sir Frederick Abel, Sir Douglas Galton, Mr. J. C. MacDonald, Mr. Edwin Clark, Sir David Salomons, Sir George Elliott, Colonel Andrews, R.A.,[1] Mr. Matthew Gray, Sir James Anderson, Mr. Norman Lockyer, Mr. H. C. Fischer, Prof. Hughes, Mr. W. H. Preece, Sir C. Bright, Major-General Webber, Mr. C. E. Spagnoletti, and Mr. Latimer Clark. Letters expressing regret at non-attendance were received from the Marquis of Salisbury, Lord John Manners, Viscount Cross, Mr. W. H. Smith, Sir H. Holland, Sir W. Grove, Sir D. Gooch, Sir A. Borthwick, M.P., Dr. von Stephan (Berlin), Dr. William Siemens (Berlin), Mr. Cracknell (Sydney), Mr. C. Todd (Adelaide), and others.

The Chairman having proposed the usual loyal toasts, proposed the toast, "To the Memory of the Pioneers of Telegraphy," asking the company to join in an expression of

reverence for those great and illustrious men who have ceased to be among us, by drinking the same in solemn silence.

The Chairman then said :—We have most of us perhaps read of that tumultuous sensation which the great Wheatstone confesses to have experienced when the message which he sent on that little journey from Euston Square to Camden Town was sent back to him by Mr. Cooke. I am perhaps not exaggerating the importance of that occasion when I venture to say that that evening as Wheatstone sat in the small cupboard of an office communicating with his colleague at a distance of some two miles, was one of the great epochs in the history of human progress; and if ever a spirit of prophecy has filled a man with something of a divine enthusiasm, it may well be that the man with whose name the system of the electric telegraph must ever be inseparably connected, may have felt his heart throb with something almost supernatural when he realized that the great work had been achieved, that the demonstration had been reached, and that the future of the science was assured. I venture to believe that when we look back upon the progress of those fifty years, we shall find in them the materials for a greater hope of the future of humanity than in almost any other record of any other period in the history of our race. I would remind you that the instrument which was employed by Wheatstone and Cooke displayed five needles, and that it was from the movements and combinations of those five needles that the whole of the alphabet was made up. Those five needles, we are told, were united by means of five copper wires laid in a groove in a triangular block of wood, and I am sure you will be interested to know that a piece of that block is in my hand at the present time, and well deserves to be preserved among the archives of science. Well of course we are with the experience of this half century well aware that this system in the first instance was crude and imperfect. Demonstrations had been arrived at, but perfection had to be reached. The difficulty that was immediately encountered in popularizing the system was obviated by the development of the railway enterprise of this country and the necessity which arose for rapid and certain communication along our lines of railway. However, some time elapsed before the real development of telegraphy in this country began. The London and Blackwall Railway was, I believe, the first to utilize the system in a practical way. In 1844 the Government of Sir Robert Peel were the first to realize how far the telegraph might be applied to the service of the State; and that year saw the establishment of a telegraph line from Waterloo to Gosport, and that I think you may say constituted the first public recognition of the value of the electric telegraph. In 1846 the first telegraph company was formed—the Electric Telegraph Company. In 1850 the first attempt was made to lay a submarine telegraph cable. A gutta-percha wire, without any metallic covering, was laid between Dover and Calais in August of that year, and you will be interested to know that I have also here a portion of that cable, which was fished up by a ship in the Channel so recently as the year 1875. In 1851 a cable was laid in substitution of this gutta-percha cable, which was protected by iron wires, and which was the commencement of a regular system of inter-communication between England and the Continent, and this cable I believe I am not wrong in associating with the name of one of those gentlemen who is happily still spared to be among us—I mean Mr. Crampton—and it must indeed be a great satisfaction to anyone who has been connected with great works of this sort to have lived, as Mr. Crampton has done, to witness their enormous development in the service of mankind. The first Atlantic cable was laid in 1858, and other companies arose during these years to compete with the first electric telegraph company, and multiplied throughout the length and breadth of England the agency of the telegraph. In 1870 the multiplication of the companies had become so great that their competition, though in some respects advantageous to the public, was yet so imperfectly regulated by State requirements, that the Government of the day determined to acquire the whole of their enterprises, and to place the telegraphs of the Kingdom under the direction of the Post Office. Now, I should like to say one or two words with regard to the instruments of telegraphy. We are aware that the first telegraphic apparatus employed by Wheatstone and Cooke was one which required five wires through which to transmit their message. It was found gradually that two wires would suffice to forward a message, and after that the progress of science enabled the operators to depend upon one. But after a time it became ascertained that a wire could be used for sending messages in two directions, and after that time four messages came to be transmitted on a single wire, two in either direction; and, as I dare say most gentlemen who are present to-night are aware, at the present time a single wire is being used at the central telegraph station in such a manner as to admit of six messages being sent in one direction, or one in one direction and five in the other, or any other combination of six messages. The first five-needle instrument was succeeded by the double needle, and the double needle by the single needle; all those systems were visual. Then came in the system of printing on a band of paper. At first the signs representing the letters were embossed on the band. This was further improved by Prof. Hughes's printing instrument, by which the actual letters were printed in ink. Then came yet another—the sounder instrument, by which messages are transmitted by sound without any record. With regard to speed, when the first electric telegraph was established the speed of transmission was from four to five words a minute on the five-needle instruments. In 1849 the average rate of transmission of a certain number of messages addressed to the *Times* newspaper was 17 words a minute. The present pace of the electric telegraph between London and Dublin, where the Wheatstone automatic instrument is employed, amounts to 462 words a minute, and thus what was regarded as miraculous fifty years ago, has multiplied a hundredfold in the course of one half century. Now you may perhaps like to know, though it is rather descending from the higher walk of this great subject, the number of telegrams which were sent through the Post Office in the United Kingdom last year. The number was 51,500,000; that is nearly 1,000,000 per week, and that number is still steadily increasing. 41,000,000, or rather more than that number, of these were inland messages, and of course a very great proportion of them were Press messages. I think you should realize the immense boon which the electric telegraph has bestowed upon the Press. I gather from such information as I have been able to obtain that the rate for Press messages, which as everybody is aware is very much less than the rate for other messages, is on the average not much more than 2*d.* per 100 words; and it is owing to these extraordinary facilities, afforded by the Post Office to the transmission of Press news, that the whole of the United Kingdom is put in possession at its breakfast table every morning of everything which it is necessary or important for anybody to know, as well as of a great many things which are neither necessary nor important. I believe that I am not wrong in saying that the cost to the public revenue of this reduced rate to the Press is not less than £200,000 a year, and that the newspapers of this country practically receive a subsidy of £200,000 a year in order to enable them to assist in the diffusion of intelligence. I imagine that the country is well satisfied that this should be so, and that there are very few people who would wish to abridge that privilege, having regard to the enormous importance to all classes of the community of being placed at the earliest moment in possession of the fullest knowledge of what is going on. But it is a fact that, owing to the recent reduction in the tariff of telegrams, the value of the telegram on the average to the State is now only 8*d.*, whereas two years ago it was 1*s.* 1*d.*; and before the State took over the telegraphs it amounted to as much as 2*s.* 2*d.* I think you may measure something of the enormous gain which the public has achieved by the acquisition by the State of the telegraph system when you look at these figures and reflect that the average price of a telegram at the present time is only about a third of what it was only seventeen years ago. I am saying this as if I were one of the public; but as Postmaster-General you must be aware that I have to regard this result with somewhat mixed feelings, and I am endeavouring, as far as I can, to denude myself of any official prepossession in putting before you from the popular side the advantages which you have obtained by the State employment of the telegraphs. I would add that if you would wish to obtain further knowledge of this most interesting subject, put in the most terse and pregnant way, you cannot do better than study a paper communicated to the Society of Arts by my friend Mr. Preece. The great agency of telegraphy which seems to form the vital principle of this planet upon which we live, this great agency which has not only gone so far towards annihilating space, but which seems at the present time to be regenerating light and revolutionizing motion, has all the future before it. Those who are enrolled in its service are probably the disciples and the apostles of a new and absolutely beneficial dispensation, and with them rests the future, in no small degree, of the human race, and the means of linking not merely ourselves to our distant colonies—and my noble friend who is beside me (Lord Onslow)

reminds me how important is the connexion between England and the younger Englands beyond the sea—but by going forward in connecting the various races of mankind by binding island to island and continent to continent. The telegraph is doing in its own quiet, its own noiseless, and its own unobtrusive way, more than all the noisiest missionaries of peace and universal brotherhood have ever accomplished.

Mr. Edwin Clark, in acknowledging the toast on behalf of "The Past," described the origin of the Electric Telegraph Company, in the organization of which he took a prominent part, the difficulties he had to encounter in curtailing expenditure, and in putting into a thoroughly sound state the wires and the system of insulation which then prevailed. He pointed out that the railway companies were really in its early stages the greatest benefactors of the telegraph. He had been deputed to examine the telegraph system prevalent in Europe in those early years in connexion with the railways, and he had recommended what had now become universal on the railway system of this country—the block system.

Mr. John Pender, on behalf of "The Present," said:—My mission to-night is to tell you what submarine telegraphy has done. I am one of those few commercial men who at an early period of telegraphy saw that there was in it the promise of a beneficent instrument for the future progress of the world. I have gone into submarine telegraphy, not as an expert, but as one of those who have come forward and taken science by the hand, and led it up to the glorious results which we have seen. Twenty years ago there was only something like 2000 miles of submarine cables; now there are 115,000 miles; and it has cost something like £38,000,000 or £39,000,000 sterling to put that amount of telegraph cable to the bottom of the sea. There was a prophecy long ago that the earth was to be girdled round in forty minutes. Why, we have got as much submarine telegraph cable as will go round the world five times, and we can send a message round the world in twenty minutes at the present moment. You ask me where does all this submarine telegraphy extend, and I reply in those beautiful lines of the poet:—

> "Far as the breeze can bear the billows' foam
> Survey our Empire, and behold our home!"

Wherever the British ship goes, or the British flag flies, there we have the submarine telegraph; and at the present moment, while I am speaking to you, human thought is travelling like lightning to every part of the world. The future of that no man can tell. Of the 100,000,000 words which are now carried by submarine telegraphy, nine-tenths are for commercial purposes. When the history of these fifty years of Her Majesty's glorious reign is written, telegraphy, and more especially submarine telegraphy, will be shown to have done more than anything else to federate the great colonies with the mother country, to spread civilization throughout the world, and to make this great world of ours as near as possible one common country.

Sir William Thomson (who was warmly received) said:—I feel that when the telegraph has been so important a bond for all the nations of the world we ought to go even beyond our fifty years jubilee and think for a moment of the great names from other countries to whom the possibility of the jubilee of the electric telegraph has been due—the great apostles of electric science in France, Coulomb and Ampère,—Ampère, whose work and whose discoveries constitute the foundation of the most important of modern telegraphic and electrical instruments generally; Ampère, whose name has become Anglicized and is invariably used in measuring the currents which produce the electric light. Then Gauss and Weber, who made the first electric telegraph. The telegraph of Gauss and Weber, and the Munich telegraph of Steinheil, and the Steinheil key, which is the manipulating telegraph key of the present day—those were the elements of telegraphy. We justly rejoice that in England so much was made of the work of those grand pioneers in science. In America the race of practical work commenced almost simultaneously with our own in the splendid telegraph of Morse. In speaking of the telegraph we almost forget time and space, and I must go back to the previous work of Henry, who anticipated in some points some of the finest discoveries of Faraday, and laid a large part of the theory of current induction, which is at the very root of some of the most splendid realizations of modern electric science, not merely for the electric telegraph, but for electric lighting. By the work of 1857—a few years before the half of the jubilee—the two brothers, Edward and Charles Bright, and Whitehouse, those three men, with Mr. Cyrus Field, reduced to practice that brilliant dream of Cyrus Field to connect England and America by means of submarine telegraphy. Then there were the brothers Werner and William Siemens working in the same direction, and the great navigator Moriarty, who was out in the *Agamemnon* in 1857, and navigated the *Agamemnon* in 1868, and was on the *Great Eastern* as navigator with Sir James Anderson. In 1865 he picked up the cable where it was broken, and in 1866, coming back a year after to the same place, hit upon it just a quarter of a mile away, by his splendid navigational powers. Canning and Clifford were also engaged in the work; then there were Varley and Jenkin (who was my special partner) with both of whom I worked for many years. I alone am here to speak for the three. Willoughby Smith, who did such fine work in 1865–66 in testing the cable, applying the newest developments of science, many of them his own inventions, to do what had never been done before, to test a submarine cable with the delicacy that was necessary under circumstances so peculiar, so utterly new. I am exceedingly sorry he is not with us this evening. But I can never forget that we scientists alone could not have done what has been done. To two men, I believe, is due the existence of the 1865 cable, and all the consequences that followed from the 1865–66 cable—John Pender and Cyrus Field. But I must remember that there are other things besides ocean telegraphs. You have told us how splendidly the land telegraphs are worked; you have pointed out how admirably under the influence of the Government system, the application of science to telegraphy has been developed. I think you may feel proud, sir, in knowing that under Government management within these last seventeen years the applications of science to telegraphy have not stood still, but, on the contrary, have been pushed forward with every possible energy and with the most marvellous success. You have told us that the rate of working between Dublin and London has reached 462 words per minute, I think we may say 500 words per minute, and that is ten times what it was ten years ago. That is something for a Government department to be proud of, and for a Government I must say there is some little political importance in the fact that Dublin can now communicate its requests, its complaints, and its gratitudes, to London at the rate of 500 words per minute. It seems to me an ample demonstration of the utter scientific absurdity of any sentimental need for separate Parliaments in Ireland. I should have failed in my duty in speaking for science if I had omitted to point out this, which seems to me a great contribution of science to the political welfare of the world.

Sir Lyon Playfair, M.P., proposed "The Scientific Societies." The scientific Societies, he pointed out, did not profess invention; they professed to lay down the laws of science and to advance natural knowledge. Men who had contributed to the advancement of natural knowledge, like Oersted and Ampère, were as much discoverers of the electric telegraph as if they had themselves actually aided in the invention. The duty of the scientific Societies was to cultivate the tree of knowledge. A great invention never came, as Minerva did, panoplied in complete armour from the brain of Jupiter. But even the brain of Jupiter could not produce this wonderful product of evolution until he had swallowed her mother, Metis, while in the first month of gestation. Our great inventors swallowed science, the mother of invention, and then produced their progeny, always, however, in a state of infancy. The steam-engine, which had done so much for human progress, has had so many inventors that a Court of law, reviewing the steps of invention, came to a solemn decision that Watt had done nothing for the improvement of the steam-engine. Scientific Societies, looking to the advancement of science for its own sake, laid the surest foundations for industrial applications. Oersted and Faraday were as true discoverers of the electric telegraph as Wheatstone, Cooke, or Morse. It was not the annihilation of space and time which was the most wonderful result of the telegraph, but it was the profound change which it had produced in our systems of government and of commerce. Who at its introduction would have supposed that the whole system of commerce would have been transformed by it, that capital would have to change the channels of its usefulness, and that the system of exchanges would undergo such profound alterations? If telegraphy had one lesson which we should lay to heart it was this—that science should be studied for the sake of knowledge, because discoveries in natural knowledge led inevitably to industrial inventions. We should, in regard to all discoveries, however unimportant they seemed, do everything in our power to pro-

mote their growth, and the growth of natural knowledge throughout the world.

Prof. Stokes said :—Scientific men know well how fascinating is the pursuit of science. Some have even gone so far as to consider that it would be polluted, if I may so speak, by being applied practically. An eminent foreign mathematician delighted in the theory of numbers because one could not conceive that it could have any practical application. An eminent English mathematician I heard express a somewhat similar sentiment. All honour be to those who are so immersed, if I may so speak, in abstract science, that they disregard and even dislike its application. They pursue science with all the more zest, they pursue it in directions which possibly otherwise might not have been followed out, and possibly in the end their own investigations may admit of applications which they never dreamt of. But for my own part my tastes do not quite lie in that direction. I like to see science connected with applications thereof, no matter to what purpose. Now, when we apply abstract science to physical subjects, we are not only enabled to investigate natural phenomena in a manner which could not otherwise be done, but the study reacts on the most abstract parts of science, and enables us sometimes to see, as if it were by intuition, truths of an abstract nature, such as, for example, propositions in pure mathematics, which we perhaps should never have arrived at if we had not viewed them through the spectacles, so to speak, of their physical application. But this is not all. When science comes to be applied to the wants of life, scientific men are placed by the practical man in the condition of making experiments which oftentimes would otherwise be impossible. When science comes to be applied to commercial purposes, it then becomes possible to construct instruments on a scale the expensiveness of which would have been utterly prohibitory to the purely scientific man. But when these instruments are constructed, it may be, for commercial purposes, the scientific man on his part is able by the favour of those who have constructed them, or of those for whom they have been constructed, to make experiments with them which oftentimes are of great interest from a purely scientific point of view.

Dr. Gladstone, responding on behalf of the "Royal Institution, said :—At the Institution there were not merely memories binding them to all those who had passed away, but they had also many relics. They preserved the log-books of Davy, Faraday, and others, and not only that, but there a great number of pieces of wire, sealing-wax, and card, all damaged, and many other things which Faraday especially used to delight to work with and to carry out in the first experiments which were suggested by the ideas that were working in his brain, and these were preserved as germs of some of their great discoveries. And here he wanted to point out one of those germs connected with telegraphy. Those relics preserved at the Royal Institution were only worth originally a few pence or shillings. How different the monster wealth which had now become the capital of those great enterprises! As far as the Royal Institution was concerned, its connexion with the electric telegraph was to a certain extent not direct, and yet it was very real. Sir Humphry Davy worked there of course largely on galvanic electricity, but he belonged to the pre-telegraphic era. Faraday himself commenced to work early on these matters, and continued to try and image in his own mind what was taking place in these phenomena. It was an important point with him that the glories of science should conduce to the benefit of man. They knew his influence with Sir Charles Wheatstone was very great, and he got him into the work chambers at the Royal Institution and talked to him about his investigations, and in the theatre brought before him some of those experiments which were afterwards performed with so much success in public. In one way and another Faraday had to do with the industrial applications of electricity, as well as with scientific discovery. They had the combination of the purely scientific man with the practical man, and then the two going together with slow, careful, conscientious investigation, followed by the energetic carrying out of the discovery in a form which benefited mankind.

Mr. Shaw-Lefevre, M.P., in proposing the "Societies representing Applied Science," said :—When I was a boy, at Eton, I recollect well the extension of the telegraph from London to Slough, and an incident of which you are all aware, the arrest of Tawell, which I believe brought the telegraph more into notice than anything else at that time. It might have been expected that the authorities of Eton, seeing a great invention of this kind brought to their door, might have been desirous of explaining it to the boys of the school, and might have been drawn out of their deep slumber of ages and done something for the scientific education of the boys then at Eton. But no thought of the kind ever entered into their minds, and the only notice taken of it at the time was that they set it as the subject for Greek verses. I and all the boys of my class commenced racking our brains to write some Greek iambics upon a subject of which we knew nothing, and to bring it into connexion with the mythology of the ancients, of which we knew a good deal. I refer to this for the purpose of showing you how little science was promoted then at our public schools. I am glad to say that things have been changed since then, but much has to be done in this direction, and there cannot be a doubt that if this country is to hold its own in the great industrial competition it must give a greater place to science in our schools, and equalize the endowments between the classical and scientific studies; and it is only by doing that I am persuaded that we can hold our own.

Mr. Bruce, President of the Institute of Civil Engineers, and Mr. Latimer Clark, past President of the Society of Telegraph Engineers, replied; and the latter, after alluding to the work done by the brothers Brett in submarine telegraphy, said :—I feel that we, as the representatives of applied science, have been somewhat neglected by the world. I feel that the politicians, some of whom have honoured us by coming here this evening, have very much neglected us. I don't allude to honours, for we shall be very well content with the position of things in that regard; but I feel that they have robbed us of much of our credit for the fact that the great effects of the jubilee which we are now assembled to commemorate have been due to the agency of the applied sciences. I do feel that politicians have been permitted to claim for themselves the credit for the wondrous benefits civilization has received from the efforts of applied science. We hear that so many shillings have been taken off a quarter of wheat, we hear that all the prosperity of the country has been due to free trade, but I say it is not so; I say they have robbed us of our honours in saying that; I say as guild and craft we ought to proclaim loudly to the world that to our efforts most of all the prosperity of the last fifty years has been due. The population of this great city and of this country when it eats its breakfast to-morrow morning will be consuming food a very large proportion of which has been brought to this country by means of applied science. It is that which has given us our prosperity. They have not taken 5*s.* or 10*s.* a quarter off wheat and corn and eatables, but they have taken off 60*s.*, 80*s.*, and 100*s.* Wheat will be brought to-morrow from places from which it could not have been brought fifty years ago for ten times what it now costs. As a guild and craft we ought to proclaim loudly that the benefits which we have conferred are the real cause of the prosperity of the great Victorian era which we meet here to celebrate.

The Earl of Onslow having proposed the health of the Chairman in a suitable address, and the Chairman having responded, the proceedings terminated.

The Postmaster-General, during the evening, despatched the following telegram to Sir Henry Ponsonby, at Osborne :—"A large dinner-party celebrating the jubilee of the electric telegraph, remember with gratitude and pride that all the progress has taken place in the happy and prosperous reign of Her Majesty and under her fostering care."

To this the following reply was received :—"The Queen thanks you for your telegram. It gives Her Majesty much pleasure to reflect on the improvements which have been made in Wheatstone's great invention, which was first practically tested in her reign."

THE CASE FOR A LONDON TEACHING UNIVERSITY.

THE questions connected with the proposal for the establishment of a Teaching University in London were discussed in a speech delivered by Sir George Young at the distribution of prizes in the Medical Faculty of University College, London, on June 1, and in a speech delivered by Dr. J. E. Erichsen at the distribution of prizes in the Faculties of Arts and Laws and of Science, at the same institution on June 30. As the case for a London Teaching University was stated by these two eminent authorities, we reprint part of what they had to say on the subject from the point of view of University and King's Colleges.

I.

Having referred to the drawbacks connected with "the system of separation between institution and institution concerning University matters" in London, Sir George Young went on to say:—

I will touch upon some of these drawbacks—drawbacks which, as I have said, I do not impute as matters of fault to any man or to any set of men, but to the mischiefs of the system; and I will draw my instances (and you are to consider that I could give you many others), as in duty bound upon this occasion, from the medical side of the question.

Well, gentlemen, in the first place we are brought face to face with a very serious and very unpleasant condition of things in the fact that several of our students, we find, are in the habit of leaving us from time to time in order to finish their course of study at other institutions where degrees are conferred, in order to qualify for those degrees. We have always been, as Broke said of the *Shannon*, "an unassuming ship," and I am not going to boast. Let us admit that there may be elsewhere teachers as eminent as those I see around me. Let us admit that there may be elsewhere possibilities of study comparable to those which are to be obtained in this place. But I will not admit—it is my duty to deny, and the point is conceded by others outside our limits—that there is anywhere a more eminent body of professors and instructors than that which has now, for two generations, led the van in matters of medical instruction of a university kind in this College. I will not admit that there is anywhere, in any part of the world, a field of study presenting greater opportunities to the student than that of London with its numerous medical schools, and with its numerous facilities for scientific study.

Next let me mention an evil, for which the University of London is not responsible—for which nothing, indeed, can be said to be responsible except the non-existence of that university which the University of London is not. Not only are medical schools, as we know well, dependent upon hospitals, but also, what is not so generally known to the public, hospitals are dependent for their administration upon medical schools. As London has spread and as one general hospital after another has been founded, each has attached to itself its own separate medical school. Each school must provide, in order to satisfy professional requirements, not merely that clinical teaching for the sake of which it is founded, but also scientific teaching of a multifarious and expensive character. In some of these schools, as is well known, it has been found impossible to provide this scientific teaching in a manner sufficiently effective for the purposes of the school. There need be no delicacy, gentlemen, in mentioning this, because, in fact, it has been most honourably acknowledged by several of these medical schools in their recent action. It was lately brought to our notice that in the case of several of them, they have practically, in some branches, given up the scientific training of their students, and have entered into an arrangement with the Government school at Kensington, by which their students should there receive that teaching which they found themselves unable to give. Well, gentlemen, at the Council of this College we had something to say—we had some objection to take—to that arrangement. With that I need not trouble you further than to say that we thought a Government department ought not to lend itself to an exclusive arrangement of this kind. We thought that it would have been better for the students themselves and for the public if the matter had been left open whether they should go to South Kensington for their chemistry and physiology, or come, if they so preferred it, to ourselves. But at the same time, gentlemen, you must not consider me in this matter to be impeaching the conduct of the other schools. As men of the world, we are quite aware that medical schools are to a certain extent rivals, and we cannot expect, merely because we asked it, that the natural jealousies of rivals should be allayed, and that a medical school in so delicate a matter should freely accept our offers of instruction for its students who, they might suppose, would perhaps be detached from their affection for their own school by frequenting this place. Well, gentlemen, what is the remedy that we should look to? I think that you will agree that we ought not, as a Council, to sit down and seek no remedy for such a state of things as this. Why, surely the remedy is that some central authority should be provided—some institution where we can meet our sister schools upon equal terms, not that wholesome emulations should be extinguished, but that the mischiefs which arise from their excess should be obviated, where, in fact, teachers and administrators might meet together for the purpose of arranging for improvements in medical education upon a common footing and without fear of mutual injury.

This, among other instances of the same kind, many of which I could give you, led some of us, as much as three years ago, into a long inquiry into the matter, and eventually into a movement for the foundation of a Teaching University in and for London. The year before last, at the meeting of the sister Faculties, the Dean of the Faculty of Science, Prof. Graham (whom, I hope, we shall see before long among us restored to health), called our attention to the movement and expressed his sympathy with it on the part of the Faculty. The President of the College, Lord Kimberley, expressed also his warm sympathy with the movement, and said (I am quoting his words, which will be found in the Report of the College of that date):—"There is nothing more dissatisfying to the minds of students and of educational men, than that in this great city there should not be some more complete establishment of some universal system. We may not see it accomplished. I do not suppose that anyone sees at present how the end is to be attained, but I am quite certain that it would be for the benefit of all the institutions of this great city that they should be gathered together and the teachers and managers brought into a close and immediate contact." With that encouragement we, many of us, took up the movement warmly, and it has now been brought to the practical stage of definite proposals and of a formal programme. We ask, in short, that the same privilege which has already been conceded to country colleges through the Victoria University shall be conceded also to us in London. Gentlemen, we cannot go to Manchester. We cannot so far ignore our position and our history as to seek for admittance from the offspring of our offspring. Besides, we ought not to be compelled to go to Manchester. The system of the Victoria University, as I have indicated, is that of an imperfect university, arising from its being scattered over several cities at great distances from each other. There is within our reach the more complete system of a localized university; for who will have the face to say that in this great population of something like four millions residing within limits admitting of daily intercourse there is not material enough—there is not ground enough—to support a local teaching university of its own?

There were working with us for a long time, in the course of this inquiry, several active members of the Convocation of the University who had themselves been interested in similar movements, and who desired to see the development of that University in the direction to which our hopes and wishes also pointed. By their exertions, and as a consequence of our movement, the Convocation and the Senate of the University of London have been brought separately to consider this matter, and have put forward from time to time certain proposals for what I must call a compromise. Those proposals have been officially communicated to us through our President, and have been, I need not say, carefully considered by the Council. They do not amount to that which we desire. They did not cover that which we claim. They are limited to this: in the first place, that there should be introduced in the Senate of the London University eight representatives of the four Faculties of the University—two to each—such representatives to be chosen by assemblies of the Faculties, and the assemblies of the Faculties to consist of representatives of the institutions throughout England which send up candidates to the University. There is also a provision for Boards of Studies to be constituted out of these same Faculties to exercise purely consultative and deliberative functions. I think that is all—all, that is to say, of a practical character. There is no doubt—and it is an important matter considered from the point of view of our argument—a proposal that in the preamble, in the objects of the University, there should be included a statement, that its purpose is to encourage education, especially in London; and there is also a proposal that as time and opportunity are given, it shall have power to found Professorships, provided that such Professorships shall not compete unfairly with our own. But, passing over these two latter proposals as not exactly of a practical character, I say that the proposal to admit eight gentlemen, representatives of assemblies the constitution of which is not very clear to me, upon the Senate of the University, and to appoint these Boards of Studies composed of representatives so gathered from all parts of England, is not what we want. It does not meet our views; and for several reasons.

In the first place, the representation which would be given to this College on such a system in the governing body of the

University, if it is to be a teaching university and not merely an examining one, is entirely insufficient. In the next place, the Boards of Studies, the Faculties, and the new members of the governing body, would represent, not London, but all England, and therefore would constitute an organization entirely different from that effective organization which we contemplate, which is to meet often, to take count of teaching-matters in London, and to do a great deal of work in the development of University education in this place. We therefore are obliged to reject these proposals, and to proceed, without any feelings of hostility and without any bitter words towards the University of London, to ask for that which we consider necessary for the effective carrying on of our own work.

Let us keep clearly before us what a teaching university really is. We need not go farther than this institution to see it, provided only that were added to us which we want—the power to confer degrees. It has not merely to provide for—it has to commend to its students the best possible teaching under the best teachers obtainable in all the subjects of the University. A university which by its very constitution is indifferent to methods of teaching and does not care how a man has obtained his knowledge, cannot be said to commend to its students any particular methods or ways of obtaining that knowledge. It rather has a contrary effect. Under these circumstances it is useless to try to set up an institution which shall combine a mixture of two principles—the principle which considers degrees merely as the marks of a liberal education, tested, no doubt, by an examination, but covering very much more than the mere showing of knowledge in examination, and the other, the rival system, which, giving up the testing of methods of education—giving up the marking of a regular education as beyond its scope—confines itself to the setting of a mark upon performance in the answering of examination-papers.

No, gentlemen, if the University of London were to move in this direction, it would spoil its own thoroughly good and honest work without doing ours. There will still remain when we have obtained this Charter, plenty of candidates for its degrees—plenty of work for it to do—plenty of honour to those who obtain them. But surely there is room for us by the side of it. There is room for an institution which shall comprise not merely this College, but King's College and the Medical Schools of London, and which, organizing them together as a Teaching University, shall give us that which we want for the efficiency of our work—an institution in which the teaching which we give is duly honoured—is not placed in an inferior position beside the teaching which is given by other universities and in other university colleges.

We ask, therefore, that a Charter to confer degrees upon all persons who have undergone a regular course of study in a college or medical school of the University and have passed the required examinations, shall be granted to a suitable governing body, upon which the governing bodies of this College and of King's College shall be properly represented, and upon which the teaching staff not merely of this College and of King's College, but also of the other Medical Schools of London, shall have their representatives. In order that the interests of the medical profession may be properly considered—in order that we may not seem to claim that which it is by no means our intention to claim—an unfair position for our own medical schools, we ask an alliance with the Royal College of Physicians and the Royal College of Surgeons, the official representatives of the medical profession in London, in order that by their means that representation may be secured upon the governing body of the University. It is obvious that from their number it would be difficult to represent directly the separate interests of eleven medical schools upon the governing body of a University; but, in so far as the teachers of the various schools have their voice—and that voice, I can assure you, we do not intend should be a small one, in the councils of the University—in so far, we shall consider the teachers of other medical schools entitled to rank on equal terms with our own. . . .

There is a movement at present on foot in the College of Physicians for a single-facultied University in London, or an institution in the nature of a University, for conferring medical degrees alone. That movement appears to us to be a part of our movement. By itself, and if the movers insist upon its being considered as essentially a separate movement, we could not look upon it with approval; for we believe that it would be fatal to the prosperity of our medical school. I will put it to you, gentlemen, How would you, the students in this College, regard a state of things under which you were called upon to work for a degree, either at the University of London at Burlington House, or at the Royal College of Physicians? If it were the case, as seems to be indicated, that the degree at Burlington House is to be connected with a very high, a somewhat unusually high, standard, and if the degree which is contemplated by the College of Physicians is to be that creditable average degree which I have indicated as one which, personally, I think ought to be established, do you not see that those medical schools, which like our own, aim at the highest teaching, would have serious difficulties in the matter? Here would be two systems in neither of which we had the least voice, two systems of examining Universities outside us competing for our students, and what would our Professors do? They would be called upon, now to train for one system and now for another, and perhaps to keep up double sets of classes, so constituted as to fit the arrangements of two rival bodies.

That is the position in which we should be placed. But if the movement on the part of the Royal College of Physicians (the Royal College of Surgeons joining in it) can be brought into accord with our own, then we shall have already obtained a part of what we seek. I will just mention one reason why I think it most desirable that you, the members of the medical profession, should take this matter seriously into consideration, and should exercise your influence with your colleagues in other institutions, in order that this point may be pressed home to them at the present stage. The visit which it was recently my duty to pay to the Privy Council Office, in order to obtain the forms necessary for carrying out our own proposals, revealed to me the fact that there exists already in that office a pile of petitions as high as this table against the proposal of the Royal College of Physicians. Now, gentlemen, against our proposals there is no petition and there is no movement. So far as I know, there is no objection in the world.

We do not conceal from ourselves that it is possible opposition may be offered as we go on. That opposition which above all others we should deprecate would be the opposition of the University of London. I have endeavoured to preserve a tone of friendship, such as I sincerely feel, towards that University. I most earnestly deprecate opposition on the part of that distinguished body to the movement which is now on foot for obtaining a University in and for London such as London ought to have. I trust it will not be led into the fatal track of the older Universities, which, by their interference, did not prevent, indeed, the foundation of the University of London, but undoubtedly spoiled it, fifty years ago. That such opposition may be apprehended by some of us we cannot ignore in consideration of the very serious matter to which I have lastly to call attention, the resignation, namely, of our President and of several members of the Council among us. Gentlemen, that these resignations have been to some extent a surprise to us, that they have been a serious cause of anxiety to us, must be obvious; but I think that they have been partly due to a misunderstanding of our aims. I think that the objections which have led to them will, to a large extent, vanish when our proposals come to be more carefully looked into. In the meantime, as for us who remain, we are not disheartened, we are not discouraged. We have at least the satisfaction, such as it is, that the Council of this College is now unanimous in the matter. We have the source of satisfaction which is afforded to us by the unanimous support of the general meeting of the College. We have at our backs the unanimous support of our distinguished body of Professors. We have at our side the unanimous assistance of the great College once our rival, but now our cordial ally. Besides King's College we have friends in every medical school in London, who are corresponding with us and working in the same direction as ourselves. We have friends and well-wishers, I may say further, in every University in England. We have friends in the Press, and we have supporters in the public, and we have received the most encouraging intimations that it will not be long before we are able to fill our depleted ranks in the Council with names which will inspire confidence, and which will materially assist us in carrying our work to a conclusion.

Finally, gentlemen, we have this more than any other as a source of encouragement—that we see our way—that we know the work that has to be done and realize the way in which we hope to effect it. Three years of study and perhaps scores of meetings and conferences have not left us entirely ignorant of the ground. We intend to make this institution greater, more splendid, more efficient, than it has been hitherto, and we expect

to succeed, because we are labouring not merely for our own aggrandisement, but for the foundation of a University in and for London which will be of incalculable benefit to University education in this mighty centre of population where we live.

II.

Dealing with the objection that a new University cannot be necessary in the capital, since we have already got the University of London, Dr. Erichsen said :—

I wish to speak with the very greatest respect of the University of London, and I entertain the highest respect for the work that has been done in that great institution during the half century that it has been in existence ; and I think that everyone connected with University College must always speak and think of the University of London with that affection with which a parent looks at his child, the University of London being the outcome of University College. We may sometimes look upon it with that feeling of mixed affection and regret with which we contemplate a child that we think has not always been so grateful as it might have been for the favours received in its early life. But, however that may be, we all speak of the University of London with, and we all feel towards it, the greatest respect and a certain affection.

But the University of London is, in truth, not a university in any sense of the term. The title is misleading and is a misnomer. By a "university" is meant an association of teachers and of students, properly organized, destined for the increase and the transmission of all learning, of knowledge in all its branches, and containing complete Faculties of Arts and Laws, Science and Medicine, and empowered to grant degrees to those of its pupils who are found to be sufficiently qualified for such a distinction. The University of London never has pretended to be a teaching institution, and, so far as its present constitution is concerned, never can be a teaching institution. If it were to become a teaching university it would require to become so completely altered in its constitution as practically to become a new institution. The University of London has only performed one of the functions of a university—that of examining candidates for its degrees. It has performed that function admirably well. The examinations have been carried to a very high standard, so much so that the ordinary pass-examination in some subjects is almost an honours examination. Yet it is only a degree-giving institution, and not a university in the sense in which a university is generally known.

Nor is it "of London" ; for, as was truly said by the Dean of the Faculty of Science in that admirable report that we listened to at the commencement of these proceedings, it is an Imperial University, which draws its candidates from almost every part of the habitable world. It has nothing to do with London except that its head-quarters are situated in Burlington House ; but, so far as London is concerned, it might just as well be situated anywhere else. The University of London, then, does not, in any way, supply the want that we wish to fill. With regard to London itself, I may say this, that even as an examining institution the University of London does not supply the desire that has sprung up of late years for academic distinctions. It does not supply the desire amongst the inhabitants of London itself. I can speak of my own profession. Of late years there has been a craving in the medical profession for the possession of degrees. As Sir George Young stated very truly in distributing the prizes in the Medical Faculty about a month ago, if there were no degrees at all we should be none the worse for it ; but one may also state something like the converse of that proposition, that if everybody has got a degree nobody is a bit the better for it, and what is common to all can be an honour to none. However, that there exists a great desire for degrees and for academic distinctions there can be no doubt. Well, do the students of the medical schools in London go to the University of London for those degrees ? Not at all. They go elsewhere. They go to Edinburgh ; they go to Cambridge ; they go to Oxford. At the present moment there are about nineteen hundred medical students at the University of Edinburgh, and nearly seven hundred of them are English. They are attracted there not so much by the superiority of teaching, because—and I say it with all respect to the University of Edinburgh, to which I have reason to be very grateful—the teaching, high as it is, and excellent as it is in all its departments, is not better than the teaching in four or five of the principal medical schools in London ; but the students go there simply in order to obtain a degree, because at the end of their studies, instead of coming out as simple Mr. So-and-So, they come out as Dr. So-and-So. Well, the others who do not go to Edinburgh, go to Cambridge or go to Oxford ; and there is a very large medical school now at Cambridge also, frequented by young men who are desirous of obtaining the degree of that distinguished University. The following incident will show how little the University of London supplies the need for degrees which is felt by London medical men. A few weeks ago there was a vacancy at one of the large hospitals of London for an assistant physician. There were no less than twelve or fourteen candidates. They were all graduates of British universities, and out of this large number of candidates, all London men, educated more or less in London, and practising in London, and attending hospitals in London, there was only one candidate who was a graduate of the University of London. All the others were graduates either of Oxford or of Cambridge. I say, therefore, that men go away from London to get their degrees at the present day. They go to Edinburgh, they go to Oxford, they go to Cambridge, they go elsewhere ; but the vast majority do not go to the University of London. That, as a degree-giving institution, does not supply the needs of London itself.

The proposed establishment of this new teaching and degree-giving university has been termed an act of hostility, a kind of declaration of war, against the University of London. Now I can say truly, speaking in the name of my fellow-members of the Council here, that there is no such feeling whatever. No such feeling has animated, I believe, any one of the Council or any person connected with this institution. This proposed university will compete, probably, to a certain extent, with the University of London, but it will compete much more with other universities. It will compete much more with the University of Edinburgh ; it will compete much more with the University of Cambridge. There is no direct competition intended with regard to the University of London. There is no reason why a new university should not be established. There is no more reason to complain of competition in the establishment of a new university than there is in the establishment of a new school. Every new school competes with every other school in existence. There is no more reason to complain of it than to complain of the introduction of a new member into any of the learned professions. Every man who becomes a lawyer or becomes a doctor may be said to compete with every existing lawyer or doctor. In the same sense the new university, if established, might be said to compete, more or less, with every existing university in the kingdom. In this case there is a competition of friendly rivalry, but nothing else ; and beyond that I cannot admit that there is any special competition with regard to any existing university.

If the University of London does not supply the want felt for higher education, how is that want to be supplied in London ? There are only one or two methods. You must either take existing institutions, or you must create a *tertium quid*, and what that *tertium quid* may be I know not. But what existing institutions are we do know, and we do know that there are two institutions in this metropolis which for the last half century have been doing the only work in London that approaches to the higher education, or approaches in any way whatever to university education. They have done that work diligently and well under great difficulties and great disadvantages, but with a fair share of success. I mean this College and King's College. Those are the two institutions ; and by the combination of those two institutions we may fairly look for the establishment of a new university in London fully capable of discharging the functions of such an institution.

I happen, from circumstances, to be personally acquainted with, I believe, every university in the kingdom ; and I can say that so far as the equipment of universities is concerned, in the way of museums, laboratories, libraries, lecture-rooms, and all other appliances—what may be termed the "plant" of a university—these two institutions taken singly are equal to most ; this one certainly is, and taken in conjunction they are superior to almost all, except the old Universities of Oxford and of Cambridge. I put them aside ; but these institutions, University College and King's College, taken in conjunction, are fully equal in all the requirements of a university to the other universities in Great Britain, the Scottish universities and the two universities of this country—one in the north and the other in the midland counties.

I cannot speak with any precision of detail of King's College, but I can speak with precision of this College ; and it may be interesting to you to know what this College really is, and what

it can present to the public in the way of supplying the requirements of a great teaching institution of university rank.

This College, in the first place, has complete Faculties of Arts, Laws, Science, and Medicine, and a School of Fine Arts, as well as a Boys' School. This College has fifty-eight professorial chairs in operation. In addition to the fifty-eight professors, there is a large teaching staff both on the general and on the medical side,—teachers, lecturers, demonstrators, and so on,—bringing up the whole members of the teaching staff to something like one hundred. Last session this College had between nineteen hundred and two thousand students. There were five hundred and fifty boys in the school. The buildings of this College, containing, as I have said, museums, libraries, lecture-halls, laboratories, and all the appliances of a university, are spread over seven acres. They cost £300,000 in construction. This College holds on trust no less a sum than £200,000, chiefly devoted to prizes, scholarships, and other objects of that kind; and it holds, besides, in trust a sum of £135,000 for hospital purposes. Its income is between £33,000 and £34,000 a year. Taking, therefore, this College alone, so far as its buildings, the contents of its buildings, and its pecuniary resources are concerned, it stands on an equal footing with several of the universities in Great Britain; and, taken in conjunction with King's College, it stands undoubtedly superior to some.

To this College, therefore, in combination with King's College, we may fairly look to the attainment of our object of establishing a Teaching University in London which will bring the higher education of London to the doors and within the pecuniary resources of the less wealthy classes of the metropolis, so that the disgrace that has hitherto attached to the metropolis of not affording a higher education, and the discredit that university education in England is to a very great extent a privilege of the wealthier and of the well-to do classes, may be wiped away. It should be within the reach of all, even of the student of the most humble means; and it would be well if this country were to take the example of Scotland in that respect, and to follow it. . . .

In this new Teaching University there are two requirements that we insist upon. One is that the candidates for its degrees should have spent a certain specified time in attendance on lectures and instruction within its walls; and the other is that the examinations should be superintended and conducted by the teaching body of the University.

With regard to the first of these two points I wish to say a few words. . . There is something more than mere knowledge that is acquired in academic instruction. There is a culture of mind and a development of the moral and social nature that cannot be acquired by solitary study; and it is for these reasons amongst others that those who are in favour of this movement are desirous that the candidates for the degrees of the new University should prosecute a portion, at all events, of their studies within the walls of the institution, so that they may imbibe something of the spirit, and that they may be in some way, too, impregnated by the *genius loci*. This has been stigmatized as retrograde; but surely there can be nothing retrograde in that which has been found by universal experience to be the better system of education, and which is adopted in every teaching university in the country.

There is another point, and that is in regard to examinations, and it is a very essential point. We feel, and we feel very strongly indeed, that the examinations should not be directed by an outside body on which there are perhaps no examiners and no teachers, but that the examinations should be conducted by the teachers themselves in the institution in which the candidate learns, I do not say by the individual teacher of each class, but by the general body of the teachers, and that is a very different thing. And, as there would be more Colleges than one in the new Teaching University, a candidate need not in any way be examined by his own teacher, although he would be examined under the direction, superintendence, and control of the general body of the teachers. In every university now, I believe, throughout the Kingdom the teachers are assisted in their examination by assessors or by extra-professorial aid, whenever it is needed, and such, of course, would be the case in the new University. We feel that examinations ought not to lead teaching, and that if examinations are allowed to lead teaching, the teaching is fettered by the examination, and you get to a system of "cram"; the higher education and the higher teaching are apt to be neglected. I recollect many years ago a circumstance illustrating this, occurring in this College in connexion with Prof. Sharpey, one of the most distinguished men ever connected with this College, the first Professor of Physiology here and, indeed, in London. There was no course, properly speaking, of Physiology given in London until Prof. Sharpey began his lectures here in the year 1836–37. Prof. Sharpey gave an elaborate course of Physiology. From the commencement he attracted crowds of students. At that time there was connected with this College a most estimable and most amiable and most excellent old surgeon, who had grown grey within the walls, as it were, of the unreformed College of Surgeons, Mr. Samuel Cooper. He was an examiner of the College of Surgeons, and I speak of him with the greatest respect; but he was never able to raise his mind beyond the requirements of the examinations of that institution. When he heard of what Prof. Sharpey was doing, he said, "What is the good of Sharpey teaching them all this kind of stuff? We do not want it at the College of Surgeons. We have never asked for it at the College of Surgeons. Why should he teach it to them?" He had no conception beyond that, and that is the frame of mind that affects every mere examiner. He has a tendency to fetter and tie down the teaching to the level of his own examinations, and it is impossible to bring him or an examining institution above that level. We therefore wish that the instruction should lead the examination, and that the examination should follow in the wake of the teaching, and not the teaching in the wake of the examination.

SCIENCE AND REVELATION.[1]

ON the present anniversary, which is the conclusion of my first year of office as President of this Institute, I propose to address a few words to you bearing on the object of the Institute, and on the spirit in which, as I conceive, that object is best carried out.

The highest aim of physical science is, as far as may be possible, to refer observed phenomena to their proximate causes. I by no means say that this is the immediate, or even necessarily the ultimate object of every physical investigation. Sometimes our object is to investigate facts, or to co-ordinate known facts, and endeavour to discover empirical laws. These are useful as far as they go, and *may* ultimately lead to the formation of theories which in the end so stand the test of what I may call cross-examination by Nature, that we become impressed with the conviction of their truth. Sometimes our object is the determination of numerical constants, with a view, it may be, to the practical application of science to the wants of life.

To illustrate what I am saying, allow me to refer to a very familiar example. From the earliest ages men must have observed the heavenly bodies. The great bulk of those brilliant points with which at night the sky is spangled when clouds permit of their being seen, retain the same relative positions night after night and year after year. But a few among them are seen to change their places relatively to the rest and to one another. The fact of this change is embodied in the very name, planet, by which these bodies are designated. I shall say nothing here about the establishment of the Copernican system: I shall assume that as known and admitted. The careful observations of astronomers on the apparent places, from time to time, of these wandering bodies among the fixed stars supplied us, in the first instance, with a wide basis of isolated facts. After a vast amount of labour, Kepler at last succeeded in discovering the three famous laws which go by his name. Here, then, we have the second stage; the vast assemblage of isolated facts are co-ordinated, and embraced in a few simple laws. As yet, however, we cannot say that the idea of causation has entered in. But now Newton arises, and shows that the very same property of matter which causes an apple to fall to the earth, which causes our own bodies to press on the earth on which we stand, suffices to account for those laws which Kepler discovered—nay, more, those laws themselves are only very approximately true; and, when we consider the places of the planets, at times separated by a considerable interval, we are obliged to suppose that the elements of their orbits have slowly undergone slight changes. But the simple law of universal gravitation, combined, of course, with the laws of motion, not only leads to Kepler's laws as a very close approximation to the actual motions, but also accounts for those slight changes which have just been mentioned as

[1] Presidential Address delivered by Prof. Stokes, P.R.S., at the annual meeting of the Victoria Institute, on Tuesday, July 19.

necessary to make Kepler's laws fit observation exactly. We are inevitably led to regard the attraction of gravitation as the *cause* which keeps the planets in their orbits.

But it may be said, what is the difference in the two cases? Is not the law of gravitation merely a simpler mode of expressing the observed facts of the planetary motions just like the somewhat less simple laws of Kepler? What right have we to introduce the idea of causation in the one case more than in the other?

The answer to this appears to be that in the one case, that of Kepler's laws, supposing them to be true, we have merely a statement of what, on that supposition, would be a fact regarding the motions of the planets, whereas in the other case the observed motions are referred to a property of matter of the operation of which in other and perfectly different phenomena we have independent evidence.

I have purposely omitted to mention the important difference between the two cases, which lies in the circumstance that Kepler's laws require correction to make them applicable to long intervals of time, whereas the law of gravitation shows no sign of failure; because, even if the former had been perfectly exact, however long the interval of time to which they were applied, I doubt if they would have carried with them the idea of causation.

To take another simple illustration, let us think of the propulsion of a bullet in an air-gun. We speak of the motion of the bullet as being *caused* by the elasticity of the compressed air. And the idea of causation comes in because we refer this particular instance of motion to a property of gas, of the existence and operation of which we have evidence in perfectly independent phenomena.

It is thus that in scientific investigation we endeavour to ascend from observed phenomena to their proximate causes; but, when we have arrived at these, the question presents itself, can we in a similar manner regard these causes in turn as themselves the consequences of some cause stretching still further back in the chain of causation? If the motion of the bullet in an air-gun be caused by the elasticity of the compressed air, can we account for the elasticity of a gas? If the retention of the planets in their orbits be due to the attraction of gravitation, can we explain how it is that two material bodies should attract one another across the intervening space?

Till a time well on in the present century, we could only take the elasticity of gases as a fact, and deduce the consequences which flow from it. But the researches of Joule and Clausius and Maxwell and Crookes and others have accumulated so much evidence in favour of the general truth of the kinetic theory of gases, that we are now disposed not to rest in the elasticity of gases as an ultimate property beyond which we cannot go, but to regard it as itself a consequence of the molecular constitution of bodies, and of the motions and mutual collisions of the ultimate molecules of a gas. Respecting the attraction of gravitation we have not at present made a similar advance. Speculations, indeed, have not been wanting on the part of those who have endeavoured to account for it. But none of these so fits into the known phenomena of Nature as to carry with it a conviction of its truth. Yet there is one indication that though we cannot at present explain the cause of gravitation, yet it *may* be explicable by what are called second causes. The mass of a body is measured by its inertia; and, though we commonly think of a body of large mass as being heavy, and though we compare the masses of two bodies most easily and accurately through the intervention of weight, yet the idea of mass may be acquired, and means might easily be suggested by which the ratio of the masses of two bodies might be experimentally determined, without having recourse to gravitation at all. Now, according to the law of gravitation, the force with which a given body attracts another at a given distance is strictly proportional to the mass of the latter. If we suppose the attracting body to be the earth, and the attracted bodies to be in one case a brass weight, and in the other a piece of marble, it follows that if they make equilibrium when placed in the pans of a true balance—I make abstraction of the effect of the buoyancy of the air—their masses are strictly equal, and, accordingly, that weight is a true measure of mass. But there is no reason *a priori*, so far as with our present knowledge we can see, why this should be so. We know that if the bodies in the scale-pans were formed, one of brass and the other of iron, and there were a magnet concealed under the table on which the operator placed his balance, the masses would not be equal when there was equilibrium. But that the law is true, and that, accordingly, weight is a true measure of mass, follows with the highest probability from the third of Kepler's laws, and was proved experimentally by Newton, by experiments with pendulums. Newton's experiment has since been repeated by Bessel, with all the refinements of modern appliances, with the result that so far as the most exact experiments enable us to decide the law is strictly true. This is perhaps the only instance, as Sir William Thomson remarked to me in conversation, in which there is an exact agreement between two quantities, and yet we are unable to give any reason why they should agree. That such is the case, holds out some prospect of scientific men being able some day to explain gravitation itself—that is, to explain it as the result of some still higher law.

Such is the nature of our progress in scientific investigation. We collect facts; we endeavour to co-ordinate them and ascertain the laws which bind them together; we endeavour to refer these laws to their proximate causes, and to proceed step by step upwards in the chain of causation. Presently we arrive at a stage at which, even after long trial, we do not see our way to going further. Yet we are not able to demonstrate that further progress in the same direction—that is, along the chain of secondary causation—is impossible. Science conducts us to a void which she cannot fill.

It is on other grounds that we are led to believe in a Being who is the Author of nature. A conclusion so important to mankind in general is not left to be established as the result of investigations which few have the leisure and ability to carry out. Doubtless, where it is accepted, the study of science enlarges our ideas respecting the greatness of that Being, and tends to keep in check notions of too anthropomorphic a character which we might form concerning Him. Still, the subject-matter of scientific study is not, at least directly, theistic, and there have not been wanting a few instances of eminent scientists who not merely rejected Christianity, but apparently did not even believe in the being of a God.

The religious man, on the other hand, who knows little or nothing of science, is in the habit of contemplating the order of Nature not merely as the work of God, but in very great measure as his *direct* work. Of course, the concerns of everyday life present innumerable instances of the sequence of cause and effect; and few are now so ignorant of the very elements of science as not to allow that the sequence of day and night, of summer and winter, is proximately due to the rotation of the earth about its axis, and the oblique position of that axis with reference to the plane of the earth's orbit. But when we get beyond the region of what is familiarly known, still more when we get outside the limits of well-ascertained scientific conclusions, and enter a region which is still debatable ground where men of science are attempting to push forwards, and are framing hypotheses with a view to the ultimate establishment of a theory in case those hypotheses should stand the test of thorough examination,—when, I say, we get into this region, a man such as I have supposed may feel as if the scientists who were attempting to explore it were treading on holy ground; he may mentally charge them with irreverence; perhaps he may openly speak of them in a manner which implies that he attributes to them an intention to oppose revealed religion.

To take a particular example. I can imagine that a man such as I have supposed may have always been in the habit of regarding each one of the thousands and tens of thousands of species into which naturalists have divided the animal and vegetable kingdoms as having originated in an independent creative act; that the supposition may have become entwined among his religious beliefs. Such a man would be apprehensive of any attempt to introduce second causes in explanation of the observed fact of the great multiplicity of species.

Akin to the feeling which I have attempted to describe is another, against which we must be on our guard. The religious man is strongly impressed with the truth of certain things which lie outside the discoveries of reason or the investigations of science, and which bear on the whole conduct of his life here, and on his hopes regarding a life hereafter. He believes these truths to be divine, and, accordingly, that no legitimate deduction of human reason is liable to come in conflict with them. But the precise mode in which a conviction of the truth of these things was arrived at depends, to a considerable extent, on each man's idiosyncrasy. His natural bent of mind, his early training, his later associations, have all a good deal to do with it. Divine truth is one thing; our own apprehension of it, and the steps by

which in our own minds it has been arrived at, are another. These are liable to human imperfection, and we may not attribute to them the infallibility which belongs to that which is divine. We are not to confound the scaffolding with the building; nor, if we are anxious for the safety of the edifice, need we therefore fear that, if the scaffolding were tampered with, the whole might come tumbling down, nor should we regard as a dynamiter a fellow-workman who would remove a pole or two.

That truth must be self-consistent, come from where it may, is an axiom which nobody would dispute; the only question can be, What is truth? Now, there are truths which we know by intuition, such as the axioms of mathematics; and there are others, again, which, though we do not perceive them by intuition, yet demonstrably follow from what we do so perceive; such, for example, are the propositions of mathematics. Then there are other conclusions which we accept as the result of the application of our reason to a study of Nature. Here the evidence is not demonstrative, and the conclusion may have all degrees of support, from such overwhelming evidence as that on which we accept universal gravitation, to what hardly raises the conclusion above the rank of a conjecture. On the other hand, there are conclusions which we accept on totally different grounds; namely, because we think that they have been revealed. Why we accept a revelation at all, is a very wide question which I cannot here enter into. That we do accept it is implied in the membership of this Institute. But, granting the acceptance of revelation, the question remains, What and how much is involved in revelation? That is a question respecting which there are differences of opinion among those who frankly accept a revelation, and with it the supernatural.

Now, the primary object of the establishment of the Victoria Institute was to examine the questions as to which there was a *primâ facie* appearance of conflict between the conclusions of science and the teachings of revelation. In order that such examination may be usefully carried out, it must be undertaken in a thoroughly impartial spirit, with a readiness honestly to follow truth wherever it may lead. It will not do to assume that the immunity from error which belongs to the divine belongs also to our apprehension of what constitutes the divine, and that therefore, if a conflict there be, the error must be on the side of science. It is true, that many statements which are really little more than scientific conjectures are represented, at least by those who take their science at second or third hand, as if they were the well-established conclusions of science. But it is true also that the progress of science has corrected the assertions of a crude theology. We are disposed nowadays to smile at the idea of any opposition between the Copernican system and the teaching of revelation; but we need not go back to the days of the persecution of Galileo to find an example of a well-supported scientific conclusion having met with a similar opposition, issuing in a similar result.

To gauge thoroughly the amount of evidence on which an asserted scientific conclusion rests, one ought to be well acquainted with the branch of science to which it relates. Still one can get a fair general notion of the evidence by an amount of reading which is by no means prohibitive, or by conversing with those who have made that branch a special study. It may be that the impression thus left on the mind will be that the votaries of science, carried away by an excess of zeal in the attempt to discover the causes of natural phenomena, have unduly, though honestly, overestimated the evidence. It may be, on the other hand, that the inquirer will perceive the evidence to be weighty and substantial, in which case it behoves him to reconsider the supposition with which he started, that the conclusion was opposed to the teaching of revelation.

One should always bear in mind the great responsibility one incurs, and the mischief one may do, by representing as bound up with revelation that which really forms no part of it. Being by hypothesis no part of it, but only erroneously tacked on to it, it may be false, and being false, it may be in opposition to a conclusion supported by the weightiest evidence, it matters not of what kind, but say scientific. What, then, will be the effect of the error committed by the upholder of revelation? The educated man of science may see through the fallacy; but will it not put a weapon into the hands of the infidel lecturer wherewith to attack revealed religion?

But whether we can agree or cannot agree with the conclusions at which the scientific investigator may have arrived, let us, above all things, beware of imputing evil motives to him; of charging him with adopting his conclusions for the purpose of opposing what is revealed. Scientific investigation is eminently truthful. The investigator may be wrong, but it does not follow that he is other than truth-loving. If on some subjects which we deem of the highest importance he does not agree with us—and yet it may be he agrees with us more than we suppose—let us, remembering our own imperfections, both of understanding and of practice, bear in mind that caution of the Apostle: "Who art thou that judgest another man's servant? To his own master he standeth or falleth."

SCIENTIFIC SERIALS.

Rendiconti del Reale Istituto Lombardo, June 16.—On the importance of the qualitative bacteriological examination of potable waters, by Prof. Leopoldo Maggi. Attention is directed to the mistake made by many chemists, who occupy themselves exclusively with the *quantitative* examination of potable waters, neglecting the much more important question of the specific quality of the germs, owing to the greater difficulty of distinguishing between the various forms of these organisms. Waters largely charged with harmless Bacteria are condemned, although perfectly drinkable, while others apparently pure, but really containing deadly germs in small quantity, are declared to be quite safe, often to the great danger of the public health. It is in fact far more a question of *quality* than of *quantity*, as shown especially by the recent researches of Chantemesse and Vidal on the Bacillus of typhus. On the other hand, Leone has experimentally shown that comparatively pure water is itself a medium of culture, so that a small quantity of innocuous Bacteria may largely increase in it without rendering its use dangerous. Some instructions are added for distinguishing between harmless organisms normally present in water as their natural element, and pathological germs, which render it quite unfit for human consumption.—Meteorological observations made at the Brera Observatory, Milan, during the month of May.

SOCIETIES AND ACADEMIES.

LONDON.

Chemical Society, June 16.—Mr. William Crookes, F.R.S., President, in the chair.—The following papers were read:—A study of the thermal properties of a mixture of ethyl alcohol and ethyl oxide, by Dr. William Ramsay and Dr. Sydney Young.—Derivatives of hydrindonaphthene and tetrahydronaphthalene, by Dr. W. H. Perkin, Jun.—The synthetical formation of closed carbon chains in the aromatic series, by Dr. F. S. Kipping.—The product of the action of ethylene bromide on ethylic acetosodacetate, by Dr. P. C. Freer and Dr. W. H. Perkin, Jun.—The synthesis of hexamethylene-derivatives, by Dr. P. C. Freer and Dr. W. H. Perkin, Jun.—An attempt to synthetize heptamethylene-derivatives, by Dr. P. C. Freer and Dr. W. H. Perkin, Jun.—The composition of shale-spirit, by Dr. A. K. Miller and Mr. T. Baker.—The magnetic rotatory power of the ethyl salts of maleic and citraconic acids and their isomers, by Dr. W. H. Perkin, F.R.S.—The temperatures at which various sulphates undergo decomposition, by Dr. G. H. Bailey.—The reaction between sulphites and nitrites of metals other than potassium, by Dr. Edward Divers, F.R.S., and Mr. Tamemasa Haga.—The action of acetyl chloride on acetoximes, by Mr. Victor Meyer and Mr. A. Warrington.—Sulphinic compounds of carbamide and thiocarbamide, by Mr. George McGowan.—Anarcardic acid, by Dr. S. Ruhemann and Mr. S. Skinner.

EDINBURGH.

Royal Society, July 4.—Mr. J. Murray, Vice-President, in the chair.—Prof. Tait communicated a paper by Mr. A. C. Mitchell on the thermal conductivity of iron, copper, and German silver. Mr. Mitchell made his experiments upon the same bars as were used by Prof. Forbes and Prof. Tait, but the surfaces were nickelized so as to prevent oxidation. The results agree well with those of Prof. Tait, and are probably as correct as the method admits of.—Mr. T. B. Sprague read a paper on the probability that a marriage, entered into by a man of any

age, will be fruitful.—Dr. A. B. Griffiths read a paper on the nephridia of *Hirudo medicinalis*, and communicated a paper by Mrs. Griffiths on degenerated specimens of *Tulipa sylvestris*.—Mr. J. T. Cunningham and Mr. Rupert Vallentin described the photosphæria of *Nyctiphanes norvegica*, Sars.—Mr. C. J. Burton read a paper on a Daniell cell for use as a standard of electromotive force.—Prof. Tait read a paper on glories. He showed that the observations made upon glories on Ben Nevis make it certain that Young's explanation of these phenomena (colours of thin plates) is not adequate. He considers that they are produced by diffraction of light reflected from the drops of water.—Mr. J. Murray submitted a report by Prof. Milnes Marshall and Mr. G. H. Fowler on the *Pennatulidæ* dredged by H.M.S. *Porcupine*.

PARIS.

Academy of Sciences, July 25.—M. Janssen in the chair.—Note on M. Gosselin's scientific labours, by M. A. Richet. This memoir on the life and work of the distinguished anatomist and pathologist, who died at the end of last April, is intended to supply the place of the customary obituary notice, M. Gosselin having expressed a desire that no discourse should be pronounced in connexion with his funeral obsequies.—Obituary notice of M. Alfred Terquem, Corresponding Member of the Section for Physics, by M. Mascart. A rapid sketch is given of the brilliant career of this physicist, who was born at Metz on January 31, 1831, and died on July 16, 1887. His numerous scientific publications deal mainly with acoustics, capillary phenomena, and heat. He is the author of an important treatise on "Roman Science in the Age of Augustus," and of a more comprehensive work on the history of physical sciences from the earliest times down to Galileo.—Note on the earthquake of February 23 at Nice, by M. Bouquet de la Grye. The diagram of the curve of the maregraph here figured as taken at the time of the seismic disturbance presents some points of considerable interest. It clearly indicates a rapid upheaval of the ground, followed by a slow subsidence, the sea returning to its normal level in about two hours after the first shock. The maximum of upheaval at Nice was 55 mm., which can scarcely have exceeded the natural elasticity of the earth's crust.—On the meteorite which fell at Jati-Pengilon, Java, on March 19, 1884, by M. Daubrée. The analysis of this meteorite, which weighed 166 kilogrammes, shows bronzite 39, olivine 33·4, iron with nickel and traces of cobalt 21·3, troilite (sulphur of iron) 5·1, chromite, 0·1; mean density 3·747. The breakage presents some exceptional features, being especially remarkable for the myriads of minute cleavage facets with a sparkling brightness like that of mica. In its general appearance it may be compared to certain very fine-grained feldspar rocks, such as leptynite, and it evidently belongs to the extremely rare category represented by the meteorites of Ensisheim (1492), Erxleben (1812), Cabarras, North Carolina (1849), Morbihan (1869), and one or two others.—Fluorescences of manganese and bismuth: general remarks and conclusions, by M. Lecoq de Boisbaudran. In concluding these protracted studies the author shows in a general way that the observations made with manganese and bismuth are also applicable to other fluorescences. He also concludes that two substances more or less active on a solvent may at times neutralize each other, reducing the two fluorescences to *nil*. A similar result has been obtained by Mr. Crookes with the rare earths.—Solar observations made at Rome during the first quarter of the present year, by M. Tacchini. In supplement to his communication of April 18, the author shows that the faculæ as well as the protuberances were most frequent in the northern solar hemisphere. The maximum of faculæ corresponds to the equivalent zone ± 10°; the solar spots were confined to ± 20°, while the protuberances reached ± 80°.—Solar observations made at Rome during the second quarter of the present year, by M. Tacchini. During this period there was a perceptible increase of all the solar phenomena, and some metallic eruptions were also recorded.—On the determination of the coefficient of elasticity of steel, by M. E. Mercadier. In a recent communication the author proved that in the relation $\frac{\lambda}{\mu}$ of the constants of elasticity λ is very nearly $= \mu$ for glass. Here he shows that for cast steel $\lambda = 2\mu$.—Danger of infection from tuberculous substances, by M. Galtier. The experiments here described fully confirm previous conclusions regarding the great resisting power of the virus of tuberculosis. It retains its activity after being subjected to temperatures ranging from 71° C. to 7° or 8° below freezing-point. It also resists the action of water and the desiccating process, as well as strong pickle, so that the consumption of fresh or corned beef from animals affected by pulmonary diseases is always attended with some danger.—On *Colochirus lacazii*, by M. Edgard Herouard. A full description is given of this new species of the genus Colochirus of the Holothurian family, found by the author in the neighbourhood of Roscoff, and by him named *C. lacazii*, in honour of M. Lacaze-Duthiers.—Contribution to the study of the evolution of the fresh-water Peridiniums, by M. J. Danysz. From his researches on the development of these organisms, as well as of the distantly-allied genera Gymnodinium and Glenodinium, the author concludes that they should be regarded rather as plants than animals. A close study of their successive phases of development, and of the nature of their substance, shows that they are true members of the vegetable kingdom.—Appearance of black rot in the neighbourhood of Agen, by M. Prillieux. An examination of some diseased grapes from this district shows clearly that they have been attacked by black rot which had already made its appearance in the Upper Hérault Valley two years ago, but which it was hoped would die out or spread no farther.—A sealed paper deposited by M. A. Leduc on May 9, 1887, and now opened at his request, describes two experiments showing that the calorific conductibility of bismuth is considerably reduced when this metal is placed in a magnetic field.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Hand-book to Government Situations: B. D. K. (Stanford).—The Conic Sections: G. Heppel (Baillière, Tindall, and Cox).—Formal Logic, Second Edition: J. N. Keynes (Macmillan).—Psychology: The Motive Powers: J. McCosh (Macmillan).—Romantic Love and Personal Beauty: H. T. Finck (Macmillan).—Crown Forests at the Cape of Good Hope: J. C. Brown (Oliver and Boyd).

CONTENTS.

	PAGE
The Jubilee of the Electric Telegraph	313
The Classification of Algæ. By Mrs. Mary P. Merrifield	313
American Mining Industries	315
Our Book Shelf:—	
Nipher: Theory of Magnetic Measurements"	316
Wilson: "Studies in Life and Sense"	316
Giglioli: "Fermenti e Microbi."—Dr. E. Klein, F.R.S.	316
Crookshank: "Photography of Bacteria."—Dr. E. Klein, F.R.S.	317
Letters to the Editor:—	
The Sense of Smell in Dogs.—W. J. Russell	317
Units of Mass, Weight, and Force.—Rev. John B. Lock	317
Chemical Affinity and Solution.—Wm. Durham	318
Early Perseids.—W. F. Denning	3
Floating Eggs.—W. S. Green	
The "Meteorologiske Institut" at Upsala, and Cloud Measurements. By Hon. Ralph Abercromby. (*Illustrated*)	319
A New Cosmogony. I. By A. M. Clerke	321
Notes	323
Astronomical Phenomena for the Week 1887 August 7–13	326
The Jubilee of the Electric Telegraph	326
The Case for a London Teaching University	329
Science and Revelation. By Prof. G. G. Stokes, P.R.S.	333
Scientific Serials	335
Societies and Academies	335
Books, Pamphlets, and Serials Received	336

THURSDAY, AUGUST 11, 1887.

THE TOPOGRAPHY OF GALLOWAY.

Studies in the Topography of Galloway; being a List of nearly 4000 Names of Places with Remarks on their Origin and Meaning. By Sir Herbert Eustace Maxwell, Bart., M.P. (Edinburgh: Douglas, 1887.)

SIR HERBERT MAXWELL will strike a sympathetic chord in the minds of many readers, who have not themselves time to search for the origin of place-names over which they have pondered, and perhaps speculated, without avail. We do not mean that the limited district so thoroughly sifted by Sir Herbert Maxwell affords illustrations for place-names everywhere, but his method of handling the subject serves as a model for the useful imitation of students in other districts where such a convenient hand-list is wanting.

On a clear day, one ascending the backbone of England, say at Cross Fell, beholds, beyond the Vale of Eden, far to the south, Ingleborough and the Shap Fells, on the west the Lake mountains, and towards the north a broad arm of the sea, which he recognizes as the Solway Firth, cutting off the wide-extended plain of the Vale of Eden, which lies spread like a carpet far below him. Beyond the Solway Firth there rises a huge hill capped with cloud and backed by hilly country, cut off by the sinuous coast-line as far as the eye can reach. The hill is Criffel, "a hill of 1850 feet," called on a map in the Bodleian Library, circ. 1330, "Mons Crefel," and by Pont, in Blaeu's Atlas, 1654, "Crafel," a hill whose peculiar granite boulders lie scattered plentifully in the drift over the new red sandstone of the Vale of Eden. It is one of the outposts of Galloway, the origin and meaning of whose place-names form the subject of a most thorough and searching investigation in the present work. These names are conveniently arranged in dictionary form in 322 pages. Many are entered and left unexplained; Sir Herbert Maxwell, with true statesmanship, leaving to others the invidious task of applying the unscientific knot-cutting, or "guessing etymology," which he so scornfully repudiates in the, if anything, somewhat prolix introduction of 44 pages.

We in England, who do not all know our *Bobelloth* and *Bethluisnion* as well as we might, come as learners to Sir Herbert Maxwell's book, which displays much real learning and a fair amount of bibliographical research. Treating, as it does, of a language which is foreign to our ears, a language rejoicing not only in "eclipses," or a vast superfluity of unsounded consonants, but of "triphthongs" or sequences of three vowels, equally unknown to our modern English, Sir Herbert Maxwell's task lies very much in expanding to the full Gaelic form the words from which the vast majority of the names are derived, and at first sight it seems almost as hopeless a task to follow him as to sit down unassisted to master Russian. We can only make one or two observations on the introduction, which digests much from O'Donovan and Joyce. "The Basque word for water is *ur*," hence "the rivers called Oure, Urr, Ure," &c. Like most other words meaning a river, it also means a "bank" of a river: *e.g.* beck; burn (bruinne, a *brink*); river (*ripa*), &c. "*Ur* denotat rivos aquarum impetuose ex alto delabentium" (Junius, "Alph. Run.," 21); cf. Lat. ora, A.S. ore, Eng. ore, the *shore*. In Norway, *ur* is the rough slope of a mountain; Irish, *ur*, a border, *brink*. The author, "dismissing as unattainable" all record of pre-Celtic speech, finds, "from the evidence of these names," that the Pictish of Galloway "belonged to the Goidhelic or Gaelic rather than to the Brythonic or Welsh branch." "No doubt," he adds, "there are names whose forms would bear being assigned to a Brythonic origin, but with these I have not ventured to deal." We will not venture either, but in such glaring cases as the "Rhinns" of Galloway; "maiden craigs" (W. Meiddyn, a cliff, precipice), common in North England; "cors," the fenny district on the coast of Kirkbean (Chalm. "Caled." iii. 234); "carse," Kirkcudbright, "carse gowan," "carse thorn," "carse land," in all of which the physical character answers to the Welsh "cors," "a *marsh*, according to the common acceptation" (Ed. Luid. "Adversaria," p. 268); and in "Corsock," New Abbey, Wel. "corsawg," *fenny* (Chalm. "Caled."), and several others, it is plain that Welsh words do occur, and therefore have to be dealt with. With the principles admirably set forth in the introduction we fully agree. One or two slips occur, as (p. 41) where the author attributes to Pont, as original, a passage copied from Camden's "Britannia." Sir H. Maxwell seems to have followed Murray's error (Note D, Append. to "Hist. of Gall.," 1822). The sentence is "Neirunto this (Vigtoune) Ptolemee placed the city Leucophibia," &c. Now, this sentence appeared in the original Latin edition of Camden, 1586, p. 480, "Galloway . . . Hac regione Leucopibiam urbem statuit Polemeus," &c., published when Pont was about nineteen; and for comparison with the passage from Pont's manuscript we give that from the first English translation of Camden, 1610:—"Neere unto this Ptolomee placed the city Leucopibia, which I know not to say truth where to seeke. Yet the place requireth that it should bee that episcopall seat of Ninian which Bede calleth *Candida Casa*, and the English and Scotish in the very same sense *Whitherne*: What say you then if Ptolomee after his maner ("suo more," 1586) translated that name in Greek Λεὺ οικιδια [*sic*], that is Whitehouses," &c.

Again, the supposed identification of *Rerigonium* with *Bargeny*, attributed by our author (p. 42) to Heylin, 1669, should be attributed to Camden. Thus, under *Carricta*, Camden, 1610, has Rerigonium, "a towne. For which *Berigonium* is read in a very ancient copie of Ptolomee printed at Rome in the year 1480, so that we cannot but verily think it was that which is now called *Bargeney*." Sir Herbert Maxwell rightly points out the anachronism between Loukopibia and Candida Casa. Horsley—with several others who have discussed the Ptolemaic names—avoids the trap, saying, with a side glance at Camden, "others from a fancied etymology place it at Whithern," which error the followers of Camden have perpetuated to our own day. The form "Lucotion" ("Brit. Chorog."), is conclusive. "Brigomono," however, in which form *Rerigonium* appears in "Brit. Chorog." (given in Gale, and Horsley, "Rom. Brit." p. 490), does offer some suggestion of Bargeny.

Of the forty-seven authorities given by the author, sixteen are works exclusively relating to Ireland. We suggest that many of the "Gallowaie" place-names are well illustrated or explained in the following works not consulted by him, viz. :—

(1) Hector Boethius, c. iii. "The Description of Gallowaie (in Holinshed, 1587, p. 9), "Aboue Nidderdale is Gallowaie," &c. Thus "the two other lakes, the *Salset* and the *Neutramen*, of equall length and bredth with the *Loch Mirton*," do not appear in Sir Herbert Maxwell's list. Of the Mull of Galloway, Boethius writes, "which the Scots call a *mule* or *nuke*. . . . The common sort name it the *mules nuke*," an evident reduplication, "newk," which occurs several times in Maxwell's list, being Old Norsk *Hnjúkr*, common in North England for a projecting hill. The forms for "the two great lakes *Rean* and *Lois*" (Ryan and Luce) are also worth entering.

(2) Chr. Irvine, "Historiæ Scoticæ Nomenclatura Latino-vernacula, multis flosculis, &c.," "enriched with many select phrases from the ancient monuments of the Scots and the aboriginal language of the Gael," Edin., 1682. Here (p. 84), "Gallovidia et Wallowithia (for it is so named by the Welshmen)" offers a form not found in Maxwell *s.v.* Galloway, and with many other forms we have before us is well worth entering. In the article (p. 186) on the word Galloway, Sir H. Maxwell, passing by Lloyd's etymology ("Church Government," appended to Stillingfleet, "Orig. Brit.," vol. ii. p. 72), cites Skene for the "stranger-Gael," but has he not observed how Prof. Rhys shakes his head at this ("Celtic Btn.," 1882, p. 153)? The form Galwychya, in "Bulla Innocenti V. *De Holmcolt.*," 1207, is worth recording, but the complete list of forms, which is a very long one, should be given.

(3) Sibbald, "Hist. Animalium in Scotiâ," 1684, Part 2, cap. iv., may be cited under Sir H. Maxwell's "Fumart Liggat" (p. 184), "*Foina* est Boethii." "Nostra arborea est. Sylvas incolat abiegnas. Nidumque super Abietes, sciurorum instar struit." The pine marten inhabits pine-woods and, squirrel-like, builds its nest in the fir-trees.

(4) W. Baxter, "Glossarium Antiq. Brit.," Lond. 1719, deals with the Ptolemaic Galloway names; but much more welcome are the "Adversaria Posthuma" of the learned Ed. Lhuyd, given as an appendix to that work. The title is "D. Edvardi Luidi de Fluvm., Montm., Urbm., &c., in Britanniâ nominibus Ad. Posth." Sir H. Maxwell has "Fínen hill" (p. 182); Luid (p. 268), "Fynnon though generally used for a well [*i.e.* spring], signified also the first or highest lakes of the great mountains." In Luid's "Adversaria" is a mine of wealth, from which we select two names, (p. 274) "Turch, *porcus*," (p. 267) Türch, a *hog*, in Brecknockshire. O'Reilly (p. 542) gives Irish, Turc, and (p. 528) Torc, Wel. Torch, a *hog*, *swine*. Sir H. Maxwell has "Turkey Hill" unexplained (p. 306), which is translated by "Swinefell, the fell or hill of the swine" (p. 299). Again, "Hespin" (p. 200) in Whithorn is left unexplained. Luid (p. 267) has, Wel. "Hespin, a sheep that yields no milk. There are two or three *brooks* of this name about Ystrad Vehlte, in Brecknockshire, so called because their channels consisting of limestone have great caverns which in summer-time take up all the water the springs afford, so that, the channels being left dry, the brooks are called *Hespin*."

(5) How could Sir H. Maxwell overlook Kirk's list of over 400 Gaelic words in Append. II. p. 99, to Bp. Nicholson's "Scottish Hist. Liby.," Lond., editn. 1776, "A vocabulary of the Irish dialect spoken by the Highlanders of Scotland, collected by Mr. Kirk," with a few words added by Ed. Lhuyd? They are in twelve chapters, several of which relate to Nature and her productions, and help to form place-names.

(6) Horsley, "Brit. Rom.," Lond., 1732, cited.

(7) "Etymology of the Names of Places in Ireland," "by a gentleman well versed in the language and antiquities of that country," contributed by "C. L." to *Ant. Repertory*, vol. iv. 1809, gives a list of eighteen words used in place-names.

(8) Thos. Murray, "Lit. Hist. of Galloway," Edin., 1822, Append., Note D. He has also a fairly full article on the forms of the name "Galloway" in Note A, the oldest of which, in a charter of Earl David, A.D. 1124, is Galwegia [? Galweyia].

(9) W. Mackenzie, "Hist. of Gall.," Kirkcudbright, 1841, has (pp. 12–22) several place-names taken from Chalmers's "Caledonia," without acknowledgment.

(10) M. M. Harper's beautiful "Rambles in Galloway," with illustrations by Faed, &c., Edin. 1876, smooths away the difficulties that lie in the path of the name-hunter, and his terse descriptions of sites are a model, *e.g.* (p. 99) "*Rusco* Castle, beautifully situated on a rising knoll in the Vale of Fleet, near the margin of the river." That is what we want to know. But "Rusco" can only be assumed to be here, as it is known to be in Nidderdale, after a man's name. Throughout Sir H. Maxwell's list, terse physical descriptions are much missed. Again, facing p. 109, Harper gives a view of "Kirkclaugh shore" that goes a long way to convince one that "Kirkstone" is meant. He gives etymological notes on place-names, no doubt from Chalmers, *e.g.* "Minnigaff," p. 133, differently explained by Maxwell (p. 254) and by Symson. Who shall then decide? On p. 151 several names. But his most important hint is in the admirable picture of the "Cow Clout Stone" (p. 187), which in the "Stat. Acct." is described by Crosbie as "a flat stone about 3 feet in diameter," in which are the marks of what might be supposed a *cow's foot*, a horse-shoe and the four nails on each side being very distinct, &c. Were cows formerly shod in Galloway as oxen are near Naples? Sir H. Maxwell has "Cowloot" (p. 128) unexplained. O'Reilly gives—Irish, *luat*, *the foot*, ("Ir. Eng. Dic.," p. 337), which explains the name.

(11) Chalmers's "Caledonia" requires mention apart, for in that work many, if not most, of the names treated in Sir H. Maxwell's painstaking work have been considered, generally with satisfaction to the reader, as will be found in vols. i. and iii., where he will be found to have anticipated some of Sir H. Maxwell's explanations, as "Loch Brack," "Loch Breac, in the Scoto-Irish the *lake of trouts*."

(12) Prof. Rhys, "Celtic Britain," 1882, a good antidote to Skene for those who may require it, has several ancient Galloway names.

(13) Dugdale, "Monasticon," gives a host of charters with the old forms of Galloway place-names. No less than twelve of the eighty-seven charters relating to the Abbey of Holmcoltram, whence the monks had a pleasant row

across Solway to their lands in Galloway, contain forms, too numerous to be enumerated here, of Kirkewinni, Kirkwyni, Kyrkewynwi, Kirkewenny, Kirkgunny, Kirkwynnyn, &c., &c., now Kirkgunzeon. The same St. Winnin probably explains Kirkennan, Kilennan, the ancient name of Buittle (Chalm. iii. c. iii.), and Kirvennie. Here also the River *Urr* is spelt "Hur"—"a portu *Hur* usque ad 'Pouesterbened'" (No. xxiv.), or "Pollesterheved" (No. lxxxii.); perhaps the same "Pow, a *slow-moving rivulet* in flat lands" (Jamieson), recorded by Sir Herbert Maxwell at New Abbey, which last was formerly called "Loch Kendelock" or "Loch Kenderloch" (Chalm. iii. 296, 305), "Loghendello" in a charter of Roland, son of Huctred (No. lxxxi., Holmcoltram, Dugd.), and now "Loch Kinder," after Cendaelaidh, King of the Picts, D. 580, q.v. We find also "Lotchitdale," "Bulla Lucii III." (No. xxi. Holmc., Dugd.), and "Lochent" (No. lxxxi. fol. 103); "Millebronna," "Bulla Alexandri III." (No. xxiv.), which, unless the same as "Millbawn," is wanting in our author's list. So also "Salternes," *i.e.* salthouses (*ib.*); "Polben," probably our author's Polbae (*ib.*); and "Sivchaye," now Southwick, according to Chalmers, iii. 296; "Glenlus," now Glenluce (No. lxxxii.); "Mayby" (*ib.*), now Mabie, in Troqueer, and "Achencork" (*ib.*); "Pollackercin" (No. lxxxi.); "Polleychos," or "Polthos" (No. lxxxii.), some stream, and "Genesik," *i.e. sandy*, "sandy-sike," or gutter (A.S. Sik), for which see "Genoch" and "Gannock" in our author's list, the last form occuring several times in Middlesex and Hertfordshire. Then we find "Mustard Garth," evidence of the Northman, in "Kirkoneville," or the manor of Kirkconnel (*ib.* fol. 112) west of the estuary of the river "Nud," now "Nith," formerly "Nid," the Novios of Ptolemy, a river-name of pre-Celtic origin, ranging from Wales to Trondhjem. "Trenguer" (lxxxiii.) preserves an *n* dropped in the modern Troqueer. "Botil" (*ib.*), A.S. Botl, perhaps "Buittle" (?). The charter of William the Lion also preserves the names Kirche Cormack (Maxwell, "Kilcormack"); S. Andreæ; "Balincros" (not in Maxwell), and "Cheletun," now Kelton.

We will now take at random a few names from Sir Herbert Maxwell's list. We cannot concede that the northern adjectival termination *et* represents A.S. wudu, as claimed by our author (Introd., p. 9), in "Aiket," &c. "Thornit," *thorny*, also occurs in Anglian North England. Are we to understand that the stream which "was ...ng along its wild and turbid waters with a *freshet* ... it" was rolling along a fresh *wood?* "Bail Fell," Old Norsk Bali, "monticule," a *grassy bank*, cf. "Bale bank," Nidderdale, so also, "Bailie Hill," but see a learned article, *s.v.* Baillie, Baille, and *s.v.* Bel, Boel, *cour intérieure*, in Duméril, "Patois Normand," 1849, which clears away the fog from "Baillie, meaning doubtful," of Jamieson. "Barean," "Barend," and "Borron," unexplained by our author, a well-known word in North England, a *rocky slope*, or *hill*, where foxes and badgers burrow. It ranges at least as far south as Kettlewell, where it appears as "Borrance," the stony screes below the limestone girdles or cliffs. It is also called "Burran," and, among the Yetholm gipsies, "Burran" means a badger. O'Reilly ("Ir. Eng. Dic.") has:—"Barran, the *tops of mountains;*" "Boireand [Barend], a large rock; a *stony, rocky district*. Is the name of several rocky districts in the north and south of Ireland. It is applied to the face of a desolate mountain in Achil," &c. Similarly used in North England. His last form "Boireand, *Borron*, a large rock," identifies the word completely. Duméril, *s.v.* "Buret," *porcherie*, records low Latin "*Burum* le *Bure* vieil anglais et le vieux français *Buron*," &c. "Bine Hill," O'Reilly has "Binn, a *hill*," which may also explain "Byng Hill," though Old Norsk "Bingr" looks tempting. However, it is not found directly applied to hills as hills. "Caughie Stone" reminds us of numerous "Cockle Hills" in North England, all on moorlands. "Hecla," the *cloak*, and "Cloak Hill" (p. 123), suggests Ir. *Cocal*, a *cloak*, which is intelligible if the cloak be *peat*, as in "Caugh Moss," Girthon. Old Norsk forms are scattered through our author's list, *e.g.* Cawvis Hill, *if* Old Norsk Kálfr, a *calf* (?), as Sir Herbert Maxwell suggests.

"Cockplay, a hill of 950 ft." (p. 124), seems to be a translation of "Cocklakes," or properly "Cocklaiks," *dry ridges on the moors*, which has been explained ("Stud. in Nidderdale") as "the *playing-ground* of the grouse or moorcock." The keepers and others familiar with their habits understand this. "Cokelayk" is often mentioned in the early charters of Holmcoltram Abbey, Cumberland, given in Dugdale. The prefix "Darn" in "Darnarbel," "Darncree," &c., reminds us of "Darnetal," a place-name at Caen. "Darne" in "Patois Normand," and "Darn" in Breton, means a *piece, portion* (Duméril). "Dub," a pool, common in North England, and in our author's list "Dub of Hass," "Duchdubs," is evidently Celtic, as it occurs as "Douve, grand fossé plein d'eau, étang," in "Patois Normand." The necessity for a brief physical description is well illustrated by the name "Knockmullin." Our author observes that the same words serve for eminences and hollows, hills and valleys, and the fact is well known. All his "Knocks" are hills, and he does not say what kind of thing "Knockmullin" is. The second half is clear—"mill," and the word might mean *mill-race*, for we have, "Patois Normand," "*Noc.* Dalle, goutiére en bois, canal qui apporte l'eau sur la roue d'un moulin." "Laicht, on the eastern shore of Loch Ryan" (Skene, "Chron. Picts and Scots." Pref. clxxv. 1867), is omitted in Maxwell's list, though we believe there is a farm of the name there yet. "Rerrick, anciently called *Dundrainan*, the hill of thorns" (Chalm. "Caled." iii. 313) renders the spelling "Rerwick" very questionable. Is there not an initial *d* dropped? "Wigg," certainly the "Wigstones" of the Nidderdale moors, a huge projecting rock, or, rather, pile of rocks, records the Gaelic "Wig," a *rock*. The old forms given by Sir Herbert Maxwell divorce the Whithorn "Wigg" from the A.S. "Wic," to which he would assign it.

We conclude an imperfect review of this important work with a question to which our personal knowledge of Runic inscriptions on British soil suggests that there is no satisfactory answer. Why send the two Runic inscriptions (Introd. p. 18) to Denmark? No Danish scholar has ever deciphered an English Runic inscription correctly from the days of Spelman and Ole Worm to July 21, 1887. We have read our own A.S. Runes from the *Futhorc* Otho B. 10 in Hickes's "Thesaurus," by the true learning of *Kemble;* and, moreover, no known Runic

inscription on British soil corresponds to the Scandinavian formula by which, "from analogy," Prof. Stephens would read those found, but unfortunately not given, by Sir Herbert Maxwell. JOSEPH LUCAS.

OUR BOOK SHELF.

The Prevention of Consumption. By C. Candler, Melbourne, Victoria. (London: Kegan Paul and Co., 1887.)

THIS may be considered a book of theories. The author promises by "his theory" to revolutionize the treatment of phthisis, and almost to bring the disease to an end among civilized nations.

The theory is briefly stated thus: "Ordinary phthisis is invariably caused by a local bacillary malaria governed by chemical light." When the author speaks of bacillary malaria, he means that the tubercle Bacillus is like a saprophyte, capable of growing and thriving in the soil, and that from the soil, which is its true birthplace and home, the Bacillus or its spores find entrance into the human system: fortunately for humanity solar light destroys many of these Bacilli.

"It will be observed," says the author, on page 191, "that it is presupposed that the consumptive, and they who are sickening with consumption, are, or have recently been, exposed to a bacillary malaria fostered by an insufficiency of solar radiation, and this is one of the inferences which urgently requires to be verified." Quite so; and this the author ought to have done himself, though he hopes that somebody else will furnish the proof.

The prevention of phthisis the author has no doubt of achieving by plenty of sunlight; and he would force the Governments to supply more sunlight to the inhabitants of big cities, where, as is known, consumption is rife. It is a pity the author does not tell us how this is to be achieved in London or Manchester during a great part of the year. E. KLEIN.

Metal Plate Work: Its Patterns and their Geometry. By C. T. Millis. (London: E. and F. Spon, 1887.)

THIS work is one of the series of Finsbury Technical Manuals, and teaches how all ordinary patterns required by sheet metal-workers can be set out on one geometric principle. It is the first work in which the setting out of such patterns has been systematized. The manufacture of every article in common use is treated as a separate problem, but the principle in all cases is that the parts composing it shall be set out mathematically, so that any worker having become accustomed to cut out his work on this principle could equally apply it to new forms. The first chapters are of the most elementary character, so that the work is not necessarily above the head of ordinary mechanics. That the book is an admirable manual there can be no question, but whether such a book will be widely consulted appears doubtful. In the opinion of two of the chief tin-plate workers in Birmingham the knowledge it imparts will save time and prevent waste of material, which results when the rule of thumb and guess-work are in vogue, whilst the workman using it will gain confidence, and his value be increased by the certainty of his pattern working out true. Nevertheless, the great mass of workmen in metals are not yet educated up to the use of such a work, and in all probability in a centre like Birmingham it will only fall into the hands of managers of manufacturing establishments and a limited number of first-class workmen. It is a book, however, that must be required by the artisan more and more to meet the rapid strides of education, and it will, we hope, command a satisfactory sale.

Walks in the Ardennes. Edited by Percy Lindley. (London: W. H. Smith and Son, 1887.)

THIS hand-book, which only costs sixpence, contains all the information the ordinary tourist is likely to want in walking in the Ardennes. The writer is very familiar with the country, and describes clearly and simply the various routes and the chief centres of interest. There are a sketch map of the Ardennes, and a good many illustrations.

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 Parietal Eye in Fishes.

IN my short paper on this subject which appeared in NATURE of July 14 (p. 246) there are one or two points which need better elucidation than I then gave them.

In the first place, for the sake of brevity, my reference to Ahlborn's valuable paper is too scanty, and I am unwilling to do any injustice to that excellent observer. I did not believe he had ever really been fortunate enough to get sections of the "pineal gland" of a fully adult *Petromyzon planeri*; for, judging from what I had found in every adult examined, I imagined that had he possessed fully adult Petromyzon he must have noticed the black pigment in the parietal eye, and moreover must also have seen and figured the deep fossa in the skull (*vide* my figure in NATURE, p. 247), in which, in the adult, the parietal eye rests. It appeared as though his descriptions and figures of the adult brain had been taken from specimens in which the metamorphosis was not quite complete. I have again studied his figures, and must admit that in other respects some of his drawings represent the brain of adult Petromyzon. The apparent contradiction seemed strange, but it is fortunately not inexplicable.

I must here mention that *Petromyzon planeri* is no longer here in Freiburg so plentiful as when Calberla worked on it, now more than ten years ago. Indeed, I have had great difficulty in obtaining adult and very young specimens. The older Ammocœtes, though not common, are not so rare.

This being the case, I could not examine the number of individuals I should have otherwise wished to do. However, I have now found one adult Petromyzon in which there was no black pigment in the parietal eye and no fossa for the eye in the skull. That the specimen was otherwise adult is certain. This find accounts for the non-discovery by other observers of the black pigment I have described. The parietal eye in Petromyzon, which is a rudimentary organ, like many other rudiment organs is probably also variable in different individuals, and is not impossible that the black pigment of the parietal eye entirely absent in the Petromyzon found in many places.

So far as I can judge at present—and I intend to further examine the point—the parietal eye in the Blindworm (*Anguis fragilis*) is also variable. It certainly varies in size and in distinctness.

The second point relates to the black pigment. Wiedersheim and Ahlborn have stated that the pineal gland in Ammocœtes possesses a gray-white pigment. Owing to scarcity of living Ammocœtes I have not verified this, but I do not for a moment doubt it, and I did not mention it, firstly because I did not think it important, and secondly because I did not wish to lengthen the paper.

I did not describe the pigment in the adult as black, but that such was the case could be inferred from the description, and in not stating its colour I was only following an excellent authority, Prof. Carrière, who, in his book "Die Sehorgane der Thiere," in many cases does not state the colour of eye-pigments. One usually assumes that a retinal pigment is black.

Thus we may say, in very young Ammocœtes the parietal eye possesses black pigment, in older Ammocœtes white pigment, and in adult Petromyzon there is a reversion to black pigment. In what relationship these three pigments stand to each other I am unable to say.

The last point concerns the hypotheses as to the origin of the eye. These were really two in number. The first of them—that which derives the paired eyes and the parietal eye from one common dorsal sense-plate—I hold to be fairly certain, and, indeed, there are many facts to support it.

The second, which derives the parietal eye as a later involution of a portion of this same plate, an involution which was supposed to have taken place after that of the paired eye "Anlage," I only believe to be conceivable. My hope of establishing it lay in the verification of an observation of Goette's; there are no facts to support it, and from more recent investigations of the development I am disposed to attach less value to it. For, from these developmental researches, from studies of the types of eye presented by vertebrates and some invertebrates, and lastly, but not least, from valuable discussion with and criticism by Prof. Wiedersheim, a new track has been found, which gives the explanation of a good deal, but the problem is too long and complicated for treatment here.

The first hypothesis mentioned above is taken as the starting-point, but for the further details there are several other questions which have first to be solved. J. BEARD.

Anatomisches Institut, Freiburg i/Br., July 20.

Physiological Selection.

LIKE so many others who have written on this subject, Mr. Rusden freely criticises my views without having deemed it desirable to read my paper. Had he taken the trouble to do so, he would have found a sufficient recognition of the general fact that instinctive habits not unfrequently serve to mitigate the swamping effects on incipient varieties of intercrossing with their parent forms. Moreover, he would have found that there are others of these habits mentioned by me which are probably much more effectual in this respect than is the one to which he draws attention. Nevertheless, it appears to me evident that all these habits taken together cannot count for much, even where they occur; while it is unquestionable that they occur only in a very small fractional part of organic nature considered as a whole—namely, in some among the more intelligent species of animals. The whole of the vegetable kingdom, an immense majority of the Invertebrata, and a considerable majority of the Vertebrata, cannot possibly have had any of their specific differentiations influenced by any of these forms of what I have already designated as "psychological selection." This sufficiently obvious consideration appears to have entirely escaped Mr. Rusden. He adduces a well-known and a comparatively limited form of psychological selection as a "simple solution" of the difficulty from free intercrossing in all cases!

The other parts of his letter merely indorse the views which are published in my paper. I there say that the theory of natural selection is not, strictly speaking, a theory of the origin of species, but a theory of the development of adaptations. Having read this statement, your correspondent writes:—"To consider the theory of natural selection as a theory of the origin of species is therefore, clearly an error. . . . The theory of natural selection is one, not of the origin of species at all, but of the preservation of particular varieties," *i.e.* those which present an adaptive character. I do not see how his agreement with my views in this matter could be more clearly expressed, and therefore I cannot understand why he supposes that he is here criticising anything which I have written. If the point of his criticism is that I imagine Mr. Darwin to have fallen into the error of regarding the theory of natural selection as (primarily) a theory of the origin of species, this would merely show again that he has not read my paper. My contention from the first has been that upon this point I am in full agreement with Mr. Darwin, and differ only from those Darwinians who differ from their master in holding that *all* specific changes are likewise adaptive changes, and *vice versâ*. It is only in the presence of this non-Darwinian assumption that specific changes and adaptive changes become synonymous terms, with the consequence that the theory of natural selection is to be regarded as in all cases the only theory of the origin of species.

And this leads me to the last point in my critic's letter. I have argued that the above-mentioned non-Darwinian assumption is opposed to observable fact, seeing that "in a large proportional number of cases" specific characters appear to be wholly useless. Nothing has surprised me so much on the part of my critics as to have found this statement vehemently challenged by so accomplished a naturalist as Mr. Wallace, and therefore I am now engaged in collecting a quantity of evidence upon the subject. But the point here is that Mr. Rusden appears to think there is some ambiguity attaching to the terms "use" and "utility." For he asks whether these words have "any real significance outside human interests and considerations." Now, I can scarcely understand how anyone at this time of day could suppose that when these words are employed in their Darwinian sense they are intended to have any reference to human interests. When an evolutionist speaks of the utility of an organ, it is hardly conceivable that anyone should understand him to mean anything else than the utility of that organ to the species which presents it. Therefore, the term "utility" is equivalent to the term "adaptation," and to say that any organ or structure is of use is one and the same thing as to say that it is adapted to the performance of a function which is of benefit to the organism or to its species. Such, at any rate, is the only sense in which I have myself employed these words; and in doing so I have, of course, followed the terminology of Mr. Darwin, as my critic might have observed without going beyond one of the quotations which he himself makes from the "Origin of Species"—namely, "I have called this principle by which each slight variation, if useful, is preserved by the term 'natural selection.'"

GEORGE J. ROMANES.

Geanies, Ross-shire, N.B., July 29.

The Droseras.

MISS ANNE PRATT in her "Wild Flowers," vol. ii. p. 155, in describing the three British species, after stating the character of the stems and flowers, remarks, "but many persons who know the plant well have never seen the flowers fully open." Two of the species, *D. rotundifolia* and *D. longifolia*, are found in a bog on a common near here, and these have lately flowered in captivity. They were transferred from their habitat and placed in a large saucer with peat and Sphagnum, under a bell glass. The flowers have expanded from 10 a.m. to noon each day, after which the sun left them. A *D. longifolia* in another position was seen to flower at 2 p.m. Moisture and sun seem the conditions to bring out the blossoms. I am not aware whether they have flowered *in situ*, as my plants were gathered in the early morning.

Ramondia pyrenaica, brought from Bagnères de Luchon ten years ago, has flowered each year on an outside rockery in my garden. J. RAND CAPRON.

Guildown, Guildford, July 28.

Comrades.

MY children and their governess, when staying in the north of Ireland lately, witnessed the following curious display of feeling, in animals not usually credited with feelings. A boar pig was in the habit every morning of going to the basket where a blind kitten of about six weeks old was kept, allowing the little thing to creep on to his back, and then taking it about and caring for it during the day. The kitten got its food at the same time as the pig, and at the same trough. In the evening the man who saw to the animals used to carry the kitten back to its basket to pass the night. "Où donc la vertu va-t-elle se nicher?"

Pollokshields, Glasgow, August 1. E. R.

A NEW COSMOGONY.[1]

II.

DR. BRAUN has earned by his excellent series of observations on sunspots (NATURE, vol. xxxv. p. 227) a title to be heard with particular respect on subjects connected with solar physics. In unfolding his views

[1] "Ueber Cosmogonie vom Standpunkt christlicher Wissenschaft. Mit einer Theorie der Sonne." Von Carl Braun, S.J. (Münster: Aschendorff, 1887.) Continued from p. 323.

regarding them in the three concluding sections of his work on Cosmogony, he by no means underrates the difficulties they present. The range of our sensible experience shrinks into absolute insignificance when compared with the exalted conditions reigning in the sun. The temperature at its surface may well reach 40,000° to 100,000° C.; near the centre it mounts probably (our author considers) to ten, possibly to thirty or more million degrees. This unimaginable vehemence of heat is balanced by an unimaginable urgency of pressure. The statement that, in the depths of the sun's interior, it reaches a maximum of 2,000,000,000 atmospheres gives only nominal expression to its value. Figures can at times keep pace with facts only on the condition of being reduced to empty and meaningless symbols.

Gravity and molecular motion—the two universal antagonists—here carry on a conflict intensified far beyond the control of "laws" derived from terrestrial observation. The correlation between elasticity on the one side, and pressure and temperature severally on the other, established by Boyle and Gay-Lussac, holds good only over a strictly limited range of conditions. Calculations founded on the supposition of its continued prevalence in the sun lead at once to manifest incongruities. Solar speculators are thus left, to a great extent, without the guidance of ascertained principles. In the region frequented by them, the scientific imagination has free play. Apposite facts are scarce; misleading analogies too much abound. It cannot then be wondered at if theories of the sun often include extravagances which it is easier for their critics to discern than for their constructors to avoid.

A futile debate has sometimes been raised as to whether the interior constitution of the sun is liquid or gaseous. The truth seems to be that neither word is properly applicable. Without unduly stretching its original meaning, neither describes with even approximate accuracy the state of things prevailing there. The notion of "critical points" has been called in question, and may be inexact. But its introduction has at least had the not unimportant effect of abolishing an artificial distinction. It has shown the separation of the various states of matter to be merely provisional. Their characteristic qualities depend upon circumstances for their development or maintenance. At transcendental temperatures and pressures, the ordinary—probably (as Dr. Braun remarks) even the scientific—criteria of gases and liquids disappear; one state merges into the other; they interchange natures; so that we may indifferently regard the sun's interior as composed of vapours compressed, in despite of their almost boundless calorific energy, to the consistence of fresh putty, or of liquids restrained from boiling by the main force of the strata loaded upon them, while expanded to four or five times their ordinary bulk, and rendered internally mobile by the prodigious elevation of the temperature. An indisputable fact, however, and one fundamental to solar physical theory, is that the sun constitutes a vast reservoir of opposing, tremendously-constrained forces, the delicate equilibrium of which cannot be disturbed, however slightly, without producing effects on a commensurate scale.

Upon such inevitable disturbances Dr. Braun founds his *rationale* of the more obvious solar phenomena. The cooling of a body like the sun does not assuredly proceed quite equably. Local excesses of temperature lead to what we may call local revolts against gravity, signified by swift uprushes from great depths of inconceivably heated substances. These are the so-called "metallic prominences." But where the forces called into play lack energy to produce, or the attendant circumstances are not sufficiently favourable to permit, an actual outbreak, an uplifting of the unbroken photospheric surface takes place, and we see a "facula." "Hydrogen-prominences" mark a medium stage of vehemence. They originate from a commotion which primarily fails to outpass the limits of the chromosphere. The injection, however, into it of a prodigious bulk of metallic vapours rapidly heats the circumjacent hydrogen; it spouts upward in a stream which aërostatic pressure tends to perpetuate, and forms, high up above the sierra-edge of the agitated ocean it springs from, a rosy cloud conspicuous by reason of its incandescence.

But the connexion here indicated, to be significant, should be invariable, which is very far from being the case. Metallic intrusions into the chromosphere are by no means a condition *sine quâ non* to the development of quiescent prominences.

Solar theorists are now for the most part agreed that spots must be ascribed *immediately* to falls of relatively cool matter upon the photosphere; they divide on the question whether the initial disturbance comes from beneath or above it. Dr. Braun ranges himself on the side of those who assimilate outbursts on the sun to volcanic commotions on the earth. Uprushes of vividly glowing substances due to the temporary preponderance of heat over pressure are answered by downrushes of obscure absorbing vapours. Spots would thus be the reactive effects of flames or prominences. Their occurrence would be impossible without preliminary eruptions. But it is at least doubtful whether in this hypothesis the real sequence of events be not inverted. The whole tenor of Mr. Lockyer's observations goes to prove that the yawning of the photosphere leads the way as a symptom of its agitation. After a spot has begun to form, its flame and facular garnishings are added. M. Trouvelot has, indeed, often perceived a nascent spot to be completely masked by towering masses of faculæ; but it is none the less there, waiting to be disclosed. Prof. Young considers the appearance of a spot to be commonly heralded by manifest disturbances of the surface; but since the disturbance is evidenced as well by the presence of "pores" (which may be termed embryo spots) as of faculæ, his authority can scarcely be invoked as decisive of the question of precedence.

This is really the touchstone of the rival theories. Outbursts from the photosphere are either the cause or the consequence of the obscurations of it termed "spots." If the former, they should unfailingly and unmistakably take the initiative. But facts certainly warrant no such rigid conclusion. Admitting then the alternative order of connexion, we can understand that descents of relatively cool matter from coronal regions, perforating the photosphere, must overturn the precarious equilibrium of heat and gravity reigning beneath it, and may thus occasion the tumultuous heavings visible as faculæ, and the amazing escapes of imprisoned vapours challenging attention as flames.

Dr. Braun's papers on the constitution of the sun were published in *Natur und Offenbarung* previous to the appearance of Mr. Lockyer's "Chemistry of the Sun." Hence, perhaps, his complaint that the observed facts regarding the solar rotation had as yet been included in no "plausible" hypothesis. We cannot think him successful in his effort to supply the want.

Adventitious arrivals of nebular supplies from interstellar space play, as our readers are already aware, an indispensable part in the theory of planetary development sketched in the earlier chapters of the work now, in its concluding portion, engaging our attention. By their agency the primitive nebula was set whirling with a motion accelerated outward, its central sluggishness persisting throughout, and modifying the whole of its long history. The inequality is perpetuated within the body of the sun itself, the innermost parts of which may require, our author thinks, as much as forty or fifty days to complete a rotation performed at the equatorial surface in twenty-five. The quickening of angular rate continues with ascent into the solar atmosphere, until, in its higher

regions, the period is reduced to ten, if not to five or fewer days. All this, we are asked to believe, is the work of the latest of our nebular annexations, which, forming an equatorial girdle round the sun, partially, and in a degree varying inversely with latitude, communicated its own more rapid movement to the superficial layers of the globe it encompassed.

The process is not even yet concluded. What Dr Braun holds to be indisputable proof of atmospheric acceleration is derived from Prof. Young's spectroscopic measurement, in 1876, of the sun's rotational velocity. But this is to lay upon the observations in question a burden of inference heavier than they will bear. The rate of equatorial movement, as computed from the observed translation of spots, is 1·25 miles a second; it came out 1·42 miles from the Dartmouth College measures. Considering, however, the extreme minuteness of the entire displacements due to this speed, amounting, for the D lines, to but $\frac{1}{7}$ of the interval between them, the discrepancy is hardly surprising; and it is well known that, in this particular class of determinations, errors lie almost always on the side of excess. Prof. Young himself, it is true, was "inclined to think" that his result betrayed an actual sweeping forward of the absorbing layers over the underlying surface; but even were the fact established, we should expect to find for it a cause less remote than the inrush of a nebula uncounted millions of years ago. Undoubtedly, however (so far we are in agreement with Dr. Braun), that cause would be found to be closely connected with the anomalies of the sun's rotation.

As regards the distribution and periodicity of spots, we are in the present work offered simple and avowed conjectures at which we need only glance in passing. The nebulous swathing not yet completely incorporated with the sun's mass impedes, and has during past ages still more effectually impeded, equatorial radiation. Hence, cooling has, in polar tracts, penetrated further into the interior, with the result of generating an internal spheroidal surface at which temperature-gradients attain a maximum, and from the middle latitudes of which special facilities are afforded for eruptive outbreaks.

But this device is assuredly not a practicable one. The highly artificial arrangement it establishes could not endure one hour. Convection-currents would speedily and without ceremony abolish it. Indeed, augmented radiation from near the poles (which is equivalent to more rapid cooling), besides being contradicted by observation, might be expected to produce just the opposite effect of intensifying the disturbances attendant on cooling. Spots and flames should then, on the hypothesis advanced, be transferred from their "royal" zones to the polar calottes.

Heat-pulsations in a period of 11½ years, occasioned, perhaps, by a slow mechanical oscillation of the sun's volume, the progressive contraction of which may be conducted rhythmically, or by regular alternations of shrinking and swelling, are invoked (certainly under every reserve) to solve the puzzle of the sunspot cycle. The difficulties attending what might be called the "disturbed thermal equilibrium" theory of solar phenomena could not be more forcibly illustrated than by the straits to which it reduces its advocates.

The study of coronal appearances compels our author to take refuge in the unassailable stronghold of electricity. We are far from asserting that he is not fully justified in this measure: the circumstances indeed seem to prescribe it; yet it is always felt to be a desperate one, for the reason that it lands us, almost completely, in the region of the unknown. It is right to add that Dr. Braun is at all times evidently loth to separate from the company of ascertained facts and laws. He advances without them only where their escort cannot be made available.

A. M. CLERKE.

MUSIC IN NATURE.

IN the February number of *Longman's Magazine*, there is a remarkable article "On Melody in Speech," by Mr. F. Weber, Resident Organist of the German Chapel Royal, St. James's Palace. The object of the writer is more comprehensive than his title expresses, for he says in his opening paragraph, "There is an infinite variety of interesting and pleasing sounds in Nature's music around us that may be noted by an attentive ear." This may be readily granted; but Mr. Weber goes on, "These sounds are mostly melodious and harmonious, or *in some harmonious connexion, and form exact intervals and chords*."

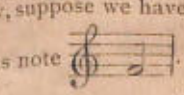
This last sentence is the point of the article. Mr. Weber is not expressing himself figuratively: he writes as a musician, and he distinctly asserts that many of the sounds spontaneously produced in Nature are truly MUSIC in the musician's, and not the poet's, sense of the term. To illustrate this assertion he has taken the trouble to identify and write down, in actual musical notes, the musical passages which he considers he has recognized in a great variety of these natural sounds, and so has challenged the public judgment on the accuracy of his theory.

Now, Mr. Weber is a gentleman of eminence in his profession, and what he says deserves attention. It is easy to say that he has given his imagination too much play in his supposed identifications; but it seems to me the subject ought to be approached from a more comprehensive point of view. The question is, Do such sounds or series of sounds constitute music? or do they not? And if not, why not? If Mr. Weber is wrong, it is probably because he has formed too hasty a view of what music really is; and this is a point that requires serious discussion.

Mr. Weber is not the first who has had this idea. Half a century ago, Gardiner, of Leicester, also a clever musician, published a book called, I think, the "Music of Nature," in which he wrote down musical passages professedly representing a vast number of natural sequences of sounds. There are many other persons, who, while they would not go to the same length as Mr. Weber or Gardiner, still believe that music may be found in the sounds of Nature, and it is worth while to see what grounds there are for such a belief.

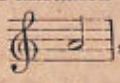
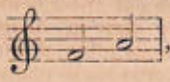
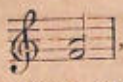
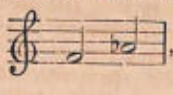
Music, in its modern form, is a very complicated structure, combining many elements, such as melody, harmony, counterpoint, tonality, measured time, rhythm, form, expression, tone-colour, and so on. But no one will suppose that the combination of all these is necessary to make what may be strictly called music. We must begin at the other end, and ask what music is if reduced to its simplest possible form? What are the fewest and least conditions absolutely necessary to constitute music, *i.e.* to give the name of music to a combination of sounds?

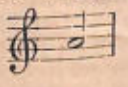
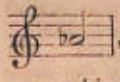
In the first place, we must have the proper material, namely *musical sounds*, and we must be particular that the sounds are really of a musical character. I am not going into acoustics. I need only say that the most essential quality necessary to give a sound this character is that it must have a *fixed and definite pitch*. A sound that is wavering and indefinite, like the sighing of the wind, or the *portamento* of a voice or violin, though it may be loosely said to be musical, is not strictly a "musical sound." It cannot be defined by the number of its vibrations, it cannot be expressed in any musical notation, and it cannot be used to form musical structure. For this purpose a sound, though it may be short, must be perfectly definite.

Now, suppose we have a sound of this kind, producing say this note  . Does the sounding of this note of itself constitute music? We must say No; for the

reason that music is an artistic *structure*, which cannot consist of one sound only. We must have other sounds to build up with it. We should hesitate, practically as well as theoretically, to give the name of music to a monotone.

Supposing, then, we add another sound, differing in pitch. Will these two sounds, heard either together or in succession, constitute music? The answer depends on the relation which the pitches of the two sounds bear to each other. For reasons which can be explained, but which it is unnecessary to go into here, it has been settled, by the universal concurrence of all nations who have made music an artistic structure, that the sounds to be used therein may not be chosen at random, but shall only be such as bear certain defined relations of pitch to each other. These relations have varied at different times and among different peoples; in our case they refer to our acknowledged musical *scales*.[1] Hence the answer to our question will be that if the two sounds are related to each other in a way accordant with our scales (but not otherwise) they may be used to form a part of our artistic musical structure, and do constitute elementary music.

Thus, suppose the second sound to vibrate quicker than the first one in the proportion of 5 to 4, or so near to this ratio that it may be mistaken by the ear for it. This will produce the note  , having a relation to the former acknowledged in our scales. Then the two notes sounded successively  , or together  , will constitute music; the former being an element of melody, the latter of harmony. Similarly in the proportion of 6 to 5,  , the same thing may be said.

But if we suppose the vibrations of the second sound to have the ratio to those of the first one of 49 to 40, giving a note about half way between  and  , we should decline to acknowledge the succession or combination as music according to our understanding of the term, though it might be so in the systems of the Greeks or of other nations.

We arrive, therefore, at the conclusion that the essential feature of music, its minimum component, must be a combination of sounds of different pitches, these pitches being moreover strictly fixed and defined, and their relations to each other corresponding to certain series agreed on and adopted as standard musical scales. Such combination will of itself constitute music; we may add all sorts of other features; but without the above essential foundation we cannot have music, in an artistic point of view.

This definition will enable us now to inquire whether or to what extent "music" in this sense is actually to be met with in Nature, or in the sources mentioned by Mr. Weber.

To begin with, the natural production of the first requisite, *i.e.* notes of fixed and definite pitch, is not frequent. Most sounds naturally produced are uncertain and wavering; precision in pitch of a sound always conveys an impression of artificiality, of its being, in fact, made purposely musical. No doubt natural sounds of definite pitch, even long sustained, do occur. The nightingale is remarkable for a beautiful long steady holding note; a quail gives three notes successively, at the same definite pitch; other birds, and occasionally some animals, give short notes clearly defined; and such notes may even be produced by inanimate causes of steady action, as a waterfall, or any substance naturally set in elastic vibration. All these, however, are exceptional.

But if there is this difficulty in getting one musically-defined note, it will be still harder to find in any natural source the occurrence of two or more such notes that are musically related to each other. The case that will probably first occur to one's mind is the song of our old friend the cuckoo, who has been immortalized by Beethoven as a musical performer. There was some time ago a controversy as to the true notes of the cuckoo, in which some eminent musicians took part; but I fancy the case is the same as the well-known fable of the chameleon: each of the witnesses was right, but the true solution of the question escaped them all. The facts would appear to be as follow.

The cuckoo gives out successively two very distinct strictly musical sounds, and his vocal organs are so proportioned that the interval between them may vary from about two semitones to five. This interval changes with the bird's age. Early in the season it is at its smallest. On May 5 last I heard on the Monte Sacro, at Orta, in Italy, a bird with a splendid voice give notes about a tone apart: late in the season I have heard them fully at a fourth interval. Of course, therefore, as the change goes gradually on they will pass through a minor third and then through a major third (which is Beethoven's interval); but the *exactitude* is all a matter of chance. I have noticed sometimes that the interval lay between a minor and a major third, so that I could not decide to which it inclined. Hence the idea that the cuckoo gives, by predetermined arrangement, any interval recognized in musical scales, is quite a fallacy.

Mr. Weber asserts that "all the animals on land, quadrupeds and bipeds, have their characteristic voices and calls in distinct intervals. The cow gives a perfect fifth and octave or tenth; the dog barks in a fifth or fourth; the donkey brays in a perfect octave; the horse neighs in a descent on the chromatic scale; the cat in a meek mood cries in a fifth, or, when excited, in a major third; proud chanticleer crows in the diminished triad and [in the diminished] seventh chord." All these Mr. Weber writes down; but I fear that more careful observation would never substantiate the idea that the intervals do really correspond with those of our very artificial scales, otherwise than occasionally by pure accident.

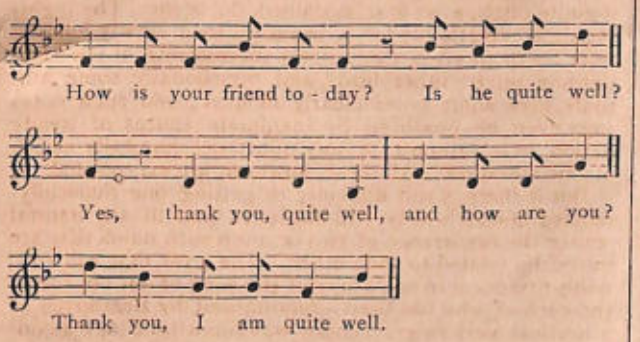
But he makes also inanimate objects conform to our musical system.[1] The wind, he says, sings certain melodies, precisely according to our scales and to our notation, in which he writes them down. This is even more incredible.

Then we come to the main topic of Mr. Weber's essay, "Melody in Speech." He gives a large number of examples, written out fully in musical notation, professing to represent natural language in different varieties: casual expressions, salutations, questions and answers, conversations, and winding up with a speech of an Oxford Professor and a sermon of an English Bishop. These representations are very curious; but it is of course open to any musician to observe for himself the language he hears every hour of the day, and to judge how far it corresponds with this alleged musical character. There is, however, a more conclusive form of test, by what we

[1] Further explanations on this point will be found in my "Philosophy of Music," Second Edition (London: Trübner; and Novello and Co., 1887).

[1] Of course we must exclude from consideration the natural harmonics of vibrating bodies, with which our system is purposely connected. Some really musical effects of the wind may be exceptionally produced in this way.

may call the reverse method. Take the following example given by Mr. Weber :—



Now, let anyone execute this as written ; and then ask the bystanders whether it represents the manner in which the sentences would be expressed in talking. I think they would say, "That is not *talking*; it is *singing*;—it is opera recitative." And so on for all the examples. Mr. Weber seems to ignore the essential difference between the two ; *i.e.* the absence, in speaking, of the requisites to constitute elementary music. In the first place, in natural speaking there are no musical sounds, properly so called, inasmuch as it is scarcely possible for a hearer to catch notes of fixed and definite pitch ; the voice tends constantly to wander about in a vague and indefinite way ; the vocal cords, under the natural prompting, have no tendency to remain at a permanent degree of tension ; to keep them so there must be an intentional artificial effort ; and hence the occurrence, in speaking, of a monotone long sustained is unusual, and has a distinct musical character.

But if we could occasionally trace, in speaking, sounds of definite pitch, we should find the other requisite—namely, definite relations between them—wholly wanting. The idea that a person, when he speaks naturally (be he musician or no musician), has our scales in his mind, and makes his voice conform to them, is altogether untenable. The moment this is done it ceases to be natural speaking, and becomes designed musical performance.

Here, therefore, we find a most positive and unmistakable distinction between natural speech and music. The person using his voice must, for the latter purpose, do two things which require predetermination and effort ; and which are therefore essentially artificial and not naturally prompted : he must execute tones of well-defined pitch, and he must give them certain definite pitch relations with each other.

Mr. Weber has, in this matter, unintentionally approached very nearly a matter much debated among philosophers, namely, the Origin of Music. Mr. Herbert Spencer, some years ago, propounded a theory that music had taken its rise from the inflections of the voice in ordinary language. This has been strongly controverted ; but Mr. Weber goes further, and asserts that ordinary language is actually *music ready made!*

He has given, as a part of his illustrations, some interesting examples of street cries. These have no doubt a really musical character ; but it is odd that he did not see the distinction between them and ordinary talking—namely, that such cries and calls are *purposely sung*, and not spoken in the natural way. Of course, conforming to this condition, they can be correctly written down, and reasoned upon as specimens of true musical melody.

There is a useful moral to be drawn from all this ; namely, a regret at the discouragement which is given to the study of the theory of music in a scientific point of view. Grove's Dictionary declares this to be useless to practical musicians, and so it is as long as they confine themselves to practice ; but when they meddle with theory the want of it must instantly make itself known. It is no disrespect to Mr. Weber to say that his article shows the loose way in which such matters are too often regarded. No one who has taken the very first steps in the philosophical study of the structure of music could entertain the idea that the sounds naturally emitted by birds, cows, or dogs, formed by the howling of the wind, or used in conversation, were entitled to be called either "music" or "melody."

WILLIAM POLE.

THE BRITISH MUSEUM (NATURAL HISTORY BRANCH).

AMONGST the many new and interesting features connected with the British Museum (Natural History), Cromwell Road, has been the opening of a new gallery to the public, containing the Historical and Type Collections in the Department of Geology and Palæontology, under the care of Dr. Henry Woodward, F.R.S., who has favoured us with the following account of the same.

Taking the exhibition cases in *chronological order*, the earliest is the "Sloane Collection." This is the most ancient portion of the Geological Collection, having formed a part of the Museum of Sir Hans Sloane, Bart., F.R.S., acquired by purchase for the nation in 1753.

The geological specimens are stated to have consisted "in what by way of distinction are called *extraneous fossils*, comprehending petrified bodies, as trees or parts of them ; herbaceous plants ; animal substances," &c., and reported to be "the most extensive and most curious that ever was seen of its kind." Until 1857 the fossils and minerals formed one collection, so that a large part of the Sloane Collection consisted probably of mineral bodies and *not organic*, but in any case only about 100 specimens of invertebrate fossils can now be identified with certainty as forming part of the original Sloane Museum. Each specimen in the Sloane Collection had originally a number attached to it, corresponding to a carefully prepared Manuscript Catalogue, still preserved, which contains many curious entries concerning the various objects in the Museum. In the course of more than 130 years, many of these numbers have been detached from the objects or obliterated in cleaning. But as all fossils at this early date were looked upon *merely as curiosities*, but little attention was paid to the formation or locality whence they were derived. Historically, the collection has immense interest to us, marking the rapid strides which the science of geology has made of late years, especially as regards its more careful and systematic methods of study.

The next collection in chronological order is the "Brander Collection," and is the earliest one in which types of named and described species have been preserved.

This collection was formed by Gustavus Brander, F.R.S., in the earlier half of the last century, and an account of the same, with eight quarto plates, was published in 1766, entitled, "Fossilia Hantoniensia Collecta, et in Musæo Britannico deposita." The descriptions of the species given in the work were written by Dr. Solander, one of the Officers of the British Museum. They were "collected in the County of Hampshire, out of the cliffs by the sea-coast between Christchurch and Lymington, but more especially about the cliffs by the village of Hordwell, nearly midway betwixt the two former places" (*op. cit.* p. 111).

Only a small number out of the original 120 figured specimens are now capable of being identified, the rest

having become, in the course of 120 years, commingled with the far more numerous and later Eocene Tertiary acquisitions, and so have lost their connexion with this admirable memoir. The engravings of the shells are equal to any modern published work descriptive of the fossils of the Eocene formation; but the names given by Dr. Solander are in many instances incorrect, according to our present knowledge of the genera of Mollusca.

The next series to which attention may be directed is the collection of Dr. William Smith. This collection, which was commenced about the year 1787, was purchased by the Trustees in 1816, a supplemental collection being added by Dr. Smith in 1818.

It is remarkable as the first attempt made to identify the various strata forming the solid crust of England and Wales by means of their fossil remains. There had been other and earlier collections of fossils, but to William Smith is due the credit of being the first to show that each bed of chalk or sandstone, limestone or clay, is marked by its own special organisms, and that these can be relied upon as characteristic of such stratum, wherever it is met with, over very wide areas of country.

The fossils contained in this cabinet were gathered together by William Smith in his journeys over all parts of England during thirty years, whilst occupied in his business as a land surveyor and engineer, and were used to illustrate his works, "Strata Identified by Organized Fossils," with coloured plates, quarto (1816; four parts only published): and his "Stratigraphical System of Organized Fossils" (quarto, 1817).

A coloured copy of his large geological map, the first geological map of England and Wales, with a part of Scotland, commenced in 1812 and published in 1815—size 8 feet 9 inches by 6 feet 2 inches wide, engraved by John Cary—is exhibited in the last wall-case on the right-hand side of this gallery, at the north end. It is well worthy of careful inspection.

William Smith was born at Churchill, a village of Oxfordshire, in 1769; he was the son of a small farmer and mechanic of the same name, but his father died when he was only eight years old, leaving him to the care of his uncle, who acted as his guardian. William's uncle did not approve of the boy's habit of collecting stones ("pundibs" = *Terebratulæ*, and "quoit-stones" = *Clypeus sinuatus*); but seeing that his nephew was studious, he gave him a little money to buy books. By means of these he taught himself the rudiments of geometry and land-surveying, and at the age of eighteen he obtained employment as a land surveyor in Oxfordshire, Gloucestershire, and other parts, and had already begun carefully and systematically to collect fossils and to observe the structure of the rocks. In 1793 he was appointed to survey the course of the intended Somersetshire Coal-Canal, near Bath. For six years he was the resident engineer of the canal, and, applying his previously-acquired knowledge, he was enabled to prove that the strata from the new red marl (Trias) upwards followed each other in a regular and orderly succession, each bed being marked by its own characteristic fossils, and having a general tendency or "dip" to the south-east.

To verify his theory he travelled in subsequent years over the greater part of England and Wales, and made careful observations of the geological succession of the rocks, proving also, by the fossils obtained, the identity of the strata over very wide areas along their outcrops.

His knowledge of fossils advanced even further, for he discovered that those *in situ* retained their sharpness, whereas the same specimens derived from the drifts or gravel-deposits were usually rounded and water-worn, and had reached their present site by subsequent erosion of the parent-rock.

In 1799, William Smith circulated in MS. the order of succession of the strata and embedded organic remains found in the vicinity of Bath. His large geological map of England and Wales is dated 1815. On June 1, 1816, he published his "Strata Identified by Organized Fossils," with illustrations of the most characteristic specimens in each stratum (4to). In 1817 he printed "A Stratigraphical System of Organized Fossils," compiled from the original geological collection deposited in the British Museum (4to). In 1819, he published a reduction of his great geological map, together with several sections across England. These have just been presented to the Museum by Mr. Wm. Topley.

Mr. Smith received the award of the *first* Wollaston Medal and fund in 1831, from the hands of Prof. Sedgwick, the President of the Geological Society, "as a great original discoverer in English geology, and especially for his having been the first, in this country, to discover and teach the identification of strata, and to determine their succession by means of their embedded fossils."

In June 1832, the Government of H.M. King William the Fourth awarded Mr. Smith a pension of £100 a year, but he only enjoyed it for seven years, as he died August 28, 1839. In 1835, the degree of LL.D. was conferred upon Mr. Smith by the Provost and Fellows of Trinity College, Dublin. The highest compliment paid him was that by Sedgwick, who rightly named him "the Father of English Geology."

The bust above the case which contains William Smith's collection is a copy of that by Chantry surmounting the tablet to his memory in the beautiful antique church of All Saints at Northampton, where his remains lie buried.

We come next to a collection, the very name of which betrays the antiquity of its origin. It is known as "Sowerby's Mineral Conchology."

This collection was begun by Mr. James Sowerby, prior to 1812, and continued by his son, Mr. James de Carle Sowerby, during the preparation of their great work entitled, "The Mineral Conchology of Great Britain," which appeared in parts, between June 1812 and December 1845, and forms six volumes octavo, illustrated with 648 plates.

The value of the work consists in the fidelity and accuracy of the figures given, and also in the fact that most of the specimens drawn are here named and described for the first time. They comprise fossils from all parts of England and from every geological formation. The small green labels mark the specimens actually figured in the work. The collection was purchased by the Trustees from Mr. J. de Carle Sowerby, January 1861. It may be interesting to record that many of the later parts were illustrated by plates drawn by the late Mr. J. W. Salter, for so many years palæontologist to the Geological Survey. When a youth, Salter was apprenticed to Mr. J. de Carle Sowerby, who was at that time both a naturalist and an engraver. The youthful apprentice afterwards married his master's daughter, and became, as is well known, one of the most brilliant palæontologists in this country.

Another curious but small series represents the "types," or "figured specimens" of "König's *Icones Fossilium Sectiles*."

This illustrated work, on miscellaneous fossils in the British Museum, was prepared by Mr. Charles König, the first Keeper of the Mineralogical and Geological Department, after its separation from the general Natural History Collections in 1825.

The engravings are rough, but characteristic, and the first "century" (or 100 figures of fossils), is accompanied by descriptions; the plates of the second "century" have names only, but no descriptions are published with them.

A far more important collection is that known as "The Gilbertson Collection."

In 1836, Prof. John Phillips published Vol. II. of his "Illustrations of the Geology of Yorkshire," which is devoted to the "Mountain Limestone District." In the

introduction he writes as follows :—"My greatest obligation is to Mr. William Gilbertson, of Preston, a naturalist of high acquirements, who has for many years explored with exceeding diligence a region of mountain limestone, remarkably rich in organic remains. The collection which he has amassed from the small district of Bolland is at this moment unrivalled, and he has done for me, without solicitation, what is seldom granted to the most urgent entreaty; he has sent me for deliberate examination, at convenient intervals, THE WHOLE OF HIS MAGNIFICENT COLLECTION, accompanied by remarks dictated by long experience and a sound judgment." He (Gilbertson) had proposed to publish on the *Crinoidea* himself, but his sketches, as well as his specimens, were all placed at Prof. Phillips's disposal. Phillips adds: "An attentive examination of this rich collection rendered it unnecessary to study minutely the less extensive series preserved in other cabinets; . . . . *most of the figures of fossils are taken from specimens* in Mr. Gilbertson's collection, because these were generally the best that could be found."

The Gilbertson Collection was purchased for the British Museum in 1841.

The collections which follow mark a distinct era in the annals of geological science. Some forty-seven years ago a little Society was founded by a few London geologists—namely, Dr. J. Scott Bowerbank, F.R.S., Searles V. Wood, Prof. John Morris, Alfred White, Nathaniel T. Wetherell, James de Carle Sowerby, and Frederick E. Edwards—for the purpose of illustrating the Eocene Mollusca, and entitled the "London-Clay Club." They met at stated periods at each other's houses, and after a time they said, "Why should we not illustrate all the fossils of the British Islands, and from every formation?" No sooner proposed than a Society was founded, called the "Palæontographical Society," in the year 1847 (just forty years ago). The first volume, issued in that year, was "The Crag Mollusca, Part I., Univalves," by Mr. Searles V. Wood (with twenty-one plates).

The "Searles Wood Crag Collection" was commenced in 1826, and occupied about thirty years in its formation. It represents the Molluscan fauna of the Red and Coralline Crags of the neighbourhood of Woodbridge, and from Aldborough, Chillesford, Sudbourne, Oxford, Butley, Sutton, Ramsholt, Felixstowe, and many other localities in Suffolk, also from Walton-on-the-Naze in Essex. The specimens so collected were employed by Mr. Searles Wood in the preparation of his Monograph on the Crag Mollusca, published by the Palæontographical Society (1848-61); with supplements in 1871, 1873, and 1879, illustrated by seventy-one quarto plates. Each figured specimen is indicated by having a small green label affixed to it.

A geological description of the Crag formation by Mr. S. V. Wood, Jun., and Mr. F. W. Harmer, was issued by the Palæontographical Society in 1871 and 1873.

The collection was presented by Mr. S. V. Wood to the British Museum, January 1856, and a supplementary collection was given by Mrs. Searles V. Wood in 1885.

The next "Palæontographical Collection" is of nearly equal antiquity and fully of equal merit. It is the Eocene Molluscan Collection formed by the late Mr. Frederick E. Edwards, about the year 1835, and was continually being added to, until a few years before his death, which happened in 1875. It was acquired for the nation by purchase in 1873.

Originally intending to illustrate the fossils of the London Clay, Mr. Edwards extended his researches over the Eocene strata of Sussex, Hampshire, and the Isle of Wight, where, assisted by Mr. Henry Keeping, he made the most complete collection ever attempted by any geologist, and it still remains unrivalled.

Mr. Edwards contributed six memoirs to the Palæontographical Society, 1848-56, also separate papers to the *London Geological Magazine*, 1846, the *Geologist*, 1860, and the *Geological Magazine*, 1865, descriptive of the Eocene Mollusca, in his collection.

Mr. S. V. Wood continued the work for Mr. Edwards, describing and figuring the "Eocene Bivalves" in the annual volumes of the Palæontographical Society for 1859, 1862, 1870, and 1877. Each specimen which has been figured is specially marked.

About 500 species have been described and figured, but the collection is very rich in new and undescribed forms.

The last collection is that of a naturalist who devoted his entire life to the study and illustration of a single class of organisms, namely the Brachiopoda. It was formed by the late Dr. Thomas Davidson, F.R.S. (of 9 Salisbury Road, West Brighton, and Muir House, Midlothian), between the years 1837 and 1886, with the object of illustrating his great work on the "British Fossil Brachiopoda," published by the Palæontographical Society, in six large quarto volumes between the years 1850 and 1886, comprising 2290 pages of text, and 234 plates, with 9329 figures, and descriptions of 969 species.

Dr. Davidson was also the author of the Report on the recent Brachiopoda collected by H.M.S. *Challenger* (vol. i. 1880); of the article "Brachiopoda," in the "Encyclopædia Britannica," ninth edition, 1875; of a Monograph of "Recent Brachiopoda" (Trans. Linnean Society, 1886 and 1887), and of more than fifty other separate memoirs mostly bearing upon Brachiopoda both recent and fossil, printed in the Transactions and Journals of the various learned Societies, &c.

His collection, both of Recent and Fossil Brachiopoda, together with all Dr. Davidson's original drawings, his numerous books and pamphlets, were presented by him to the British Museum through his son Mr. William Davidson, February 1886. By his direction the entire collection of recent and fossil species are to be kept together in one series, for the convenience of reference of all men of science who may wish to consult the same.

NOTES.

ON Tuesday the Technical Instruction Bill was read a second time. The second reading having been moved by Sir W. Hart Dyke, Mr. S. Leighton proposed an amendment which would have led to the rejection of the measure, on the ground that a new change ought not at present to be imposed on ratepayers. The amendment was negatived; but in dealing with it Mr. Goschen and Mr. W. H. Smith found it necessary, as Mr. Mundella complained, to adopt a very "apologetic" and "persuasive" tone. The fact seems to indicate that a good many members of the House of Commons do not even yet realize that an adequate system of technical instruction is absolutely necessary to enable this country to hold its own in the industrial and commercial warfare of the present age.

IN a memorandum on the Scotch Technical Schools Bill, which has been introduced by the Lord Advocate, it is stated that, "as there is a School Board in every parish and burgh in Scotland, it is unnecessary to extend the powers of the Bill to any other local authority." The Bill is not to take effect in any parish or burgh until after the triennial election of a new School Board next year; and the resolution of a Board to establish a technical school requires confirmation at a second meeting of the Board, and also by the Scotch Education Department. While the subjects to be taught in the technical schools will be determined by the Department of Science and Art, the schools will in other respects be under the Scotch Education Department. No scholar will be admitted into a technical school until he has passed the fifth standard, which in Scotland frees from the obligation to attend an elementary school. The proposal that adults above the age of twenty-one shall not be

eligible for admission to technical schools maintained out of the rates ought to be rejected. If adults desire to attend these institutions, there seems to be no good reason why their wish should not be gratified.

On Wednesday, the 3rd inst., the Local General Committee appointed to make arrangements for the Manchester meeting of the British Association assembled in the Town Hall, Manchester, to receive the report of the Executive Committee. Mr. Alderman Goldschmidt presided, and there was a numerous attendance. In the report, which was adopted, the Executive Committee gave a most satisfactory account of the efforts which had been made to secure the success of the meeting. Attention was especially called to the number of eminent foreign visitors who are expected to be present, and it was definitely announced that among these visitors will be the Emperor of Brazil, who has been a member since 1871. Of the members of the Association more than a thousand have already expressed their intention of attending the meeting, and many ladies and gentlemen have sent in their names as new members or associates. The Executive Committee pointed out that at previous meetings of the Association a large proportion of the visitors had received offers of hospitality, and they expressed a confident hope that Manchester would not fail in this respect. Numerous excursions have been arranged for Saturday, September 3, and Thursday, September 8; and offers of hospitable entertainment have been received and accepted in connexion with several of these. The Duke of Devonshire has invited a party to Bolton Abbey; and invitations have been received from the Duke of Westminster to visit Eaton Hall; from W. H. Foster, Esq., and Sir Thomas Storey, to visit Hornby Castle and Lancaster; from W. Morrison, Esq., M.P., to visit Malham and Gordale; from the Rev. M. Farrer, to visit Ingleborough; and from the Directors of the London and North-Western and Lancashire and Yorkshire Railway Companies to visit their Locomotive Works at Crewe and Horwich. Hospitable offers have also been received in connexion with excursions to Northwich, Buxton, Stonyhurst, Tatton, Macclesfield, Gawsworth, Clitheroe, the Liverpool Docks, the Longdendale Reservoirs, and Worsley. The Liverpool Marine Biological Committee have arranged for a day's dredging expedition; an invitation has been received for a visit to the Isle of Man at the close of the meeting; and several other excursions have been organized. Many of the principal works and mills will be open for inspection during the meeting.

The following is the programme of the Manchester meeting of the British Association:—Wednesday, August 31, President's Address, in the Free Trade Hall, at 8 p.m. Thursday, September 1, Sectional Meetings, 11 a.m. to 3 p.m.; *conversazione* at the Royal Jubilee Exhibition, by invitation of the Executive Committee of the Exhibition, 7.30 to 11 p.m. Friday, September 2, Sectional Meetings, 11 a.m. to 3 p.m.; lecture by Prof. H. B. Dixon, F.R.S., on "The Rate of Explosions in Gases," in the Free Trade Hall, at 8.30 p.m. Saturday, September 3, Sectional Meetings, 11 a.m. to 1 p.m.; excursions; lecture to working men by Prof. George Forbes, F.R.S., on "Electric Lighting," in the Free Trade Hall, at 8 p.m. Monday, September 5, Sectional Meetings, 11 a.m. to 3 p.m.; lecture by Colonel Sir Francis de Winton, K.C.M.G., R.A., on "Explorations in Central Africa," in the Free Trade Hall, at 8.30 p.m. Tuesday, September 6, Sectional Meetings, 11 a.m. to 3 p.m.; *conversazione* at the Town Hall, by invitation of the Right Worshipful the Mayor of Manchester. Wednesday, September 7, General Meeting in the Chemistry Lecture Theatre of Owens College, at 2.30 p.m. Thursday, September 8, excursions.

The Royal Archæological Institute has had a very successful series of meetings in Salisbury and the neighbourhood. On Tuesday, the last day of the Congress, the members were entertained at luncheon at Rushmore Park by General Pitt-Rivers, the President. He conducted the party to Woodcutts, where he has lately discovered the remains of a Romano-British village. The skeletons dug up show that the people, whoever they were, that inhabited this village were very inferior in stature, the males being on an average only 5 feet 2 inches in height, and the women only 4 feet 10 inches. General Pitt-Rivers has in his museum a very large collection of articles that must have been in daily use among them, including coins, both British and Roman, brazen, silver, and gilt fibulæ, knife-handles, chains, tweezers, bracelets, locks, padlocks, flint arrow-heads, fish-hooks, and horse-shoes, to say nothing of a bowl of Samian ware and the bricks of a hypocaust. The members, having seen all that General Pitt-Rivers had to show them, agreed that he "had kept the most interesting of all the days and the best of his treasures for the last."

Science announces the death of Dr. Charles Rau, Curator of the Archæological Department of the National Museum at Washington. America owes to him the excellent arrangement of the large prehistoric collections at Washington. His writings on American archæology, contained in the annual reports of the Smithsonian Institution and in various journals, and his recent work, "Prehistoric Fishing in Europe and North America," secured for him a high place among American archæologists.

The subscription made by some friends of the late Dr. Walter Flight, F.R.S., has resulted in a sum of £317. This amount has been handed over to Mr. Basil Martineau, of Hampstead, and Mr. Henry Basset, F.C.S., of Barnsbury, who have kindly consented to act as trustees on behalf of Mrs. Flight. The Committee of the Fund, anxious to avoid expense, trust that the subscribers will excuse the printing and circulating of individual notices to the above effect.

We have more than once referred to the scheme promoted from Kew for the establishment of minor Botanical Gardens in the several West Indian Islands. The gardens of the Windward Islands are to be in correspondence as far as relates to the supply of useful plants, and information concerning them, with the chief Botanical Department in Jamaica. The Island of Grenada has been the first to take advantage of the new scheme. Its newly established Botanic Garden was opened to the public on July 18. Barbadoes has recently recorded its adhesion. We learn with much regret that the members of the group of Leeward Islands decline at present to take any part in the scheme.

The *Annalen der Hydrographie und maritimen Meteorologie* of the German Hydrographic Office for July contains the first part of the discussion of the daily synoptic weather charts of the North Atlantic Ocean and adjacent parts of the continents for the autumn of 1883, viz. for the months September to November (see NATURE, June 16, p. 159), thus commencing for the period undertaken by the Meteorological Council. The discussion is accompanied by nine charts, showing clearly (1) the paths of the barometric minima, (2) the position and changes of the barometric maxima, and (3) the mean position of the isobar of 765 mm. (30·119 inches) for fifteen periods of three to nine days each. The same number also contains a comprehensive discussion of the rainfall of Mauritius and neighbouring portions of the Indian Ocean, compiled from the observations published by Dr. Meldrum and all other available sources, by Dr. W. Köppen.

The volume of "Hourly Readings" of the self-recording instruments at the Observatories in connexion with the Meteorological Office for the year 1884, the last part of which is just published, contains two elaborate appendixes:—(1) The harmoni

analysis of the diurnal range of air-temperature at seven Observatories for each month of the twelve years 1871-82. The numerical results have been obtained directly from the continuous photographic records by means of Sir William Thomson's harmonic analyzer. A comparison of the monthly means obtained from the machine with those got by calculation shows that the results obtained by the former are as accurate as those obtained very much greater labour by the latter method. (2) Tables and formulæ, by Lieut.-General R. Strachey, R.E., C.S.I., to facilitate the computation of harmonic coefficients in the form $p_1 \cos n\,15°$, &c., and in the form $P \sin (n\,15° + \tau°)$. Tables are calculated for the coefficients of p and q from hourly values, harmonic coefficients from five-day means, non-periodic corrections, and multiples of the usual sines. Tables of multiples of the natural and logarithmic sines, &c., have been previously published, but we know of nothing which at all compares in detail and usefulness with the tables now in question, for the calculations which they are intended to simplify.

DR. VON LENDENFELD'S account of his investigations in the Australian Alps, published in Ergänzungsheft No. 87 of *Petermann's Mitteilungen* (see NATURE, July 21, p. 283), contains some interesting observations on the meteorology of that district, especially with respect to rainfall. He found that the mountainous part of the continent has much more rain than any other part of temperate Australia south of the zone of tropical rains. At Hiandra, to the north of the Kosciusko group, it amounts to 61 inches; while between the mountains and the coast the amount is small, being only 18 inches at Cooma. Some places to the west of the mountains, and still in sight of them, suffer much from want of water. Generally speaking there appears to be no connexion between the weather on the Alps and that on the coast. On the latter most rain falls in autumn, while on the mountains the spring is especially wet. In the middle of summer (January and February) rainfall is least both on the mountains and on the coast. In the lowlands precipitation always falls as rain, while on the mountains snow falls at all times of the year, and it never rains in winter. The amount of dew is exceedingly great, but as this is only taken into account in the total amount of the rainfall, the climate of the mountains appears drier than it really is. There are not sufficient observations to determine the temperature and wind conditions accurately, and these can only be estimated from the behaviour of the snow. At heights exceeding 1000 metres (3280 feet) the snow lies for a month or two, and above 2000 metres it is met with in places, even in the height of summer. Snow-drifts are found exclusively on the eastern slopes, which clearly proves the prevalence of westerly winds in winter.

IT has been arranged at the Hong Kong Observatory that Mr. Knipping will, as heretofore, investigate typhoons within the area of the Japanese weather-maps, as well as north and east of that area. Father Faura will investigate the typhoons in their passage across the Philippine Archipelago, and those that approach very near the coast of Luzon. Dr. Doberck will investigate typhoons at sea south and west of Mr. Knipping's district, from information collected from men-of-war and merchant vessels, and typhoons in China from the facts recorded in the returns of the Imperial Chinese Maritime Customs.

A NOVEL series of voltaic combinations, in which solutions of alterable compounds are substituted for the attackable metals, has lately been investigated by Dr. Alder Wright and Mr. C. Thompson (Journ. Chem. Soc., August 1887). The chief feature of the new cells consists in the replacement of the zinc or its equivalent by a plate of carbon, platinum, or other conducting but unchanged substance, immersed in a solution of some easily oxidized or chlorinated compound, and opposed to a similar plate in contact with the solution of a substance capable of being readily deoxidized or dechlorinated. The plate in contact with the oxidizable fluid acquires the lower potential or becomes the negative pole, while the other plate takes the higher potential and forms the positive pole with regard to the outer circuit. An almost endless variety of new combinations may thus be employed, some of which may be expected to develop considerable energy. A convenient cell, of electromotive force 1·5 volt, consists of a U-tube, into one limb of which is poured a solution of sodium sulphite, while a solution of "chromic liquor," that is, a mixture of sulphuric acid and potassium bichromate solution, is run into the other, a little moderately concentrated sulphuric acid being previously placed in the bend to prevent too rapid diffusion of the two. On placing the two platinum plates in their respective solutions and completing the external circuit by a wire, a constant current is maintained, owing to oxidation of the sulphite to sulphate, and reduction of the chromic acid to chromium sulphate. In all the cases examined the currents were remarkably steady, and capable of performing measurable amounts of electrolytical work.

SIR PHILIP MAGNUS has presented his Report on the technological examination held in 1887, under the direction of the City and Guilds of London Institute for the Advancement of Technical Education. A special feature of this year's examination is that forty-eight candidates were examined in nine subjects, under the direction of the Institute, in New South Wales. Examinations were held in Sydney, Bathurst, and Newcastle. The question papers were sent out to Sydney, and the answers of these colonial condidates were forwarded to London, where they were examined together with those of other candidates, the date of the examination having been so timed as to render this arrangement possible. Sir Philip Magnus considers that the increase in the total number of candidates examined and of those who have passed is satisfactory. In 1886, 4764 candidates were examined, of whom 2627 passed; in 1887, 5508 were examined, of whom 3090 passed. The increase in the number of candidates in 1887 is 744 as against 796 in 1886. During the past session 365 classes were held in 121 different towns. Manchester heads the list of provincial centres from which the largest number of candidates have passed, the number being 183 as against 169 in the previous year. Next in order comes Glasgow with 169 as against 163, Leeds with 114 as against 81, Blackburn with 73 as against 10, Huddersfield with 69 as against 70, Belfast with 66 as against 74, Bradford with 63 as against 80.

THE results of the survey and last census of India are that the area of the peninsula of Hindostan is 1,382,624 square miles, and the population 253,891,821. Although immense tracts of country are annually cultivated, according to the most recent survey ten million acres of land suitable for cultivation have not as yet been ploughed. At the same time, 120 millions of acres are returned as waste lands.

ON July 21 the people of Nancy were astonished by the sudden appearance of an immense number of common ants, which were brought by a very strong wind. Most of the insects were wingless.

ACCORDING to the *Free Press* of Singapore, a work which has occupied much of the recent attention of those Government officials connected with the Land and Survey Departments of the Straits Settlements has been completed, and has been sent to England. This is a map of the Malay Peninsula, based upon one produced in 1879 under the direction of the Straits Branch of the Royal Asiatic Society, but altered and improved by subsequent exploration.

IN a recent number of the *Revue Scientifique*, M. Arnaudeau develops the idea of a double postal tube between Dover and Calais, to be suspended in air. Each tube should be

1 metre in diameter, and of thin metal, allowing the supports to be far apart, say 800 metres. In the tube, a train of ten to fifteen small waggons should run on rails on a floor, the motive power compressed and rarefied air actuating a piston. The lower part of one tube should hold telegraphic, and that of the other telephonic wires. The metallic foundation-piers, some of which should be as much as 70 metres high, should be of truncated-pyramid shape, and capable of floating at first, but gradually filled with masonry and water, and sunk to the bottom. These should support tall pillars having suspension-cables at the top. By the pumping out of the water, these piers could be raised and shifted if necessary.

In the Exhibition recently opened at Havre there is an interesting collection of specimens of poisonous fishes. Some are poisonous when eaten; others are merely venomous. Among the first are many Sparoids, a Tetrodon, and many *Clupea*, which are abundant near the Cape of Good Hope. In the Japan Sea is found a very peculiar Tetrodon, which is sometimes used as a means of suicide. It brings on sensations like those produced by morphia, and then death. Another interesting collection in the Exhibition is that of a number of Bacteria, and pathogenetic microbes. This collection was formed by Prof. Cornil, of Paris.

At the annual meeting of the Seismological Society of Japan, on May 27, Prof. S. Sekiya exhibited an interesting model of his own design, showing the motion of the ground at the time of an earthquake. The actual motion was magnified fifty times. At the same meeting, Prof. Milne read a paper on the effects produced by earthquakes upon the lower animals. Animals often show signs of alarm not only while an earthquake is going on, but before the shock is felt. Prof. Milne's friend, Mr. James Bissett, of Yokohama, testifies that thirty seconds before the first shock on the 15th of last January one of his ponies suddenly got up on its feet and pranced about in the stall, evidently terrified at the coming shake. A pony at Tokio was observed to act in a similar manner. Prof. Milne has had many opportunities, just before earthquakes, of confirming the fact that pheasants scream; and several observers have assured him that in like circumstances frogs suddenly cease croaking. Of all animals, geese, swine, and dogs are said to give the clearest indications of an approaching earthquake. It is said, too, that many birds show uneasiness, hiding their heads beneath their wings, and behaving in an unusual manner. Prof. Milne suggests that some of the lower animals may be sensitive to small motions which we do not notice. The terror manifested by intelligent animals like dogs and horses may be, he thinks, the result of their own experience, which has taught them that slight tremors are premonitory of movements more alarming. In the case of pheasants, frogs, and geese, alarm may be due solely to the tremors. Strange behaviour on the part of animals several hours or days before an earthquake Prof. Milne attributes for the most part to accidental causes. In volcanic districts, however, as he shows, it has sometimes happened that before an earthquake certain gases have emanated from the earth; and where this has occurred the smaller animals have not only been alarmed, but sometimes killed.

A violent shock of earthquake was felt on August 26 at Laghouat. It caused much consternation, these phenomena being very rare in the vicinity.

Early in December, an Exhibition of winter flowers, plants, and fruit will be opened at Mayence.

The locust plague has done much damage this year in Algiers. All the efforts of the authorities to cope with it have proved fruitless, and it is feared that the evil will be not less formidable next year, the eggs deposited being numberless.

The annual meeting of the North of England Institute of Mining and Mechanical Engineers was held on Saturday last in the building of the Newcastle Exhibition. Sir Lowthian Bell delivered his presidential address. Dealing with the progress of railroads and navigation, he pointed out that fifty years ago the tonnage sailing under the British flag might be taken at 750,000, of which a little above 50,000 consisted of steamers. By the end of 1885 this country possessed 3,456,562 tons of sailing ships, and 3,973,483 tons of steam vessels, making a total of 7,430,045 tons. Referring to compound engines, he said they all knew how the dangers attending the use of steam at a high pre sure had been met by the introduction of the compound system, in which, by the use of three cylinders, a great addition to the expansive force of the steam was now extensively employed. To such an extent had this been carried that 350 tons of coal were now doing the work which formerly required 750. Mr. T. W. Bunning read a paper in which he advocated the federation of the different mining and mechanical associations in the kingdom on the lines of the Society of Chemical Industry. Sir Lowthian Bell was re-elected President.

The additions to the Zoological Society's Gardens during the past week include a Malbrouck Monkey (*Cercopithecus cynosurus* δ) from West Africa, presented by Mr. T. Sutton Flack; a Blue and Yellow Macaw (*Ara ararauna*) and a Red and Yellow Macaw (*Ara chloroptera*) from South America, presented by Mr. W. Reid Revell; a Green Turtle (*Chelone viridis*) from the West Indies, presented by Mr. James McGregor; two Griffon Vultures (*Gyps fulvus*), European, a Dark-green Snake (*Zamenis atrovirens*), European, deposited; a One-streaked Hawk (*Melierax monogrammicus*) from West Africa, an Elegant-grass Parakeet (*Euphema elegans* ♀) from South Australia, purchased.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 *AUGUST* 14–20.

(For the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on August 14

Sun rises, 4h. 45m.; souths, 12h. 4m. 30·3s.; sets, 19h. 24m.; decl. on meridian, 14° 24′ N.: Sidereal Time at Sunset, 16h. 56m.

Moon (New, August 19, 6h.) rises, 23h. 52m.*; souths, 7h. 40m.; sets, 15h. 35m.; decl. on meridian, 18° 18′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Decl. on meridia ° ′
Mercury ...	3 9	10 49	18 29	17 58 N.
Venus ...	8 45	14 28	20 11	3 57 S.
Mars ...	1 55	10 5	18 15	22 32 N.
Jupiter...	11 11	16 21	21 31	10 24 S.
Saturn...	2 40	10 36	18 32	20 27 N.

* Indicates that the rising is that of the preceding evening.

August.	h.	
16 ...	— ...	Venus at her point of greatest evening brilliancy.
16 ...	20 ...	Mercury at greatest elongation from the Sun, 19° west.
1 ...	21 ...	Mercury in conjunction with and 0° 33′ nort of the Moon.
19 ...	— ...	Total eclipse of the Sun. The central line of totality passes from south of the Baltic across Europe, Asia, and Japan. At Greenwich and in England and Scotland generally the sun will rise partially eclipsed a few minutes only before termination of the eclipse. In Ireland the eclipse ends before sunrise.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·3	81 16 N.	Aug. 16,	20 28	*m*
R Arietis	2 9·7	24 32 N.	,, 20,		*M*
Algol	3 0·8	40 31 N.	,, 19,	3 48	*m*
R Comæ Berenices	11 58·5	19 25 N.	,, 15,		*M*
T Ursæ Majoris ...	12 31·3	60 7 N.	,, 16,		*M*
δ Libræ	14 54·9	8 4 S.	,, 19,	21 32	*m*
U Coronæ	15 13·6	32 4 N.	,, 16,	0 53	*m*
U Ophiuchi	17 10·8	1 20 N.	,, 16,	2 28	*m*
			,,	22 36	*m*
X Sagittarii	17 40·5	27 47 S.	,, 17,	21 0	*m*
U Sagittarii	18 25·2	19 12 S.	,, 18,	1 0	*m*
β Lyræ	18 45·9	33 14 N.	,, 14,	4 0	*M*
R Lyræ	18 51·9	43 48 N.	,, 16,		*m*
η Aquilæ	19 46·7	0 43 N.	,, 14,	2 0	*m*
δ Cephei	22 25·0	57 50 N.	,, 20,	0 0	*m*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A. °	Decl. °	
Near γ Andromedæ ...	25	42 N.	Swift; streaks.
,, μ Persei	61	49 N.	Very swift; streaks.
,, δ Draconis ...	291	70 N.	Swift; short.

NEW GUINEA EXPLORATION.

ON March 15 last a private exploring Expedition, commanded by Mr. Theodore Bevan, left Thursday Island for New Guinea in the steamer *Victory*, which had been placed for six weeks at Mr. Bevan's disposal by Mr. Robert Philp, the owner. Mr. Bevan's object in undertaking this expedition was to ascertain whether it was possible to reach the mountains in the interior of New Guinea by means of the Aird or other large rivers flowing into the Gulf of Papua, and to establish, if possible, friendly relations with the natives in the neighbourhood of the gulf, with the view of paving the way for future explorations.

We reprint from the *Sydney Morning Herald* of May 23 the following account of the expedition :—

The expedition has proved the existence of spacious waterways leading far into the interior of the island, the two most important—and magnificent rivers they seem to be—having been named the Douglas and the Jubilee. These discoveries may be destined to be of considerable importance to Australia, for a flourishing industrial European community may in the not very remote future settle on the banks of these waterways. Northern Queensland, from its situation, may naturally be expected to reap the greatest advantages from the opening up of New Guinea, but, directly or indirectly, the habitation of its fertile plains and valleys with pioneer settlers must prove beneficial to the metropolis of New South Wales. A comprehensive account of the expedition will be published in due course, illustrated by a chart showing the new discoveries, and by photographic views of new mountain ranges and previously unknown tribes of natives, but a brief description of some of the principal discoveries made will probably be read with interest.

Of the country in the vicinity of the Aird, very little up to the present is known, and at Thursday Island old experienced hands looked upon it as little short of madness, having regard to the supposed treacherous channels existing, and the hostility of the natives, to attempt to enter the rivers which discharge their waters into the gulf. Mr. Bevan, however, paid little regard to the grim forebodings, and the Expedition was fortunate in reaching Cape Blackwood in the month of April, at a time when the waters are invariably smooth, and when there is little reason to fear tempestuous weather. The exploring party soon set to work. Several minor streams were discovered, particulars concerning which will be given in due course, but, as already indicated, two new fresh-water rivers of magnitude were found, disemboguing their waters through various mouths into the Gulf of Papua. Both these pursue a devious course amidst ranges of hills, washing the base at times of lofty mountains. The rivers are longitudinally about 60 miles distant from each other. The first one—the Douglas—is reached by the Aird, up which the *Victory* steamed, and it became manifest that the Aird was only one of several mouths of the main stream, which was navigated for a distance of 130 miles, but which, however, in reality took the party inland only about 80 miles, by latitude, northward of Cape Blackwood. The explorers left this river through a channel marked upon the Admiralty chart as dry land, and this brought them into Deception Bay. The existence of this passage, in which there is from four to eight fathoms of water, proves Cape Blackwood to be an island. It may here be stated that for the first 30 miles up the Aird the country was found to be of deltaic formation, with alluvial islands scattered here and there; but beyond that the main stream of the Douglas becomes a compact watercourse, flowing between rising ground on either side. The country about the delta is flat, covered with scrub, and the banks are well defined. On the higher waters of the Douglas there is a practically uninhabited forest country, which in parts could be easily cleared. Two important fresh-water tributaries to the Douglas were discovered, one of which has been named the Burns and the other the Philp. A new range of mountains observed in this vicinity was named by the leader after his uncle, Mr. Thomas Bevan, an ex-Sheriff of London.

The Gulf of Papua has been explored up to Orokolo, and to the westward of that village are what appear to be fine rivers, but these were reported by the natives to be separate mouths of one river, and the natives' report has been confirmed by Mr. Bevan, who, proceeding up a sixth large channel to the west of Bald Head, came upon the main river, which fed the delta and cut inland at right angles into the five other rivers. There was a heavy break on each bar of the first five openings, probably due to south-east weather on the Queensland coast, but a smooth-water passage was found into the sixth opening. The time at the disposal of the party was too limited to enable them to survey each opening of the river, so a westerly course was pursued, and the *Victory* steamed up a large channel running in a northerly direction from Bald Head to the point of its confluence with other waters. A week was spent in examining the rivers coming in from the north-west, but although high land was seen it could not be reached by any branch in that direction. The easterly passages were next tried, and a channel was found running easterly and north-easterly, almost at right angles, into two other streams. Taking the branch running inland, they proceeded a few miles further, and found it led into two other streams, one going inland and the other with a current towards the sea as before. Yet again did they meet two other streams, and still steaming up the one leading inland, they, on going 5 miles further, came upon another, and this time the last arm leading seaward. Here they found themselves on a fresh-water river nearly half a mile wide, with a steady current flowing towards the sea. A magnificent panorama of rising country was now opened up. Range over range of hills stretched into the distance, capped by some towering blue mountain peaks, and so clear was the atmosphere that even the high mountains, which must have been leagues away, seemed close at hand. They were all clad with trees, and upon the face of them could easily be distinguished the water-gullies, brightly illuminated by the glistening rays of the sun. The river was navigated 110 miles from Bald Head, or about 50 miles in latitude from Orokolo, its chief trend being in an easterly and north-easterly direction, although the course was unusually serpentine. In honour of Her Majesty's having completed the fiftieth year of her reign, this river, probably the finest in British New Guinea, has been named the Jubilee. The ranges into which the waters carried the little steamer, drawing 9 feet of water, were named the Albert Victor.

Very little trouble was experienced with the natives during the expedition. Only once was the party attacked, and that was when going up the Aird—about 20 miles from its mouth—probably by the same tribe that attacked Capt. Blackwood forty-two years ago. The hostile blacks fired several flights of arrows, some of which fell harmlessly by the vessel's side, but they dispersed at the sound of the steamer's whistle, and after a few shots had been fired wide; neither the attacking nor attacked sustaining the slightest hurt. Through this untoward circumstance Mr. Bevan was unable to obtain the name of the tribe. Another tribe, who evinced their peaceful intentions by carrying green bows in their canoes, were found inhabiting the country behind Aird's Hill. A third tribe was met with 48 miles inland, as the crow flies, from Cape Blackwood, and these

called themselves the Tumu. At the confluence of the Douglas River with Deception Bay, a fourth, the Moko tribe, was found. The Kiwa Pori tribe, the fifth met with, were ascertained to be the inhabitants of the country close to Bald Head, in the Papuan Gulf. The Birumu tribe were seen about 16 miles north-west of Bald Head, and the Evorra, the seventh and last tribe, were found about the same distance north-east from Bald Head. With all, except of course the first, friendly relations were established. Mr. Bevan's previous experience of New Guinea natives and knowledge of some of their habits and dialects were exceedingly serviceable to him; and with the exercise of a little patience he was enabled to inspire them with the fullest confidence. Several natives were induced to go on board the steamer, and were photographed. Only three of the tribes could be spoken of as large, the one possessing the greatest numerical strength being the Kiwa Pori, which numbered from 400 to 500 men. The result of Mr. Bevan's observations is that the country is practically uninhabited except along the coast. No natives were seen on the Jubilee River beyond 25 miles from the coast-line.

The best of the land—and fine rich soil it is—appeared to lie between the head of the deltas of both rivers and the foot of the hills, where it looked exceedingly fertile, and covered in places with a palm scrub which could be readily cleared. Sago, tobacco, bananas, bread-fruit, and sugar-cane were found to be indigenous. As already stated, the country about the deltas is alluvial and flat, and then in turn come sandstone, limestone, and ironstone, as well as the stratified rocks which mark the earlier geological periods. Mr. Bevan hopes, at no distant date, to be able to complete the work of which this preliminary expedition he has now made is but the precursor. In the animal, vegetable, and mineral kingdoms, there is a splendid field for men of science. About eighty ornithological specimens have been obtained by the party, and a few snakes, lizards, and fishes, which will be examined at the Australian Museum. A large and varied ethnological collection has also been obtained by Mr. Bevan in exchange for trade from the tribes with whom he established friendly intercourse. Some of the prominent features in the landscape have been named after Mr. Richard Wynne, Mr. F. E. Joseph, Dr. Ramsay, Messrs. Harrie Wood, C. S. Wilkinson, E. Fosbery, and other well-known Sydney citizens.

A word is necessary with regard to the climate, which is described as by no means unhealthy. The temperature varied from 72° F. at daybreak to about 86° in the shade at noon. The party returned to Thursday Island within the time stipulated by the owners of the *Victory*, in excellent health, and with unimpaired physique. The cost of the expedition was from £500 to £600, but from this a considerable sum, represented by the value of the collections, must be deducted. In response to Mr. Bevan's application, the Government have placed at his disposal a competent draftsman to aid him in making up his plottings.

THE INSTITUTION OF MECHANICAL ENGINEERS.

THE Institution of Mechanical Engineers held their summer meeting last week, at Edinburgh, under the presidency of Mr. E. H. Carbutt. The meetings were held in the Library Hall of the University, the members being received by the Marquis of Tweeddale, the chairman, Sir William Muir, Principal of the University, and other members of the Reception Committee. The two papers first read on Tuesday related to the Forth Bridge and the machinery employed in its construction. Both papers we reprint to-day. The discussion on the first of them referred mainly to the subjects of expansion and contraction under variations of temperature and to wind-pressure, and in reply the author of the paper, Mr. E. M. Wood, explained that 1½ inches per 100 feet was allowed for expansion, or double the amount usually thought sufficient; whilst, as regards the wind-pressure, the highest registered had been 35½ lbs. per square foot, whilst 56 lbs. was allowed for. All the speakers who discussed the paper of Mr. Arrol, the contractor for the bridge, referred in high terms to the skill and ingenuity exhibited throughout. Later on in the day the members made an excursion to the Forth Bridge, Mr. Arrol and the heads of the various departments at the works acting as guides. A striking feature was the comparative noiselessness with which the work was carried on, owing to the successful use of hydraulic power in riveting.

We regret to learn that on the day of the visit to the bridge two men had lost their lives owing, it is believed, to the staging on which they were employed giving way; this raises the number that have been killed at the Forth Bridge works to six during the last two months, the number of men employed averaging between 3000 and 4000.

The third paper read was by Mr. F. J. Rowan, on electro-magnetic machine tools, which were invented by him to overcome the difficulties of riveting by hand; they perform their work in a very complete way. The conditions of the work itself involve the separation of the riveting portion of the apparatus from the bolster or holder-up, whilst the riveting process requires that the two portions of the machine should be rigidly held together. This is effected by magnets so arranged on opposite sides of the plating with their poles of unlike denomination facing each other, that they are drawn towards each other, thus pressing the plates together, and insuring the proper condition for riveting. The riveting itself is effected by an electric motor, which by means of gearing and a cam, lifts the hammer against a spring, the amount of compression imparted to the spring in lifting being regulated by hand.

The first paper read on Wednesday was descriptive of the electric light on the Isle of May, by Mr. D. A. Stevenson. The machinery, boilers, and engines, are placed near the base of the island, and close to the water-supply, as it was found that the saving which would be effected by not having to convey fuel to the top of the island, or to pump up water, would compensate for the loss of energy due to the resistance of extra length of the electric conductor. The electric generators are two De Meritens alternate-current machines, each weighing 4½ tons. The induction arrangement of each machine consists of five sets of twelve permanent magnets, sixty in all, each magnet being made up of eight steel plates. The armature, 2 feet 6 inches in diameter, is composed of five rings with twenty-four bobbins on each, arranged in groups of four in tension and six in quantity, and makes 600 revolutions per minute. With the circuit open, each machine develops an electromotive force of 80 volts, with the circuit closed through an arc 40 volts. An average current of 220 amperes is developed, thus yielding 8800 watts of electrical energy, or 11·7 horse-power in the external circuit. In the dioptric arrangement constructed by Messrs. Chance to the author's design, the condensing principle has been carried further than in any apparatus previously constructed. The principle consists in darkening certain sectors by diverting the light from them and throwing it into the adjoining sectors so as to reinforce their light. The author agreed with the conclusion arrived at by the Trinity House that taking first cost and annual maintenance into account, electricity should only be used for important landfall-lights; where, however, the most powerful light was desired, independently of cost, the electric arc had no rival. Some interesting observations have been carried on for the last five months which prove the electric light to be the most penetrating of all lights in all states of weather. Every night at twelve o'clock the light-keepers at St. Abbs Head, twenty-two miles distant, where there is a first-order flashing light, and one of the most powerful oil-lights in the service, observe the Isle of May light; whilst the keepers there observe the St. Abbs Light. The result of these observations has been that the Isle of May light has been seen one-third more frequently than the other. The paper was discussed by Sir James Douglas and several other speakers. A paper was read on the construction of the Tay Viaduct, by Mr. F. S. Kelsey, the resident engineer. This bridge is two miles long, and has taken five years to construct, having been opened for traffic on June 20 last. A paper on the dredging of the lower estuary of the Clyde, by Mr. C. A. Stevenson, was read. Both these papers, which were fully discussed, are of technical rather than scientific interest.

In the evening a *conversazione* was given by the Lord Provost, magistrates, and Council of the city in the Museum of Science and Art. Sir William Thomson gave a very exhaustive lecture on waves, concluding with an important suggestion. It seemed to him that inasmuch as wave resistance depends almost entirely on surface action, it might be diminished relatively very much by giving a great deal of body below the water-line. High speeds of 18 or 20 knots might thus be obtained. By making ships like the old French ships, swelling out below the water-line, there would be a large additional displacement and carrying power, and little addition to wave disturbance.

THE STRUCTURE AND PROGRESS OF THE FORTH BRIDGE.[1]

AS a visit to the works of the Forth Bridge is included in the programme of the present meeting of this Institution, the author trusts that a short sketch of the preliminary proceedings, with a description of the structure and progress, from one engaged on the work from its outset, will prove of interest in explaining the reasons and means adopted for connecting the railways on opposite shores of the Firth of Forth, at the site of the historic ferry and still existing Hawes Inn, whose time-table for the departures of the ferry-boat is so quaintly alluded to in "The Antiquary."

Previous Proposal.—For many years, suggestions for establishing direct communication between the southern railways running into Edinburgh and the Fifeshire lines, with the object of more direct access to Perth and the north, had been frequently considered by the companies interested in that route; but until an Act of Parliament was obtained in 1873 for the construction of a suspension bridge, designed by the late Sir Thomas Bouch, for crossing the Forth at the site of the present works, no proposal gave prospect of successful issue. Although the type of bridge then proposed was not one generally considered applicable for the passage of railway trains, yet no positive objection seems to have been taken to it, inasmuch as a contract was entered into for its construction, workshops were erected at the site, and foundations were started. But after the severe gale at the close of the year 1879, so destructive to a viaduct in an equally exposed position, it was deemed prudent to suspend operations; and the directors of the North-Eastern, Midland, and Great Northern Railway Companies, which each have an interest in obtaining direct access to the eastern and northern districts of Scotland, requested their respective consulting engineers, Mr. T. E. Harrison, Mr. W. H. Barlow, and Mr. (now Sir John) Fowler, to confer together and report upon the possibility of some other plan for making through communication between the existing lines at the point already selected. Tunnelling was out of the question on account of the depth of the water; the proposals therefore took the form of bridges.

Present Plan.—On May 4, 1881, the engineers submitted their joint report, unanimously agreeing that the steel bridge on the cantilever and central-girder system, designed by Sir John Fowler and Mr. Benjamin Baker, was not only the least expensive, but the best suited for the situation. The soundness of this decision has since received confirmation in the fact that seven long-span bridges have been or are now under construction in different parts of the world, and many more are proposed on the principle adopted for the Forth Bridge. For the substitution of this design in place of the suspension bridge contemplated in 1873, the Forth Bridge Railway Company appointed Sir John Fowler and Mr. Baker their engineers, and obtained an Act of Parliament in July 1882. The financial obligations for the construction of the bridge having been undertaken by the railway companies interested in the through route—namely, the North-Eastern, Midland, Great Northern, and North British—tenders were invited for the work, and from the applications received two offers were selected; and with the combined firm of Messrs. Tancred Arrol and Co. a contract was made in December 1882 for the entire execution of the work.

General Dimensions.—The total length of the bridge will be 8,090 feet, or 380 feet over one mile and a half. There are two main spans of 1700 feet each, two side spans of 675 feet each, with the ends counterbalanced and anchored to the masonry, and three intervening piers; these together make up about a mile of the total length, and the remainder is composed of fifteen approach spans of 168 feet each, and of masonry arches and abutments. For a length of 500 feet in the centre of each of the two 1700-feet spans there is a clear headway for navigation of 150 feet above high water; the rails being placed at a level 6 feet higher. From the base of the deepest pier to the top of the cantilevers the total height is 450 feet, or only 10 feet less than the Great Pyramid of Egypt.

The cross sections of the main spans are of trapezoidal form, 330 feet in height from centre to centre of the members over the piers, and 33 and 120 feet in width across top and bottom respectively, and tapering towards the ends of the cantilevers, thus giving a form which is eminently suitable for withstanding lateral pressure. The girders carrying the railway are supported at intervals inside the cantilevers, &c., by trestles or cross frames, and a continuous lattice-work parapet 4½ feet above the rails extends the whole length of the bridge.

Load, and Wind Pressure.—In addition to its own weight the bridge is being constructed to support, without exceeding in any member the unit stresses permitted by the Board of Trade, a load equivalent to trains of unlimited length equal to 1 ton per foot run on each line of railway, or passing trains consisting each of two engines and tenders at the head of sixty coal trucks weighing 15 tons each; and also to withstand a lateral wind pressure of 56 lbs. per square foot of exposed surface of train and structure. The magnitude of the lateral pressure may be judged from the fact that over the mile length of main spans the estimated surface exposed to a point blank wind at right angles to the bridge amounts to a little more than 7½ acres; the pressure of 56 lbs. per square foot on this surface would therefore be equivalent to a total of more than 8000 tons. In addition to lateral winds, the direction from any point of the compass has been provided for, even including the imaginary condition of each group of main piers becoming the centre of a whirlwind. Effects of temperature will be provided for in the rails, and at the junctions of the central girders with the cantilevers; and the bearings on both the main piers and under the weighted ends of the cantilevers have provision made for allowing movements due to changes of temperature and to the elasticity of the cantilevers under lateral pressure. The lateral play allowed is limited, so that the whole of the piers may act in concert to resist combined actions of all forces tending to disturb the normal state of rest of the 50,000 tons of permanent load. As a further provision 48 steel bolts of 2½ inches diameter, secured 24 feet down in the masonry by anchor plates, hold down the bed plates with an initial tension of 2000 tons; the nuts and saddle-plates are so arranged as to allow freedom of lateral movement to the skew backs; but any lifting would at once be prevented by the anchorage coming into action, which however could only happen under the assumed circumstances of a wind pressure more than double that already mentioned, acting over the whole estimated surfaces. The maximum pressure on the base of the piers will be a little over 6 tons per square foot.

Forms of Parts.—The enormous forces to be resisted have been met by adopting the most suitable forms of parts for withstanding the stresses. Tubular members are used for compression, and open-braced box-forms for tension. These parts vary in size as required. Though the tubular form has scarcely been used in this country for bridge members since its employment by the late Mr. Brunel, no difficulty has arisen in connexion with its use; even the junctions are dealt with as readily as the generality of the work.

Masonry.—The masonry for the main piers, above the whinstone concrete filling of the caissons, consists of a casing of Aberdeen granite, inclosing and bonded into a hearting of Arbroath stone set in cement, and strengthened by three massive wrought-iron belts built into the stone-work. The deepest pier weighs about 20,000 tons. The remainder of the masonry of the piers and abutments is of a similar class, whinstone being largely used in the interiors.

Steel.—For the principal members of the superstructure subject exclusively to compression, the steel used has a tensile strength of from 34 to 37 tons per square inch, with at least 17 per cent. of elongation in a length of 8 inches; for the other parts 20 per cent. of elongation, with 30 to 33 tons tensile strength. The rivet steel has 25 per cent. elongation, and 26 to 28 tons tensile strength per square inch. The whole of the steel is manufactured by the Siemens process. No sheared edges or punched holes are permitted.

Work started.—No time was lost by the contractors in starting the work. The land was at once entered on; the old workshops were put in order, and the extensive range of offices, stores, workshops, and yards was commenced, which now cover fifty acres. Meanwhile the centre line of the bridge was fixed, and the position of the piers determined. The foundations of those on land were begun simultaneously with the building of temporary jetties for gaining access to the piers that had to be sunk below water-level. These jetties, which are still used for conveying the material, are in themselves no small work; the southern or Queensferry jetty extends 2200 feet from the shore, and is connected with the workshops by an incline worked by a rope driven by a stationary engine. In order that the operations might be carried on continuously day and night when needful, electric light installations, supplemented by lucigens, were laid

[1] Paper read by Mr. E. Malcolm Wood before the Institution of Mechanical Engineers, on Tuesday, August 2.

over the works and piers; and telephonic communication was established between the offices and all the centres of operations. The workshops and yards were rapidly completed, and furnished with tools, of which many are of a special and novel description. Ever since the commencement the work has progressed without interruption, and has gradually assumed the gigantic proportions of the present time. Over 3000 hands have been employed continuously for the last year; and during the present summer months the number has been increased to 3600. The majority find lodgings in the neighbourhood of the bridge; and the remainder make use of a special train service to and from Edinburgh, and a steamer to and from Leith, put on for their use night and morning.

Materials.—The materials for the permanent work have been obtained throughout from producers of repute: Aberdeen granite from Messrs. Fyfe; Portland cement from Messrs. Hilton and Anderson and Bazley White; Siemens steel from the Steel Company of Scotland and the Landore Steel Works. All the steel has been subjected to rigid examination, and has passed the ordeal of specified tests before leaving the makers' works; a few specimens showing its high quality are exhibited. The materials delivered up to the present time have included—

Granite	550,000 cubic feet.
Portland cement	21,000 tons.

The amount thus far erected has been—

Masonry in piers and abutments ...	129,500 cubic yards.
Steel in approaches and main spans	19,000 tons.
Steel for main spans, prepared ready for erection, about	20,000 tons.

By the time the first consignment of steel arrived, the shops were ready for the preparatory operations, and the whole establishment was rapidly organized to deal in the most complete manner with the work to be executed. Hydraulic power is freely used, from the extremely neat form of shop crane to the 2000-ton press for curving the tube-plates. With the exception of the main-pier caissons, made by Messrs. Arrol Brothers of Glasgow, and the superstructure of the approach spans by Messrs. P. and W. Maclellan of Glasgow, the whole of the work has been turned out of the shops at the bridge, their present capacity being an output of 1300 tons of finished work per month.

Shop Practice.—The procedure in the shops may be described as follows. The flat plates and bars are first straightened. The plates to be curved are heated to a uniform red heat in a gas furnace, and while red-hot are moulded in dies under hydraulic pressure to the required form, stacked and coated with ashes, and allowed to cool slowly and equally; any subsequent warping is taken out by placing them again in the press when cold, and giving them a final squeeze into the correct shape. The butts of the bars are cold sawn, and the edges and butts of the flat plates are planed in the usual manner. The ends of the curved plates are planed in a novel form of machine, in which the tool travels in a circular path readily adjusted to the radius of the curved plate. On completion of the planing, the plates are taken to the tube yard, and are built up round the longitudinal ribs and internal stiffening frames, which have previously been fitted together in moulds to the exact diameter required: so that the plating of the framing at once gives the tube its proper form. The plates are in 16-feet lengths, and break joint alternately over the stiffeners at 8-feet intervals. Means are adopted to keep the tubes in line while the rivet holes are pierced by a travelling annular drilling frame, which is mounted on wheels and carries a boiler and engine driving ten drills by cotton ropes. A pair of drills are attached to each bed; and as the beds can traverse the circumference of the tubes, while the drills can traverse the length of the beds, the whole outside of the tube is commanded, and the holes are completed with accuracy to insure their precise coincidence when the parts are rebuilt at the site. As fast as each section of 8 feet length is finished, the machines are propelled along the rails to take up a new position; they thus travel gradually in successive stages over the whole length laid down. The tee and trough-shaped parts are built together in the shops, and the holes are drilled by adjustable vertical and horizontal drills, fitted to a travelling carriage; the power is transmitted to the machines by ropes from the shop shafting. Numerous radial machines are also in use for the secondary parts. For dealing with special parts, many ingenious and somewhat novel workshop appliances have from time to time been brought into use, beyond those here mentioned. All the parts of the junctions are carefully fitted together in the yard in the exact positions they will relatively occupy in the bridge. After each member has been prepared, the pieces are painted, marked, and stored until required for erection.

Founding Piers.—With the founding of the piers below water commenced the more difficult part of the undertaking; but without any sensible delay the whole of the piers have been successfully sunk and completed. The foundations for piers in shallow water were put in either by tidal work or by open cofferdams, and the excavation was carried down to boulder clay or rock. Though these were of individual interest themselves, from the size and difficulties met with, they are dwarfed by the magnitude of the operations connected with the deep-water piers, of which those in the south group are embedded in the boulder clay in one case at 90 feet below mean water-level, while at Inchgarvie they rest on a level bench cut out of the sloping whinstone rock at a depth of 72 feet.

Caissons.—The caissons for all the deep piers are 70 feet in diameter at base; the cutting edges and shoe are of steel, and the upper parts of wrought-iron. They were first built on ways on the south shore, and were launched with sufficient ballast on board in the form of concrete to insure their stability while towed out to their berths at the end of the jetties, where guide piles and dolphins were used to place them in correct position. Temporary wrought-iron cofferdams were built upon the top of the caissons, timber working decks constructed, cranes and concrete mixers fixed, air-pressure connexions made good, and sinking operations commenced with a pressure in the excavating chamber sufficient to drive out the water; the air-compressing machinery was placed on the jetty alongside. The Inchgarvie caissons had to be equipped with all these fittings while moored to the south jetty, so as to be ready for work on arrival over their rocky bed.

The working chamber was illuminated by electric lights; and communication was effected with it through three shafts with air locks on the level of the upper deck. The two shafts for the skips bringing up the excavated material were constructed with horizontal sliding shutters, worked by hydraulic rams, in place of the usual swing doors. The winding drum for bringing up the skip from the working chamber was not in the lock itself, but driven by an engine outside. On arrival of the skip in the lock, the lower slide was shut to, and the blow-off cock opened for releasing the pressure, the top slide drawn back, and the hook of the discharging crane was coupled to the skip by hand. This direct and rapid method of transit for the excavated materials greatly facilitated the sinking; the whole operation from arrival of the skip in the lock to its removal lasted only about three-quarters of a minute in ordinary working, the sequence of the movements being automatically controlled by interlocking gear. The air locks in the third shaft for the men were constructed with a view of rapidly changing shifts, and had double chambers, each capable of holding seven men.

The silt overlying the harder deposit was expeditiously expelled from the working chamber by means of ejection pipes passing into water outside, the air-pressure being sufficient to blow out charges of silt and water mixed in a box which communicated by a valve with the ejection pipes. On reaching the boulder clay, portable steel diggers, actuated by hydraulic cylinders placed between the roof and the implements, were brought into use to break up this hard material.

At Inchgarvie a modification of this system was required for sinking the deep piers into the hard whinstone rock, which had a natural slope of 1 in 4½. Bags of sand and concrete were deposited in two piles on the deeper side of the site to be occupied by the caisson, which had been launched with massive timber blocks in the chamber, to rest upon this artificial bed; these blocks and the edge of the caisson touching the rock on the shallower side were the first bearings it took when lowered at the site. The whole of these primary operations required extreme care to provide for differences of weight on the base, due to the depth of water at different states of the tide. Then by means of rock drills and ordinary quarrying operations inside the air-chamber the rock was excavated until the caisson was sunk to a level bench cut out of the sloping rock. In these caissons the full pressure of air due to the head of water was maintained during the sinking, and it was found advisable to change the gangs every four hours; the maximum pressure reached at high

tide was 33 pounds per square inch above the atmosphere. The last of these caissons was got down to its final depth in October 1885.

In sinking the southern group of caissons, the air-pressure hardly ever exceeded 22 pounds per square inch, the silt and clay acting as a lute; and the working shifts were of six hours' duration, about twenty-seven men being down at a time.

Recovery of Canted Caisson.—With the exception of the north-west pier in the southern group, the whole of the piers were completed with regularity. But the caisson for that particular pier, weighing with concrete some 3000 tons, while ready at the site for placing in its final position, by some means became waterlogged on New Year's Day, 1885, and on the tide falling slid forwards on the mud about 15 feet, and canted over through 25°. After an ineffectual attempt to right it by pumping, regular siege was laid to it; but not until the autumn following, after nine months of incessant work, was a timber jacket or cofferdam completed, which enabled the pumps at last to obtain command over the leaks. The caisson then floated again, and after repair was sunk in position in the ordinary manner, arriving at its final depth in 1886. After the excavation had been completed, the chambers were rammed with concrete and grouted up, the concrete and anchorage and masonry were completed, and the temporary cofferdam was ready for removal.

Men Employed.—No difficulty arose in obtaining a sufficient number of men inured to work under air-pressure, as M. Coisseau of the firm of MM. Couvreux and Hersent, of Paris, brought his staff of trained excavators from the Antwerp harbour improvement works, and contracted for the work to be executed under air-pressure.

Raising Viaduct Girders.—After the masonry of the approach viaduct piers had been carried up to a convenient height, a temporary stage was built, upon which the girders were erected and riveted up. Steel cross-beams with pairs of hydraulic jacks were placed under the ends of the girders over the piers; and a stage surrounding the piers was suspended from the main girders. From this platform the men in charge of the rams conducted the operations of lifting and blocking up the girders; and the masons afterwards completed the stonework in the vacant spaces. By this plan the girders were raised to their final height in July of the present year. The whole of the ten spans on the south side were lifted simultaneously as soon as they were all riveted up. The materials for the piers were first raised in trucks, by a steam hoist on the jetty, to a tramway laid on beams between the bottom members of the girders, and afterwards lowered into position by winches over each pier, these winches being driven by running ropes from engines on the girders at alternate piers. These approach spans now require only the parapet and a few other details for completing them in all respects, ready for the permanent way.

Erecting Steel Work over Main Piers.—On the completion of the masonry, the operation of erecting the steel work was commenced on the northern piers early in 1885 by riveting up the bed plates, and lowering them into position over the heads of the foundation bolts. Their surfaces were afterwards smoothed by emery wheels, and coated with crude petroleum, to prepare them for receiving the bearing plates of the cantilever bases or skewbacks. These, as already mentioned, have freedom for a limited amount of sliding, and the gauges at present attached show that the sliding movements follow the changes of temperature as anticipated.

The skewbacks, forming the junction of five tubular and five rectangular members over the piers, were then erected, and were connected with the horizontal members at the same level, which had been built together on a stage. After the connexions had been riveted up, a commencement was made upon the upper parts over the piers; these parts have since been erected without any form of fixed scaffolding, and the operation is still in progress over the Inchgarvie piers.

The lifting gear for raising the erecting platforms consists of a pair of plate frames, one below the other, fixed inside each 12-feet pier-column, by pins passing through the wings of the frames and the ribs of the column. The lower frame supports a hydraulic lifting press; and upon the ram rests a through box-girder cross-beam, at right angles to the length of the bridge, passing through voids in the columns, where plates are temporarily left out for this purpose. These cross-beams support lattice-girders in pairs, one on each side of the column, which extend a little more than the full length of the side of the quadrangle formed by the piers. Upon the top of all comes the main deck, furnished with gantries, cranes, oil-heated rivet-furnaces, &c., complete in all respects for carrying on the chief operations of erection. On the bottom level of the girders is a lower deck, with the ends housed in to form temporary shelters for the men. The box and other girders are built up of parts which will eventually be used in the permanent structure. Communication between the level of the jetty and the platforms is made by hoists, drawn up between wire-rope guides by the winding engine on the level of the jetty, which lifts the material by wire ropes to the platform; safety clutches are attached to each cage, for seizing the guide ropes in case the hauling gear were to give way. During lifting operations, access to the platforms is gained by ladders laid up the cross-bracing between the main columns over the piers.

The process of raising the platforms is as follows. Water-pressure at about 30 cwts. per square inch is conveyed from pumps on the jetty to the lifting presses by wrought-iron piping taken up the inside of the columns, and is turned into a cyinder, lifting the load off the series of pins in the top frame. The pins are then withdrawn, and the ram lifts the box-girder, carrying with it the loose frame, until opposite the next series of holes in the ribs of the columns, into which the pins are then inserted; the pressure is released, and the box-girder again rests upon the upper frame. In the return stroke the ram, hanging by its shoulders from the upper frame, by means of its piston form now hauls up the lower frame, from which the pins have been withdrawn; and when this has been repinned, it is ready to support the press for another upward stroke. By this means the platforms have been gradually raised, generally through lifts of 16 feet at a time, until arrived at the summit. On their way up they have been utilized for building the tubular cross-braces and other work; and at the present time those at the southern and northern piers form the stage for erecting the top members between the heads of the main columns. The platforms at Inchgarvie are now only 40 feet below the height to which they will have ultimately to be raised.

In building the pieces together, they are connected by service bolts, until the hydraulic riveters are brought into action. For the open work the riveters are of the gap type; but for the closed tubular work, a special adaptation was devised by Mr. Arrol, by which the rivets are closed in any part of the built tubes. When these machines arrive at the top of the columns, after having completed the riveting on the way up, they are taken apart ready for application elsewhere.

Erecting Cantilevers.—The building out of the first projecting bays of the cantilevers is being conducted on the system just described, with such modification as to suit the altered circumstances. The bottom members are first erected, and have been built by means of overhanging frames in panels, resting upon the completed portions of the tube, and so constructed that, as fast as the work is riveted up by the annular riveting machines, and the forward portions of the cage-like framing are brought into bearing, the back frames can be unshipped and taken forwards to the working face. Upon the top of this framework a movable hydraulic crane is placed for lifting the pieces into position, which are brought alongside from the pier by carriers suspended from a single rail of angle bar. As soon as the limit is reached at which these members can support the projecting work, inclined supporting stays are introduced, which connect the bottom member at this part with a temporary horizontal tie stretching between the main columns at about the level of the cross-bracing; thence the inclined stays slope down again, and are attached to the bottom member on the other side of the pier. After this has been done, platform girders with decks are built at a convenient level to rest on cross-beams carried by rising frames, which are introduced between the corners of the first vertical member of the bridge: this member having been pierced beforehand with a series of pin-holes, in readiness for a lifting action similar to that used in the main columns. The ends of the platforms nearest the piers are raised by suspension bars, by the action of hydraulic rams attached to the main columns at a higher level. From these platforms, as in the previous cases, the erection of all parts commanded by them is carried on as they rise.

The erection of the secondary parts proceeds simultaneously with that of the main members, the railway girders being built by corbelling out from the supports, and the other parts by light stages when the parts themselves cannot serve as a means of support to extend the work. As will readily be understood, the erection of these sections calls for greater nerve and judgment on

the part of the men employed than does that of the portions previously described.

In conclusion, the author desires to express his indebtedness to Sir John Fowler and Mr. Benjamin Baker, through whose kindness he has been enabled to place before the Institution the foregoing particulars respecting an undertaking which, as shown by the magnitude of the works now being carried on, constitutes one of the greatest engineering feats ever attempted.

THE MACHINERY EMPLOYED AT THE FORTH BRIDGE WORKS.[1]

THE greater part of the machinery at the Forth Bridge works is original in design and novel in construction, chiefly because of the unusual nature of the work to be carried out. It may be roughly classed under the following heads: hydraulic bending and setting, planing, drilling, erecting, and riveting. In designing the machinery and tools to accomplish these different kinds of work, there had ever to be kept in view rapidity of production, with a very high quality of work in the finished structure. An idea of the quantity of machinery provided to deal with the material passing through the shops may be partly formed from the fact that it is capable of finishing 1500 tons in a single month.

Hydraulic Bending and Setting Machinery.—To bend and twist the large steel plates required in the construction of the tubes and their connexions, a great variety of hydraulic presses had to be provided. The largest of these is capable of exerting a pressure of 1600 tons between the dies. It consists of four 24-inch cylinders, resting on two longitudinal girders bedded in concrete. From each cylinder rise two iron columns, which carry a fixed table overhead. On the top of the rams another table is placed, which can be raised or lowered at will. Between these two tables are placed the blocks which stamp the plates to the desired shape. In most cases this shape is the arc of a circle, but in others the form is very varying, while in some instances the plates are flanged as well as bent or twisted. In nearly every case, after a plate has been set while heated, it requires to be finally adjusted when cooled. To dispense with the heating of the plates gives unsatisfactory work, and is in many cases impossible. In no instance is this plan of bending adopted to any extent without annealing the plates both before and after the work has been put upon them. Much of the final adjusting of the plates is done by presses consisting of a simple ram fixed to the upper of two girders, which are bound together at the ends, the lower girder serving as the seat for the block on which the plate is placed. Numerous other forms of presses are employed for lighter work.

Planing Machinery.—A special class of machinery is employed to plane the edges of the plates. In the case of most of the plates this requires to be done very carefully, because in the structure of the bridge a certain percentage of the stress in compression is taken up by the plates butting, instead of wholly by the rivets as in the tension joints. This statement applies to all plates in the tubes.

The sides are first of all planed on what may be looked upon as an ordinary planing machine. It is provided however with special double side-cheeks, between which are two fixed swivelling tool-boxes, one on each side of the machine. These tool-boxes can when desired be transferred to a special cross-slide, as it is sometimes more convenient to work with one box in the cross-slide rather than with both between the side-cheeks. Both tools act together and cut continuously—that is, during the backward as well as the forward travel of the table. The plate to be cut is fixed upon a curved block, which in turn is securely bolted to the table.

For planing the ends of the curved plates a special machine had to be designed and built, in which the plates are secured to a fixed table, while the tool is made to travel backwards and forwards in a swinging pendulum that receives its motion through a connecting-rod from a travelling saddle. The tool cuts both ways in this instance also, and is fed to its work by hand.

The planing machines employed to finish the rectangular plates for girder work are of the usual pattern for plate-edge planing, but with the addition of an end slide provided with a separate tool for planing one end of the plate at the same time that one of its sides is being similarly treated. This machine finishes a plate at two settings, with the certainty that the ends are at right angles to the sides.

In some machines two saddles are upon the main slide, and in others two tools are in one saddle; both devices have their advantages. The facing of the tees, angles, and other sections is done as a rule by cold steel saws, in order to secure good butting.

Drilling Machinery.—As will be inferred from the varying character of the work, the drilling is performed by various classes of machines. The principle kept in view is that, wherever possible, girders, tubes, &c., should be drilled only while their various parts are temporarily built and held together by bolts in the position they will finally occupy in the finished structure; in this way the highest class of work is obtained.

For drilling the tubes, the machines, each complete in itself, are made large enough to embrace the entire circumference of the tube. They consist of a wrought-iron under-frame or carriage, on which are placed the engine and boiler. On it are also fixed two large cast-iron annular rings or headstocks, embracing the tube, round which ten drilling slides and heads travel circumferentially. The slides are moved around the rings and consequently around the tubes by a worm at each end, gearing into a worm-wheel that forms part of the rings. The motion of the drill-heads on the slides is longitudinal, or parallel to the tubes. These two motions easily permit of the ten drills working at any part of the circumference of the tube comprised between the two annular rings, which embrace a length of 8 feet. When this length is finished, the whole machine is travelled forwards, and is again ready to drill a new length of 8 feet. The tube rests on timber blocks, which are removed from the front and placed behind as the machine travels forwards. In the case of the lighter tubes, the rate of drilling is as high as 12 lineal feet of tube per shift of ten hours; this represents about 800 holes drilled

The booms of all girders are drilled separately on blocks, thus leaving the bracings to be drilled to template, which is done by radial drills at another time. The machines employed to drill the booms are of a wholly different kind from those used for the tubes. They are moved along rails, running on each side of the blocks upon which the booms are built, and parallel with them. They consist of a double carriage with upright columns, connected together by means of a cross-beam and sundry other framing for carrying the shafts, pulleys, &c. To the columns and cross-beam are secured slides, to which the fixed drill-heads are bolted on the front of the machine; while to the back are attached radiating arms, each carrying a single drill. In this way there are both fixed and swinging drills on the two sides of the machine, capable of drilling holes in either a horizontal or a vertical plane. The fixed drills serve for all holes in the regular pitch, while the movable drills take what may be called odd holes, such as those where the struts and ties are to be secured to the booms. All the fixed drills are self-feeding, but the movable ones are fed by hand. The number of drills simultaneously at work varies greatly; at times as many as thirteen have been employed together on a single boom.

Other machines having radials with only single drills are used for a special class of drilling, and are found to work to great advantage. With the exception of a few special tools, the remaining drilling is done by radials capable of making a complete circle round the column on which they are supported. Tables are placed on each side of these machines, and the work is fixed on one of the tables; and as the drills are placed at a convenient distance from one another, all the drilling required is easily accomplished without a second shifting of the work.

Erecting and Riveting Machinery.—To erect and rivet such large quantities of material at the immense height at which much of it requires to be done demands a large quantity of special plant for riveting and other purposes. The ordinary class of riveting is accomplished by means of small portable riveters, consisting of two arms held apart by links and stays; one arm acts as the holder-on, while the other carries the hydraulic cylinder for supplying the power, the cylinder and arm together forming one casting. For some of the more difficult work, where neither could this form of riveter be employed nor could the work be done by hand, small direct-acting hydraulic cylinders were used; the die for forming the rivet-head was here fixed into the piston. Two 4-inch cylinders were usually employed, held

[1] Paper read by Mr. William Arrol before the Institution of Mechanical Engineers on Tuesday, August 2.

to their work either by hard wood packing placed against the permanent structure, or by temporary girders brought into proper position. In these machines the pressure employed was 3 tons per square inch. A large amount of excellent work was performed by these machines in positions where it was practically impossible to do it otherwise.

The riveting of the vertical columns of the piers is done by riveting machines attached to the under sides of the lifting platforms. They are lifted with the platforms, and do their wo k while the platform is at rest. They consist of two longitudinal girders or uprights, one on the outside and the other on the inside of the column. Along the face of each girder a riveting cylinder is raised or lowered by hydraulic power. The inside girder has a trunnion at top and bottom, fitting into a step in two temporary diaphragms for supporting the thrust of the rams in riveting. It is turned round on the trunnions at will, so as to rivet up an entire length of 16 feet of the tube both circumferentially and longitudinally. The outside girder and riveting cylinder when at work always face the inside. The outside girder is attached at top and bottom to two wrought-iron rings, which encircle the column, and not only furnish the necessary support but also permit of the machine being moved round the column by hydraulic power as required. Over 800 rivets have been closed in a day by one of these machines.

In the erection of the large piers of the bridge, hydraulic power is utilized to a great extent. The principle adopted is to build the piers from off a platform raised by hydraulic pressure as the work of erection proceeds, utilizing the piers themselves in process of building as the support of the rising platform.

THE CHEMISTRY OF THE RARE EARTHS.

IT is now nearly twelve months since the chemical world was agitated by the memorable departure made by Mr. Crookes, in his address to the Chemical Section of the British Association, in attempting to translate into language thoughts which had been irresistibly forced home to the minds of many men of science as to the insufficiency of the theories of our modern chemical philosophy to account for the presence in our midst of those objects of ever-increasing interest, the chemical elements. It will be remembered that, both in the address referred to and in his lecture at the Royal Institution on the "Genesis of the Elements," Mr. Crookes based a large portion of his arguments upon the remarkable experiences which he himself had met with in endeavouring to separate the constituents of the rare earths contained in several sparsely distributed minerals. It may be of interest just to recall the main conclusions drawn by the lecturer from his experiments upon the substances yielded by the laborious but fruitful process of fractionation. Yttrium, which only two years ago was supposed to be a simple substance, fell under that *nil desperandum* sorting influence into five components, each of which presented a distinct phosphorescent spectrum; samarium, one of the constituents of old didymium, was found to consist of two and possibly of three ingredients; and finally, the two components of didymium itself, into which it had been separated by Dr. Auer von Welsbach, were shown by Mr. Crookes, M. de Boisbaudran, and M. Demarçay to consist themselves of several.

Contemporaneously with the work which has been carried on by these and other experimenters, Drs. Krüss and Nilson, who have at their disposal tolerably large quantities of Scandinavian minerals containing rare earths, have been engaged upon work of a similar nature, and have lately published in the *Berichte* of the German Chemical Society results of the highest interest, not only confirming the conclusions above referred to, but announcing that, "in place of the rare metals erbium, holmium, thulium, didymium, and samarium, we must now accept the existence of more than twenty elements."

Considering the interest which Mr. Crookes's addresses have called forth, and the important bearing of this contemporaneous work upon a subject of such paramount importance to the first principles of chemistry, it may be of advantage to give a short description of the experiments which have led to results of such magnitude.

The minerals examined were specimens of thorite from Brevig and Arendal, in the province of Christiansand, of wöhlerite from Brevig, cerite from Bastnäs, fergusonite from Arendal and Ytterby, and of euxenite from Hitterö and Arendal. The nitrates of the earths contained in these minerals gave very beautiful absorption-spectra, and a precise measurement of the positions of the lines and bands in these spectra resulted in the surprising observation that in certain minerals only a particular few of the absorption-bands of the nitrates of some of the rare earths were visible; thus, only one line out of all the lines considered to belong to the nitrate of holmium, the metal which Soret called X, was visible in any intensity in the spectrum of the nitrates from thorite of Brevig; moreover, this particular line is but insignificant, among many much more intense, in the usual spectrum of the nitrate of holmium. The more intense lines were either not at all or only faintly visible in the spectrum of thorite of Brevig; hence it is concluded that Soret's X must consist of at least two ingredients, of which one is found free in thorite of Brevig, and gives this one line of wave-length 428·7.

In these observations a single 60° prism of dispersion $A - H_\gamma = 4° 18'$ was preferred, inasmuch as weak lines or bands cannot be distinctly seen with more dispersion, and the position of maximum darkness becomes more difficult to fix; the spectroscope was fitted with the most refined micrometer arrangement for the accurate determination of the wave-lengths, so that the whole of the work may be checked by future observers. Before passing to the discussion of the main results of the experiments, a brief description of the procedure in case of one of the minerals examined may not be without interest. Thorite of Brevig is a typical specimen of the Scandinavian rare earth minerals, and its treatment was as follows. After removal of the thoria, which was required for the purpose of determining the atomic weight of thorium, the solution in ice-cold water of the sulphates of the mixed earths was precipitated by oxalic acid, leaving the iron, manganese, and uranium in solution. The oxalates were then ignited and the residual earths again converted to sulphates; the sulphates were converted to hydrates by precipitation with ammonia, and the hydrates dissolved in nitric acid, by which a lovely pink solution of the nitrates was obtained. As small quantitities of thorium and cerium were still contained in the mixture, the nitrate solution was evaporated to dryness, and the residue ignited, whereby the thorium and cerium nitrates were converted into insoluble basic salts. The filtered solution of the residue then contained the nitrates of the didymium and yttrium metals, and gave the following absorption-spectrum:—

Thorite of Brevig.

Observed position of max. darkness.		Previously observed wave-length.	For	Intensity of the absorption-bands.
Reading of micrometer.	Observed wave-length.			
2354	728·3	728·3	Di	Tolerably strong.
2381	708·2	708·2	Di	Very faint.
2411	686·0	684·0	Tm	Extremely faint.
2420	679·3	679·4	Di	Faint.
2457	654·1	654·7	Er	Faint.
2480	640·6	640·4	X	Very faint.
2505	626·1	626·1	Di	Faint.
2568	591·5	591·5	Di	Faint.
2596	579·2	579·2	Di	Tolerably faint.
2605	575·4	575·4	Di	Faint.
2689	539·6	...	?	Very faint.
2713	531·3	531·3	Di	Faint.
2721	529·2	530·0	Di	Extremely faint.
2740	523·6	523·1	Er	Strong.
2747	521·6	521·5	Di	Very faint.
2781	512·2	512·2	Di	Faint, broad.
2872	485·9	485·5	X	Very faint.
2888	482·3	482·0	Di	Very faint, but sharp.
2913	476·5	477·7	Sm	Faint.
2944	469·2	469·0	Di	Strong.
2974	462·3	463·2	Sm	Faint.
3068	445·6	445·1	Di	Very strong.
3076	444·2	444·7	Di	Very strong.
3164	428·7	428·5	X	Strong.
3240	417·3	416·7	Sm	Strong.

Similar observations were made upon the nitrates derived from the other minerals above mentioned, the actual wave-lengths being in every case determined so that the position of the lines can be open to no doubt whatever.

Ten years ago the erbium earths were considered as the oxide of a single element, but we now know that they consist of the oxides of scandium, ytterbium, thulium, erbium, terbium, holmium, and yttrium. Out of the rich data furnished by the present observations the observers believe they can prove that all those erbium earths whose nitrates give absorption-spectra are not oxides of simple bodies, but mixtures of the oxides of various new elements. Yttrium, as before mentioned, has already been shown by Mr. Crookes to consist of five constituents, and it will be interesting to see what light the workers in Stockholm have thrown upon the nature of some of the remainder.

M. Lecoq de Boisbaudran showed that by fractionation of Soret's X two new substances were arrived at, which he named holmium and dysprosium, but these are now shown to be themselves compound, for one part of dysprosium is not present in thorite of Brevig or cerite of Bastnäs, although present in the mixture called X; in fact, de Boisbaudran's dysprosium lines Dyα, Dyβ, and Dyδ are found to belong to three different elements; and the other constituent, the holmium of de Boisbaudran, is probably made up of no less than four distinct components.

As the introduction of fresh names is rapidly increasing the difficulty of work in this direction, Krüss and Nilson prefer to simply label the components by affixing the letters of the Greek alphabet to already accepted symbols. The metal called by Soret X is therefore constituted as follows:—

	Wave-length of characteristic line in absorption-spectrum of nitrate.
Xα	640·4
Xβ	542·6
Xγ	536·3
Xδ	485·5
Xε	474·5
Xζ	451·5
Xη	428·5

of which

Thorite of Brevig contains...	Xα, Xδ, Xη.
,, Arendal ,, ...	Xβ, Xγ, Xε, Xζ, Xη.
Wöhlerite of Brevig ,, ...	Xγ, Xζ, Xη.
Cerite of Bastnäs ,, ...	Xα, Xη.
Fergusonite of Arendal ,, ...	Xβ, Xγ, Xδ, Xε, Xζ, Xη.
Fergusonite of Ytterby and euxenite of Arendal and Hitterö contain	Xα, Xβ, Xγ, Xδ, Xε, Xζ, Xη.

We are now accustomed to distinguish as erbium that body whose nitrate solution exhibits, in addition to a large number of lines in the violet and ultra-violet, two principal lines of wave-lengths 523·1 and 654·7 respectively, of which the former is the most intense. But in euxenite of Hitterö much greater difference is shown, one being extremely strong, while the other is barely visible; therefore here again the observers consider themselves in face of at least two elements, Era and Erβ, one giving 523·1 and the other 654·7. Moreover, they have succeeded in separating the two almost completely by a method of fractionation similar to that employed by Mr. Crookes.

Cleve, in 1879, gave the name of thulium to the metal whose oxide formed the strongest base present in the mixture of erbium earths; and its salts, according to Thalèn, exhibit two absorption-bands, 684·0 and 465·0, of which the former is the most intense. Again, the variations are found to be too great for the supposition of a single earth to be tenable, one line being entirely absent in fergusonite and thorite of Arendal, while the other is strong; hence thulium must also consist of two ingredients, Tmα and Tmβ.

The observations with regard to didymium are all the more interesting as entirely confirming Mr. Crookes's statements, and Drs. Krüss and Nilson even go further in proving either that our interpretations of the indications of spectrum analysis are grossly wrong or that didymium is composed of not less than nine distinct elements. Dr. Auer von Welsbach's symbols for praseodymium and neodymium, the two constituents of didymium which he actually separated, are discarded, and the same nomenclature adopted as in the case of holmium.

	Characteristic line in absorption-spectrum of nitrate solution.
Diα	728·3
Diβ	679·6
Diγ	579·2 and 575·4
Diδ	521·5
Diε	512·2
Diζ	482·0
Diη	469·0
Diθ	445·1
Dii	444·7

The name samarium was given by M. de Boisbaudran to an element identical with Marignac's Yβ, an ingredient of the old didymium. The nitrate of this metal gives seven absorption-bands according to Thalèn, but it is surprising that in thorite and euxenite of Arendal the line 416·7 is tolerably strong, and even very strong, without another samarium line to be seen in the spectrum; the conclusion is inevitable that there must be in this substance a constituent whose nitrate gives the line 416·7, and to this the name Smα is given, all other samarium lines being provisionally supposed to belong to Smβ.

The main result of this splendid work, therefore, appears to be that, instead of holmium, erbium, thulium, didymium, and samarium, we must, if we follow Krüss and Nilson, recognize the existence of at least twenty-two new elements, the fate of some of which may be, in the near future, to be subjected to still further subdivision. If we add to these the results previously obtained by Mr. Crookes with respect to yttrium, the astounding revelation is presented to us that instead of six we find ourselves in face of twenty-seven, or a clear gain of at least twenty-one new elements.

But now comes the vital question—Are these really new elements, or are they only different molecular aggregations of the atoms of a few, as suggested by Mr. Crookes? It certainly seems very remarkable that so large a number of elements should be crowded together about the central series of the periodic system, and we appear to have a repetition of the same phenomenon, in a much intensified degree, as obtains in the cases of nickel and cobalt, rhodium, ruthenium, and palladium, and of iridium, osmium, and platinum. It may, however, be interest-in this connexion to remember that this precise state of things was predicted by Mendelejeff himself (*Ann. Chem. Pharm.* Suppl. 8, p. 158), and in no way militates against the new element theory. Krüss and Nilson, rather than be obliged to have recourse to the introduction of new or auxiliary theories of spectrum analysis, prefer to rest upon the simpler and apparently more straightforward assumption that these substances, whose nitrate spectra show such marked differences, are indeed *bonâ fide* new elements. The accuracy of this view will doubtless be strongly contested, but in any case the result appears likely to be equally striking; for, if future work shows its want of accordance with facts, then an entirely new field of research has been opened, and the generally accepted ideas of the structure of matter must of necessity undergo a complete metamorphosis.

A. E. TUTTON.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, June 16.—"On Figures of Equilibrium of Rotating Masses of Fluid." By G. H. Darwin, M.A., LL.D., F.R.S., Fellow of Trinity College and Plumian Professor in the University of Cambridge.

The intention of this paper is, first, to investigate the forms which two masses of fluid assume when they revolve in close proximity about one another, without relative motion of their parts; and secondly, to obtain a representation of the single form of equilibrium which must exist when the two masses approach so near to one another as just to coalesce into a single mass.

When the two masses are far apart the solution of the problem is simply that of the equilibrium theory of the tides. Each mass may, as far as the action on the other is concerned, be treated as spherical. When they are brought nearer to one another this approximation ceases to be sufficient, and the departure from sphericity of each mass begins to exercise a sensible deforming influence on the other.

The actual figure assumed by either mass may be regarded as

a deformation due to the influence of the other considered as a sphere, on which is superposed the sum of an infinite series of deformations of each due to the deformation of the other and of itself.

But each mass is deformed, not only by the tidal action of the other, but also by its own rotation about an axis perpendicular to its orbit. The departure from sphericity of either body due to rotation also exercises an influence on the other and on itself, and thus there arises another infinite series of deformations.

It is shown in the paper how the summations of these two kinds of reflected influences are to be made, by means of the solution of certain linear equations for finding three sets of coefficients.

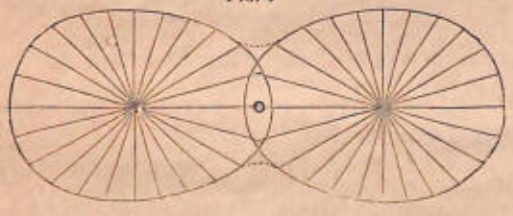
The first set of coefficients are augmenting factors, by which the tide of each order of harmonics is to be raised above the value which it would have if the perturbing mass were spherical. The second set correspond to one part of the rotational effect, and belong to terms of exactly the same form as the tidal terms, with which they ultimately fuse. The third set correspond to the rest of the rotational effect, and appertain to a different class of deformation, which are in fact sectorial harmonics of different orders. The term of the second order represents the ellipticity of the mass due to rotation, augmented, however, by mutual influence. All the terms of this class, except the second, are very small; their existence is, however, interesting.

From the consideration that the repulsion due to centrifugal force shall exactly balance the attraction between the two masses, the angular velocity of the system is found. It is greater than would be the case if the masses were spherical.

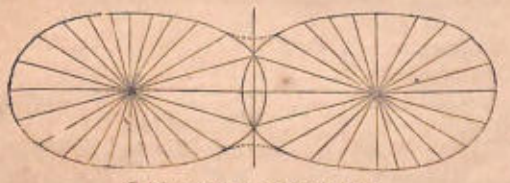
The theory here sketched is applied in the paper numerically, and illustrated graphically in several cases.

When the masses are equal to one another they are found to be shaped like flattened eggs, and the two small ends face one another. Two figures are given, in one of which the small ends nearly touch, and in the other where they actually cross. In the latter case, as two portions of matter cannot occupy the same space, the reality must consist of a single mass of fluid consisting of two bulbs joined by a neck, somewhat like a dumb-bell. In the figure conjectural lines are inserted to show how the overlapping of the masses must be replaced by the neck of fluid.

FIG. 1



Section perpendicular to axis of rotation.



Section through axis of rotation.

A comparison is also made between the Jacobian ellipsoid of equilibrium with three unequal axes and the dumb-bell. It appears that with the same moment of momentum the angular velocity is nearly the same in the two figures, but the kinetic energy is a little less in the dumb-bell. The intrinsic energy of the dumb-bell is, however, greater than that of the ellipsoid, so that the total energy of the dumb-bell is slightly greater than that of the ellipsoid.

Sir William Thomson has remarked on the "gap between the unstable Jacobian ellipsoid . . . . . and the case of the smallest moment of momentum consistent with stability in two equal detached portions." "The consideration," he says, "of how to fill up this gap with intermediate figures is a most attractive question, towards answering which we at present offer no contribution."[1] This paper is intended to be such a contribution, although an imperfect one.

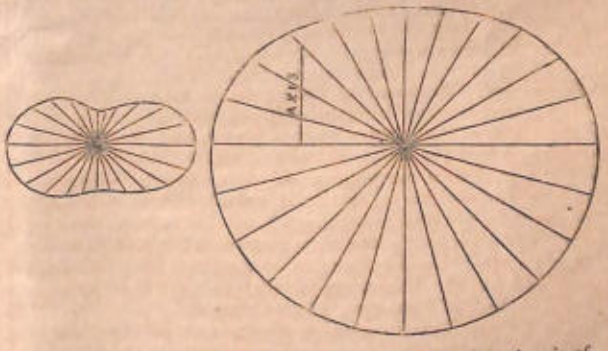
M. Poincaré has made an admirable investigation of the forms of equilibrium of a single rotating mass of fluid, and has specially considered the stability of Jacobi's ellipsoid.[2] He has shown by a difficult analytical process, that when the ellipsoid is moderately elongated, instability sets in by a furrowing of the ellipsoid along a line which lies in a plane perpendicular to the longest axis. It is, however, extremely remarkable that the furrow is not symmetrical with respect to the two ends, and there thus appears to be a tendency to form a dumb-bell with unequal bulbs.

M. Poincaré's work seemed so important that, although the figures above referred to were already drawn a year ago, this paper was kept back in order that an endeavour might be made to apply the principles enounced by him, concerning the stability of such systems. The attempt, which proved abortive on account of the imperfection of approximation of spherical harmonic analysis, is given in the appendix to the paper, because, notwithstanding its failure, it presents features of interest.

The calculations in this paper being made by means of spherical harmonic analysis, it is necessary to consider whether this approximate method has not been pushed too far in the computation of figures of equilibrium which depart considerably from spheres. A rough criterion of the applicability of the analysis is derived from a comparison between the two values of the ellipticity of an isolated revolutional ellipsoid of equilibrium as derived from the rigorous formula and from spherical harmonic analysis. As judged by this criterion, which is necessarily in some respects too severe, the figures drawn appear to present a fair approximation to accuracy.

Since, as above stated, the rigorous method of discussing the stability of the system fails, certain considerations are adduced which bear on the conditions under which there is a form of equilibrium consisting of two fluid masses in close proximity, and it appears that there cannot be such a form, unless the smaller of the two masses exceeds about one-thirtieth of the larger. It seemed therefore worth while to find to what results the analysis would lead when two masses, one of which is twenty-seven times as great as the other, are brought close together. As judged by this criterion the computed result must be very far from the truth, but as the criterion is too severe, it seemed worth while to give the figure. The smaller mass is found to be deeply furrowed in a plane parallel to the axis of

FIG. 2.



Ratio of masses 1 : 27. Upper half of figures section through axis of rotation. Lower half section perpendicular to axis.

rotation, so as to be shaped like a dumb-bell, and although this result can only be taken to represent the truth very roughly, yet it cannot be entirely explained by the imperfection of the analytical method employed. It appears then as if the smaller body were on the point of separating into two masses, in the same sort of way that the Jacobian ellipsoid may be traced through the dumb-bell shape until it becomes two masses.

M. Poincaré has commented in his paper on the possibility of the application of his results, so as to throw light on the genesis of a satellite according to the nebular hypothesis, and this

[1] Thomson and Tait, "Natural Philosophy" (1883), § 798 (i). He also remarks elsewhere that by thinning a Jacobian ellipsoid in the middle, we shall get a figure of the same moment of momentum and less kinetic energy.

[2] *Acta Math.* vii. 3 and 4, 1885.

nvestigation was undertaken with such an expectation. He remarks, however, that the conditions for a separation from a mass which is strongly concentrated at its centre, are necessarily very different from those which he has treated mathematically.

However, both his investigation and the considerations adduced here seem to show that, when a portion of the central body becomes detached through increasing angular velocity, the portion should bear a far larger ratio to the remainder than is observed in our satellites, as compared with their planets; and it is hardly probable that the heterogeneity of the central body can make so great a difference in the results as would be necessary if we are to make an application of these ideas.

It seems then at present necessary to suppose that after the birth of a satellite, if it takes place at all in this way, a series of changes occur which are still quite unknown.

Paris.

Academy of Sciences, August 1.—M. Janssen in the chair.—On the silicates of thorine, by MM. L. Troost and L. Ouvrard. It was lately shown by the authors that the study of the double phosphates formed by thorine and zircon with phosphoric acid and potassa or soda furnished no argument for associating thorine with zircon. Their further researches on the combinations of thorine and silica have yielded a compound substance, in which this base seems to be still further removed from zircon. The silicates of thorine were prepared by heating a mixture of silica and thorine with the chloride of calcium used as a solvent, and by varying the conditions two silicates were obtained, differing entirely in their composition and crystalline form. The crystals belong to the orthorhombic system, with density 6·82 at 16° C., analysis yielding 18·01 silica and 81·80 thorine. This compound corresponds to the formula $2ThO \cdot SiO_2$ (Th = 58·1), or $Th'O_2 \cdot SiO_2$ (Th' = 116·2). There is no isomorphism between this silicate of thorine and zircon $ZrO_2 \cdot SiO_2$; but here thorine may be regarded as playing the part of a bioxide. This conclusion has been confirmed by the recent experiments of MM. Krüss and Nilson, who, when determining the vapour-density of thorium, obtained numbers approaching, but always inferior to, that corresponding to the formula $Th'Cl_2$.—New fluorescences with well-marked spectral bands, by M. Lecoq de Boisbaudran. The new fluorescences here described are specially remarkable both for the number and the position of their distinct rays. They are often very bright, and are obtained by taking as agents the oxides of Sm, Za, Zβ, and as solid solvents alumina or gallina. Alumina with 1/50 of samarine shows a red, an orange, and a green band, whose positions differ little from those occupied by bands obtained from the inversion of the induction-spark on a solution of chloride of samarium. The red is extremely weak, the orange more visible, the green easily distinguished, although less luminous than the orange.—Fluorescence of spinel, by M. Lecoq de Boisbaudran. The natural spinels give both a red fluorescence, whose spectrum has been carefully described by M. Edm. Becquerel, and also occasionally a greenish fluorescence. It is here shown that the former is due to the presence of chromium, the latter to that of manganese. By introducing 1/1000 of MnO into the composition of artificial spinel, the beautiful green fluorescence gives the same green band, but considerably more intense. By replacing the manganese with 1/100 of oxide of chromium, there is developed a magnificent red fluorescence presenting all the characters of that of the ordinary natural spinels.—Heat of formation of some crystallized tellurides, by M. Ch. Fabre. It is shown that several metallic tellurides may be obtained by heating in nitrogen a mixture of powdered tellurium and filings of the metal. The tellurides of iron, nickel, cobalt, and thallium not hitherto obtained, are crystallized, resisting hydrochloric acid and sulphuric acid at a low temperature, but slowly changing in a moist atmosphere. Reduced to a fine powder they are easily dissolved in bromine and the water of bromine yielding the corresponding bromide, hydrobromic acid, and tellurous acid. A comparison of the heats of formation of the crystallized tellurides and selenides seems to show that in the same group, according as the equivalent weight of the metalloid combined with the metal increases, the quantity of heat liberated by the combination diminishes. But in order to verify this hypothesis, it would be necessary to determine the heat of formation of the corresponding crystallized sulphides.—On the succinimidoacetic and camphorimidoacetic ethers, by MM. Alb. Haller and G. Arth. In order to obtain these ethers, the authors have employed the sodified derivatives of succinimide and camphorimide, the latter behaving like its analogues in the presence of the alkaline metals.—On a new isomere of benzine, by M. G. Griner. Besides the dipropargyle belonging to the fatty series discovered by M. L. Henry, the author has obtained another isomere of benzine, which does not combine with ammoniacal cuprous chloride, and consequently is not acetylenic. Its simplest formula would seem to be $CH_2—C\equiv C—C\equiv C—CH_2$.—Remarks in connexion with the observations of M. Grawitz on the preparation of the chromates of aniline and their applications, by MM. Ch. Girard and L. L'Hote. The authors repeat that they were the first to isolate and study the bichromate of aniline, a crystalline salt, of which they gave the formula and chemical properties, and from which they have succeeded in preparing certain colours such as mauvéine, pheno-safranine, violaniline, &c.—On the effects of salting on pig's flesh affected by charbon, by M. F. Peuch. The experiments here described show that even in thoroughly salted bacon the charbon is not killed, but its virulence is destroyed.—On a new microbe determining indigotic fermentation and the production of blue indigo, by M. E. Alvarez. The author's experiments show that indigo is a product of fermentation determined by a special microbe greatly resembling those of pneumonia and rhinosclerome, which also have the power of setting up indigotic fermentation. The microbe of indigo also possesses pathologic properties determining either a passing local inflammation, or even rapid death with congestions of the viscerae and fibrine exudations.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

A New Mode of Geometrical Demonstration, with Examples: D. Mair (Brown, Aberdeen).—Terra: A. A. Anderson (Reeves and Turner).—Annales de l'Observatoire de Nice, tome ii. (Paris).—Food Adulteration and its Detection: J. P. Battershall (Spon).—Electricity: W. Larden (Longmans).—British Dogs, Nos. 9 and 10: H. Dalziel (Gill).—Bees and Bee-keeping, vol. ii. parts 9, 10, 11: F. R. Cheshire (Gill).—McGill University Annual Calendar, Faculty of Medicine (Montreal).—On the Education of Engineers: H. Dyer (Munro, Glasgow).—Hints to Meteorological Observers, 2nd edition (W. Marriott (Stanford).—Archives Néerlandaises des Sciences Exactes et Naturelles, xxi. (Harlem).—Brain, part xxxviii. (Macmillan).—Quarterly Journal of the Royal Meteorological Society, April (Stanford).—Meteorological Record, vol. vi. No. 24, vol. vii. No. 25 (Stanford).—Annalen der Physik und Chemie, 1887, No. 8 (Leipzig).—Beiblätter u den Annalen der Physik und Chemie, 1887, No. 7 (Leipzig).

CONTENTS.

	PAGE
The Topography of Galloway. By **Joseph Lucas**	337
Our Book Shelf:—	
Candler: "The Prevention of Consumption."—**Dr. E. Klein, F.R.S.**	340
Millis: "Metal Plate Work"	340
Lindley: "Walks in the Ardennes"	340
Letters to the Editor:—	
The Parietal Eye in Fishes.—**J. Beard**	340
Physiological Selection.—**Dr. George J. Romanes, F.R.S.**	341
The Droseras.—**J. Rand Capron**	341
Comrades.—**E. R.**	341
A New Cosmogony. II. By **A. M. Clerke**	
Music in Nature. By **Dr. William Pole, F.R.S.**	
The British Museum (Natural History Branch). By **Dr. Henry Woodward, F.R.S.**	345
Notes	347
Astronomical Phenomena for the Week 1887 August 14–20	350
New Guinea Exploration	351
The Institution of Mechanical Engineers	352
The Structure and Progress of the Forth Bridge. By **E. Malcolm Wood**	353
The Machinery employed at the Forth Bridge Works. By **William Arrol**	356
The Chemistry of the Rare Earths. By **A. E. Tutton**	357
Societies and Academies	358
Books, Pamphlets, and Serials Received	360

THURSDAY, AUGUST 18, 1887.

THE PHYSIOLOGY OF PLANTS.

Lectures on the Physiology of Plants. By Julius von Sachs. Translated by H. Marshall Ward, M.A., F.L.S. (Oxford: Clarendon Press, 1887.)

IT is significant of the progress which the science of botany has made in the last twelve years that the accepted text-book of Sachs, which was published in 1873 as a single volume of 850 pages, is now represented by three volumes with an aggregate of about 1900 pages. The anatomical treatment in the original text-book was condensed, and wanting in detail; it is now replaced by the comparative anatomy of De Bary. Book II. of the text-book, which dealt with special morphology, has been re-edited by Goebel as a separate work, a translation of which has recently appeared (see NATURE, vol. xxxv. p. 577). The physiological portion of the original text-book, entirely remodelled and re-written by its author, appeared in Germany in the form of the "Vorlesungen über Pflanzenphysiologie," a translation of which is the book now under review. In producing this as the last of the series of three volumes above mentioned, the Clarendon Press has completed an undertaking which must earn the heartiest thanks of English students of botany.

It is not improbable that the publication of this volume will mark a stage in the history of the science in this country. The period of dependence mainly on translated text-books has now been of some duration, and it is not to be expected or desired that translation should altogether cease. Nevertheless it is unsatisfactory to receive the bulk of our supply at second hand, and subject to those delays which necessarily attend translation. It is to be confidently hoped that a period of home production, which has already begun, will now ensue, and thus demonstrate at once the healthy growth of the science amongst us, and the fact that there is still alive that skill of exposition in which this country has not been deficient in the past.

Written in the lecture form, and in an easily-flowing style, which the translator has successfully reproduced, the book is aimed at, and should surely attract as well as satisfy, "students and cultivated readers." The first five lectures are devoted to organography, and writing as a physiologist, with the express purpose of preparing the way for a physiological treatment of the subject, the r has adopted a method of "physiological organophy," protesting against that "purely formal morphology," which has been prevalent during the last thirty or forty years, and which he complains of as having left the physiological relations of organs entirely out of account.

In laying down his system of "physiological organography," the author ranges all organs in five categories: (1) root, (2) shoot, (3) sporangia and spores, (4) archegonia, (5) antheridia. It will be noted at once that the term shoot is used in a comprehensive sense including leaf and stem where these are distinguishable. The shoot as a whole is thus co-ordinated with the root, a method which commends itself physiologically as more suited to the time than the old distinction of stem, leaf, and root as co-ordinate categories. Secondly, it will be observed that the time-honoured attempt to recognize in the sporangium the result of metamorphosis of some part of the vegetative system, in fact, to define it as representing a metamorphosed leaf, pinna, &c., is abandoned, and Goebel's generalization that the sporangium is an organ of independent nature is accepted. The author then proceeds to apply his method of treatment to the vegetative organs. Referring, by way of illustration, to numerous plants, he distinguishes as *typical forms* of root or shoot those which "present the essential peculiarities in great perfection," he recognizes as *rudimentary* those parts of plants low in the scale, in which "the organic differentiation generally is not so far advanced as in the typical ones," and as *reduced forms* those in which it may be assumed "that, in consequence of special modes of life, more simply organized forms have again arisen from those more highly organized." Lastly, he designates as *metamorphosed forms* those "which have, it is true, been derived later from the typical ones, but which contribute to the greater perfection of the entire organism," such as flowers, tendrils, &c.

In the application of this system the rhizoids of mosses, of fern prothalli, and liverworts, the organs of attachment of various Algæ, and even the mycelium of Fungi, are designated "roots," while the term "shoot," including the distinctive parts of stem and leaf, where these are distinguishable, is applied indiscriminately to the aërial parts of vascular plants, mosses, Algæ, and even to the fructification of the Fungi. While accepting this method as throwing a certain light on the various forms of plants, when regarded from the physiological point of view, it cannot be too strongly impressed upon us that it is in no sense a substitute for the purely formal morphology. This is clearly stated by the author himself, when he says (p. 72) that his method "is by no means intended to exclude the purely formal comparison as it has hitherto been conducted under the name of morphology; its effect on the latter is only to be that of explaining and enlightening." While reading these admirably-written lectures, some whose bent is strongly physiological may think that pure morphology has had its day, and is effete, while the true and only point of view is the physiological; but it is not the author's object to teach this doctrine, and it is to be regretted that, in order to avoid any uncertainty of interpretation, a more distinct terminology was not introduced. The author, who draws a clear distinction between the morphological "member" and the physiological "organ," might well have devised a system of terms applicable exclusively in this physiological sense, and so have both cleared the way for his own views and have saved from risk of error those whose morphological sense is dull.

A concise exposition of the internal structure of the plant follows, the cellular character of most plants being contrasted with the structure of the Cœloblastæ, which Sachs has designated "non-cellular plants." This part, though illustrated by many of the familiar figures from the old text-book, has been entirely re-written in accordance with more recent researches. It is not merely a descriptive and comparative treatment; the physiological end is constantly kept in view, so that the first twelve lectures may be regarded as preparatory to the more purely physiological part which follows. After a short explanation of the external conditions of vegetable life,

and of the physical properties of plant-tissues, the remainder of the book is assigned to physiology properly so called, and it is divided into four parts dealing respectively with nutrition, growth, irritability, and reproduction. It is impossible, within the limits of a short review, to give an adequate idea of this comprehensive work; but it may be stated at once that it is as a whole decidedly preferable to the physiological part of the old text-book, which it has replaced. Its superiority is based not solely on its more modern view and larger sphere of observation, but also on its more clear construction. The information it contains is more easily accessible to the student, and to this end the addition by Prof. Marshall Ward of a thorough working index will materially conduce.

It remains to mention certain points in the book which for various reasons will be of special interest to English readers. Sachs's views on the transfer of water in plants are well known from his other writings. Here he puts forward in a concise form his opinion that the transfer is effected through the substance of the lignified walls. Much has been written since the first publication of these lectures to shake confidence in Sachs's view, and a defence of his position against recent attacks would now be of greater interest than the plain statement of his own case which is here given. In the succeeding chapters, on the regulation of the stream of transpiration, and the consequent supply of salts in solution, and on the general nutrition of plants, there is little to demand detailed notice. The writing is clear, and works up the results of recent investigation in a very readable form.

In the next part, which treats of growth, there is much fresh material to interest English readers, the most notable being that in Lecture XXVII. Here Sachs gives a really masterly epitome of his researches on the arrangement of cells in embryonic tissues, reducing to a system what was before 1878 a chaos of isolated observations, and leading up to the important conclusion that "the mode of cell-division depends only upon the increase in volume, and the configuration of the growing organ," and further, that apical cells, where they occur, are merely to be regarded as gaps in the system of construction. After a series of seven lectures on irritability, the volume closes with a discussion of reproduction, both from the comparative and physiological points of view.

Regarded as a whole the book is certainly a remarkable one. Prof. Sachs is a man who does not undervalue his own work, and who has no fear of stating his own convictions; and this volume may fairly be taken as expressing his opinion on vegetable physiology in 1882. In this respect it will always be a valuable work, and will maintain an historic interest long after the actual views expressed in it are either superseded, or have passed out of the range of controversy. F. O. B.

A DICTIONARY OF PHILOSOPHY.

A Dictionary of Philosophy in the Words of Philosophers. Edited, with an Introduction, by J. Radford Thomson, M.A., Professor of Philosophy in New College, London. (London: R. D. Dickinson, 1887.)

TO those who like to pick up information in a scrappy way, this volume will no doubt prove useful. Chancing, for example, on the word *realism*, and feeling somewhat hazy as to its exact meaning, the inquiring reader turns to his "Dictionary of Philosophy," and under the head "Realism or Dualism" finds a statement from Fleming of the theory "as generally held," and short paragraphs descriptive of (1) Sir W. Hamilton's natural realism, (2) Herbert Spencer's transfigured realism, (3) the reasoned realism of George H. Lewes, and (4) intuitive realism, McCosh. Still unsatisfied, he turns to the "Theories of the Concept," and learns of the doctrine of realism from Monck, Whately, and Mill; of its varieties (extreme realism and moderate realism) from Ueberweg; of its origin from Ferrier, Maurice, and Ueberweg; of its truth and error from Noah Porter and Whately; and he is perhaps rather shocked, in conclusion, to learn from Mill that it is "an abandoned doctrine."

An introduction (of 35 pages) has been written by the editor, "for the sake of beginners in philosophical studies, with the view of affording to such readers a general survey of the field of thought before them." We think the editor might have added, "and as an incentive to turn for explanations to the body of the work." We doubt whether the beginner would gain much from a "Sketch of the History of Philosophy" so short as that given in the fourth part of the introduction. We quote, by way of example, the description of post-Kantian German philosophy, with one sentence of which we are in complete accord:—

"The course of philosophy in Germany since the time of Kant has been very remarkable, but is very difficult thoroughly to trace. The following are, however, the chief developments:—(1) German idealism advanced with very rapid strides. It is common to say that Fichte's subjective idealism was followed by the objective idealism of Schelling, and that by the absolute idealism of Hegel. But such a description can convey no meaning to the ordinary reader. (2) In reaction from this tendency was the modern German materialism, expounded by Moleschott, Vogt, and Büchner—a modification of the ancient atomism, according to conceptions of modern science. (3) A development of one side of Kant's philosophy was the pessimism of Schopenhauer and Von Hartmann. According to the former of these, the absolute existence which Kant held to be unknown is will, whilst the latter lays the greatest stress on the unconscious. These thinkers are, however, better known for their theory of human life, of which both take a gloomy and despondent view. (4) Herbart by no means accompanied the progress of post-Kantian idealists; he is characterized by Schwegler as 'extending the monadology of Leibnitz.' (5) Ulrici and Lotze may be taken as examples of German philosophers who hold by the spiritual interpretation of human nature."

The arrangement of the body of the work is as follows:—Two preliminary sections are devoted respectively to (1) "Designations, Definitions, and Divisions," and (2) "The Mind." In the latter are subdivisions on (1) mind, (2) the intellect, (3) faculties of the intellect, (4) personality and the ego, (5) the nature of man, and (6) consciousness. Then follow four main divisions: (A) the psychology and philosophy of cognition, including three sections on ancient, mediæval, and modern schools; (B) the psychology and philosophy of feeling, with paragraphs on æsthetics; (C) the psychology and philosophy of the will, with a section on free-will and determinism; and (D) moral philosophy of ethics, with a concluding section on the immortality of man. The

sections on cognition do not seem to be very happily arranged; but a double index—an index of names and an index of subjects—renders it easy to make use of the volume as a dictionary. It would have been well, however, if a synoptical table of contents had also been added.

Turning now to one or two points of more special interest to the man of science, we think that the promise in the preface that "a fair representation has been secured of the teaching of the physiological and evolutional psychologists of our own time," is by no means fully redeemed. Barely a page and a half is devoted to "The Brain and Nervous System." The page on "Sensibility and Muscularity" is not very satisfactory; while the information conveyed in the three pages or so devoted to "The Five Senses" is sufficiently meagre. Such observations as Goldscheider's on "pressure-spots" and "temperature-spots" are not alluded to. We have come across no mention of Lotze's theory of local signs. But it would be easier to enumerate the few elementary points that are mentioned than the many important generalizations that are ignored.

Looking up *evolution* under "Modern Philosophical Schools," we find Mr. Herbert Spencer's well-worn definition preceded by that given by Mr. Sully in his article in the "Encyclopædia Britannica," an extract happily chosen. Two or three paragraphs on mental evolution from "The Principles of Psychology" are then cited. Mr. Sully's criticism of the Spencerian position is succeeded by Mr. Stirling's sweeping and not very acute criticism of the evolution theory in general. A paragraph from Mr. Fiske, on evolutional religion, concludes the two pages and a quarter devoted to this subject. There are indeed other incidental quotations, but we cannot say that the doctrine of evolution is adequately represented.

Nothing, however, is easier than to find fault with the execution of a work of this kind. We trust the labours of the editor and of the "collator of experience" have not been expended in vain. There are in this "Dictionary" a great number of well-selected passages from philosophers of all shades of opinion; and there must be many men with but little leisure for philosophic study who will be glad to make or to renew acquaintance with the thoughts and the speculations here presented.

C. Ll. M.

OUR BOOK SHELF.

Hay Fever and Paroxysmal Sneezing. By Morell Mackenzie, M.D. Fourth Edition. Pp. 96. (London: J. and A. Churchill, 1887.)

PERHAPS none of the minor ills to which humanity is prone has given rise to so much discussion as the subject under review. We have the views of those who regard it as a complaint due to "pollen"; of those, again, who look upon it as a neurosis, in which the much maligned and little understood "sympathetic system" is considered to play the chief part; and of others who attribute this and kindred disorders to the hurtful consequences of the presence of swellings, exostoses, bony ridges, &c., in the nasal cavities. The latter school relies on a mode of treatment which in its endeavours to clear the nose of all so-called obstructions, by the free use of the saw, the drill, the gouge, the dental engine and electric motor, &c., reminds one more of the efforts of a mechanic, anxious to bring the nasal cavities into comparison with a polished eburnated cylinder, than of the intelligent practitioner. This kind of thing is being carried to excess, and an earnest protest must be made against the officious and meddlesome surgery of the nasal passages which is advocated amongst a certain class of modern specialists.

It is an old idea that hay-fever is produced in persons having a certain nervous erethism, or predisposition, by the contact of the pollen of certain flowering grasses with some portion of the upper respiratory tract, or the conjunctiva. Dr. Mackenzie is an advocate of this view, and he regards the action of this pollen as more dependent upon its "vital, than chemical or physical characteristics." Those grains with the longest pollen-tubes (Liliaceæ) are less irritating than the pollen of Graminaceæ, the pollen-tubes of which are quite rudimentary. Pollen rubbed into the noses of hay-fever patients is exceedingly irritating, and is more active than alum or tannin. Dr. Mackenzie thinks that the absence of vibrissæ, or want of mobility of the alæ nasi, or dryness of the mucous membrane, leads to the entry of pollen into the nasal cavities. Many interesting facts are referred to in this book which substantiate the author's views; and it is difficult to come to any other conclusion, in the face of such an able exposition, than that, whatever may be the condition of the sympathetic or central nervous system, which in a word constitutes the necessary "predisposition," the introduction of pollen into the eyes, nose, or throat, is necessary for the production of "hay-fever." Some interesting experiments are related by Dr. Mackenzie on dredging the atmosphere during the hay-fever season, with the object of counting the pollen-grains floating in the air. While these were enormously increased during the month of June when hay-making was general, and diminished during July in the country, even the air of the streets of London was only on one or two days during this season found to be free from pollen-granules. Thus persons, even in the heart of a large town, are not free from this external irritant.

The section on paroxysmal sneezing is very good. The author regards the affection as one of the respiratory centre, the afferent impulse of which is conveyed by the trigeminal nerve-fibres. Dr. Mackenzie rightly condemns much of the unscientific jargon written about the power of isolated ganglia, such as Meckel's ganglion, to be directly concerned in these conditions, and justly refers the nervous mechanism to the cerebro-spinal centres, quoting at length Gaskell's recent researches on the sympathetic nervous system, on which, indeed, he founds his views. The author's ideas are set forth with great ability and moderation, and this book forms a valuable contribution to the discussion of this much vexed subject. The treatment of these complaints is fully dealt with in the book.

The Owens College Course of Practical Organic Chemistry. By Julius B. Cohen, Ph.D., F.C.S. (London: Macmillan and Co., 1887.)

WHATEVER may be the failings of this little book, there is no doubt it is a step in the right direction—that of making what is called organic chemistry really a practical study, as is the case with inorganic. The introducers of the author, in a short preface, seem to imply that the practical study of organic chemistry should of necessity be connected with, and indeed lead up to, research. Now, however desirable it may be that a considerable number of people should do organic research, there are a great many cases where the student of chemistry will gain as much as will be useful to him by simply making some careful preparations, just as is done with ordinary quantitative analysis, with no intention of making analysis a profession.

It has no doubt been a standing disgrace in this country that, up to within the last few years, organic chemistry

has been a "*black-board*" subject in most, if not all, schools; and for this neglect there is little excuse, for a great number of most important experiments may be made without more expenditure than in the case of ordinary quantitative analysis.

A somewhat similar plan of work to the one in this little book has been followed for the last three or four years at the summer course of the Normal School of Science, and no doubt other Colleges where chemistry is a leading subject will have adopted some plan of practical organic instruction. The publication of this book will save some trouble to teachers in directing the preparations. The book is divided into two parts, and, curiously enough, what is generally considered the easier, viz. marsh-gas derivatives, are put in the second part. The author gives as his reason for this, that the selected examples offer fewer difficulties. That is a matter of opinion to some extent, and may depend on the course of lectures the student is hearing at the time.

In Part I., after the purification of alcohol, ether, benzene, and short descriptions of boiling-point determination and fractional distillation, we pass on to formation of benzene derivatives, commencing with bromobenzene, ethyl benzene, &c., to typical members of different families, ending with ethyl benzoate. The descriptions of process to be followed are short, but generally to the point, and are preceded in each case by references to the literature on the subject, which is a very valuable addition, and should be useful to beginners. The appendix, consisting of notes on the preparations, is very good, but would have been better placed, probably, in the text, or in connexion with the most typical substance of a group or family. As to the physical constants, melting and boiling points and specific gravity only are mentioned. Surely a great number of substances, the preparation of which is described, allow of their vapour-densities being taken by Victor Meyer's method? Beyond that there is little to complain of. The book is fairly well adapted for its ostensible purpose.

My Microscope. By a Quekett Club Man. (London: Roper and Drowley, 1887.)

IT is impossible to give in a small volume of some sixty pages a clear description of the microscope and the wonders it reveals. Still the author has managed to make his little essays interesting; and if there is not much depth in his work, he has perhaps written enough to induce some of those who are not already the possessors of a microscope to get one. It is surprising that he has not laid more stress on the advantages of a binocular over a monocular instrument.

LETTERS TO THE EDITOR.

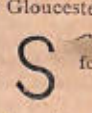
[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*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.*]

Sun and Fire Symbolism.

THERE is a phase of sun and fire symbolism in our very midst which seems hitherto to have received but little attention, viz. the presence of such symbols as crests or in the coats-of-arms of many of the oldest noble families and landed gentry of the British Isles. We find them in the greatest numbers in the armorial bearings of our Scottish families, and those belonging to the most northern counties of England; probably for the same reasons that they are most numerous on objects which have been found in the northern portions of Scandinavia, *i.e.* that the light and warmth of the sun were naturally prized in such districts, and also because they have there survived longer, owing to the isolated position of the inhabitants depriving them of free intercourse with the outer world.

In a letter in NATURE (vol. xxxv. p. 558) headed "The Svastika both as Sun and Fire Symbol," I gave illustrations of some of the emblems of the sun and of the svastika as a fire symbol, and also alluded to the wheel as being in use in some countries to this day as a preservative against fire. A type of fire symbol exists in some parts of England at our very doors. In Gloucestershire and Herefordshire—possibly also in some of the other south-western counties of England—it is not an uncommon circumstance to see on the external walls of some of the older houses one or two pieces of iron in this form  and sometimes thus  . It seems evident that they could not have added much support to the building, since they were bolted on to it at one point only—the centre.

A most interesting explanation of them was given a few years ago by an old servant of our family who died about five years ago; his age went with the century. He was a Gloucestershire man, and on being asked the reason of the  form of these irons, he replied "that they were made thus in order to protect the house from fire, as well as from falling down."

In the little village of Kingstone, in Herefordshire, it is still the custom for the people on the eve of May-day to take two short pieces of wood and nail them in this form  over the door of a house or a stable, removing the one of the previous year. On inquiry why this was done, the reply was, "To scare the witches or the evil spirits away."

In the crests and armorial bearings of many of our families we find at least three distinct forms of sun and fire symbolism.

(1) The *sun* in splendour.

(2) *Fire*, represented sometimes by a mountain in flames.

(3) The *sun* as a ring, or as a simple circle, the heraldic terms for this latter type being amulets (Collins's "Peerage of England," London, 1779) and annulets (Sir Bernard Burke's "Peerage, Baronetage, and Knightage," London, 1880).

I propose now to give examples of a few of the most typical of each kind.

Blount, Bt.—This family is of French extraction, and formerly Lords of Guisnes, in France; their crest is an armed foot in the sun. Motto, *Lux tua via mea.*

Blunt, Bt.—Probably originally the same family. These latter have for a crest the sun in glory, charged in the centre with an eye, issuing tears.

In the Earl of Clancarty's arms—the Trenches came from Poitou in 1575—on the first and third quarters is the sun in splendour, and in the centre an escutcheon with the coronet of a Marquis of the Netherlands, charged with a wheel with six spokes. (The wheel is still used as a preservative against fire, both in Holland and in Denmark.)

Musgrave, Bt., of Hayton, has, for his crest, two arms in armour embossed, sustaining the sun; so has also Musgrave, Bt., of Tourin, co. Waterford; and their arms are the same.

The rising sun and the sun in his splendour is also borne—

By the Marquis of Lothian, by the Earls of Stamford and Warrington, by Lords Polwarth and Hammond—Lord Polwarth's crest is a lady richly attired, holding a sun in her right hand and a half moon in her left; and it also forms the crest of Tyrwhitt, Bt., Fairbairn, Bt., the Earls of Antrim, Nicholson, Bt., where it is placed between two stars of eight points, and of many more families.

We find fire symbols in connexion with the sun in the armorial bearings of Macleod of Lewis. Their crest is the sun in splendour, and in their arms they have a mountain in flames on the

first quarter and the three-legged Manx man in the second quarter; the motto belonging to this latter is *Quocunque jeceris stabit.*[1] The Duchess of Sutherland (Countess of Cromartie) bears this symbol in her arms for Macleod, in the first and third quarters.

The crest of the Earls of Seafield is a salamander in flames; the Marquis of Hertford has a phœnix in flames out of a ducal coronet; Mackenzie of Scatwell, co. Ross, has on the second quarter of his arms a rock in flames, and on the third quarter three legs of a man armed, for Macleod of Lewis; Lord Ongley, a phœnix in flames holding in its beak a fire-ball; Verney, Bt., a demi-phœnix in flames looking at the rays of the sun; and Carmichael, Bt., has in the second and third quarters of his arms, a cup with flames of fire issuing therefrom.

We will now turn to the third portion of our subject—the sun as a ring, or a simple circle—bearing in mind that the former type is in heraldry called *amulets* or *annulets*, and that the simple circle is styled a *bezant*.

The Earl of Lonsdale has in his coat-of-arms six annulets forming a triangle; the Earl of Bantry one annulet in the first, and ten bezants in the fourth quarter of his coat-of-arms; Barron, Bt., on a Saltier gu., five annulets. Amory Bagge, and Bailey, Bts., bear also annulets.

In the possession of a member of the writer's family is a seal of rock crystal, on which is the motto *Luceo non uro*;[2] beneath this is a baron's coronet (for the Barony of Strange, which came to the Dukes of Athole through the female line), and below this, again, the sun in glory. It is believed (but none are now living who know this for a fact) that this seal formerly belonged to Marjory, second wife of the fourth Duke of Athole, eldest daughter of James, sixteenth Lord Forbes, and widow of John Lord Macleod, who died s.p. in 1789; she married the Duke in 1794, and died in 1842, having had by him a son and a daughter who predeceased her.

The Isle of Man belonged at one time to the Macleods—when, is apparently not known—but in 1405-6 it came into the possession of the Stanleys (afterwards Earls of Derby), through Sir John Stanley, Kt., who in conjunction with Roger Leke received a commission to seize the city of York and its liberties, and also the Isle of Man upon its forfeiture by Henry Percy, Earl of Northumberland. The Stanleys held the Isle of Man until the death of Ferdinando, the fifth Earl, without male heirs, when the Barony of Strange—which title had been borne by the second Earl, who died in 1522—fell into abeyance between his daughters, and the earldom went to his brother William, sixth Earl, who bought from his nieces their claims on the Isle of Man. His son, again, the seventh Earl, was summoned to Parliament in 1627 as Baron Strange, under the impression that his father had enjoyed it; this was, however, not the case, and the summons was virtually a new peerage, which eventually devolved upon the ducal house of Athole, through the marriage of Amelia Anna Sophia, youngest daughter of the seventh Earl of Derby, by his wife Charlotte de la Trémoille, daughter of the Duc de Thours in France, with John, second Earl and first Marquis of Athole. Failing male heirs to her father, Charlotte, daughter of the second Duke of Athole, became Baroness Strange on his death in 1764, and also succeeded to his rights in the Isle of Man. She married her cousin, John Murray, who became the third Duke.

Another seal, also in the writer's family, has an impression which differs essentially from the armed legs of the Isle of Man. It is known to have been the private seal of the fourth Duke. The Manx emblem correctly described is "the three legs of a man armed ppr. conjoined in the centre at the upper part of the thighs, placed in triangle garnished and spurred *or*": but on this seal the three legs are bare, and conjoined in the centre by a sun with rays. In fact, it is the trinacria of Sicily.

HARRIET G. M. MURRAY-AYNSLEY.

[1] Signifying, "However you throw me I stand." This is true of the svastika, a fire symbol likewise.

[2] "I give light, but I do not burn."

Bishop's Ring.—The Sky-coloured Clouds.

DURING a recent visit to the Alps I carefully looked for Bishop's ring, and found that it was generally visible at high altitudes in the middle of the day when the sky was clear. On the whole, the higher one ascended, the plainer it was, up to a height of 9000 feet, beyond which I did not go. On one occasion it was visible nearly or quite as low down as Chamounix, altitude 3400 feet; but this was the lowest point I saw it from. It was always extremely faint, so much so that if I had not seen it previously in its greater brightness I should not have noticed it at all,—indeed, it could usually only be detected by a careful comparison of the colour of different parts of the sky. Its dimension seems the same as heretofore.

About sunrise and sunset this circle continues occasionally conspicuously visible here, but it is long since I saw it in the middle of the day, when the sky has been really clear; sometimes, however, I have seen a similar circle, but with much duller colours and extremely feeble, giving one the impression that it was lower in the atmosphere than Bishop's ring as caused by the volcanic dust, and that it might be caused by smoke.

Last evening, and still more this morning, there was a bright display of the "sky-coloured clouds" (if I may so call them). I seldom or never saw them more brilliant than they were this morning, when I observed a circumstance as regards their colour that I have not noticed before: the greater part of them was coloured as usual, the lower part a dull yellowish green, and the upper part a bright, though light, blue; but there was a border of yellow above the blue, very faintly lit up it is true, but unmistakable. The display was almost confined to that part of the horizon between north-north-east and north-north-west, the cloud-mass evidently not extending further east or west to any extent, and the upper border after 2 a.m., at least, was evidently the actual southern edge of the cloud sheet.

I had the opportunity of watching these clouds gradually fade away in the increasing daylight, showing that in all probability they did not evaporate, but simply became invisible owing to the increasing light of the sky, and perhaps also to their losing light themselves. It is again evident, therefore, that they are of an exceedingly filmy and transparent nature, and indeed can hardly be considered real clouds at all.

Their motion was very slow, but appeared on the whole much as usual—viz. from a north-easterly direction.

Sunderland, July 30. T. W. BACKHOUSE.

The Electricity of the Contact of Gases with Liquids.

SINCE the delivery of Helmholtz's famous Faraday Lecture, "the charge on the atom" has been assumed by physicists, notwithstanding the very serious objections urged by Maxwell against such a theory. A re-perusal of the latter, some eighteen months ago, excited me to make some experiments on the subject. It occurred to me that by allowing such solutions as potassic iodide and chlorine water to react in an insulated vessel some information might be obtained as to the equality or inequality of the atomic charges.

My object at present is not to give an account of the many experiments of this kind which I made, but briefly to call attention to one result to which they led, and I shall describe only such experiments as are necessary for this purpose.

One of the electrodes of a quadrant electrometer was "put to earth," the other was connected to an insulated stand on which was placed a porcelain dish containing a small quantity of distilled water. The instrument was in a rather sensitive state. A high-resistance Daniell, through which a current never passed, gave a deflection of 94 divisions either way. A small fragment of potassium was cast on the water. The spot went rapidly to the left, indicating a negative charge on the porcelain dish, and a positive charge on the escaping hydrogen. A second fragment of K was thrown on the liquid in the insulated dish. The spot moved 28 divisions to the left, then turned and went up the scale to the right 300 divisions. A third piece of K was thrown on the liquid in the insulated basin, and the spot moved 40 divisions to the right. This behaviour was extremely perplexing. The connexions were looked up, and the experiment repeated with like irregular results.

Na was used instead of K, and although the deflections then obtained were also irregular, the tabulated results showed a contrast. When Na was used, 40 per cent. of the deflections were *first* to the left; when potassium was used, 70 per cent. of the deflections were *first* to the left. Speaking broadly, this seemed to indicate that with K the hydrogen came off charged positively, with the sodium it came off charged negatively; and as I thought that such a result would throw some light on the atomic charges I tried very hard to eliminate what I then believed to be accidental exceptions, and to prove that in reality such was the case. But I tried in vain.

Retaining the same method of testing the electrification, other

combinations, such as H_2SO_4 and Na, H_2SO_4 + Zn, HNO_3 and K, &c., were tried, and in most cases irregular deflections such as those above described were obtained. Ultimately I got two constant and definite results: (1) Na thrown on strong and pure acetic acid invariably left a positive charge on the insulated dish, the escaping hydrogen being negative; (2) a fragment of zinc thrown into strong HCl invariably left a negative charge on the insulated dish, the escaping hydrogen being positive.

This last is a gross and unmistakable result. In fact its very magnitude was for some time a source of embarrassment. I shall not stop to describe the steps by which the next experiment was reached, but shall proceed at once to describe it; and I shall venture to give it somewhat in detail, as the title at the head of the paper is mainly founded on it.

The electrometer was not in a very sensitive state. The high-resistance Daniell aforesaid gave a deflection of 38 divisions on either side. A glass beaker 7½ inches high and 5 inches in diameter was placed on the insulated stand. A porcelain dish, 2¾ inches in diameter and 1¼ inches high, was nearly filled with a 10 per cent. solution in distilled water of strong HCl, and placed at the bottom of the glass beaker just described. The insulated stand was now connected to one pair of quadrants, the other pair were put to earth. The "spot" stood at 378 on the scale. Three small fragments of granulated zinc were now dropped into the dilute HCl in the insulated dish. A very slight effervescence at once appeared. This gradually increased but never became violent. No trace of spray could be detected at the end of the experiment above the lower half of the beaker. In 4 minutes from dropping the zinc the spot could be perceived moving, and in 4½ minutes more it moved 28 divisions to the left, indicating the charge on the dish negative and the escaping hydrogen positive. The insulated stand, &c., was now disconnected from the quadrants. The spot maintained its position on the scale. In 1½ minute after, the quadrants were again connected to the insulated stand: the spot moved instantly 20 divisions more to the left. In 1½ minute more it had moved 10 divisions further to the left, but with a slower pace, and it presently stopped and turned back, at first slowly, taking 5 minutes to go back the 68 divisions to the zero. In 4 minutes more it had moved 80 divisions to the right. The insulated stand was once more disconnected from the quadrants, and at the end of 2 minutes they were re-connected, when the spot instantly bounded up 55 divisions further to the right. It continued to move in the same direction until the effervescence ceased owing to the acid being exhausted. A quantity of the zinc survived. On short-circuiting the quadrants the spot returned to within 4 divisions of the original zero.

As the reaction between zinc and hydrochloric acid proceeds, the quantity of chloride of zinc in solution continually increases, and so it appears demonstrated that when hydrogen passes through hydrochloric acid it acquires a positive charge, when it passes through chloride of zinc it acquires a negative charge. I believe that this inference may be safely very much generalized, but for the present I forbear. In confirmation of it, however, it may be well to mention that at any stage of the last experiment a deflection may be obtained to right or left as required by adding an excess of saturated chloride of zinc (for the first), or of hydrochloric acid (for the second).

When it is known that the sign of the charge on escaping hydrogen depends upon the substance it has been in contact with, the very irregular results with K and Na already mentioned become less mysterious.

J. ENRIGHT.

Newton's Laws of Motion.

THERE is a point in connection with Newton's laws of motion which the text-books on dynamics, which found the science upon those laws, seem to me to leave very inconveniently and unnecessarily mysterious. The point to which I allude is the meaning of the words "rest or uniform motion in a straight line" in the first law. The difficult words are "uniform" and "straight," which of course are each of them meaningless until it is explained what the motion is with reference to; but this explanation is not given explicitly in any of the books on dynamics which I am acquainted with; and a comparison of their various statements leaves me in some doubt as to what is intended to be implied. May I therefore appeal to those of your readers who accept Newton's laws to say whether the following is correct?

I find that Law III. is interpreted by the most influential authorities, such as Maxwell and Tait, to mean that force occurs only as one side of a mutual action, consisting of two equal and opposite forces between two portions of matter. I am therefore led to suppose that the freedom from force action, which is spoken of in Law I., should be explained (by means of Law III.) as meaning isolation from the influence of all other matter; and that Law I. must be considered as containing a definition of an arbitrary meaning to be given in dynamics to the words "rest or uniform motion in a straight line," namely, that it is the motion possessed by any particle isolated from the influence of all other matter, which influence is to be traced by its mutual character. Law I. would then go on to say, as an experimental result, that all isolated particles move with reference to one another in a way consistent with this definition.

In order to reach this conclusion I find it necessary to interpret some statements in text-books in a somewhat awkward fashion (*e.g.* Maxwell, "Matter and Motion," article xl.), and to suppose some others to be incorrect; hence my doubts, and my appeal for their resolution.

W.

August 9.

On the Constant P in Observations of Terrestrial Magnetism.

ON page 304 of vol. ii. of their excellent treatise on "Practical Physics," Messrs. Stewart and Gee give the usual expression for the constant depending upon the distribution of magnetism in a pair of magnets employed for measuring terrestrial horizontal force; namely—

$$P = \frac{A - A'}{\frac{A}{r^3} - \frac{A'}{r_1^3}}.$$

Instead of this awkward and troublesome form, I would suggest

$$P = \frac{r_1^3 r^3}{r_1^3 - r^3}\left(1 - \frac{A'}{A}\right),$$

which can be readily deduced from Gauss's original equations, and is much better adapted to logarithmic computation; especially when r and r_1 remain constant throughout a series of observations, and Gaussian logarithms are used to form the factor $(1 - A'/A)$.

WM. HARKNESS.

Washington, D.C., August 1.

The Stature of the Human Race.

IN your "Notes" of last issue, p. 348, you mention General Pitt-Rivers conducting a party of the Royal Archæological Institute to Woodcutts, where skeletons dug out show that the people who inhabited the ancient Romano-British village were of very inferior stature, the males being only on an average 5 feet 2 inches, and the females 4 feet 10 inches. I think it would be a very interesting inquiry to ascertain the average height of the human race in the past, as it appears to me from such data as I have been able to collect that the human race has continuously increased in average stature. I have measured a great many Roman coffins, where I happened to come across them, and my average shows that the Roman could not have greatly exceeded 5 feet 5 inches. In taking measurements of ancient armour, I find that the English aristocracy have decidedly increased in average height within 500 years. For a paper I read before our local Society, I measured twenty-five mummies in the British Museum as nearly as I could through the cases, making estimate for [illegible] ping, and I found the average height of males 61 inches, females 55 inches. The mummy of the celebrated Cleopatra measures about 54 inches, about the height of the present average European girl of 13. The most ancient mummy of an Egyptian king yet discovered measured 52 inches. With research I have no doubt interesting data could be obtained on this subject. Limiting the matter to my own observations, I have formed the idea that the average stature of the human race increases at about the rate of 1·25 inches per 1000 years.

WM. F. STANLEY.

Cumberton, South Norwood, August 13.

A Spider allowing for the Force of Gravity.

THE manœuvres of the small hunting spider, so common on the West Coast of Africa, are always attractive, and my interest

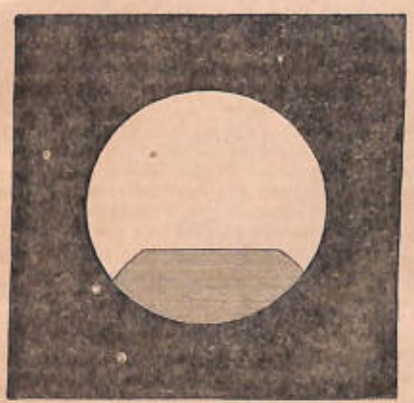
in them had been specially aroused by seeing a house-fly, which had previously narrowly escaped capture, swoop down on his mortal enemy and touch him on the back with his claws (as though twitting him on his failure), the spider apparently taking no notice whatever. On seeing, therefore, one of these spiders stalking a small moth on my wall in Cape Coast Castle, I devoted my attention to the operation.

After moving off several times the moth at length settled on the ceiling, and I thought the chase was over. The spider, however, followed on to the ceiling, and approaching within striking distance (about two inches) anchored his web; then moving round in a circle from the moth until he was about equi-distant from his anchor and his prey, he made his spring. He had evidently calculated how much loose web he would require to reach his prey, for when he fell (as was inevitable from the force of gravity) he was suspended in mid-air by the loose web. The spider regained the ceiling by his own web, having narrowly missed a good meal. C. B. LYSTER.

19 Waterloo Crescent, Dover, August 12.

The Lunar Eclipse of August 3.

IT would be interesting to know if the following phenomenon was observed at other places. At 9.30 p.m., local time, at Hamburg, a small cumulus cloud was observed a little distance below the moon, and the darkened part of the lunar surface was taken to be part of the cloud, from its upper edge being flattened. Ten minutes later the cloud had passed away, but the



Sketch of Lunar Eclipse of Wednesday, August 3, 1887 (as observed at Hamburg).

flattened appearance on the moon remained, and it was evident that the earth's shadow was distorted, as seen in the annexed sketch. Several persons noted the peculiarity, which was visible until about 10.30 p.m. in a very clear sky. H. H.

August 8.

BOTANY OF SAN DOMINGO.

THE vegetation of this, the largest of the West India Islands next to Cuba, has long been almost totally unknown to botanists. The absence of all but the scantiest data about its flora has made any general conclusions as to the main facts of the geographical distribution of plants in the West Indies very uncertain. It has usually been supposed that any attempt to explore any part of the island botanically would present almost insuperable difficulties. The following extracts from a letter from San Domingo received at Kew from Baron Eggers, who has laboured so assiduously in the investigation of West Indian botany, will be read therefore with much interest. W. T. THISELTON DYER.

Puerto Plata, Sto. Domingo, July 11, 1887.

I HAVE now been about three months in this island. I arrived in Samana on April 4, and the following day in this place. After having spent a couple of weeks in exploring the lower mountains here (2600 feet), I proceeded to Santiago, where again I spent some time in exploring the Vega Real and the Monte Christi range. From Santiago I went further into the interior to Jarabocon and the Valle de Constanga (3860 feet), from where I made an expedition up to the highest peaks I could find (Pico del Valle, 8680 feet), and which I succeeded in climbing, though with considerable hardship and fatigue. From this Sierra I returned to Santiago, and from thence to Puerto Plata, where I have latterly been exploring the region to the east towards the rivers Yasica and Jamao.

This, in short, is an outline of my travels here. I have been so far very fortunate, as I have succeeded in penetrating to regions where no European seems ever to have been before: my collections are very rich—about 1200 species—and my health has not suffered from the rather hard life here.

This island is, to a considerable extent, in a state of uncivilization: the roads are frightful, and hardly deserve that name; in fact, there is not one single good road in the whole island. You could hardly believe that the principal road from Santiago to Puerto Plata, on which the greater part of the traffic of the island goes, in the rainy season is impassable often for weeks. With regard to the vegetation, it does not strike me as being very luxuriant. It is much less so than I expected, and is certainly less luxuriant than that of Dominica.

The Cacti, which are a good criterion with respect to dryness of climate, are seen very frequently in the Vega by Santiago; higher up, the mountains in the interior are covered with pine forests to an immense extent. There the soil is gravelly and rather sterile. I found the pine growing from 600 feet up to the very highest peaks. The Sierra and Monte Christi, a coast range, consists of Tertiary limestone, and has no pines at all. But here you find also Cacti, Acacias, and Agaves not unfrequently. Palms are comparatively scarce—only about six or seven species are known (*Oreodoxa, Sabal, Thrinax, Euterpe*, and one called "Yarey" here, which I believe is a species of *Thrinax*), comparatively few *Orchideæ*, and no *Cycadeæ* at all. I believe in the south, near San Domingo, there is a *Zamia*; and, on the whole, the eastern part of the island is more moist, especially near Samana Bay and along the river.

Of remarkable plants I have found here a *Clavija*, which seems to be known only from Trinidad among the West India Islands, *Phyllocoryne jamaicensis*, a *Stanhopea* or *Lælia*, and several tree ferns. In the high mountains, of course, I found a greater number of interesting species: several *Tupæ*, two *Ericaceæ*, two Fuchsias, of which one has a most beautiful large pendulous flower, *Ranunculaceæ*, Ferns, *Loranthus*, and others which of course were all unknown there. The *Juglans cinerea* grows here at a height of about 1800 feet; I obtained a number of seeds.

Among *Coniferæ* I should especially mention a splendid *Taxodium*, the wood of which is dark red and very odorous. It is called Sabium here.

The *Cacti* are, no doubt, very rich and interesting, but as they require to be preserved in alcohol, and the means of transport are so very difficult, I have not made any collections of them this time. The beautiful *Rudolphia rosea* grows from the coast up to 4000 feet.

On the stems of the pines a number of curious Bromeliads are growing, none, however, very conspicuous; at about 1000 feet a bulbous Oxalis with white flowers is found, commonly among the pines in the sandy soil. A number of herbaceous *Synantheræ* were found among grasses in the upper regions above 7000 feet.

The *Podocarpus* of Jamaica I did not see here at all. A number of beautiful *Echites* are found in the lowlands, as well as some striking Orchids (*Bletia, Laliopsis*); also two remarkable Coccolobas, the immense-leaved *C. macrophylla*, and another species with somewhat lesser

leaves. The first-named has, as you know, large dark purple flower spikes of 2 feet long; the other, on the other hand, only short spikes with small white flowers. On these Coccolabas are found several nice Epidendrums.

The savannahs are frequent and extensive here, and afford a number of smaller plants of various descriptions.

In several parts of the island there are tracts of mahogany, which are cut for export.

The climate is generally cooler than in the smaller islands. I found the nights quite fresh. In the higher mountains, of course, it was quite cold at night. On the Pico del Valle I passed one night. We had a large fire blazing all night; in the morning, at 6 o'clock, the thermometer only showed 11° C.

Rivers and brooks are innumerable, but on account of the freshets and the violent current after rain, hardly any aquatic plants are seen, at least in this part of the island. I missed the beautiful *Pontederæ* of Porto Rico.

I send you to-day, by mail, seeds of the only palm which I have been able to obtain, a species of *Euterpe*, which is common here above 1200 feet, and the fruit of which is much eaten by half-wild hogs. It is called "Manacla" here, and grows to a height of about 30 to 40 feet.

Towards the end of the year I propose continuing my explorations of the West Indies, having in view a further investigation of this island, especially of the east and south, and furthermore of the Bahamas (especially Andros) and the two islands of Tobago and Grenada, both of which, I believe, are very little explored. The northern part of Dominica is also still *terra incognita*, unless something has been done there since my visit in 1879 and 1880. This island is particularly interesting to me. I believe it is one of the most luxuriant of the West India Islands.

CONSTITUTIONAL FORMULÆ AND THE PROGRESS OF ORGANIC CHEMISTRY.

IF the mere increase in the number of known facts were an accurate measure of the growth of a science, the question as to the progress of organic chemistry would be easily answered. Let the reader open a text-book on chemistry of fifty or sixty years ago, and he will find, sheltering itself under the wing of the inorganic chemistry of that day, the half-fledged science of organic chemistry. Then let him turn to Beilstein's gigantic *Handbuch der organischen Chemie*, with its more than two thousand large closely-printed pages—a mere classified catalogue of the known facts, written moreover in the highly-condensed elliptical style appropriate to catalogues. Here is increase.

But life is not measured by days, nor chemistry by new compounds; and the reader might resent the invitation to appraise the progress of organic chemistry by this rough quantitative method. A qualitative analysis is necessary here.

But how? The really important facts, even with the aid of the most judicious selection, could hardly be packed within the compass of a single article; nor would they be interesting, or, in such compression, even intelligible, to the non-chemist. There are of course the usual *pièces de résistance* in the shape of the coal-tar colours, and the various naturally-occurring compounds that have been artificially prepared; but probably the general reader has heard enough of these already, and might feel inclined to ask whether organic chemistry has nothing further to say for itself.

There is, however, a peculiarity of organic chemistry which distinguishes it from the remainder of the science. The aim of all chemistry is to ascertain the constitution of matter, and the said peculiarity of organic chemistry is, that it expresses its views on this important subject in greater detail, more precisely (or, as some will have it, more dogmatically), than inorganic chemistry. Its articles of belief on this head are embodied in its constitutional formulæ.

Here we touch on matter which every chemist will at once recognize as debatable. But, for good or for evil, these constitutional formulæ are, apart of course from the dry facts, the main scientific outcome of organic chemistry: they form the particular contribution which organic chemistry has been able to make towards solving the central problem of all chemistry—the constitution of matter. At present they crown the edifice of organic chemistry. Are they the keystone of an arch, or a mere meaningless architectural embellishment? This is the most general question which organic chemistry can put to itself at the present moment, and an attempt to answer it is the most fitting mode of reviewing the past work of the science. Let us therefore turn our attention to these constitutional formulæ, and ask ourselves what they are: what their meaning is, their scope, their justification.

According to some unfriendly critics, constitutional formulæ have done incalculable harm to chemistry by causing chemists to desert accurate experiment and observation for idle speculation—to substitute for the arduous work of the laboratory the easy task of stringing together atomic symbols, according to certain rules, on paper. There may in some cases have been some small measure of truth in this accusation—in other words, there may have been some occasional abuse of constitutional formulæ; but the injustice of the accusation as a whole is sufficiently proved by the fact that the most successful experimenters of the day in the domain of organic chemistry are enthusiastic supporters of constitutional formulæ, and confess to having been guided by these formulæ at almost every step of their researches. This actively-hostile attitude towards constitutional formulæ is fortunately becoming daily rarer.

Another class, not of detractors, but of rather lukewarm friends, of the constitutional formula, regard it as a convenient mnemonic device, by the aid of which the composition of otherwise hopelessly complex compounds may be successfully impressed on the memory. It is perfectly true that constitutional formulæ do perform this important function; but an impartial review of the case will, we imagine, lead to their being rated somewhat more highly than this.

A third class may be described as the undiscriminating admirers—the injudicious friends—of the constitutional formula. To them the constitutional formula is a final expression of the position of the atoms in the molecule—a picture of the molecule itself. This is a phase of belief which many pass through in making their first acquaintance with organic chemistry, and its existence is due to the circumstance either that the teacher is so much engrossed in impressing the complex array of facts and theories upon the mind of the student that he has not time to introduce philosophic limitations and doubts, or that he considers such an addition only calculated to render an already somewhat tough intellectual fare totally indigestible by a beginner. However this may be, it is certain that the faith of the beginner is quite as frequently appealed to as his reason.

We shall best be in a position to discern the meaning and to estimate the value of these constitutional formulæ, if we consider to what necessity they owe their origin, and how far they fulfil the purpose for which they were devised.

The atomic theory, as propounded by Dalton, satisfied for a time the requirements of chemists. For every properly-analyzed compound a more or less simple atomic proportion could be calculated, and this atomic proportion was expressed in the empirical formula of the compound. These empirical formulæ were combined into

equations, and the equations formed a complete expression of the reactions, so far as the weights of matter taking part in them were concerned. Now it was experimentally proved that every definite compound possessed a constant qualitative and quantitative composition, and it seemed to chemists as something of the nature of an axiom that to a given composition one and only one compound could correspond. So convinced were they of the truth of this unproved and, as the event showed, totally erroneous proposition that, when in 1823 and 1824 the first cases of *isomerism*, or identity of composition together with diversity of properties, were discovered by Wöhler and Liebig, the results obtained by these eminent chemists were generally set down to faulty analysis. But the cases of isomerism multiplied rapidly, and chemists had to make their account with this altered state of things. But here the inadequacy of the empirical formulæ became evident. Wherever a case of isomerism occurred, the empirical formula was ambiguous, and the equations in which it was employed were neither a complete nor a precise expression of the reactions.

To some, the discovery that constancy of composition no longer involved constancy of properties may have seemed to sap the very foundations of chemistry as then understood. But this was not the case. The discovery necessitated an extension of the atomic theory, not its abolition. In fact, isomerism afforded a remarkable proof of the correctness of the view that matter consisted of atoms. On the alternative hypothesis that matter fills space continuously and homogeneously, isomerism is incapable of explanation; as it is inconceivable that the same given quantities of the same given kinds of matter, continuously and homogeneously filling space, should produce more than one compound. A difference of properties in such a case bespeaks a difference of *arrangement* of the component parts; and further, as each such compound displays, even in the state of the finest mechanical subdivision, perfect uniformity, the component parts, by the arrangement of which the difference of properties is produced, must be exceedingly small. We are thus led back to the atomic theory, whilst at the same time the extension is indicated which it was necessary to make in this theory in order that isomerism might find its proper place and explanation under it. It was necessary to determine, so far as possible, the *mode of arrangement* of the atoms in the various compounds. The results of this attempt are embodied in the constitutional formulæ which have been employed by chemists at various times.

The method resorted to in solving this problem was very similar to that which had been employed in determining the ultimate composition of compounds. Just as when, after isolating from a compound, or introducing into a compound, some particular kind of elementary matter, chemists concluded that the compound actually contained that particular kind of matter, so, when in a reaction a particular group of atoms was eliminated bodily from a compound, or introduced bodily into a compound, they concluded that this group existed as such in the compound. Unfortunately, the conclusion is not always quite so warrantable in the case of atomic groups as in the case of elements. The reaction, for example, by which an atomic group is eliminated from a compound involves the destruction of the parent compound, and in this process, which is generally more or less violent, it is only too easy for the atomic groups to undergo re-arrangement. In this way, alcohol (C_2H_6O), from the fact that it may be split up into ethylene (C_2H_4) and water (H_2O), was at one time regarded as containing these two atomic groups—a view which at all events is not that at present held. We thus see that from two different reactions, two totally distinct and mutually incompatible constitutional formulæ may be deduced for the same compound.

It would carry us too far to trace all the steps by which constitutional formulæ gradually became more precise and less self-contradictory, but a few important discoveries may be mentioned which have mainly tended to bring about this result. In the first place, the development of the idea of the molecule as distinct from that of the atom, and the discovery of a means of determining the molecular weight of bodies, led to the division of isomerides into two classes: those in which the proportions of the various atoms were the same, but the total number of atoms in the molecule was different—this mode of isomerism being distinguished as *polymerism*; and isomerism proper, in which both the proportions of the various atoms, and the total number in the molecule, are the same in the different compounds. But the knowledge of the molecular weight aided chiefly in the construction of constitutional formulæ by determining the exact number of atoms in the molecule, and thus facilitating the task of arranging these atoms by stating precisely how many atoms had to be arranged. The law of valency also exercised a most important influence, simplifying matters by greatly limiting the number of legitimate arrangements. In fact, in the case of some of the simpler compounds, such as methane (CH_4), ethane ($CH_3.CH_3$), propane ($CH_3.CH_2.CH_3$), methyl alcohol ($CH_3.OH$), and others, only one mode of arrangement is, according to the laws of valency, possible for each compound.

A modern constitutional formula, therefore, takes the various atoms of a compound in the proportions indicated by the empirical formula, and in the absolute number prescribed by the molecular weight, and arranges them in that way which, within the limits of the laws of valency, will best account for the reactions of the compound.

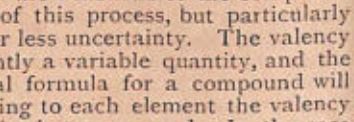
Let us consider what elements of uncertainty are involved in each of the various operations here enumerated.

The correctness of the empirical formula of a compound, as calculated from its percentage composition, depends upon the correctness of the atomic weights assigned to its component elements. In the case of organic compounds, to the consideration of which we shall confine ourselves here, the atomic weights of the component elements may be regarded as determined with a degree of probability approaching to absolute certainty. (This does not, of course, refer to the question whether the atomic weight of an element has been determined within 1/10,000 more or less of its true value, but whether, for example, in the case of carbon the atomic weight is 12, or some multiple or sub-multiple of 12.) The empirical formulæ of correctly analyzed organic compounds may therefore be regarded as standing on as sure a foundation as almost anything in the range of science which is a matter of deduction and not of direct observation or experiment. As regards the second point, the molecular weight, an almost equal certainty may be said to prevail in most cases in which the compound can exist in the state of vapour. Avogadro's law, that "when two gases or vapours are at the same temperature and under the same pressure, the number of molecules in unit of volume is the same in both gases or vapours"—this law, originally advanced as an hypothesis, has been shown to follow as a mathematical deduction from the kinetic theory of gases, a theory almost as well established at the present day as the atomic theory itself. Avogadro's law places in our hands a means of determining the molecular weight of substances which are capable of existing in the form of vapour, the only uncertainty attaching to its determinations being that occasioned by the tendency which many compounds exhibit, either to undergo decomposition, or to be incompletely vaporized, in passing into the gaseous state. But in the case of all compounds capable of existing undecomposed in the gaseous state throughout any considerable range of temperature, the molecular weight may be determined with a very high degree of probability. In cases where the compound is not volatile

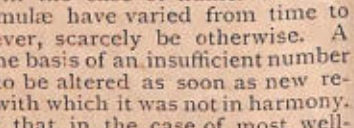
without decomposition, recourse must be had to indirect means in the determination of the molecular weight, and there is consequently more or less uncertainty in such determinations. As regards the third operation in the construction of a constitutional formula—the arrangement of the atoms so as to satisfy their valencies, and at the same time to account for the reactions of the compound in question—both parts of this process, but particularly the latter, involve more or less uncertainty. The valency of an element is frequently a variable quantity, and the validity of a constitutional formula for a compound will depend upon our attributing to each element the valency which it really possesses in that compound. In the case of organic compounds, however, this source of uncertainty is reduced to a minimum. Carbon is, with one certain and one or two doubtful exceptions, always a tetrad; hydrogen and oxygen are constant in their valency; and the character of nitrogen as a triad or as a pentad is generally easy to determine. The chief source of uncertainty lies in the difficulty of expressing the reactions of a compound by its constitutional formula, great scope being left here for arbitrary interpretation. To this is due the fact, on which the opponents of constitutional formulæ lay so much stress, that in the case of numerous compounds the accepted formulæ have varied from time to time. This could, however, scarcely be otherwise. A formula constructed on the basis of an insufficient number of reactions would have to be altered as soon as new reactions were discovered with which it was not in harmony. And it must be admitted that, in the case of most well-studied compounds, very few changes have been made in the constitutional formulæ since these were constructed on the principles of valency. In the case even of the more complex compounds, the constitutional formulæ show a tendency to become finally settled as soon as sufficient experimental material has been accumulated.

In this light, then, a constitutional formula is to be regarded as a symbolic expression, constructed according to the laws of valency, and embodying in a very condensed form the reactions of a compound. By a knowledge of the rules according to which such a formula is constructed—by a knowledge of chemical precedent, as it were—we ought, from an inspection of the formula, to be able to predict the reactions of the compound; to say beforehand, for example, how many substitution-products of a particular class a given compound ought to yield, and so on. The value of a good working hypothesis lies in the fact that it can predict: if it can predict nothing, it is worth nothing. Now, with regard to the question before us, we find that if we correctly embody in a constitutional formula a certain number of reactions—a number sufficient to warrant its construction—it will correctly predict an enormous number of reactions which were not in the least contemplated during its construction. Let us take the example of acetic acid:—

Starting with methyl iodide, which admits of only one constitutional formula, we convert it into methyl cyanide by heating it with potassium cyanide, $CH_3I + KCN = CH_3(CN) + KI$, thus substituting a monad group, CN, for the monad iodine atom. The constitution of this methyl cyanide is, however, not rendered clear by this reaction: it might be either $CH_3.CN$ or $CH_3.NC$ according as the cyanogen group is united to the carbon of the methyl by means of carbon or by means of the nitrogen. Both these compounds are in fact known. The one formed in the foregoing reaction has the first of these two constitutions, inasmuch as, when heated with acids or alkalies, it parts with its nitrogen in the form of ammonia, yielding acetic acid, $CH_3.CN + 2H_2O = CH_3.COOH + NH_3$. In this *hydrolysis*, or decomposition of the compound with assumption of the elements of water, the nitrogen atom of the cyanogen group is removed, whilst the carbon atom remains in combination; and we therefore conclude that it was by means of this carbon atom, and not by means of the nitrogen atom, that the CN-group was united to the carbon of the methyl group—a conclusion confirmed by the behaviour of the isomeric methyl cyanide, in which the carbon atom of the NC-group can be eliminated, leaving the nitrogen attached to methyl. The only question remaining to be solved is the constitution of the monad group, COOH, which might be



either $-C(=O)-O-H$, or $-C(-H)(=O)-O$. From what is known from other sources concerning the mechanism of the process of hydrolysis, the first formula is the more probable: that it is correct is shown by the behaviour of acetic acid towards the trichloride of phosphorus, by which reagent the hydroxyl group (OH) can be removed and replaced by a monad chlorine atom, whilst the resulting acetyl chloride ($CH_3.COCl$) may be reconverted by the action of water into acetic acid ($CH_3.CO.OH$). Acetic acid therefore contains a monad group, OH, exchangeable for chlorine; and the first formula is correct. Uniting this COOH-group (carboxyl) to the methyl group, and expanding these radicles, we have the fully-dissected formula of



acetic acid, $H-C(H)(H)-C(=O)-O-H$.

What does this formula tell us? What does it predict? Of the four hydrogen atoms, three are directly united to carbon, and one is distinguished from the others by being indirectly united to carbon by means of oxygen. We know that hydrogen, when directly united to oxygen (for example, in water) may be displaced by electro-positive elements such as metals; and we find that in acetic acid one hydrogen atom is distinguished from the others by this property. We know that hydrogen atoms in direct union with carbon (as in the hydrocarbons) may be displaced by electro-negative elements such as the halogens. This we find to occur in the case of acetic acid: there are three atoms of hydrogen which may be successively displaced by chlorine and other electro-negative atoms or groups. That these three chlorine atoms are attached to the same atom of carbon, and that therefore the three atoms of hydrogen which they have displaced are also so attached, is shown by the fact that potassium trichloracetate, when warmed with a solution of potassium hydroxide, yields chloroform ($CHCl_3$)—

$$\begin{matrix} CCl_3 \\ H \end{matrix} \Big\vdots \begin{matrix} CO.OK, \\ OK \end{matrix}$$

The same reaction with ordinary potassium acetate (a higher temperature being, however, required in this case) yields marsh gas (CH_4)—

$$\begin{matrix} CH_3 \\ H \end{matrix} \Big\vdots \begin{matrix} CO.OK. \\ OK \end{matrix}$$

In both these reactions the molecule is divided at the point of union of the carbon atoms. Apart from this disruption, each carbon atom retains the same atoms in combination with it after the separation which were attached to it before. In the reaction with phosphorus trichloride already referred to, the molecule of acetic acid is divided at the point of union of the hydroxyl group (OH) with the acetyl group ($CH_3.CO$). That such separations are possible without disturbing the atomic arrangement of the separated groups renders the construction of constitutional formulæ possible. But the point to be noted is that all the foregoing reactions and many others—in fact all the reactions of acetic acid—are

satisfactorily explained by the constitutional formula, and are, so to speak, embodied in the formula.

A constitutional formula is thus founded on reactions and predicts reactions. In this lies its chief value. A constitutional formula which is not founded on reactions possesses a very slight value indeed. The constitutional formula of a complex mineral silicate, for example, is not an expression of the reactions of that silicate, inasmuch as the silicate has not hitherto been induced to yield any variety of reactions worth mentioning: it is merely the simplest, and perhaps the most symmetrical, way of arranging the component atoms consistently with their valency, and in accordance with certain analogies in the constitution of salts of oxygen-acids. Not even the molecular weight of the silicate is known, and this knowledge is the first step towards the construction of a constitutional formula which shall have any great value. But the beginner, who has not always the genesis of the various formulæ before his eyes, is apt to put all constitutional formulæ into one category, and to view all with equal trust or distrust according as his temperament happens to be sanguine or sceptical.

The chief opponents of constitutional formulæ are to be found among inorganic chemists. Constitutional formulæ are essentially a creation of organic chemistry. We have seen that they mainly originated in the necessity of explaining the phenomena of isomerism. Now isomerism, which is the rule in organic chemistry, is entirely the exception in inorganic chemistry. The constitutional formulæ of inorganic chemistry are thus an artificial growth: they are the result of an attempt to transplant into inorganic chemistry methods and analogies derived from organic chemistry, and it cannot be affirmed that these borrowed growths have altogether flourished in the new soil. Where the organic formulæ have guided the chemist through the labyrinths of the various classes of compounds, predicted reactions, laid down the number of possible isomerides, and shown the way to the synthesis of natural compounds so high in the scale of complexity as alizarin and indigo, the same methods applied to inorganic chemistry have led to no tangible result higher than that of checking a few doubtful formulæ by means of the laws of valency. The reasons of the failure have already been indicated. But the partial failure of constitutional formulæ in inorganic chemistry is hardly an argument against their use in organic chemistry, where they have achieved the most signal success.

Up to this point we have regarded the constitutional formula simply as a symbolic device, by means of which reactions and cases of isomerism may be expressed and predicted. The question now arises: Is it anything beyond this? A constitutional formula is primarily a certain definite arrangement of atomic symbols. Is there anything like this atomic arrangement in the molecule itself, or even anything corresponding with it?

It is in the highest degree improbable that there is anything *like* it in the molecule itself, but quite possibly there is something *corresponding with* it. That the constitutional formula cannot be like the molecule in the sense of being a picture of it is manifest from a variety of considerations. To mention one out of several: a constitutional formula represents the atoms as points connected with one another in a certain definite way by lines of attraction, without reference to any actual positions in space which these atoms may be supposed to occupy; for the sake of convenience they are represented as lying in the plane of the surface on which the formula is drawn. Now the kinetic theory of gases informs us that the atoms within the molecule are not to be conceived of as occupying their positions in a state of rest: each executes some form of vibration or rotation. This view is quite compatible with the existence of definite relations of attraction between given atoms within the molecule. To borrow an illustration from astronomy, we might in the constitutional formula of acetic acid, for instance, regard the two carbon atoms as the two suns of a double star, and the atoms directly attached to the carbon suns as planets—one with a satellite. The parts may execute their respective motions without disturbing the stability of the whole, any more than the stability of the solar system is disturbed by the motions of its parts. Now, it is evident that a constitutional formula which represents the atoms as motionless in a plane cannot be a true image of the molecule—cannot be *like* it.

That the constitutional formula, however, in some way *corresponds with* the molecule, is shown, not only by the chemical evidence which we have already discussed, but, what is more important, by a number of physical considerations. That the physical properties of a substance are dependent on the arrangement of the atoms within the molecule is evident from the fact that in isomeric compounds the melting-point, boiling-point, specific gravity, and other physical properties generally vary for each isomeride. A comparison of the physical properties of similarly-constituted compounds shows that in many cases very definite relations can be traced between constitution and physical properties.

Very important information has been gained in this way by studying the behaviour of organic compounds towards light. Thus a number of these compounds when in the liquid state, or in solution, cause the plane of a ray of polarized light, if passed through them, to turn through a certain angle. It was observed by Le Bel that all such optically active substances contained in their constitutional formulæ at least one *asymmetric* carbon atom—that is, a carbon atom united to four dissimilar atoms or groups; and an ingenious hypothesis has been put forward by Le Bel, and in greater detail by Van't Hoff, to account for this concatenation. The researches of Gladstone and Dale, Landolt, Brühl, and others on the molecular refraction of organic liquids have demonstrated an intimate connexion between the refractive power of a liquid on the one hand and its constitution on the other, so that observations on the refractive power may be employed in ascertaining the constitution of such compounds; and Perkin has recently shown that the "magnetic rotatory power" of organic liquids—the power which such liquids possess, when placed in a strong magnetic field, of turning the plane of the polarized ray—may be utilized in the same manner. Again, the selective absorption which organic liquids exercise on light of different wave-lengths is closely connected with the constitution of these liquids; and the presence of certain organic radicals in the formula of a compound is manifested by certain definite absorption-bands which make their appearance in the photographed spectra of the infra-red (Abney and Festing) and of the ultra-violet (Hartley).

Other remarkable relations between constitution and physical properties are manifested in what is termed the *molecular volume* of organic liquids at their boiling-points—a subject first investigated by Kopp, and later by Thorpe, Ramsay, Lossen, and others. By the *molecular volumes* of compounds are understood the relative volumes which quantities of these compounds taken in the proportion of their molecular weight occupy. Kopp found that the molecular volume of a liquid organic compound at its boiling-point is the sum of the atomic volumes of its elements; and that, whereas the atomic volumes of carbon and hydrogen are constant, the atomic volume of oxygen varies with its mode of combination, having two distinct values: one value for an oxygen atom attached with both its affinities to the same atom of another element, and a second value for an oxygen atom attached to two different atoms.[1] Sulphur exhibits a similar definite variation in atomic volume in accordance with the mode of distribution of its affinities.

[1] Kopp distinguished "intra-radical" and "extra-radical" oxygen. The above is a re-statement of his views in terms of modern formulæ.

In all the foregoing instances we have a successful correlation of the results of physical chemistry with those obtained by the pure chemist in the deduction of constitutional formulæ. Many who withheld judgment, or even condemned, when the chemist was his own witness, may listen to him more favourably now that he is supported by the independent testimony of the physicist.

These investigations into the physical properties of organic compounds are of relatively recent date. There is little doubt that as they are extended new and important laws will be deduced. Much is to be hoped from thermo-chemistry, incoherent as many of its utterances as yet are. By following the path of physico-chemical research, chemists may even hope to arrive at a dynamical representation of the molecule which shall be as much more powerful as an instrument of research than the present merely statical constitutional formula, as that is more powerful than the empirical formula which preceded it. It is hazardous to try to fix a limit to scientific advance in any direction, but it is probable that the modern constitutional formula represents the limit to what purely chemical research can accomplish in determining the constitution of matter. Much will still have to be done by purely chemical research in working out the details of the existing system: the constitution of the more complex compounds will be ascertained on the lines of our present formulæ; new valuable natural compounds will be synthesized. But mere chemical reactions can probably never settle questions of intramolecular dynamics; in these help must come from physical chemistry. Moreover, the physical methods of research supplement the chemical methods in one important particular. By chemical methods we can never study the molecule as it actually exists. Our synthetical methods give us information concerning the molecule only at the moment of its formation; our analytical methods equally confine themselves to the moment of its destruction. The physical methods supply this want: they enable us to study the existing compound. Of these physical methods, one of the most promising, although one of the most recent, is the optical method, which has yielded results of the utmost importance both in inorganic and in organic chemistry. The ray of light which passes from the fixed star to the earth gives us information concerning the composition of the atmosphere of the fixed star; and it is perhaps not too much to hope that the ray of light which has threaded its way through and between the molecules of a compound, and has been modified by its contact with these, will, if properly interrogated, furnish some information concerning the structure of these molecules. Indeed, in the case of the rotation of the plane of polarized light by organic liquids, of their absorption spectra and their indices of refraction, this information has in a measure been obtained.

To sum up. The constitutional formula is not an ultimate expression of the whole truth as regards molecular structure. But it is certainly a very useful and convenient symbolical expression of certain aspects of the truth. We all hope that it may one day be superseded by some higher and more complete generalization. But it will be absorbed and assimilated, not rejected and contradicted, by that generalization. *Non omnis morietur.*

THE YALE COLLEGE MEASUREMENT OF THE PLEIADES.[1]

THE Messrs. Repsold have established, and for the present seem likely to maintain, a practical monopoly in the construction of heliometers. That completed by them for the Observatory of Yale College in 1882 leaves so little to be desired as to show excellence not to be the exclusive result of competition. In mere size it does not indeed take the highest rank; its aperture is of only 6 inches, while that of the Oxford heliometer is of 7½; but the perfection of the arrangements adapting it to the twofold function of equatorial and micrometer, stamps it as a model not easy to be surpassed. Steel has been almost exclusively used in the mounting. Recommended as the material for the objective-cell by its quality of changing volume under variations of temperature nearly *pari passu* with glass, its employment was extended to the telescope-tube and other portions of the mechanism. The optical part of the work was done by Merz, Alvan Clark having declined the responsibility of dividing the object-lens. Its segments are separable to the extent of 2°, and through the contrivance of cylindrical slides (originally suggested by Bessel) perfect definition is preserved in all positions, giving a range of accurate measurement just six times that with a filar micrometer. (Gill, "Encyc. Brit." vol. xvi. p. 253; Fischer, *Sirius*, vol. xvii. p. 145.)

This beautiful engine of research was in 1883 placed in the already practised and skilful hands of Dr. Elkin. He lost no time in fixing upon a task suited both to test the powers of the new instrument and to employ them to the highest advantage.

The stars of the Pleiades have, from the earliest times, attracted the special notice of observers, whether savage or civilized. Hence, on the one hand, their prominence in stellar mythology all over the world; on the other, their unique interest for purposes of scientific study and comparison. They constitute an undoubted cluster; that is to say, they are really, and not simply in appearance, grouped together in space, so as to fall under the sway of prevailing mutual influences. And since there is, perhaps, no other stellar cluster so near the sun, the chance of perceptible displacements among them in a moderate lapse of time is greater than in any other similar case. Authentic data regarding them, besides, have now been so long garnered that their fruit may confidently be expected at least to begin to ripen.

Dr. Elkin determined, accordingly, to repeat the survey of the Pleiades executed by Bessel at Königsberg during about twelve years previous to 1841. Wolf and Pritchard had, it is true, been beforehand with him; but the wide scattering of the grouped stars puts the filar micrometer at a disadvantage in measuring them, producing minute errors which the arduous conditions of the problem render of serious account. The heliometer, there can be no doubt, is the special instrument for the purpose, and it was, moreover, that employed by Bessel; so that the Königsberg and Yale results are comparable in a stricter sense than any others so far obtained

One of Bessel's fifty-three stars was omitted by Dr. Elkin as too faint for accurate determination. He added, however, seventeen stars from the Bonn *Durchmusterung*, so that his list comprised sixty-nine, down to 9·2 magnitude. Two independent triangulations were executed by him in 1884-85. For the first, four stars situated near the outskirts of the group, and marking the angles of a quadrilateral by which it was inclosed, were chosen as reference-points. The second rested upon measures of distance and position-angle outward from Alcyone (η Tauri). Thus, two wholly unconnected sets of positions were secured, the close accordance of which testified strongly to the high quality of the entire work. They were combined, with nearly equal weights, in the final results. A fresh reduction of the Königsberg observations, necessitated by recent improvements in the value of some of the corrections employed, was the preliminary to their comparison with those made, after an interval of forty-five years, at Yale College. The conclusions thus laboriously arrived at are not devoid of significance, and appear perfectly secure, so far as they go.

[1] "Determination of the Relative Positions of the Principal Stars in the Group of the Pleiades." By William L. Elkin. Transactions of the Astronomical Observatory of Yale University. Vol. I., Part I. (New Haven: 1887.)

It has been known for some time that the stars of the Pleiades possess a small identical proper motion. Its direction, as ascertained by Newcomb in 1878, is about south-south-east; its amount is somewhat less than six seconds of arc in a century. The double-star 61 Cygni, in fact, is displaced very nearly as much in one year as Alcyone with its train in one hundred. Nor is there much probability that this slow secular shifting is other than apparent: since it pretty accurately reverses the course of the sun's translation through space, it may be presumed that the *backward* current of movement in which the Pleiades seem to float is purely an effect of our own *onward* travelling.

Now the curious fact emerges from Dr. Elkin's inquiries that six of Bessel's stars are exempt from the general drift of the group. They are being progressively left behind. The inference is obvious, that they do not in reality belong to, but are merely accidentally projected upon, it: or, rather, that it is projected upon them; for their apparent immobility (which, in two of the six, may be called absolute) shows them with tolerable certainty to be indefinitely more remote—so remote that the path, moderately estimated at 21,000,000,000 miles in length, traversed by the solar system during the forty-five years elapsed since the Königsberg measures, dwindles into visual insensibility when beheld from them! The brightest of these six far-off stars is just above the eighth (7·9) magnitude; the others range from 8·5 down to below the ninth.

A chart of the relative displacements indicated for Bessel's stars by the differences in their inter-mutual positions as determined at Königsberg and Yale, accompanies the paper before us. Divergences exceeding 0″·40 (taken as the limit of probable error) are regarded as due to real motion; and this is the case with twenty-six stars besides the half-dozen already mentioned as destined deserters from the group. With these last may be associated two stars surmised, for an opposite reason, to stand aloof from it. Instead of tarrying behind, they are hurrying on in front. An excess of the proper movement of their companions belongs to them; and since that movement is presumably an effect of secular parallax, we are justified in inferring their possession of an extra share of it to signify their greater proximity to the sun. Hence, of all the stars in the Pleiades these are the most likely to have a measurable annual parallax. One is a star a little above the seventh magnitude, distinguished as *s* Pleiadum; the other, of about the eighth, is numbered 25 in Bessel's list. Dr. Elkin has not omitted to remark that the conjecture of their disconnexion from the cluster is confirmed by the circumstance that its typical spectrum (as shown on Prof. Pickering's plates) is varied in *s* by the marked character of the K line. The spectrum of its fellow-traveller (No. 25) is still undetermined.

It is improbable, however, that even these nearer stars are practicable subjects for the direct determination of annual parallax. By indirect means, however, we can obtain some idea of their distance. All that we want to know for the purpose is the *rate* of the sun's motion; its *direction* we may consider as given with approximate accuracy by Airy's investigation. Now, spectroscopic measurements of stellar movements of approach and recession will eventually afford ample materials from which to deduce the solar velocity; though they are as yet not accurate or numerous enough to found any definitive conclusion upon. Nevertheless, M. Homann's preliminary result of fifteen miles a second as the speed with which our system travels in its vast orbit, inspires confidence both from the trustworthiness of the determinations (Mr. Seabroke's) serving as its basis, and from its intrinsic probability. Accepting it provisionally, we find the parallax of Alcyone = about 0″·02, implying a distance of 954,000,000,000,000 miles, and a light-journey of 163 years. It is assumed that the whole of its proper motion of 2″·61 in forty-five years is the visual projection of our own movement towards a point in R.A. 261°, Decl. + 25°.

Thus, the parallax of the two stars which we suspect to lie between us and the stars forming the genuine group of the Pleiades, at perhaps two-thirds of their distance, can hardly exceed 0″·03. This is just half that found by Dr. Gill for ζ Toucani, which may be regarded as, up to this, the smallest annual displacement at all satisfactorily determined. And the error of the present estimate is more likely to be on the side of excess than of defect. That is, the stars in question can hardly be much nearer to us than is implied by an annual parallax of 0″·03, and they may be considerably more remote.

Dr. Elkin concludes, from the minuteness of the detected changes of position among the Pleiades, that "the hopes of obtaining any clue to the internal mechanism of this cluster seem not likely to be realized in an immediate future;" remarking further: "The bright stars in especial seem to form an almost rigid system, as for only one is there really much evidence of motion, and in this case the total amount is barely 1″ per century." This one mobile member of the naked-eye group is Electra; and it is noticeable that the apparent direction of its displacement favours the hypothesis of leisurely orbital circulation round the leading star. The larger movements, however, ascribed to some of the fainter associated stars are far from harmonizing with this preconceived notion of what they ought to be. On the contrary, so far as they are known at present, they force upon our minds the idea that the cluster may be undergoing some slow process of disintegration. M. Wolf's impression of incipient centrifugal tendencies among its components certainly derives some confirmation from Dr. Elkin's chart. Divergent movements are the most strongly marked; and the region round Alcyone suggests, at the first glance, rather a very confused area of radiation for a flight of meteors, than the central seat of attraction of a revolving throng of suns.

There are many signs, however, that adjacent stars in the cluster do not pursue independent courses. "Community of drift" is visible in many distinct sets; while there is as yet no perceptible evidence, from orbital motion, of association into subordinate systems. The three eighth-magnitude stars, for instance, arranged in a small isosceles triangle near Alcyone, do not, as might have been expected *a priori*, constitute a real ternary group. They are all apparently travelling directly away from the large star close by them, in straight lines which may of course be the projections of closed curves; but their rates of travel are so different as to involve certain progressive separation. Obviously, the order and method of such movements as are just beginning to develop to our apprehension among the Pleiades will not prove easy to divine.

A. M. CLERKE.

NOTES.

STRENUOUS efforts have been made to secure that the arrangements for the observation of the total solar eclipse of August 19 shall be adequate. "A large number of astronomers," says the *Times* of the 15th inst., "will be distributed along the central line, fully equipped with instruments suited to the particular work they intend to do. The Russians themselves have most energetically organized a very complete set of observations, meteorological and otherwise, at widely-distant stations, viz. Krasnoiarsk in Siberia, Perm in the Ural Mountains, and Viatka in Central Russia; while Prof. Mendeljew goes to Pavlovsk, near St. Petersburg; Prof. Bredichin, of the Moscow Observatory, to Kineshma; and Dr. Podsolnotschnaja will be stationed near Tver. Several foreign astronomers will also visit Russia, and have received very hospitable treatment at the hands of Prof. Struve and the other Russian authorities. From

England, Dr. Copeland, of Lord Crawford's Observatory, and Father Perry, of Stonyhurst, have accepted an invitation from Prof. Bredichin to two members of the Astronomical Society, and have already joined him at Kineshma; and Mr. Turner, from the Greenwich Observatory, will occupy a station selected by Prof. Struve. Prof. Young and Prof. M'Neill, from America, have gone to Tver; and two other American astronomers will also make observations. Prof. Tacchini and Dr. Riccò, from Italy, have gone to Viatka; and two German delegates and one French have also been sent." We may add that there will be an American photographic and spectroscopic station in Japan.

SEVERAL very good speeches were delivered last Saturday in the course of the debate on the Education Estimates. Mr. Mundella did excellent service by insisting, as he had often done before, on the necessity for a higher standard of education in our elementary schools. A great many people seem still to be of opinion that the State discharges all its obligations in this matter if it secures that children shall learn to read and write. But what is the good of teaching children to read and write if they are not also taught how to put the power, when they have acquired it, to a proper use? The chances are that reading and writing, if education goes no further, will soon be forgotten. Long ago this was pointed out by M. Thiers, who showed that children in France who could read and write at the age of eleven ceased to be able to do either before they entered the army as conscripts. If education is to be of real value, it must be carried on to an age when boys and girls are capable of taking an interest in "things of the mind," and they must receive instruction in subjects which they are likely to find attractive. This was urged with much force by Sir John Lubbock, who argued that history and natural science should receive far more attention than is now devoted to them in elementary schools, and that manual instruction ought to be added to the list of the optional class subjects.

FROM the Aiken (South Carolina) *Recorder* of the 19th ult we learn that Dr. Henry William Ravenel died on July 17, after a protracted illness. He was a native of the State in which he died, and early in life botany was his favourite pursuit, and fungi his specialty. Soon after graduating he engaged in cotton planting, and continued it for twenty years. Subsequently he devoted more time to botany, and during the last few years of his life he was Botanist to the South Carolina State Department of Agriculture. The infirmity of deafness prevented him from taking any other post. He published a few short papers, chiefly on the plants of his native State; but he was more widely known from his "Fungi Caroliniani Exsiccati," of which he issued a number of fasciculi; and the "Fungi Americani Exsiccati," which he prepared in conjunction with Dr. M. C. Cooke. He was a member of several scientific Societies, and in 1886 the degree of LL.D. was conferred on him by the University of North Carolina.

THE Curatorship of the Natural History Department of the Science and Art Museum of Dublin is now vacant, owing to the resignation, through ill-health, of Mr. A. G. More. Mr. More has been associated with the Institution for twenty years. He succeeded to the Curatorship six years ago, on the death of Dr. Carte, and the condition of that portion of the Museum over which he presided testifies to-day to his abilities as an administrator, and to the exceptional skill of himself and those who have been associated with him. As a botanist he is best known as joint author with the late Dr. Moore of the "Cybele Hibernica"; as a zoologist his name is honourably associated with British ornithology. Numerous notes and papers, scattered throughout various journals, give evidence of his scientific attainments and activity; and by no means an inconsiderable portion of his experience lies buried in publications on the Irish fauna and flora, for, with characteristic good-nature, he has always been willing to help local naturalists with his experience and critical knowledge. His loss is greatly to be deplored, and we wish the directorate good fortune in the choice of his successor.

THE Autumn Congress of the Sanitary Institute of Great Britain will be held at Bolton on September 20 and following days, under the presidency of Lord Basing. The Council invite papers on subjects included in the programme, and will be very glad to receive the personal co-operation and support of all who are interested in the diffusion of sanitary knowledge.

ON Saturday last, M. Jovis, accompanied by M. Mallet, made a balloon ascent from Paris, hoping that he might reach a height greater than that attained by any previous aëronaut. The balloon began to ascend at 7.15 a.m., and was visible until 8.10, when it disappeared, having reached, as was supposed, a height of between 7000 and 8000 feet. About eleven o'clock it came down in Belgian Luxembourg. The altitude reached was 22,000 feet. This is far below the "record" of Messrs. Glaisher and Coxwell, who rose to a height of 37,000 feet.

THE eighth Bulletin of Miscellaneous Information issued from the Royal Gardens, Kew, has just been published. It contains a series of valuable notes on the Tree Tomato (*Cyphomandra betacea*), the Chocho (*Sechium edule*), the Arracacha (*Arracacia escu'enta*), and the Cherimoyer (*Anona Cherimolia*). All these food-plants have been recently introduced from the West Indies to the East Indies. The notes are preceded by the following statement:—"The introduction of the Arracacha was first attempted, at the instance of the Government of India, in 1879, but, after many failures, was only successfully accomplished in 1883. The Chocho was introduced to Ceylon by means of a single plant, which survived the journey direct from Jamaica to Ceylon, in January 1885. The Tree Tomato and Cherimoyer were introduced by seeds, which travel well and are more convenient for distribution than plants. In a few years, no doubt, all these plants will be widely distributed throughout the East, and they will be found useful additions to the vegetable diet of both Europeans and natives. Already the Chocho introduced to Ceylon as recently as 1885 is to be found in the local markets; and the Tree Tomato is mentioned 'as a most valuable acquisition to Southern India.' All the four plants here mentioned are likely to thrive at hill-stations in India and in all districts suitable for coffee and cinchona cultivation. They are sub-tropical rather than tropical in their requirements, and hence no doubt they will be found of service in South Africa, in certain parts of Australia, Northern New Zealand, and in hilly districts generally throughout our tropical possessions. The information here summarized will indicate their usefulness as food-plants, and the sources both in the Old and New World from which future supplies of seeds and plants may conveniently be obtained."

SOME doubt has existed as to whether the Chinese have not one or more kinds of plants in use as ginger that are unknown elsewhere. In the Annual Report on the Botanical and Afforestation Department, Hong Kong, for the year 1886, Mr. Charles Ford, Superintendent, says he has taken steps for cultivating all the kinds of plants generally included by the Chinese as ginger, with the hope that he may be able to study them in such a manner as to secure all possible information in connexion with this subject. While at San Ui he was fortunate in obtaining from cultivated plants good flowering specimens. These he dried, and, together with specimens of the roots (properly rhizomes), forwarded to the Director of Kew Gardens for a study of them to be made there, where they can be compared with other kinds, or with specimens of the same kind from other places. The specimens he procured were, without doubt,

Zingiber officinale, the species commonly in cultivation in other parts of the world. It is possible that some other plant, which is not a true ginger, may be used in making the celebrated Canton preserved ginger, but all the information Mr. Ford has yet obtained points to the species *Zingiber officinale* as the only kind which the Chinese use for this purpose. The ginger cultivated on the Lo-Fau Mountains has a wide reputation amongst the Chinese as being of unusual efficacy in medicine. This superior quality may be derived from peculiarities of soil or climate which communicate to the plant exceptional properties.

A REMARKABLE relation is shown to exist by Dr. C. Bender (*Ann. der Physik und Chemie*, 1887, 8 B., p. 873) between certain physical constants and chemical valency. In experimenting upon the density, expansion, and electrical resistance of several salt solutions, and mixtures of the same, the curious fact was noticed that a very simple relation existed between the number of gramme-molecules of the various salts required per litre of water at 15° C. to make up solutions the physical constants of which should remain unaltered on mixing. It is a well-known fact that on mixing two chemically-inactive salt solutions the physical constants generally diverge very considerably from the arithmetical mean of those of the constituents. But Dr. Bender finds that it is possible to prepare "corresponding" solutions, which on mixture shall retain their physical constants unchanged, the constants of the mixtures forming the arithmetical means of those of the constituent solutions; and further, the strengths of these "corresponding" solutions expressed in gramme-molecules per litre bear extremely simple relations to each other. For example, with respect to density and expansion, a solution of sodium chloride containing 1 gramme-molecule per litre at 15° corresponds with a solution of potassium chloride also containing a gramme-molecule, or a barium chloride solution containing half a gramme molecule, barium being divalent; corresponding with these are also a solution of ammonium chloride containing $\frac{2}{3}$ gramme-molecule, and a lithium chloride solution in which $\frac{2}{3}$ gramme-molecule is dissolved in a litre of water. With respect to electrical conductivity, the following also correspond:—Solutions of NaCl, LiCl, and $\frac{1}{2}(BaCl_2)$, each containing n gramme-molecules; and of KCl and NH_4Cl, each containing $\frac{3}{4}n$ gramme-molecules per litre. Hence "corresponding solutions" are those whose gramme-molecule contents, respect being had to valency, stand in a simple relation to each other.

THE *American Meteorological Journal* for the months April to July last contains a reprint of a lecture delivered by Prof. Cleveland Abbe, in December last, before the Franklin Institute, on some popular errors in meteorology. We can only draw attention here to a very few of the points taken up. The author first attacks the astro-meteorological predictions made up for a long time in advance, and shows that every effort to demonstrate any appreciable influence of the moon or planets on our atmosphere has signally failed. He refutes the singular belief that animals or birds know more about future weather than man himself, and attributes their migrations and hibernating habits to the results of experience of many past ages, or to natural causes beyond their control; and he shows that what is true of animals is still more clearly true of vegetables, so that nearly all the rules for weather-prediction founded on the behaviour of plants, on the falling of soot in the chimney, &c., relate simply to hygroscopic phenomena, of which a hygrometer will give more accurate indications. The efforts to show that the destruction or growth of forests affects the climate are objected to on the ground that we have not enough observations of rainfall and temperature properly comparable with each other to justify any conclusion whatever. With reference to the fact of less rainfall being caught in gauges high above the ground, the author explains that, although the drops grow as they descend through clouds, they rarely grow after they have nearly reached the ground; the stronger winds to which the gauge is exposed when set high up, carry the drops to one side, and so the higher gauge catches less than the lower one.

THE Monthly Weather Review of the United States for May contains much useful information, and possesses additional interest from the fact of its publication so soon after date. Eleven barometric depressions are traced in the North Atlantic, two of which traversed the ocean from coast to coast. Among the notices of meteors, one of extraordinary size seems to have fallen in a field near Wellsburg, N.Y., making a pit 40 feet wide and 20 feet deep; an effort is to be made to find the meteor. A special feature in these Monthly Reviews is the reports of fogs in the vicinity of the banks of Newfoundland and in the trans-Atlantic routes. Notes on their possible prediction have been published by Sergeant E. B. Garriott in the last three issues of the Review, and ship-masters have been requested to send special reports relative to the fog-banks observed. From the observations already made it is assumed that the differences in the temperature of the air which cause the development of dense fog, are occasioned by the *deflection* of the regular prevailing air-currents by cyclonic areas advancing from the interior of the continent. A knowledge of the movements of these cyclonic areas would, in the opinion of Sergeant Garriott, allow of the prediction of fog in time to send telegraphic warning to ships leaving British ports. Further investigation of the subject by the Signal Office will show whether these hopes are capable of practical realization.

THE Danish Meteorological Institute has published its *Meteorologisk Aarbog* for 1885, with the exception of that portion relating to the colonies, which appears to be one year in arrear of the other parts. The work is divided into three sections. (1) Observations taken in the kingdom of Denmark at 10 principal stations, 102 climatological stations, and 171 for rainfall. At 8 of the principal stations the observations are printed *in extenso*; and there are also monthly and yearly *résumés*. The correction for gravity at lat. 45° is given for the means of the barometric observations, in accordance with the recommendation of the International Meteorological Committee (Paris meeting, 1885). (2) Colonial stations, containing observations in the Faroe Islands, Iceland, Greenland, and one station at Santa Cruz in the West Indies. (3) Observations of air and sea temperature, &c., taken on 21 light-vessels round the Danish coasts. These latter observations are very valuable for determining various questions connected with the range of sea-temperature of the coasts, and the migrations of fish, &c. The Reports of the Meteorological Council show that such observations have been taken for some years in this country, although not regularly published. The Danish observational system dates from 1861, when it was under the charge of the Agricultural Society. The Meteorological Institute has published its year-books since 1873.

AT a recent meeting of the Pekin Oriental Society, Dr. Dudgeon read a paper on "*Kung-fu*, or Taoist Medical Gymnastics." *Kung-fu* means labour, and is applied to the science of movement, including, among other things, massage, shampooing, and other operations on the body practised with the object of preventing and curing disease, and for the comfort and sense of bracing which they confer. One of the thirteen departments in the Chinese great Medical College is that of pressing and rubbing. An early Chinese work on this subject was translated by the Jesuits in 1779, and first drew the attention of Europe to the subject and stimulated inquiry. Ling, a Swede, introduced the movement cure into Europe; but here it rests on definite anatomical knowledge, whereas in

China it can lay claim to no such foundation. The Taoists adopted the practice at a very early period to ward off and cure disease; but in later times charms, incantations, and magic seem to have taken its place. Dr. Dudgeon described the general principles of the art, including active, passive, and breathing movements, and the *rationale* of the Chinese system of medicine on which it is founded. The life of man depends upon the existence of air circulating throughout the system. The vital principle is supposed to reside at a point one inch below the navel; from here the two principles of nature emanate. Thence, according to Chinese notions, proceeds the breath in expiration, and thither it goes in inspiration. The great object of life and also of *Kung-fu* is to nourish this original air, and avoid disease by preventing the admission of depraved air. Dr. Dudgeon gave a description of the various movements prescribed for various diseases. Some of these are complicated, and many ridiculous, but the practice appears to hold its place still in Chinese medicine.

At a recent meeting of the French Société d'Encouragement, M. Grosfils, of Verviers, described a new method he had hit upon for preserving butter. The principle of it is, to hinder the crystallization of salicylic acid added to the butter, and so maintain its antiseptic power indefinitely. This he effects by means of lactic acid, which is a pretty strong solvent of salicylic acid. The composition he had arrived at consists of 98 parts of water, 2 parts of lactic acid, and $\frac{1}{500}$ of salicylic acid. This will preserve good butter indefinitely, even at high temperatures and in hot countries. M. Grosfils estimates that the butter, supposing it retains 5 per cent. of its weight of liquid, will retain 1 part of salicylic acid to 100,000. Lactic acid beyond 2 per cent. gives a slightly acidulated taste which might affect the saleability of the butter: this may be removed by simple washing with water, or, better, with skim-milk containing a little bicarbonate of soda. The preparation of a kilogramme of butter by M. Grosfils' process does not cost more than one or two centimes.

It appears that, after some years' experiment, M. Jovis, Director of the Aëronautic Union of France, has found a satisfactory varnish for textile materials. It is of great flexibility, contains no oleaginous base, and, while adding little to weight, confers great impermeability. A piece of calico coated with it will retain hydrogen several days, and is not only not disaggregated by the matters applied, but even by use increases their dynamometric force; a matter of great importance for marine cordage, sails, tents, &c. The varnish is also suitable for paintings, wainscoting, &c., and it is exempt from mouldiness. It can be exposed to very varied temperatures without alteration. Lastly, the sub-products can be utilized for coating walls, railway-sleepers, &c. Such is the account presented to the Société d'Encouragement, to which the Aëronautic Union has applied for help to give this new industrial branch a worthy development.

We have received the Transactions of the Norfolk and Norwich Naturalists' Society for 1886-87. This is the eighteenth annual volume issued by this flourishing Society. The papers are numerous and varied, beginning with the presidential address of Sir Peter Eade, which is devoted to the subject of germ life, more particularly as it affects human and animal life. Mr. Seebohm follows with two papers on the birds of the Lena Delta and of the extreme north of Alaska, and Mr. Harvie-Brown contributes a paper on the birds of Priest's Island. Sir Peter Eade gives an account of two land tortoises (*Testudo graeca*) in confinement; and there are two papers on new or rare Norfolk plants. Mr. J. W. Gurney, Jun., has a paper "On the Periodic Movements of Galls on the Norfolk Coast," and the Rev. H. A. Macpherson writes on "Hybrid Finches." Mr. Francis Day gives descriptions of some remarkable forms of eels found in Saham Mere, Norfolk; and Mr. Southwell has a paper on the "Smelt Fishery in Norfolk," as well as his annual report on the herring fishery from the ports of Yarmouth and Lowestoft. Mr. A. W. Preston continues his meteorological notes. Two papers of more than local interest are contributed by Lieut.-Colonel Feilden and Mr. Herbert Geldart, the former on zoological, the latter on botanical, researches carried on during a voyage to Hudson's Bay on board the *Alert*, which, in the summer of 1886, visited and relieved the various meteorological stations in that locality. There are some interesting communications in the form of "Miscellaneous Notes and Observations"; and last, but by no means least, is Part II of the "Fauna and Flora of Norfolk," being Section II. of a list of the birds observed in the county by Messrs. Gurney and Southwell.

The journal *Caucase* states that the Imperial Society of the "friends of natural science, ethnography, and anthropology" are devoting particular attention to the zoology of the Caucasus. In 1885 the Society sent a mission to study the fauna of Erivan and of the coast of the Black Sea, and this year it has sent out two expeditions, one to study the fauna of the coast of the Caspian, the other that of the environs of Tiflis and the Lakes Gotchka, Paleoston, and others.

The death is announced of Dr. Johann Krejci, Professor of Geology at the University of Prague and a member of the Bohemian Parliament.

The Imperial Leopold-Caroline Academy of Naturalists, at Halle, celebrated its two-hundredth anniversary on August 7.

A volcanic eruption lately occurred in the Island of Galita, on the Algerian coast. The streams of lava were numerous, and the light of the fire was visible for forty miles around.

On July 26 a severe shock of earthquake was felt at Obernzell, Wegscheidt Messnerschlag, in Lower Bavaria.

A severe earthquake was noticed in Ecuador on August 2, at 6.29 p.m. Great damage was done in many cities, but Cuenca suffered most, many of the houses falling in, and others being seriously damaged. Shocks of earthquake were also felt in several places in Indiana, Kentucky, Tennessee, and on the eastern banks of the Missouri.

The additions to the Zoological Society's Gardens during the past week include a Red and Blue Macaw (*Ara macao*) from Central America, presented by Dr. and Mrs. T. W. Allright; a Carrion Crow (*Corvus corone*), European, presented by Mr. George Nicholson; a Fieldfare (*Turdus pilaris*), presented by Colonel Verner; a Hive of Bees, presented by Mr. T. Bates Blow; four Geckos, four Frogs from Italy; two Lineated Chalcis (*Chalcides lineatus*) from the South of France; two Dark-green Snakes (*Zamenis atrovirens*), two Natterjack Toads (*Bufo calamita*) from Germany, purchased; a Bennett's Wallaby (*Halmaturus bennetti*), two Viscachas (*Lagostomus trichodactylus*), three Wood Hares (*Lepus sylvaticus*), born in the Gardens; a Bronze-spotted Dove (*Chalcopelia chalcospilos*), two Hybrid Spotted Zenaida Doves (between *Zenaida maculata* ♂ and *Z. auriculata* ♀), bred in the Gardens.

OUR ASTRONOMICAL COLUMN

New Variable of the Algol Type.—Mr. E. Sawyer announces in No. 159 of *Gould's Astronomical Journal* his discovery that the star 155 (*Uran. Argent.*) Canis Majoris is a variable of the Algol type. A diminution in the light of the star was first observed on March 26; the star was then observed again on March 29 and 30, and April 6, 7, 9, and 10, and appeared on each occasion to be of about its normal brightness. On April 11 at 8h. 15m. it was again found to be faint, but had recovered brightness by 9h. On April 19 another minimum was

observed and the recovery of light successfully watched. The next night seemed to show the commencement of another minimum, but the star was low at the time of observation. The epoch would appear therefore to be some aliquot part of eight days; if the observation of April 20 is accepted, it will be about 1d. 3h. It is uncertain, as yet, whether the star has been observed at actual minimum; but the diminution of light remarked has amounted to about half a magnitude. As the star is the first certainly variable star in the constellation, it will probably be called R Canis Majoris. The place of the variable for 1875·0 is R.A. 7h. 13m. 49s., Decl. 16° 9′·7 S.

Mr. Sawyer gives in the same number of the *Astronomical Journal* some observations of Y Cygni, the new Algol-type variable discovered by Mr. Chandler last December. They give a general confirmation of the period, viz. 2d. 23h. 56m., deduced by Mr. Chandler from his own observations.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 AUGUST 21–27.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on August 21

Sun rises, 4h. 56m.; souths, 12h. 3m. 1·1s.; sets, 19h. 10m.; decl. on meridian, 12° 9′ N.: Sidereal Time at Sunset, 17h. 9m.

Moon (at First Quarter August 25, 20h.) rises, 7h. 37m.; souths, 14h. 10m.; sets, 20h. 30m.; decl. on meridian, 2° 55′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Decl. on meridian. ° ′
Mercury ...	3 15	10 54	18 33	17 49 N.
Venus ...	8 39	14 10	19 41	6 18 S.
Mars ...	1 51	9 56	18 1	21 49 N.
Jupiter...	10 49	15 57	21 5	10 46 S.
Saturn...	2 18	10 13	18 8	20 16 N.

Occultations of Stars by the Moon (visible at Greenwich).

August.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. ° °
22	65 Virginis	6	20 34	21 26	92 307
27	B.A.C. 6981	6	17 59	19 14	51 277

August.	h.	
21	14	Venus in conjunction with and 9° 13′ south of the Moon.
22	0	Venus at greatest distance from the Sun.
23	13	Jupiter in conjunction with and 4° 12′ south of the Moon.
25	2	Mercury at least distance from the Sun.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·3	81 16 N.	Aug. 21,	20 8	*m*
			26,	19 47	*m*
Algol	3 0·8	40 31 N.	,, 22,	0 37	*m*
			,, 24,	21 25	*m*
β Libræ	14 54·9	8 4 S.	,, 26,	21 6	*m*
U Coronæ	15 13·6	32 4 N.	,, 22,	22 36	*m*
U Ophiuchi... ...	17 10·8	1 20 N.	,, 21,	3 14	*m*
		and at intervals of		20 8	
X Sagittarii... ...	17 40·5	27 47 S.	Aug. 24,	22 0	*m*
W Sagittarii ...	17 57·8	29 35 S.	,, 24,	20 8	*m*
U Sagittarii... ...	18 25·2	19 12 S.	,, 21,	0 0	*M*
β Lyræ...	18 45·9	33 14 N.	,, 23,	21 0	*m*
			,, 27,	2 0	*M*
δ Cephei	22 25·0	57 50 N.	,, 27,	0 0	*M*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
From Pisces ...	60	11 N.	Swift.
Near κ Cygni ...	291	60 N.	Slow, brilliant trained meteors.

GEOGRAPHICAL NOTES.

THE *Bollettino della Società Geografica Italiana* for June contains a valuable contribution to the study of the ethnical relations in the Ogoway and Lower Congo basins, by the Cavaliere A. Pecile, who was associated for three years with Count Giacomo di Brazzà in his exploration of the new French protectorate in the equatorial region north of the Congo. All the multifarious tribes of this extensive region, which stretches from the coast inland to the Ubangi affluent of the Congo, are divided into two essentially distinct groups, that is to say (1) the original settled populations, either aborigines in the strict sense of the term, or such as have occupied their present homes from prehistoric times; and (2) those that have made their appearance in comparatively recent times on the Ogoway and Lower Congo continually pressing forward from the interior towards the coast. To the former group belong the Batekes, Adumas, Avumbos, Mbocos, Ondumbos, Mboshi, and many others; to the latter the Bakales, Pauens (Fans), Okandas, and Obambas of the Ogoway, and the Apfurus, Bayanzi, and others of the Congo and its northern affluents. One of the most important results of the author's researches is the light that he throws on this mysterious forward movement of the inland tribes, which is not confined to the equatorial regions, but extends almost uninterruptedly northwards to Upper Guinea and Senegambia. Here the chief aggressive populations are the Toucouleurs (mixed Berbers), Fulahs, and Mandingans, all now Mohammedans; in the Ogoway and Congo basins the Bakales, Fans, and Bayanzi, all still pagans, and mostly cannibals. These have already reached the coast at many points, pressing forward from a vast and almost impenetrable forest zone, which stretches from the seaboard eastward probably to the Niam-Niam country in the heart of the continent. But the author believes that he has discovered the very cradle of the fierce Bakale and Fan peoples about the head waters of the Ivindo (2° 30′ N.), where the old settlements still exist whence the first waves of migration flowed westwards. This general westward movement is described as taking place unconsciously, or through a sort of vague instinct attracting the over-crowded inland populations towards the centres of trade on the coast. Their interests naturally impel them in the direction whence come the European commodities so much coveted by all the inland populations. The Bakales appear to have preceded the Fans by many years, their migrations being chiefly directed towards the lagoons of the Lower Ogoway, where they are now settled between the local Galoa and Inenga tribes. The Bayanzi, who have acquired the ascendency along the right bank of the Lower Congo, seem to have come originally from the same regions as the Fans, whom they resemble in physical appearance, character, language, and usages. But while the latter are "land-lubbers," displaying absolute horror of the water, the Bayanzi have always been great fluvial navigators, so that their original home may have been the Upper Ubangi, slowly advancing down this great artery to its junction with the Congo. In general the settled aborigines are of blacker complexion and more decided Negro type; the intruders much fairer, taller, with more regular features, less woolly hair, more animated and intelligent expression. At the same time they are also more ferocious and very decided cannibals. This point, about which some doubts had been expressed, was confirmed in a startling way by the fate of three Aduma boatmen belonging to the Expedition, who happened to be left behind near a Fan village on the banks of the Ogoway, and whose skeletons were afterwards found carefully picked (*diligentemente scarnati*) by the villagers. The Fans are continually on the look-out for captives to supply their cannibal feasts, whereas the somewhat more pacific Batekes are anthropophagists rather through the necessity of procuring a flesh diet in their present territory, which is nearly destitute of large game. A chief source of their supplies are the unfortunate slaves, or the humbler members of the tribe, who are denounced by the medicine-men as the cause of any calamity, such as the sickness or death of a chief, and who are always sacrificed and eaten to propitiate the evil spirits, and at the same time to satisfy the craving for human flesh.

THE BRITISH ASSOCIATION.

THE Manchester meeting of the British Association promises to be brilliantly successful. It will probably be attended by a larger number of persons than have been present at any

former meeting; and, as we have repeatedly noted, ample preparations are being made for the hospitable reception of visitors. The meeting will be rendered especially interesting by the foreign men of science who will take part in the proceedings. To the lists, already printed, of these distinguished visitors we may now add the names of the American chemist Dr. Alfred Springer, and Dr. H. F. Weber, of Zürich. Dr. Sterry Hunt, F.R.S., of Montreal, has also expressed his intention of being present.

From an article in the *Times* of the 15th inst. we reprint the following account of the work which is expected to be done in most of the Sections:—

"Coming down to the Sections, we find the Presidential Chair of Section A (Mathematics and Physics) occupied by the Astronomer-Royal of Ireland, Sir Robert S. Ball, who is not only among the most eloquent of scientific orators, but one of the two great recognized wits of the Association, the other being a brother Irishman, Dr. Haughton. We may therefore expect something unusual in the way of presidential address from Sir Robert. The subject of the address will, we believe, be that part of the science of theoretical mechanics known as 'The Theory of Screws.' Its treatment will be peculiar and somewhat imaginary; it will indeed be 'a dynamical parable,' and contain a little more humour than is usually met with in such addresses. The general proceedings of the Section are likely to be of considerable interest. The report on the very important subject of electrolysis may possibly lead to a lengthy discussion, in which some of the more distinguished foreign visitors may be expected to take part. There may also be a discussion on the report of the Committee on Electrical Standards. Sir William Thomson will most likely exhibit his milliampere balances and read a paper on their application. Some interesting electrolytic results may be expected from Owens College, and Mr. Haldane Gee will exhibit a comparison-magnetometer. Electricity will occupy a prominent place in the Section. Mr. W. H. Preece will probably read a couple of papers. More results of Profs. Thorpe and Rücker's new magnetic survey may be communicated. Prof. Hull will treat of the effect of continents in altering sea-level. The Ben Nevis Observatory will have another word to say on high-level meteorology, and some papers on heat will come from the Glasgow University laboratory.

"The Geological Section will be presided over by that able palæontologist, Dr. Henry Woodward, of the British Museum. Dr. Woodward, in his address, may be expected to touch on some of the more important topics that have been recently engaging the attention of geologists—the progress of the geological survey, the relations which exist between palæontology and biology, and recent special researches in various directions. There will, we believe, be a discussion by a combined meeting of this Section with the Section of Economic Science, on the question of gold and silver, in which the geologists will mainly deal with the subject of supply. Another important discussion will be on the burning topic of the arrangement of museum collections—whether palæontologists should arrange their finds to suit themselves, or whether the fact of their extinction should be ignored and these specimens be mixed up with their extant fellows.

"The Geographical Section will this year have the honour of being presided over by the chief of the Metropolitan Police, Sir Charles Warren, himself an experienced practical geographer. He will probably in his address deal with one branch of the leading geographical topic of the day—the uses of the study of geography to the practical statesman. In this, the popular Section of the Association, there will be not a few papers of popular interest. The King of the Belgians is sending over two representatives to speak on the Congo Free State, while Capt. Coquilhat, an old Congo official, will read a paper on his own account. Mr. A. Colquhoun, who is in England on short leave, has promised a paper on Burmah and another on Formosa. Mr. John Forrest, the Surveyer of Western Australia, will read a paper on that colony. Mr. Steains, a young engineer just returned from the Botocudo country in South America, will have something racy to tell of an almost unknown people. Dr. L. Wolf, of Leipzig, who has done so much important work on the southern tributaries of the Congo, will give to the Section the results of his journeys. One of the most important papers will be on the new survey of Siam, on which Mr. M'Carthy, the official surveyor, has been engaged for seven years, and the beautiful maps of which he has brought home with him. Various aspects of geographical education will be brought forward by Mr. H. T. Mackinnon, Mr. E. G. Ravenstein, and others, while the subject of Antarctic exploration may receive some attention.

"With Dr. Giffen as President of Section F (Economic Science) we may expect an address which will be worth listening to by all interested in our national progress. The subject will be 'The Recent Rate of Material Progress in England,' and the lessons to be adduced will doubtless come home to all in their suggestiveness, so far as holding our own with other nations is concerned. The papers which are promised for the Economic Section are likely to be of unusual interest. The bimetallist movement has a very strong hold in Manchester, and it is expected that Prof. J. S. Nicholson, of Edinburgh, will be present to advocate this cause. Very valuable light will be thrown on the subject by Mr. E. Atkinson, of Boston, who has been commissioned by the United States Government to inquire into European feeling on this important issue. M. Walras and Mr. Dana Horton will also contribute papers on monetary matters. The status and working of limited liability companies is another subject of special interest in Lancashire; this will be dealt with by Mr. G. Auldjo Jamieson, of Edinburgh. There will also be an important discussion on a topic which is attracting attention all over the country—the depression in the value of land, and the reasons which have brought it about. Dr. Arthur Ransom will contribute an interesting statistical investigation on phthisis areas in Manchester and Salford. Another day will be devoted to a group of papers on subjects connected with foreign trade. Mr. F. Hardcastle, M.P., will read on the classification of the exports of cotton piece goods in Board of Trade returns; Mr. A. E. Bateman, of the Board of Trade, will have a paper on the statistics of our foreign trade, and what they tell us; Mr. Marshall Stevens will write on freights; and Mr. W. Westgarth and Prof. Leone Levi will deal with Australian and American protectionism.

"The economists will also give a day to education, especially in its technical aspects. With this Mr. W. Mather will deal, while Sir Philip Magnus will read a paper on schools of commerce. An interesting paper on farthing dinners in elementary schools will be contributed by Mr. Sargant, of Birmingham, who has made careful and minute observations on the working of the system. Two reports will also be presented to this Section—on the monetary standard, and on the lists by which wages are regulated in the cotton trade. The latter is an elaborate document, and will in all probability give rise to much interesting discussion.

"From Prof. Osborne Reynolds, as President of the Section of Mechanical Science, we are sure to have an address that will be of as much interest to the student of pure science as to those who deal only with its applications. As might be expected, the Manchester Ship Canal is sure to receive considerable attention in this Section, and we may expect a lively discussion on the papers by Mr. W. Shelford on 'Improvements of Access to the Mersey Docks,' Prof. O. Reynolds on 'The Tides in the Mersey,' and Mr. E. Leader Williams on 'The Manchester Ship Canal.' Another paper of the same class will be that by Mr. T. A. Walker on the Severn Tunnel, which is likely to be of special interest. Other papers likely to be of more than average interest will be those of Mr. Gisbert Kapp on the maximum work of dynamos, Mr. H. White on improved railway sleepers, Mr. A. S. Biggart on the Forth Bridge works, Mr. Arthur Rigg on a revolving engine, and Mr. Henry Davey on expansive working in directing pumping-engines.

"Prof. Sayce, as President of the Anthropological Section, is sure to give an address of real human interest. The science of anthropology is young, but it embraces many lines of inquiry. Prof. Sayce will very wisely confine himself to his own line—to the study of language and the evidence we may derive from it as to the history and development of mankind. He may broach some theories that will surprise orthodox anthropologists, and will have a good deal to say upon the Celts. We believe Canon Isaac Taylor is writing a paper on a subject kindred to that discussed by Mr. Sayce, and is expected to occupy the whole of Friday morning. Mr. Stuart Glennie will also contribute a paper on the same subject, and Mr. Akin Károly promises some contributions to the remote history of mankind. Mr. Flinders Petrie's collection of Egyptian squeezes and photographs will form the subject of a paper by the Rev. H. G. Tomkins. The report of the Egyptian Photographs Committee, and that on the North American Indians, will both be of considerable interest."

To this statement it is only necessary to add that Dr. E. Schunck will preside over Section B (Chemistry), and Prof. A. Newton over Section D (Biology).

THE JAPAN EARTHQUAKE OF JANUARY 15, 1887.[1]

SOON after the occurrence of the earthquake of January 15 last, which caused considerable damage to property in and near Yokohama, the authorities of the Imperial University directed the writer to visit the places which had been affected by the shock, and to make a full report of all the circumstances. The results thus arrived at form the subject of the present paper. Before proceeding with this, however, it seems desirable to give some particulars respecting the principal shocks which have been felt in the Empire since 1879.

The earthquake of February 22, 1880, is the severest that has been experienced in the Plain of Musashi during the last ten years. The damage done to buildings was very much greater than on the recent occasion. Its origin was in the Bay of Tōkyō.

On October 25, 1881, Nemuro, in Yezo, was visited by a somewhat destructive shock. Fissures were opened in the ground, and the damage to property was not inconsiderable.

The well-known Atami Spa and its neighbourhood were convulsed on the morning of September 29, 1882, by a sudden and severe movement, which damaged embankments, destroyed an historical monument, and did sundry other mischiefs.

The earthquake of October 15, 1884, originated in the Bay of Tōkyō, and affected the Plain of Musashi. It overturned a considerable number of chimneys, cracked walls, and broke articles in museums and elsewhere. In Tōkyō, the greatest horizontal movement, in a soft ground, was 42 mm., or double the amount observed on January 15 last. However, the total damage, taking the whole affected area into account, was smaller.

The seismic waves in the disturbance of October 30, 1885, extended over the whole of Northern Japan and part of Yezo, shaking a land area of 34,738 square miles. But, though of great extent, they fortunately did little harm.

On July 23, 1886, quite a destructive earthquake visited Shinano and the neighbouring provinces, overthrowing several houses, and forming fissures in roads and hill-sides. The shock also stopped the flow of a hot spring at Nozawa. The part most severely shaken was a mountainous district some 2000 feet above the sea, including the famous active volcano of Asama, and many extinct craters. This case was an unusual one, as most of the larger earthquakes in Japan extend along the sea-shore.

Next in the list comes the severe shock of last January.

It thus appears that this Empire is visited by a more or less destructive earthquake *almost once a year*, and that the Plain of Musashi is affected in like manner *at intervals of a few years*.

The shock of last January was of most unusual violence. It originated near the coast, about 35 miles south-west of Tōkyō, and the seismic waves propagated nearly 200 miles to the west and north-east along the Pacific seaboard. On the north-west they approached but do not quite reach the shore of the Japan Sea. They shook, in all, about 32,000 square miles of land area.

In Tōkyō the disturbance began at 6h. 51 m. 59s. p.m., with slight tremors. After thirty seconds from the commencement, the greatest horizontal motion (21 mm.) was recorded. The time taken to complete one to-and-fro motion of the ground was 2·5 seconds. The maximum vertical motion was only 1·8 mm., being, as usual, very small compared with the horizontal movement. The principal motion continued for more than two minutes, during which time no less than *sixty distinct shocks* occurred. The maximum velocity and maximum acceleration, which measure the overthrowing and shattering power of earthquakes, have been calculated from the above numbers, and found to be respectively 26 mm. and 66 mm. per second. These numbers, considering the range of motion, are small; or, in other words, the oscillations of the ground were comparatively gentle and slow, which serves to explain the fact that but little harm was done to property in the capital. In Yokohama, Hipp's seismograph registered a horizontal motion of 35 mm.

The origin of the shock was in a narrow band of country running from west to east in the province of Sagami, parallel to the coast, at a distance from it of about seven miles. It emanates from the western or mountainous parts of the province, passes through the southern foot of Oyama (4125 feet above the sea-level), and reaches the Bay of Yokohama in a total distance of about 30 miles. I believe the most probable cause of the shock to have been faulting or dislocation of the earth's crust along the band above named. This inference is supported by the fact that the parts of the country through which the western half of the band passes consists of rocks of different geological formations, interwoven in such a way that their junctions present lines of weakness favourable to earth-snaps. The topographical features of the district—high mountains on the north, and comparative low plateau and sea-shore on the south—also lend strength to this conclusion. Unequal distribution of loads on the earth's surface tends to facilitate bending and folding of the rocks.

It is along the above-named axis or band that the effects were most striking. They were mainly confined, however, to a small breadth on either side of it, so that places as little as two or three miles to the north or south experienced a well-marked diminution of seismic energy. This is not the first instance in the history of the severer shocks in which the destructive effects have been practically limited to a small area near the origin.

More especially on the hilly or western portion of the origin, land-slips and cracks were numerous. The cracks mostly took place in banks, hill-sides, or other situations favourable for their formation. The writer counted no fewer than seventy-two in a distance of seven miles, the largest measuring a foot wide and five hundred feet long, and all of them running parallel to the axis of origin, which is also parallel to the general contour of the country. Several wells became turbid. In some of artesian character the water permanently decreased; in others it increased. There is a ferry across the large river Banyū where it is crossed by the axial band; but the water was so agitated by the shock that for some time afterwards the boat could not be used. The water in one of the rivulets on the west became muddy. The shock was severely felt on board of vessels in Yokohama harbour, the people in many of them rushing on deck under the impression that they had been run into. The effects upon these vessels were doubtless caused partly by motion communicated through the cables, and partly by agitation of the water due to movements of the sea-bottom. The earthquake was preceded by the usual warning roar or rumbling, as of distant cannon, emanating apparently from the western part of the origin-band. In that district, too, the after-shocks on the same night were five in number, while in Tōkyō there was only one. There were four tremors near the origin during the night of the 16th.

Dwelling-houses in country towns and villages are always built of wood. Their frame-work is of timbers from four to seven inches square, crossing one another at right angles. The uprights are placed about three feet apart, and stand on rows of squared stones or boulders, the intervening spaces being filled with bamboo-laths, on which is laid the mud-plaster that forms the walls. Tiles and straw are principally used for the roof-covering. In the district near the origin these wooden houses shook with great violence. Several of them were more or less twisted, cracked, or unroofed. Sliding doors, covered with paper or of wood, which serve as shutters, partitions, and windows in Japanese houses, broke and were shot out of their grooves. The joints between the frames were in some cases badly loosened. Although there are thousands of wrecked houses, in the district of origin, on the verge of falling down, and looking as if a strong breeze would be enough to blow them over, the buildings of this class nevertheless withstood the violence of the earth movements so far as to escape actual demolition. The writer saw only two small rotten hovels which had

[1] Paper by S. Sekiya, Professor of Seismology, Imperial University, Japan. Reprinted from the Journal of the College of Science, Imperial University, Japan, vol. i. part iii. The earthquake, the distribution and effects of which are described in this paper, is the shock which formed the subject of an article in NATURE for June 2 (p. 107), in which one of the autographic records obtained by the author with Prof. Ewing's seismographs was reproduced in *facsimile*. The diagram in question, which showed a greatest horizontal movement of 7½ millimetres, was one of those referred to near the end of this paper as having been obtained on the stiff elevated soil where the University is built, and where the amplitude of the motion was little more than one-third of the motion shown by seismographs of the same construction on the lower alluvial soil.

been thrown down. This circumstance shows the tenacity of wooden framed structures. Prof. T. Mendenhall, in a report [1] on the recent catastrophe at Charleston, says:—"As was to be expected, buildings constructed of wood suffered much less than those of brick. The interior of wooden buildings, however, would often exhibit a scene of total destruction, furniture, book-cases, &c., having evidently moved with great violence."

Fire-proof stores, or *Kura*, suffered severely as to their walls. These buildings have wooden frames, strongly joined by horizontal and vertical pieces, and closely covered with laths, the whole making up a compact box-like structure. The roof is tiled, and carefully plastered with a mud which has a slight cementing property, to the thickness of from three to nine inches. This plaster is put on in several layers, each layer being added after the preceding one has dried. The whole process is an expensive one. The walls, on account of their great thickness and the poor tenacity of the mud, are easily cracked or stripped. As many as sixty or seventy per cent. of the *Kura* suffered from the recent shock. It is evident that these thick-walled structures should be replaced by brick buildings, which are equally fire-proof and much stronger.

It may be mentioned, however, that the frameworks of *Kura*, after having been entirely stripped, have withstood the most violent earthquake on record.

In Yokohama, houses are built of different types and with a variety of materials, so that they afford a fair field for the comparison of seismic effects. It is very fortunate that, judging from the effects wrought by the recent earthquake on both land and buildings, the seismic intensity in this town was less than one-third of that in the western or hilly parts of the origin-band. But for this, the results would have been highly disastrous.

The houses which suffered most were the composite structures of wood and stone. They are built of wooden frames encased with stone blocks, each of the latter measuring 2 feet 9 inches long, 9 inches wide, and 6 inches thick, and being clamped to the wooden planks inside by three iron nails. The nail, called *Kasugai*, is 5 inches long and $\frac{7}{16}$ inch square, and bent at right angles at its two ends. The stone is soft and brittle, being volcanic rock of the worst quality. In time the iron nails get rusty, and the stones are so acted on by rain and frost as to be easily cracked, or detached from the wooden frames, even by moderate shakings. These buildings, erroneously called European houses, already exist in abundance, and unfortunately increase each year in number. They are generally constructed with bad materials and on faulty principles; the object of the builders being to attain fair protection from fire, along with the appearance of a stone building, at the least practicable cost.

Two brick structures received serious damage, cracks having been formed, as usual, at the corners of the buildings and over the windows. The seismic vibrations, however, left no traces on the Town Hall, the Custom House, Prefectural Office, and other well-built structures of brick or stone.

In Yokohama, wooden houses sustained no damage worth mentioning. Joints were more or less loosened and tiles occasionally fell down from the roofs. The tiles that are fastened to the framework of wooden houses, to form walls, were in some cases detached in large quantities. There are decidedly many improvements which might be made in the present wooden buildings, both of Japanese and so-called European styles, especially in the arrangements of their joints, the scientific distribution of materials, &c. If these and other defects were properly remedied, such dwellings might be made pretty safe as against earthquakes. In sites little liable to danger from fire, one may find, in this country, wooden houses built three and even four centuries ago. Wood, no doubt, will continue for a long time to be the chief building material in this country.

In Japan, however, fire is a more constant and even more dread enemy than earthquakes, while terrible conflagrations are often brought about by destructive shocks. Hence, brick and stone should, and probably will in time come to be largely employed for building, especially in towns. The question, then, is to select certain types of brick or stone houses which are best calculated to resist earthquake shocks. Sheet and bar iron houses, as used in Australia, would make very efficient earthquake-proof buildings, although they are not free from several objections.

After the terrible catastrophe of 1883 in the Island of Ischia, the Italian Government appointed a Commission [1] to consider the reconstruction of the buildings in that island. The Commission, after investigating the different modes of construction most suitable for earthquake countries, submitted models of houses in wood, and in combinations of wood and masonry, which were adopted. The Commission recommended that buildings should be chiefly constructed with an iron or wooden framework, carefully joined together by diagonal ties, horizontally and vertically, the spaces between the framework being filled in with masonry of a light character. Not more than two stories above ground were to be allowed, &c., &c.

In Italy, brick houses are joined by iron tie-rods; and similar devices are now, to a certain extent, used in this country. Concerning the erection of brick or stone houses in Japan, much valuable information is to be obtained from the Italians, who, like ourselves, have lived for centuries amidst terrible shakings, and who, no doubt, have gained much experience in the constructive arts suitable to the conditions of our existence here.

A prominent feature in the effects of the recent earthquake was the overthrowing of brick chimneys in Yokohama, especially on the Bluff. Soon after the shock, circulars were sent round to the principal residents, asking for information as to the effects of the shock on the buildings occupied by them. More than fifty answers were received, and the facts embodied in them have been of great value in preparing this paper. The writer takes this opportunity of expressing his warmest thanks for the kind assistance thus rendered to him. From these answers, from the Police Reports, and from actual observations, fifty-three chimneys appear to have been destroyed. In one instance a heavily-coped chimney fell in a large mass through the roof, and severing a strong beam of 1 foot by 8 inches on the second story, penetrated to the ground floor.

About one-half of the chimneys thrown down during the shock were cut in two at their junction with the roof; while some dislodged the tiling and did sundry other damage to the buildings at their points of contact. Evidently the chimneys and the houses moved with unequal range and with different vibrational periods. Prof. Milne has more than once recommended that chimneys should be built thick and squat, without heavy ornamental mouldings or copings; and be, if possible, disconnected from the roofs. Those houses in which his suggestions had been adopted suffered no damage on January 15.

Generally, the relations of the seismic effects to the geological, topographical, and other features of the various localities were found to corroborate previous experience. That the seismic vibrations in hard ground are very much less than in soft soil was well illustrated on the recent occasion. At the University, where the ground is hard and firm, the seismograph recorded only 8 mm. horizontal motion, as compared with 21 mm. registered by a similar instrument placed on soft soil a mile distant. Totsuka is a small town, with a single long street running along the foot of a hill; one side of the street, however, is built on made-up ground. Most serious damage was done on that side, while the opposite houses suffered very much less, though not more than twenty feet distant. Houses built on cliffs and hill-brows received more damage than those situated at the base or on the flat summits of the same hill. To observe the effects of marginal vibration, the writer recently placed one seismograph at the steep edge of a loamy hill thirty-eight feet in height, and another similar instrument at its foot. The motions, thus far measured, at those two levels are found to be in the ratio of 2 to 1. A third instrument will shortly be set up on the flat summit of the same hill. Observations of a similar nature, on different rocks and at various heights, will form the subject of a further paper. It is probably owing to marginal vibration that houses on the Bluff of Yokohama are always heavy sufferers from earthquakes.

The extensive and rapidly increasing use of kerosene lamps in Japan constitutes a grave danger in severe shocks. The lamps now in common use are of very brittle materials, contain the most combustible of oils, and are usually poised on ill-balanced stands. In the great earthquake of 1855, at a time when kerosene was unknown in this country, fire broke out in Yedo at more than thirty points, setting a very large part of the city in a blaze. In the event of another such shock, the mischief which would be produced from this cause alone is awful to contem-

[1] The *Monthly Weather Review*, U.S. Signal Service, August, 1886.

[1] Proceedings of the Institution of Civil Engineers, vol. lxxxiii., Session 1885-86, part i.

plate. Great credit will be due to any one who can invent a convenient earthquake safety-lamp, which, it is to be observed, will also constitute a valuable safeguard in ordinary daily life. It is true, so-called safety-lamps are sold in Tōkyō, but they are very ineffective and miserable affairs. The use of metallic oil-holders would doubtless greatly lessen the danger.

During his inquiry the writer was shown sixteen lamps that had been broken in the recent earthquake. In one instance the kerosene caught fire, and it was with great difficulty that the residents extinguished it by the aid of wet mats.

MINERALS AT THE AMERICAN EXHIBITION.

ONE of the most conspicuous features of the American Exhibition is the remarkable collection of minerals brought over and exhibited by Mr. A. E. Foote, of Philadelphia. Many of the specimens, which are extremely fine, have been obtained during collecting-expeditions undertaken by Mr. Foote himself, and several new species and varieties have been made known to science through his indefatigable labours.

The central feature is a hexagonal pavilion covered with mica, and surmounted by a model of a snow crystal. Each side of the pavilion is devoted to a separate mineral region of the North American continent—except the first, which is filled with a collection of gems and ornamental stones. Here are rough and cut specimens of a precious ruby, topaz, opal, williamsite, with examples of malachite and azurite beautifully banded and taking a fine polish.

A lapidary who has had several years' experience in making rock-sections for the British Museum is constantly employed close by.

Minerals from the region near the Pacific coast come next. Wulfenite, a rare species, some the finest specimens ever seen, is here exhibited in large groups of orange-red crystals; also brilliantly red vanadinites and large bright crystals of chessylite or azurite associated with velvet tufts of malachite. All these are from the marvellous country that Humboldt called New Spain. The deep-red garnets from Alaska in their sombre settings of gray mica-schist are especially noteworthy. Among the minerals of the Rocky Mountain region are wonderful crystals of the green Amazon-stone; ore from the famous Bridal Chamber at Lake Valley, New Mexico, so rich that the heat of a match will cause it to melt and fall in drops of nearly pure silver. A space the size of a moderate-sized room produced about £100,000. The precious turquoise comes from Los Cerrilloz, New Mexico, where Montezuma got his chalchuhuitls that he valued above gold and silver. The Indians still make long pilgrimages for the sacred stone.

Most striking among the minerals of the Mississippi Valley and Lake region are the blendes and galenas from South-West Missouri, a district that now produces over one-half of all the zinc mined in the world. It was formerly so abundant that farmers built their fences with it. Masses of the lead-ore weighing ten tons were found within 12 feet of the surface. Here Indians formerly procured the lead for their bullets, placing the ore in hollow stumps and building a fire over it.

From Arkansas come fine rock-crystals or hot-spring diamonds, with powerful lodestones, arkansites, and hydrotitanites.

From the Lake Superior region come copper, chlorastrolites, and zonochlorite, a remarkable gem-like mineral.

In the case devoted to the North Atlantic coast region is rhodonite, so much used by the Russians in their ornamental work, in fine crystals. The mines at Franklin, N.J., produce also many minerals found nowhere else in the world, such as franklinite, named after the illustrious philosopher; anomolite, a new species recently described by Prof. G. A. König, of the University of Pennsylvania; troostite, jeffersonite, blood-red zincite, &c., &c. Cacoclasite, a new species in fine crystals, associated with pink titanite, comes from the same region, as do the remarkable crystals of apatite. These are among the finest specimens ever seen, and associated with them are the brilliant twin-zircons. From the apatite are manufactured hypophosphites to stimulate the appetite, and superphosphates to grow wheat and corn.

The last case devoted to the South Atlantic coast region contains amethysts, sapphires, aquamarines, tantalite, gummite, and uranolite, huge sheets of mica, &c., &c.

Next to the wall opposite is a very extensive collection illustrating the mineralogy of Pennsylvania, which, besides the well-known coal, iron, and other ores that have made the State famous, includes very extraordinary specimens of the rare mineral brucite, from which the medicine, Epsom salts, may be made; diaspore in fine crystals, corundum for polishing purposes, chromite for producing brilliant yellows, &c., &c.

Adjoining, in cases and drawers, are the college and educational collections indispensable for the studies of mineralogy, geology, and chemistry.

The collection of American Geological Surveys and other scientific works is very extensive, over fifty volumes from Pennsylvania alone being shown. We have devoted so much space to the description of the extensive exhibit made by Mr. A. E. Foote, of Philadelphia, that we can only refer to the minerals shown by Kansas and other States, by the Denver and Rio Grande and C. B. and Q. Railroads, and by various mining companies.

THE FOLK-LORE OF CEYLON BIRDS.

A CORRESPONDENT of the *Ceylon Observer* of Colombo, referring to the interest excited by Mr. Swainson's new book on "The Folk-Lore and Provincial Names of British Birds," notes some points in the folk-lore of the birds of Ceylon, obtained largely in conversation with natives. The devil-bird (*Syrnium indrani*) stands *facile princeps* for his evil reputation; his cry heard in the neighbourhood of villages is a sure harbinger of death, and the superstitious natives are thrown into great consternation by its demoniac screech. The legend about the bird is as follows:—A jealous and morose husband doubting the fidelity of his wife killed her infant son during her absence and had it cooked, and on her return set it before her. She unwittingly partook of it, but soon discovered that it was the body of her child by a finger which she found in the dish. In a frenzy she fled to the forest, and was transformed into a *ulania*, or devil-bird, whose appalling screams represent the agonized cries of the bereaved mother when she left her husband's house. The hooting of owls in the neighbourhood of houses is believed to bring misfortune on the inmates. The magpie robin, though one of the finest of the song-birds of Ceylon, is similarly tabooed; it has a harsh grating screech towards evening, which is considered ominous. The quack of the pond heron flying over a house is a sign of the death of one of the inmates, or of a death in the neighbourhood. If the green pigeon (*Nika kobocya*) should happen to fly through a house, as it frequently does on account of its rapid and headlong flight, a calamity is impending over that house. Similarly with the crow. But sparrows are believed to bring luck, and are encouraged to build in the neighbourhood of houses, and are daily fed. The fly-catcher bird of Paradise is called "cotton thief," because in ancient times it was a freebooter, and plundered the cloth merchants. As a penalty for its sins it was transformed into a bird and doomed to carry a white cotton attached to its tail. The red wattle lapwing, the alarm bird of sportsmen, has the following legend connected with it:—It is said to represent a woman who committed suicide on finding herself robbed of all her money, amounting to thirty silver pieces, by her son-in-law. The cry of the bird is likened to her lament: "Give the silver, give the silver, my thirty pieces of silver." Its call is heard at all hours, and the stillness of night is broken with startling abruptness by its shrill cry. Another story about it is that when lying in its nest in a paddy field, or a dry spot in a marsh, it lies on its back with its legs in the air, being in continual fear that the heavens will fall and crush its offspring. The story current about the blue-black swallow-tailed fly-catcher (*Kawudu panikkia*) and its mortal enemy, the crow, is that the former, like Prometheus of old, brought down fire from heaven for the benefit of man. The crow, jealous of the honour, dipped its wings in water and shook the drippings over the flame, quenching it. Since that time there has been deadly enmity between the birds. The Indian ground thrush (*Pitta coronata*) is said to have once possessed the peacock's plumes, but one day when bathing the peacock stole its dress; ever since the *Pitta* has gone about the jungle crying out for its lost garments. According to another legend, the bird was formerly a prince who was deeply in love with a beautiful princess. His father sent him to travel for some years, and on his return the princess was dead. He still wanders disconsolately about calling her name. It is also said that the peacock, being a bird of sober plumage, borrowed the brilliant

coat of the *Pitta* to attend a wedding, and did not return it. The disconsolate *Pitta* wanders through the jungle calling on the peacock to restore its dress—hence the cry, *ayittam, ayittam* (my dress, my dress). The cry of the hornbill (*Kandetta*) is inauspicious and a sure sign of drought. The bird is doomed to suffer intolerable thirst; not being able to drink from any stream or rill, it has the power only to catch the rain-drops in its bill to quench its thirst, and keeps continually crying for rain.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

THE following is the list of candidates successful in the competition for the Whitworth Scholarships, 1887:—James Whitaker, 21, engineer student, Burnley, £200; John Calder, 20, mechanical engineer, Glasgow, £150; John Smith, 22, carpenter, Belfast, £150; Nicholas K. Turnbull, 21, mechanical engineer, Glasgow, £150; James C. Talbot, 23, engineer, Southampton, £150; Arthur F. Horne, 25, mechanical engineer, Moreton-in-Marsh (formerly of Glasgow), £150; Edward J. Duff, 23, engineer, Glasgow, £150; Robert N. Blackburn, 20, engineer apprentice, Liverpool, £150; William Thomson, 20, engineer apprentice, Glasgow, £150; William W. F. Pullen, 20, engineer apprentice, Cardiff, £100; Edwin Griffith, 20, engineer apprentice, Glasgow, £100; Frederick C. Tipler, 23, assistant chemist, Crewe, £100; Thomas H. M. Bonell, 24, analytical chemist, Swindon, £100; Richard J. Redding, 22, metallurgist, Plumstead (Woolwich), and Arthur W. Sisson, 25, mechanical draughtsman, Lincoln (equal), £100 each; Arthur H. Abbott, 22, engineer, Great Yarmouth, £100; George Hough, 23, engineer, Wolverton, £100; Harry G. Christ, 19, engineer apprentice, London, £100; Harry D. Griffiths, 21, engineer apprentice, Cardiff, £100; Denholm Young, 24, engineer apprentice, Edinburgh, £100; Benjamin G. Oxford, 20, engineer apprentice, Liverpool, £100; Bernard H. Crookes, 21, engineer student, Liverpool, £100; George J. Wells, 23, engineer, London, £100; John Eustice, 23, engine fitter, Camborne, £100; Augustus H. H. Bratt, 24, engineer, Plumstead (Woolwich), £100.

SOCIETIES AND ACADEMIES.

LONDON.

Entomological Society, August 3.—Dr. D. Sharp, President, in the chair.—Mr. J. W. Peers and Mr. R. G. Lynam were elected Fellows.—Jonkeer May, the Dutch Consul-General, exhibited a pupa and two imagos of *Cecidomyia destructor* (Hessian fly) which had been submitted to him by the Agricultural Department.—Mr. W. White exhibited, and made remarks on, a specimen of *Philampelus satellitia*, Linn., from Florida, with supposed fungoid excrescences from the eyes. Mr. Stainton said he was of opinion that the supposed fungoid growth might be the pollinia of an Orchis. Mr. Poulton expressed a similar opinion, and the discussion was continued by Mr. Pascoe and Dr. Sharp.—Mr. White also exhibited a specimen of *Cetephia alchymista*, bred from a pupa collected last autumn on the south coast.—Mr. McLachlan sent for exhibition a number of oak-leaves infested by *Phylloxera punctata*, Lichtenstein, which he had received from Dr. Maxwell Masters, F.R.S.—Mr. Champion exhibited two rare species of *Curculionidæ* from the Isle of Wight—viz. one specimen of *Baridius analis*, and a series of *Cathormiocerus socius*. He remarked that *C. maritimus*, Rye, had been placed in recent European Catalogues as a synonym of the last-named species, but that this was an error. He also exhibited a series of *Cicindela germanica*, from Blackgang.—M. A. Wailly exhibited, and made remarks on, a number of living larvæ of *Antheræa pernyi*, *A. mylitta*, *Telea polyphemus*, *Platysamia cecropia*, *Attacus cynthia*, *Callosamia promethea*, and other silk-producing species. He also exhibited imagos of the above species, imagos of *Antheræa yama-mai*, and a number of species of Diurni from Sarawak.—Mr. Poulton exhibited crystals of formate of lead obtained by collecting the secretion of the larva of *Dicranura vinula* on 283 occasions. The secretion had been mixed with distilled water in which oxide of lead was suspended. The latter dissolved, and the acid of the secretion being in excess the normal formate was produced. Prof. McIlola promised to subject the crystals to combustion, so that their constitution would be proved by the final test.

EDINBURGH.

Royal Society, July 15.—Special Meeting.—Dr. J. Murray, Vice-President, in the chair.—Prof. Tait submitted a communication by Sir W. Thomson on the stability of the steady motion of a viscous fluid between two parallel planes.—Sir W. Turner communicated a note by Mr. George Brook on the epiblastic origin of the segmental duct in teleostean fishes, and birds.—Prof. T. R. Fraser read a preliminary note on the chemistry of strophanthin.—Mr. J. J. Coleman described a new diffusimeter and other apparatus for the study of liquid diffusion.—A paper by Mr. Frank E. Beddard was communicated by Prof. Sir W. Turner.—Dr. Murray read a paper on the mean height of the land of the globe. The lower limit he gives is, in round numbers, 1900 feet. The higher limit, which he believes to be more nearly correct, is about 2100 feet.—Mr. J. T. Cunningham, of the Scottish Marine Station, read a paper on the *Chætopoda sedentaria* of the Firth of Forth.

July 18.—Sheriff Forbes Irvine, Vice-President, in the chair.—The Chairman intimated the foundation by Dr. Gunning of the Victoria Jubilee Prize, and the conditions of award which have been approved by the donor. The first award of the prize was made to Sir W. Thomson, for a remarkable series of papers on hydrokinetics which he has communicated to the Society.—Mr. W. Durham read the second part of his paper on the laws of solution.—Prof. Tait communicated a paper by Prof. W. Burnside on the partition of energy between the translatory and rotational motions of a set of non-homogeneous elastic spheres. The rotational energy is equal to two times the translational energy.—Dr. H. R. Mill submitted a paper on the salinity, temperature, &c., of the Firth of Forth.—Prof. Tait communicated a paper by Mr. Albert Campbell on the direct measurement of the Peltier effect. Mr. Campbell has experimented with three pairs of metals. His results agree in every case with Prof. Tait's thermo-electric diagram. The agreement in the case of iron and nickel is of special importance.—Dr. Alex. Scott communicated a paper on vapour-densities at high temperatures.—Prof. Tait read a paper by Dr. G. Plarr on the determination of the curve, on one of the co-ordinate planes, which forms the outer limit of the positions of the point of contact of an ellipsoid which always touches the three planes of reference.—Mr. Buchan read a paper by Mr. A. Rankin on the mean temperatures of the various winds at Ben Nevis Observatory.—Prof. Crum Brown read a paper on ferric ferricyanide as a reagent for detecting traces of reducing gases. This reagent gives a test depending on the production of colour, which is a more delicate test than one which depends on its disappearance.—Prof. Tait communicated some results on the compressibility of water, of mercury, and of glass. The average compressibility of a 20 per cent. aqueous solution of common salt per atmosphere for the first 100 atmospheres is 0·0000316. It diminishes rapidly with the percentage of salt in solution. The compressibility of common lead glass is 0·0000027 at a temperature of 19° C.—Prof. Berry Hayecraft submitted a description of experiments to show the truth of Sir J. Lister's theory of coagulation.—Dr. Murray communicated a paper by Mr. Adam Dickie on the chemical analyses of sea-water from the Clyde sea-area.—The Chairman mentioned the number of papers read during the session, classifying them under various heads. He also read the Jubilee address which had been presented to Her Majesty by the Secretary of State on behalf of the Society.

PARIS.

Academy of Sciences, August 8.—M. Janssen in the chair.—Observations of the minor planets, made with the great meridian instrument of the Paris Observatory during the first quarter of the year 1887, communicated by M. Mouchez. The right ascensions and polar distances are given of Leto, Sophrosyne, Undine, Hebe, and nine other minor planets at various dates with Paris mean time, all comparisons being referred to the ephemerides published by the Berlin *Jahrbuch*, except those of Undine, which are referred to those published in No. 288 of the circulars of the Berlin *Jahrbuch*. The observations were made by M. O. Callandreau.—Fresh documents on the relations existing between the chemical and mechanical work of the muscular tissue, by M. A. Chauveau,

with the co-operation of M. Kaufmann. In order to complete his series of preparatory determinations on the mechanical work of the muscular tissue, the author has attempted to determine the quantity of heat produced by the muscles which function effectively in the physiological conditions of the normal state. By the methods and new processes here described he claims to have overcome the great difficulties inherent to studies of this nature. His experiments show once more that a large amount of heat is generated while the muscle operates, and of this only a small quantity is absorbed by the work performed. Repeated experiments will be needed accurately to determine this quantity. From the experiments already made, he infers that it mostly ranges from one-seventh to one-eighth of the total, the coefficient of the latter being 0·000323 calories, and that of the heat transformed into work generally from 0·000041 to 0·000034 calories.—New fluorescences with well-defined spectral rays, by M. Lecoq de Boisbaudran. Here the author studies alumina with the earth Za_2O_3; but as this earth has not yet been obtained in a pure state, he has been compelled to employ a substance still mixed with some other rare earths, notably $Z\beta_2O_3$; $Z\alpha$, however, being greatly in excess of $Z\beta$. Alumina containing 1/1200 of Za_2O_3 impure, heated with sulphuric acid and moderately calcined to a red (between the fusions of silver and copper), yields a greenish-yellow fluorescence, faint and without measurable spectrum. With 1/50 of Za_2O_3 in the alumina, a green fluorescence is obtained, slightly yellow and dull. The spectrum consists chiefly of the bands of $Z\beta$, which apparently differ but little from those obtained by reversion with a solution of $Z\beta Cl_6$. The presence of $Z\alpha$ is indicated to the right of the two yellow and blue bands; but the green band of $Z\beta$ is the strongest in the spectrum, having two nebulous maximums, of which that to the right is the most intense. The author also announced that he had obtained some very fine fluorescences by highly calcining alumina containing a little didymium or praseodymium.—The partial lunar eclipse of August 3, observed at the Observatory of Bordeaux, by M. G. Rayet. Under a three-prismed spectroscope, mounted on the great equatorial (0·38 metre) the transition from the adumbrated to the luminous part of the disk appeared very abrupt. While the spectrum of the former was limited by the lines D and F, with a maximum of intensity towards E, that of the part in transition extended abruptly towards the red as far as Angström's atmospheric group α. But the spectrum of the moon especially near the eclipsed part, was too pale to permit the use of a slit narrow enough to show the atmospheric lines. The α group and the very numerous lines near D were alone distinctly visible under the form of bands.—On the tides of the Tunisian coast, by M. Héraud. The observations made during the hydrographic survey of this coast have enabled the author to study the tidal movement, the existence of which in the Gulf of Gabes and on the adjacent seaboard has long been demonstrated. These tides appear to be the most important and regular in the whole Mediterranean basin; but they are perceptible only on the section of the coast to the south of Mehediah. They continually increase in magnitude as far as Gabes, where they acquire a maximum of 2 metres at the mean spring-tides, thence decreasing to 1 metre at Zarzis and on the Tripoli frontier. The tidal wave appears to come from the east, the mean period being apparently about 24 hours. All the observed circumstances would seem to show that the relation of the lunar to the solar wave is less than that of the absolute actions of the sun and moon.—A comparative study of the old, eruptive and sedimentary rocks of Corsica and the Eastern Pyrenees, by M. Ch. Depéret. During a recent trip to Corsica the author had an opportunity of determining some very close analogies between these two geological systems. Thus the central part of the granitoid mass at Ajaccio is formed of a porphyroid granite disseminated with black mica, passing thence on either side insensibly to a granulitic granite, a true transitional formation between the granulite type and granite. Analogous formations occur in the Eastern Pyrenees, as, for instance, in the granitoid mass stretching east and west between the valleys of the Aude, Têt, and Boulsane. Here also the central part, extending from the forest of Salvanère to Belesta, consists of a porphyroid granite passing on both sides imperceptibly over to a granite with two micas and granulitic texture. A comparative study of the eruptive and sedimentary rocks in both regions reveals similar resemblances. In Corsica the Cambrian limestone everywhere worked as marble is absolutely identical with that of the Pyrenees.

BERLIN.

Physiological Society, July 1.—Prof. du Bois Reymond, President, in the chair.—Dr. Martius communicated the results of his researches, by the graphic method, on the movements of the heart. When a sound is passed into the œsophagus, and connected with a Marey drum, cardiopneumatic curves are obtained whose interpretation is still a matter of controversy. In order to arrive at an experimental decision on this point, Dr. Martius has recorded simultaneously on the same individual the cardiopneumatic curves from the œsophagus and the curve of impulse of the ventricular apex as obtained from the wall of the thorax. It appeared from this that the curve of ventricular impulse is of doubtful interpretation; its shape was always the same; but it was impossible to determine with any certainty which part of the curve corresponds to the systole, and which part to the diastole. Dr. Martius has therefore registered the occurrence of the heart-sounds by auscultation and making signals which were recorded on a rotating drum on which the curves of cardiac impulse were being registered, having first ascertained that his personal equation was without influence on the results. In this way he was able to show that the first sound of the heart, corresponding to the closing of the auriculo-ventricular valves, coincides with the first rise of the curve from the base-line, while the second sound, or closing of the semilunar valves, coincides with the second smaller rise of the curve. The *first* rise and fall of the curve corresponds therefore to the cardiac systole. The speaker explained the shape of the *whole* curve as follows:—At the commencement of the systole the auriculo-ventricular valves are shut, as also are the semilunar valves since the aortic blood-pressure has not yet been overcome. During this period the contracting cardiac muscles alter the shape of the heart, the apex moves forward, and so the curve rises. As soon as the pressure in the ventricle is greater than that in the aorta, the semilunar valves open and the blood begins to pour out of the ventricle; as the result of this the apex of the heart moves back, and the curve falls till it reaches the base-line at the conclusion of the systole and commencement of the diastole. At this instant the semilunar valves close and the shock thus produced is communicated to the heart, and makes itself evident on the curve as the second or valvular rise. Thus finally the first rise of the curve of cardiac impulse corresponds to that period of systole during which all the valves are closed; the first apex of the curve marks the instant at which the semilunar valves open; the first fall of the curve indicates that portion of the systole during which blood is flowing out of the ventricle; the systole ends with the commencement of the second or smaller rise in the curve. Dr. Martius has been able to strengthen this analysis of the cardiac movements, so important both physiologically and pathologically, by observing that the duration of the rise and fall of the curve of systole varies in different individuals: thus he finds, conformably with the explanation given above, that in patients with low aortic blood-pressure, the rising portion of the curve of cardiac impulse is very short, while the falling part is considerably lengthened, resulting from the low aortic pressure allowing the semilunar valves to open sooner. On the other hand, in a case of arterial sclerosis, he found the rising part of the curve considerably lengthened, since the aortic blood-pressure was greater, and was only overcome at a later period of the systole.—Dr. Goldschneider presented and explained plates illustrating the topography of the sense of temperature. The sense of heat and cold was determined for the whole surface of the body, and arranged in a series corresponding to twelve degrees of intensity. As a general result, it was found that the sense of cold is more extended than that of heat; that both senses are more developed on the trunk than on the extremities; that the sense of temperature is less acute in the median line of the body; that the distribution of this sense over the surface of the body is quite different from that of the sense of touch; and that the points of exit of the nerves possess little or no sense of temperature.

July 15.—Prof. Munk, President, in the chair.—Dr. Jacobsen gave an account of some acoustical experiments which he has carried out with a view to determining the law according to which the amplitude of vibration of a tuning-fork diminishes as it gradually comes to rest. According to theory, the diminution in the amplitude of vibration takes place in geometrical progression; Hensen had, however, found that the logarithmic decrement at first diminishes, and then, when the vibrations have become extremely small, increases

again. The speaker has made experiments with tuning-forks, recording the vibrations of the arms by means of brushes writing on a rotating drum; in another series of experiments, which are not yet concluded, he has photographed the vibrations at equal intervals of time. The result of his work is that the vibrations diminish in geometrical progression, thus according with theory.—Dr. Wertheim gave an account of his experiments to determine the number of visual units in the central portions of the retina. In continuation of the experiments of Dr. Claude du Bois-Reymond, who has determined the number of visual units in the fovea centralis and found them equal in number to the cones, Dr. Wertheim, employing the same method, has determined the number of visual units to a distance of 2·5 millimetres from the centre. A sheet of tinfoil pierced with uniform holes was illuminated from behind, and then the distances were measured at which the holes began to be just visible as separate objects, as their image was made to fall on parts of the retina *successively* further and further towards the periphery. After having found in the fovea centralis the same number of visual units as had Dr. du Dois-Reymond, he then observed that their number decreases rapidly towards the periphery up to a distance of 1·5 millimetres, then remains constant for a short space, then diminishes again rapidly, and then gradually as far as the limits of the retinal area which he investigated. The speaker found that the first rapid decrease extends as far as the limits of the macula lutea. The anatomical statements respecting the limits of the yellow-spot and the number of cones outside this area did not permit of his drawing any conclusion, other than the above, from the optical experiments. The same numbers were obtained when red and green light was used.—Dr. Goldschneider has carried out a series of experiments to test Leyden's theory that ataxy, when not of central origin, is caused by injuries to centripetal nerves. By passing strong electric currents through the first phalanx of one finger he anæsthetized the second and third phalanx, and then found that the movements of flexion and extension of the finger no longer gave a regular curve of rise and fall as traced by the tip of the finger: the movements executed by the finger were irregular, sometimes going beyond and sometimes falling short of the desired extent. The sensation of passive movement was also considerably lessened. The speaker hence concluded that the ataxic movements are caused by the interference with the sensations arising from passive movements of the limbs. He added to this an hypothesis as to the nature of ataxy and the seat of the muscular sense in the limbs.

July 27.—Prof. Munk, President, in the chair.—Dr. Sandmann spoke on respiratory reflexes originating in the nasal mucous membrane. In order to study the possible connexion between asthma and diseases of the nose, which has been so often supposed to exist, the speaker has made experiments on the respiration in rabbits and cats whose nasal openings had been completely occluded. In addition to confirming the phenomena which had been already described by earlier observers, he found that the changes in volume of the thorax were the same as in normal animals, whereas the intra-thoracic pressure was considerably increased when breathing was carried on entirely by the mouth; similarly the respiratory undulations of the blood-pressure tracing were increased in amplitude. He next investigated more closely the respiratory reflexes which originate in the nasal mucous membrane; of these three are known—namely, inhibition of respiration, sneezing, and coughing, as a result of stimulation of the nose. Inhibition of respiration was observed to occur, according to the strength of the stimulation, either in the phase of expiration, or of inspiration, or merely as a more pronounced expiration. Sneezing was brought about by tickling the nasal mucous membrane, and was found to consist of a deep inspiration with simultaneous closing up of the pharynx and mouth by the application of the tongue to the palate, followed by an explosive expiration. When the stimulation is slight, only the deep inspiration is produced; if the stimulation is strong, the deep inspiration is followed by a somewhat lengthy inhibition of the same, which is frequently accompanied by slight expiratory movements; when the stimulation is of moderate strength an ordinary sneeze is the result. After section of the phrenic nerves the deep inspirations were no longer observed. Dr. Sandmann, by section and removal of the mucous membrane in rabbits, has further examined the various regional areas of the same, and found that sneezing can only be produced by tickling a limited area of the mucous membrane. On the rabbit this area is found in the entrance to the nose on the anterior surface of the lowest nasal muscle; but in addition to this place, the same reflexes may be produced by stimulation of the front part of the septum and roof of the nasal cavity. Sneezing cannot be produced by stimulation of any other portion of the nasal mucous membrane. In man the region of the posterior nasal openings is also connected with the reflexes involved in sneezing in addition to the regions mentioned above. An anatomical investigation of the areas whose stimulation leads to sneezing showed that they are supplied entirely by the ethmoid nerve. Stimulation of this nerve in the orbit was followed regularly by sneezing, which could therefore be produced to a certainty by stimulating the trunk of the nerve. The third kind of respiratory reflex—namely, coughing as a result of nasal stimulation—could not be experimentally produced in the cats and rabbits used in these experiments.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

The Distribution of Rain over the British Isles, 1876: G. J. Symons (Stanford).—First Lessons in Science: Dr. J. W. Colenso (Ridgway).—A Treatise on the Principle of Sufficient Reason: Mrs. P. F. Fitzgerald (Laurie).—Prolegomeni di Filosofia Elementare, Terza Edizione (Torino).—Bulletin de l'Académie Royale des Sciences de Belgique, No. 6, 1887 (Bruxelles).—Journal of the Royal Microscopical Society, August (Williams and Norgate).—Bulletin of the California Academy of Sciences, vol. ii. No. 6.—Boletin de la Academia Nacional de Ciencias en Cordoba, Junio 1886 (Buenos Aires).—Journal of the Anthropological Institute, May and August 1887 (Trübner).

CONTENTS.

	PAGE
The Physiology of Plants	361
A Dictionary of Philosophy	362
Our Book Shelf :—	
Mackenzie: "Hay Fever and Paroxysmal Sneezing"	363
Cohen: "The Owens College Course of Practical Organic Chemistry"	363
"My Microscope"	364
Letters to the Editor :—	
Sun and Fire Symbolism.—Mrs. J. C. Murray-Aynsley. (*Illustrated*)	364
Bishop's Ring.—The Sky-coloured Clouds.—T. W. Backhouse	365
The Electricity of the Contact of Gases with Liquids.—J. Enright	365
Newton's Laws of Motion.—W.	366
On the Constant P in Observations of Terrestrial Magnetism.—Wm. Harkness	366
The Stature of the Human Race.—Wm. F. Stanley	366
A Spider allowing for the Force of Gravity.—Major C. B. Lyster	366
The Lunar Eclipse of August 3.—H. H. (*Illustrated*)	367
Botany of San Domingo. By W. T. Thiselton Dyer, C.M.G., F.R.S.; Baron Eggers	367
Constitutional Formulæ and the Progress of Organic Chemistry	368
The Yale College Measurement of the Pleiades. By A. M. Clerke	372
Notes	373
Our Astronomical Column :—	
New Variable of the Algol Type	376
Astronomical Phenomena for the Week 1887 August 21–27	377
Geographical Notes	377
The British Association	377
The Japan Earthquake of January 15, 1887. By Prof. S. Sekiya	379
Minerals at the American Exhibition	381
The Folk-Lore of Ceylon Birds	381
University and Educational Intelligence	382
Societies and Academies	382
Books, Pamphlets, and Serials Received	384

THURSDAY, SEPTEMBER 8, 1887.

HYDROPHOBIA.

THE culmination of scientific knowledge in any special direction frequently appears to the casual observer as a sudden and unforeseen event, although actually the result of a combination of well-ascertained facts accumulated during many years. The introduction of rabies from among the inexact to among the more exactly known diseases has been however so rapid as to very fairly substantiate this popular belief. It is now scarcely more than three years ago since the self-sacrificing labours of M. Pasteur helped us to pass from superstition to accurate knowledge of the real nature of rabies or hydrophobia, and this passage from the pre-scientific to the scientific epoch of the subject was actually perfectly abrupt. Nearly the same thing may also be said of the discovery by Koch of the *Bacillus tuberculosis*, for in both cases the scientific grasp of the subject undoubtedly commenced with the discovery how the virus might be isolated for purposes of experiment. While, however, this is strictly the case with tuberculosis, it is but partially true for rabies, for though there cannot be a shadow of doubt that the micro-organism which is the virus of rabies will soon be demonstrated in pure cultivations, this one factor is yet wanting to place it on the same level as that of tuberculosis. This difficulty in obtaining pure cultures, though a serious defect in our information, is yet most interesting, for it affords a distinct interpretation of several facts in the etiology of the disease, which, as we shall see directly, proved obstacles to early inquirers, and which yet at the same time, when viewed in the present light of science, are most encouraging to those who are anxious to see this miserable evil extinguished for ever.

All observers, notably Mr. Dowdeswell, are agreed that a micrococcus can be demonstrated in the tissues of the spinal cord of animals affected with the disease, but unless we accept the doubtful results of Fol no one has yet succeeded in cultivating this micrococcus. Those familiar with the difficulties of "rearing" pathogenic organisms will readily understand this obstacle in a disease with such a long incubation period as rabies.

M. Pasteur at the outset of his investigations attempted to solve the problem in this direction, but fortunately for science soon abandoned it in view of the probability that the virus would best be dealt with by endeavouring to obtain it in quantity from the central nervous system, since from the symptoms it evidently there produced its greatest effect, and so might be expected to be more especially present. He therefore made an emulsion by crushing in sterilized *bouillon* portions of the central nervous system, specially the spinal cord. With this emulsion he inoculated the disease from animal to animal by injecting a small quantity of it beneath the dura mater. By this simple procedure he established the first of his most important discoveries, viz. the real incubation period of the disease. At the same time too, as is usual in instances of a genuine scientific advance, the one important discovery led to a further one, since he has thus presented science with an infallible means of determining whether an animal had really suffered from the disease or not.

To the public this test has already been of the utmost value, since, as is well known, the characteristic lesions in the alimentary canal, &c., being absent or but feebly marked in the early stages of the disease, the possibility of thus giving a definite opinion in cases of doubt by the aid of Pasteur's method has frequently been the means of affording the utmost relief to the minds of those who have been victims to the lingering dread of hydrophobia. We are not aware however that the slightest public expression of gratitude has ever been expressed in this country towards that experimental science which in M. Pasteur's hands has led to such an important result, or that we, who by reason of rabies being endemic among us are profiting, and will profit, enormously by the light thus shed on the subject, have acknowledged our indebtedness to him in any way.

From his researches M. Pasteur was led on to formulate certain deductions which might be accepted as the logical consequence of the theory of the disease thus shown by him to be indubitably zymotic.

In the first place, M. Pasteur considered it probable that he could attenuate the virus which he had just discovered the possibility of handling with certainty, and we may add safety. This he soon accomplished by the method of drying.

It might naturally be supposed that, having proceeded thus far, he would have been led to attempt protection from, and prevention of, the evil effects of the disease by inoculating with this attenuated virus. Indeed it is unfortunately the popular belief that he did do this, and that his efforts to cure the malady are conducted upon this plan.

As a matter of fact, however, M. Pasteur, with a much wider prescience of the facts and theories of zymotic disease in general, considered that the well-known arrest of development which happens to virulent organisms as a consequence of their growth in tissues or in artificial culture media was due to the production by their metabolic processes of katalyzed substances whose presence was inimical to the active growth of the microbe, and that therefore these substances might be regarded and used as antidotes to the virus. Acting on this assumption, he proceeded to endeavour to protect animals by injecting into them considerable quantities, not of the virus, as generally supposed, but of these antidotal substances, or possibly of one alone. Of course, knowing that as a rule attempts to isolate in a state of chemical purity such products usually failed or seriously altered them, and having already ascertained that the baleful influence of the virus could be abrogated or rendered nugatory by the process of drying, he proceeded to employ the simple method of injection emulsions of dried spinal cords. The injection of such emulsions was naturally performed understanding that the drying would probably also impair to a certain extent the protective value of the antidotal substance; to insure, therefore, the introduction into the animal of as large a quantity as possible he considered it advisable to inject emulsions on successive days from spinal cords which had been dried for shorter and shorter periods, till ultimately, having commenced by inoculating from cords which had been dried for fourteen days, and which had been proved to be perfectly innocuous, he arrived at a

cord which had been dried for only twenty-four hours. Of course the introduction of such virulent matter as this had no effect on systems already armed by the previous inoculations of antidotal substance. It is greatly to be regretted that owing to general ignorance of this fundamental principle of M. Pasteur's method the many and violent discussions upon his treatment have been rendered absolutely useless. We trust, therefore, we may be excused for having dwelt thus at length upon it.

As soon as M. Pasteur had demonstrated to the French Academy that he could secure the *protection* of dogs by injecting them in the manner above described, he was induced to attempt the *prevention* of the disease in man by similar injections after the bite. This attempt soon developed into a regular practice, from the numbers of patients who flocked to Paris seeking treatment in consequence of the unhappy prevalence of rabies in those European countries which had neglected to provide for its easy extermination by suitable legislation.

From the Report of the Committee commissioned by the Local Government Board to inquire into M. Pasteur's treatment, it would appear, as might have been expected, that a comparatively large proportion of M. Pasteur's patients were bitten by dogs which were not rabid; but it is also evident from the same Report that when deductions have been made for these cases the death-rate among the remainder was far lower than even the lowest estimate ever formed of the mortality from hydrophobia among persons bitten by reputedly rabid dogs.

While this gratifying result was accomplished, it was at the same time evident that the method was by no means perfected, and the lamented death of Lord Doneraile (from the bite of a tame fox which had been infected by a dog), affords an illustration of this, for the deceased nobleman was subjected to treatment within a few days after receiving the virus. But while the inoculations did not prevent a fatal issue, there seems good reason to believe that they notably modified the distressing features of the malady, for in a brief account before us it is stated that the inability to swallow fluids only appeared twenty-four hours before death, and there was at no time spasm produced by swallowing moist solid food. The same gratifying modification appears to have also been present in another case that recently was observed in St. George's Hospital. Should this modification prove to be general, M. Pasteur will have deprived the malady of its worst tortures.

Before leaving the consideration of this part of the subject it is to be noted that the virulent opposition with which M. Pasteur's efforts in the cause of humanity were met went to the length of charging him with having actually caused the death of some of his patients by his inoculations. This charge, though not supported by any exact evidence whatever, was also inquired into by the above-mentioned Committee appointed by the Local Government Board. Indeed, they had a special opportunity of doing so, for one of the laboratory servants of Mr. Horsley (who carried out the experiments for the Committee), being bitten most severely by a rabid cat, died six weeks later (the usual incubation period of the disease) with the paralytic form of hydrophobia rather than the excitable form. Rabbits inoculated from the spinal cord of this man died with the shortest possible period of incubation. As the Committee point out, this would seem to have lent colour to the idea that the inoculations themselves were fatally virulent, had not similar instances of short incubation periods been occasionally observed to follow inoculation from similarly rabid animals. Stress was also laid upon the mode in which the man was inoculated—namely, by what M. Pasteur called the intensive treatment, and which he adopted in cases of very severe injury. But the whole question was dismissed by M. Pasteur altering this mode of treatment in order that there should not even be the semblance of the possibility of such an accident.

It seems to us that, in England at any rate, there is quite another view to be taken of this question of rabies and its scientific prevention—in fact, that its complete extermination should be ensured in preference to efforts to treat it after it has attacked anyone.

The data upon which legislation should be based are now fortunately at hand. The House of Lords recently appointed a Select Committee, under the chairmanship of Lord Cranbrook, the President of the Privy Council, to inquire into the whole question of the social bearing of the disease, and the means which have been adopted to get rid of it in foreign countries. The Report of that Committee is published, and we have been permitted in addition to inspect the evidence laid before it. This evidence is a most instructive comment upon the manner in which the facts of modern science are sometimes treated as being of only equal value with the most absurd misstatements dictated by charlatanism and abandoned self-interest.

As a whole, however, the Report is one with which we have good reason to be satisfied in many ways, for it recognizes the great value of the simplest means of preventing the spread of the disease, viz. the muzzle.

Those of us who remember the senseless anti-vivisectionist opposition which met the police edict enforcing this salutary measure in London, will not be surprised of course to find in the evidence before the Committee the same thing repeated, but, as was inevitably the case, deprived this time of all its deceptive influence.

For the experience of the working of the muzzle in London, where it brought the number of deaths from hydrophobia down from 27 in 1885 to 0 in the last quarter of 1886, and indeed we believe we may also say the first six months of 1887; the experience of its working in Nottingham, where the cases of rabies varied directly in number according to the way in which the muzzle regulations were enforced; of Prussia, where the disease is almost extinct, being one-fiftieth part of that in Great Britain; of Scandinavia, where it is absolutely extinct—all disproved the baseless theories and assertions of those who, under the guise of pseudo-zoophilism, endeavour to perpetuate in man and the lower animals the torments of this horrible disease. As will doubtless have been already surmised, the whole of this factious opposition to the above-mentioned beneficent legislation came from the small clique of anti-vivisectionists who were unhappily represented on the Committee itself in two of its members, viz. Lords Mount-Temple and Onslow.

The Lords' recommendation of the muzzle, however, is marred by one defect, and that a very serious one.

Here is a disease, a zymotic disease, the virus of which, as we have just seen, is most difficult of isolation, and evidently easily destroyed by ordinary conditions when it has left living tissues; a disease, too, which is fortunately infrequent compared to many others, and again fortunately one which would become extinct if not kept in existence by transmission from dog to dog; a disease, in short, which needs nothing but the firm administration of the most ordinary rules of preventive medicine to be destroyed utterly,—and yet, in deference to the professional agitation before mentioned, the Lords' Committee only recommend the use of the muzzle when the disease is "prevalent." If this means, and it is capable of being interpreted in two ways, that the Lords' Committee think the muzzle should be applied only when the disease is epidemic, nothing more regrettable can be imagined. To the scientific mind it seems almost incredible that a legislative body should hesitate to grasp the opportunity, the easiest ever offered, of eradicating a disease so painful and utterly incurable when once the symptoms have declared themselves; but here unfortunately is the example we referred to above of scientific fact overridden by vulgar prejudice. For this disease, acknowledged by all who have studied it to be transmitted solely by inoculation of one animal by another, is endemic in Great Britain, is paramount in the manufacturing districts and great cities; and yet the Lords' Report, instead of recommending the universal application of the muzzle, which would abolish the evil from these its centres, is content to leave it to the local authority—Heaven save the mark!—to apply the remedy when the disease has already made sufficient havoc (!) as to call for its suppression. It is sad, too, to see that this view, which we must call narrow, runs through the whole Report, but it is gratifying to find that that Committee, at any rate, fully appreciates the high worth of M. Pasteur's invaluable test of the disease.

In conclusion the Lords say that, should M. Pasteur's method of treating the disease be found of value, provision should be made for its introduction into England. While heartily concurring in this recommendation, we cannot but feel grieved that the necessity for it in England should be permitted to exist; for in this country, like Scandinavia, the introduction of the disease can be prevented; so that if proper measures were taken England would enjoy the same complete immunity from it that Sweden does at the present day. With reference to the adoption of M. Pasteur's mode of treatment into this country, a most fundamental difficulty arises at the outset, viz. that we have no public laboratory where investigations of this kind could be carried on for the nation, and that therefore an institution of the kind would have to be established for this and kindred subjects of inquiry. At the present time there is, unfortunately, little hope that this want—which we have before so frequently pointed out is nothing short of a national disgrace—will be adequately met; and, as a matter of fact, questions of this sort are usually decided at the Brown Institution, the nation being thus lamentably dependent upon the assistance of a private charity.

Oddly enough this necessity has just been provided for in France by the institution of a *quasi*-private laboratory—the Pasteur Institute. We are, however, strongly of the opinion that questions of this kind are of an Imperial character, and as such should be dealt with by the central Government in a properly-fitted institution.

Hydrophobia from time immemorial has been the most dreaded of all diseases, and justly; but no doubt this dread has been intensified by ignorance of its causation, an ignorance which, having existed for more than 2000 years, has just been dissipated to an enormous extent by the scientific labours of M. Pasteur. This advance is, of course, a source of vexation to the misanthropic anti-vivisectionists, who are shamefully endeavouring to bolster up the exploded theory of spontaneous generation in order to hamper the efforts of preventive medicine to stamp out the disease, regardless also of the evidence from countries where it has been so rooted out, and where, owing to its importation being prevented, it has never appeared again.

And while we have made this great step forward in our knowledge of the nature and etiology of the disease, we have at the same time learnt, thanks again to M. Pasteur, how to protect animals from its ravages, how to prove or disprove its existence in the absence of clinical or anatomical evidence, and, although this is still *sub judice*, apparently how its fatal effects may be warded off in the human being, and, if not successfully prevented, possibly ameliorated.

Finally, a most satisfactory outcome of this increase in our scientific knowledge is the revelation to us that by the adoption of certain legislative means we may destroy the evil once and for all.

POPULAR BOOKS ON BIRDS.

Ocean Birds. By J. F. Green. With a Preface by A. G. Guillemard, and a Treatise on Skinning Birds, by F. H. H. Guillemard, M.D. With Illustrations by Frances E. Green. 4to, pp. viii.-93. (London: R. H. Porter, 1887.)

Bird Life in England. By Edwin Lester Arnold. 8vo. (London: Chatto and Windus, 1887.)

TWO books deserving the above title are before us. It is well known that some of the most interesting works on ornithology have been written by men who do not profess to be scientific naturalists, but who exhibit an intelligent acquaintance with their subject and also possess a faculty of description the want of which adds so much to the dullness and heaviness of style with many more ambitious writers. Anyone who has made an ocean voyage knows full well that the hours are often apt to hang heavy on the hands of the passengers; and if this is true on board a steamer, it is much more true in the case of a sailing-vessel. Mr. Green therefore has compiled a volume which aims at giving assistance to voyagers in the southern oceans, providing short descriptions of the species of sea-birds most commonly met with; and as the author has travelled much by sea himself, it may be taken for granted that he knows the wants of an ornithological inquirer on board a vessel, and has done his best to supply the information. A "Glossary of Terms," and a chapter on the preparation of bird-skins, have been furnished by the author's friend Dr. Guillemard, whose excellent account of the voyage of the *Marchesa* is one of

the most readable of modern books of travel. The work is divided into three parts, the first treating of the petrels, the second of the frigate-birds, gannets, and tropic-birds, and the third of the gulls and terns. Mr. Green has given a very correct account of all the best-known species belonging to these groups, and for a second edition he may find a few useful notes on some of his marine friends in the volume published by the Royal Society on the Transit of Venus Expedition to Kerguelen Island. One at least of the notes here published is given by Mr. Green, but only as an extract from our volume of "Aves" in "Cassell's Natural History." The illustrations which accompany the work may be sufficient to identify the various species represented, especially the albatrosses, but they are rather roughly done, and that of the flying petrel is nothing but a caricature. No figures taken from mounted birds are ever satisfactory, and Miss Green's illustrations are no exception to the rule.

Mr. Arnold's little work will rank with any that we know of for pleasant reading, either from a sportsman's or an ornithologist's point of view. Some of the descriptions of game and wild-fowl shooting are exceptionally good, and carry with them a scent of the moor and the sea. Despite an acquaintance with several standard works on birds, the author seems to cling with respect to some of the more pretentious but second-rate books which pass muster as histories of British birds. It is, however, somewhat of a treat to find His Royal and Serene Highness the Prince of Mantua and Montferrat (!) spoken of under his original title of Groom Napier, though we should never call him a "first-class" authority. Many well-known names are wrongly spelt throughout the book, and these shortcomings should be corrected in a subsequent edition, when we should also like to see that Seebohm's excellent "History of British Birds" has come under the author's ken. It is to works on natural history like Mr. Arnold's, where real instruction is conveyed in elegant English, so that the acquisition of knowledge is rendered pleasant and easy, that we owe so much of the interest which has of late years been awakened in scientific pursuits; and we should be captious indeed were we to point out small errors in a book the perusal of which has given us so much enjoyment. Not the least useful feature of the work is a chapter by Mr. Brodie Innes on "Grouse Moors and Deer-Forests."

Should the works under review pass into a second edition, we should be glad to point out to the authors certain emendations which have occurred to us, of too little moment, perhaps, to mention in a review, but which would add somewhat to the finish of the volumes.

R. BOWDLER SHARPE.

OUR BOOK SHELF.

First Lessons in Science; designed for the use of Children. By the Right Rev. J. W. Colenso, D.D. (London: Ridgway, 1887.)

THIS book was written more than a quarter of a century ago for the use of a class of natives of the diocese of Natal, who were learning to read English. Since then the greater part of it has been rewritten in order to adapt it to the necessities of European children. As far as possible the earlier lessons are written in words of one syllable, so that they are well fitted for the use of those for whom they are intended.

The object of the work is to furnish the readers with useful information concerning the things around them, in place of the usual childish stories contained in the first books of English; at the same time presenting only such facts as ought, according to the good Bishop, and we quite agree with him, to be known by everyone. We venture to think that in this respect the native students under Bishop Colenso's care were much better off, having these lessons in their possession, than the boys and girls of our own schools who were learning English at the same time.

By far the greater part of the book is devoted to astronomy, to which subject it forms really an admirable introduction. This of course necessitates the introduction and explanation of many geometrical and optical terms, all of which are put forth in the best possible way. The physical features, and orbital and apparent motions of all the members of our system, including comets and meteorites, are fully considered, as are also the apparent motions of the stars.

The reasons are also given why the observed place of a heavenly body should be corrected for refraction, parallax, aberration, precession, and nutation. Kepler's laws and the law of gravitation also come in for a fair share of attention.

Some of the figures should be brought up to date. We are told that the earth is 96,000,000 miles from the sun, and that between forty and fifty minor planets are known; whereas the distance of the sun is between 92,000,000 and 93,000,000 miles, and no less than 268 minor planets are now on our lists.

It is to be regretted that books of this kind, written in clear, simple language, are not more appreciated by those responsible for the selection of reading-books for our elementary schools.

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.*]

On the Constant P in Observations of Terrestrial Magnetism.

IN regard to the quantity P, depending on the distribution of magnetism in a pair of magnets employed for measuring terrestrial horizontal force, for the calculation of which Prof. Harkness, in NATURE for August 18, p. 366, gives a simplified expression, may I be allowed to mention that a yet more simple modification of the ordinary formula has been used in the Greenwich observations since the year 1878, in which, the difference between A and A_1 being small,

$$P = (\log A - \log A_1) \times \frac{r_1^2 r^2}{(r_1^2 - r^2)\ \text{modulus}}.$$

When the two distances employed are, as is usual, always the same, the factor becomes a constant, being, for $r = 1{\cdot}0$ foot and $r_1 = 1{\cdot}3$ foot, $= 5{\cdot}64$. The advantage of the form is that as, in the calculation, the logarithms of A and A_1 are first arrived at, their difference multiplied by $5{\cdot}64$ at once gives P. Mention of this is made in the introduction to the Greenwich Magnetical Observations for 1878, and in those for some following years, although omitted from the more recent volumes.

WILLIAM ELLIS.

Royal Observatory, Greenwich, August 24.

The Svastika on English Walls.—The Solar Eclipse of August 19.

I GREATLY fear that practical builders will be uncourteous enough to smile at Mrs. Murray-Aynsley's idea (NATURE, August 18, p. 364) that the S-shaped iron bars seen on the walls of houses are fire-emblems or survivals of sun-worship. They are common enough in every county of England and elsewhere; in fact, wherever the scamping of jerry builders or the lapse of time has caused walls to give way or bulge outwards. The bolt in the centre is not merely to hang them up, but is the end of a long and strong iron bar passing right through the building and attached to a similar curved brace on the other side, or at any rate fixed to some firm unyielding part of the masonry. The curved shape is simply chosen as that which embraces and gives support to the greatest area of brick or stone surface without the necessity of having a solid, continuous plate.

A propos of sun-worship, it is sad to reflect how much good a little of it might have done in inducing that august but capricious luminary to show himself to the thousands who looked in vain for him on the morning of the recent eclipse. He seems to have shone in splendour in longitudes east of the Urals, where his worshippers abound, but to have hid himself in anger from nearly the whole of unbelieving, scientific Europe.

At Twer, between St. Petersburg and Moscow, where I was myself, the early dawn was beautifully clear; but first a dense ground-mist enveloped us, and then, when enough wind sprang up to clear this away and give us a glimpse of the sun about six-tenths eclipsed, a heavy bank of rain clouds came up and put an end to all hopes of observation. The commencement of totality was pretty well marked by a sudden intense gloom, not, however, greater than (if even as great as) a London fog.

At Berlin placards were extensively posted up a little later in the day stating that "in consequence of the unfavourable weather the eclipse was postponed until the next day." This might have been believed in France or Ireland, but it is harder to take in the Teuton than the Celt. H. G. MADAN.

Eton College.

Large Meteors.

A PEAR-SHAPED fireball, rivalling Venus in brilliancy, passed over Cardigan and Radnor in Wales on August 21 at 11h. 2m. It was observed by Mr. D. Booth at Leeds, and by the writer at Bristol; but the two paths, though likely to be very accurate as regards the *direction* of flight, are somewhat discordant in the beginning and end points. The radiant of the fireball was at 264° + 61° in Draco, and agrees with the two following showers:—

1871 August 20–25	...	...	264 + 64 Tupman.
1887 August 14–23	...	...	264 + 62 Denning.

The meteor referred to appears to have been observed at Bristol much earlier in its track and when considerably higher in the atmosphere than when noticed at Leeds. The mean of the two places gives a height of 80 miles over a point 6 miles east of Aberystwith to 45 miles above a place 7 miles west of Rhayadergwy. The earth point was near Hay, Herefordshire.

It would be important to hear if this fine meteor was observed at any stations in the Midlands, in Wales, or on the south-east coast of Ireland. As seen from Leeds it passed through *Scutum Sobieski*, and at Bristol close to the star ι *Draconis*.

Another fine meteor about equal to Venus was observed here on August 30 at 14h. 25m. It left a bright streak in its path of 18° from 19° + 27° to 5° + 14°. Radiant at 46° + 43° near β Persei. W. F. DENNING.

Bishopston, Bristol, August 31.

Colliery Explosions and Atmospheric Pressure.

THERE are few questions so much in need of a satisfactory solution as the relationship which exists between colliery explosions and changes of atmospheric pressure. Before anything was known of the weight, and variations in the weight, of the air, before the barometer was discovered, miners had learned to connect the state of their working-places with weather changes. The old pits were very shallow, the workings very limited, and the ventilation practically left to take care of itself, so that it is not difficult for us to understand the effect of temperature rather than pressure on the atmosphere of the mine. "Trefoil damp," "pease bloom damp," &c., sufficiently indicate the summer prevalence of the danger; in winter "the damps were scarcely felt or heard of." In the early part of the present century Mr. John Buddle, the Newcastle viewer, having watched his barometer and the mining reports became strongly of opinion that "accidents from fire-damp always occur with a low barometer." Faraday and Lyell's Report on the Haswell disaster of 1844 dwelt upon the importance of officials taking into account the variations of the barometer in the management of mines. Since then numerous public Commissions and private inquirers, English and foreign, have investigated the connexion supposed to exist between the exudation of gas and a *falling* barometer. The earlier decisions may be said to favour Mr. Buddle's opinion, but of late years there appears to be a tendency to declare that the effect of a low or falling barometer has been considerably over-rated—that in reality it has little or no influence. Under whatever conditions of pressure explosions formerly occurred, it is perfectly clear from the experience of recent years that disasters take place, as a rule, when there is an excess and not when there is a deficiency of pressure.

Mr. Dobson's Report to the British Association in 1855 showed from a large, though imperfect, number of observations that up to the year 1854 accidents from fire-damp were most frequent in the summer months June and July, the minimum at the end of January; the results being taken to prove indisputably the general dependence of explosions upon the seasons of the year.

In the papers communicated to the Royal and to the Meteorological Societies between 1872 and 1874 by Messrs. Scott and Galloway, it was however shown from 1369 accidents in twenty consecutive years that the maximum occurred at the end of January, the minimum in the middle of September.

These very different results may be regarded as indicating the great revolution which has taken place not only in the time at which explosions occur, but also in the conditions of mining operations. Pits are now of enormous depths, with most extensive galleries, and the ventilating appliances are of the most elaborate description. Possibly these changes have modified very greatly the effect of weather variations. It must be remembered that gas exists in mines under two quite distinct conditions, that in the goaves and waste places being free and in direct contact with the air, while the gas occluded in the solid coal or imprisoned in faults is not in direct contact with the atmosphere. In the former case it is generally agreed that the accumulations of gas expand or contract with the changes of atmospheric pressure. In the latter case we know that the gas exists in the coal at a pressure of many atmospheres, so that it is highly improbable that it is affected directly by the rise and fall of the barometer. Indirectly, however, it would seem that a very important effect results, but in direct opposition to the idea that it escapes only with a falling barometer.

Serious explosions are almost exclusively confined to deep mines, where the management is perfect, and where every care is taken to insure safety. Mystery surrounds each disaster, and it is left to individuals to trace them to coal-dust, gas, or some other favourite theory. Fortunately the illiterate manager has given way to a different order of men, and from the interest taken by mining engineers there is reason to believe that much of the uncertainty which at present envelops the question will be removed before long. Barometers are now common to all mines, and they are studied with more or less interest by the officials. For years past it has become clear to them that there is no apparent connexion between the escape of gas and a falling barometer: the firemen "in ordinary cases can forestall the barometer by from twelve to twenty-four hours." This conclusion, based upon the ordinary observations of officials during their daily routine of duty, has been confirmed by more precise and carefully-planned systems of collecting information.

Following the Seaham disaster of September 1880 (when the centre of an anticyclone was over the northern counties), Mr. Corbett arranged hourly observations, day and night, for several months, showing the atmospheric pressure, the measurements of gas which had escaped into the workings, and by means of water-gauges the movements of the gas in parts of the workings sealed from contact with the air. The water-gauges indicated an out-bye pressure as much as 33, 35, 41, and 48 hours before the barometers began to fall, while gas in measurable quantities was to be found many hours before the mercury gave signs of falling. On the Continent somewhat similar observations have

been made at Saarbrück and Karwin. The Austrian inquiry showed that "where after a rapid rise of the barometer it continued to rise slightly, or remained stationary for some time at its maximum, a gradual increase in the volume of gas in the air would set in; or if, after a rapid fall in the barometer, it continued to fall gradually, or remained stationary at its lowest point, a decrease in the quantity of gas would become apparent."

Evidently, therefore, from these researches the greatest danger is not, as a rule, to be apprehended when the barometer is low or falling, and this is supported by actual disasters, the majority taking place under anticyclonic conditions of pressure. While Mardy, Pendlebury, Penygraig, Seaham, and many others add to the verdict, it will suffice to deal with some explosions of the present year, and see if they do not bring home to us a new view of the natural forces at work far down below the surface of the earth.

From the simultaneous observations made at 6 p.m. on Friday, February 18, the Meteorological Office reported:—"The barometer is now rising in all parts of the United Kingdom, and an anticyclone is apparently advancing from the westward." An hour later thirty-nine lives were lost in an explosion in the Rhondda Valley. The anticyclone continued on its course to the Continent, and by the morning of Wednesday, February 23, when so much damage was wrought by the earthquake, the centre had reached Southern Europe. On March 1 the anticyclone was a little further north, and over the neighbourhood of the Chatelus Mine, near St. Etienne, where ninety lives were sacrificed. Still moving northward, the night of March 4–5 found the highest barometer readings over Belgium and the Netherlands, when 144 miners perished at Quaregnon, near Mons.

In the last week of May another anticyclone moving from south to north was marked by the loss of one life at Darcy Lever on the 25th, three lives near Wigan on the 26th, and seventy lives at Udston, near Glasgow, on the 28th.

An anticyclone over Western Germany on the night of June 7–8 marked about sixty deaths at Gelsenkirchen. As this area moved to the westward, a slight earthquake was felt near Strasburg on the 11th, and a severe one in La Vendée on the 15th.

Clearly Mr. Buddle's strong opinion is not applicable to the second half of the century. The knowledge that gas is found escaping with a rising barometer, and that so many explosions take place as indicated, has led mining officials to blame the mercury for not falling even before the gas begins to escape, their idea being that pressure has actually decreased, but that barometers are many hours before taking up the changes. The idea may be dismissed as an erroneous one. The cause must be sought for in another direction, not the direct action of variations of atmospheric pressure on the gas as it leaves the coal, but the effect on the earth's crust and indirectly on the occluded gas. Whatever be the true cause of earthquakes, there seems to be no reason to doubt that fluctuations of atmospheric pressure cause undulations of the earth's crust. Prof. Darwin, taking a probable estimate for the elasticity of rocks, has calculated that with a range of two inches of the barometer we are at least three or four inches nearer the earth's centre when the instrument stands very high than when it is very low, and concludes: "It may be that the incessant straining and unstraining of the earth's surface is partly the cause of earth-tremors, and we can at least understand that these strains may well play the part of the trigger for precipitating the explosion of the internal seismic forces." The seismological records of Japan show that earthquake shocks are twice as numerous under the predominant anticyclone of the winter months, as they are in the summer with lower pressure. As a result of the discussion of earthquakes in Jamaica, Mr. Maxwell Hall concludes that "at the time of an average earthquake shock the barometer is a little above its average height. This is due to the circumstance that the winter months, December, January, and February, when the barometer is above its monthly average, are more liable to shocks than other months of the year; and that the hours from 8 p.m. to 2 a.m., when the barometer is above its diurnal average, are similarly more liable to shocks than other hours of the day." Explosions of fire-damp follow a similar rule; they are most numerous in the winter months, when the range of pressure is greatest, and usually when the barometer is very high. Allowing for the flexure of the earth's surface, we can conceive that with the downward movement under increasing pressure the pent-up gases are forced into the workings of our deep mines; it may be indeed these movements cause infinitesimal fissures in the coal-seams through which the gas passes into the workings at a time when it has been customary to believe there was least danger. There is some degree of probability in this from the fact, so frequently noted in great explosions, that there is a suddenness in the appearance of the gas which is not a common experience in shallow workings.

Taking into consideration all the recorded facts, they point to the conclusion that far greater weight should be attached to a period of high atmospheric pressure than has hitherto been deemed necessary. In any future discussion of this important subject it is to be hoped further evidence will be forthcoming, and that instead of endeavouring to connect every disaster with a low barometer, the distribution of pressure as a whole be taken into account.

The influence of coal-dust upon explosions has not been touched upon, but it may be remarked that the dry atmosphere of an anticyclone renders the dust more inflammable than the dampness of a low-pressure system, so that there is a double reason for giving closer attention to mines under anticyclonic conditions.

HY. HARRIES.

MEASUREMENT OF SPECIFIC HEAT.

HAVING regard to the comparatively large experimental error introduced by thermometers into specific heat measurements, a null method appeared to be desirable. The following method occurred to me about two months ago, but not having access to a physical laboratory, I have not been able to practically test its accuracy.

Two exactly similar calorimeters (A and B) are taken, each containing a coil of thin Pt wire of resistance R, so arranged as to be completely immersed in the liquid. A contains a mass, M (including water equivalent), of water; B the same mass of substance the specific heat of which is being measured. The wires are arranged in bridge fashion, so that the ratio of the currents flowing through the two wires may be made to take any value.

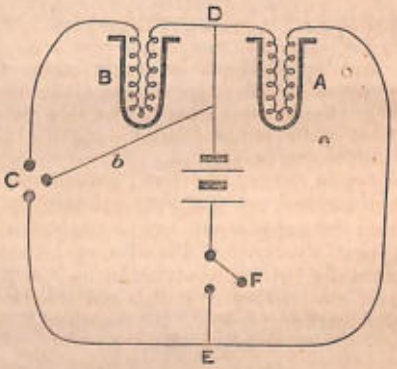


FIG. 1.

A differential thermometer (not indicated in the sketch, for sake of clearness) shows the least difference of temperature between A and B. Probably the most delicate and convenient arrangement is to use two thin Pt wires balanced in the arms of a bridge, using a very sensitive galvanometer.

First consider the calorimeter B containing the substance. It receives a quantity of heat, H, from a current, C, flowing through a resistance, R, for a time, t. Hence

$$H = \frac{C^2Rt}{J} = \theta MS$$

(where θ is the rise of temperature, and S the mean specific heat for that interval).

Similarly in A, containing water,

$$H^1 = \frac{C_1^2Rt}{J} = \theta M.$$

For by the conditions obtained θ is the same in both A and B. Hence $S = \left(\frac{C}{C_1}\right)^2 = \left(\frac{r_1}{r}\right)^2$, where r_1 and r are the resistances of the two circuits. It is obviously unnecessary to make the resistances, and the masses of liquids, equal, but the equation is thus simplified. If a smaller mass of water, m, be taken, then $S = \frac{m}{M} \cdot \left(\frac{r_1}{r}\right)^2$, thus increasing the delicacy of the method.

Since in the adjustments a considerable amount of time would be necessary to allow the calorimeters to attain thermal equilibrium after each trial, the following modification may prove more simple and more practical :—

The calorimeter B is arranged so that by a switch-key, C, the current can be diverted through a wire of exactly equal resistance, b, so that the current is the same by either path. The resistance from D to E is the same either way. The key F is pressed down for a time, t,

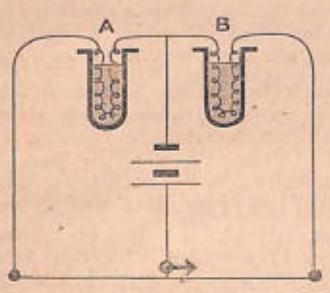


FIG. 2.

until the needle is largely deflected; then the current is switched from B and passed through A alone, until the needle is just brought back to zero, in total time, T. Then, neglecting for the present the slight error due to cooling, in A . . . $\theta M = \frac{C^2RT}{J}$, in B . . . $\theta MS = \frac{C^2Rt}{J}$, $\therefore S = \frac{t}{T}$.

Since T and t can both be made large, this should give very accurate results. It is evidently especially applicable to the measurement of the rate of increase of specific heat with temperature, since the liquids may have any initial temperature.

In conclusion, I may say that I should not have published this method in such an incomplete state, and unsupported by experiment, but I noticed to-day (September 5) that Profs. Stroud and Gee intend to read a paper before the British Association on "A Null Method in Electro-Calorimetry," and it is possible this may refer to a similar method.

GEORGE N. HUNTLY.

THE HESSIAN FLY.

I AM sorry to say that reports from correspondents acquainted with the attack of the Hessian fly show its presence now in an almost continuous line along the northern and eastern coast from Cromarty on the Moray Firth in Scotland down to Kent.

I have this morning received specimens of the *puparia* from the parish of Urquhart, in Morayshire, the most northerly locality from which I have at present received the so-called "flax-seeds."

The amount of presence varies very much. In the locality above mentioned (that is, the district from Aberdeen to Cromarty), the traces of attack are reported as to be found from 25 to 30 miles inland, but the injury slight, not more than one straw in fifty being affected, and the grain of fair quality. It is severe in some parts of Perthshire, and is found also in the eastern counties adjacent.

In East Lothian, Haddington, and Berwickshire attack is only reported from a few places at present, and in Northumberland from one locality.

Beginning again on the two sides of the Humber the attack widens much in area as it is traced south. It passes through Lincolnshire and Cambridgeshire, touching an easterly part of Northamptonshire, till it extends over the district commonly known as the eastern counties, including besides great attack in Hertfordshire, and some in Bedfordshire; and it also occurs in Kent.

In the southerly or westerly parts of England it occurs at Lymington and Petersfield in Hampshire, and to a considerable extent near the College of Agriculture, Downton, near Salisbury; and I have one report of it from near Bridgwater, and it also occurs at Goring Heath, Oxfordshire.

The above localities are where I know of its presence from specimens sent to myself, or, in a few cases, from information given me by correspondents whom I know to be acquainted with the appearance of the *puparium*, and the characteristics of the attack.

It very likely may occur elsewhere, but I am only just giving a general sketch of extent of infested area from personal knowledge.

It strikes me as a very curious point that the attack should so markedly cling to the sea-side, excepting in a few isolated instances, or where the inland area is continuous with the sea-side district.

It is very satisfactory to observe that although the season has been so altogether *extraordinarily* favourable to various kinds of insects affecting corn-stems, yet that in very many instances reported to me the injury caused to wheat by Hessian fly has been slight

On this fact I venture to think we may ground a hope that, either from the varieties of wheat which we use being kinds suited to do what is called "resist" attack, or from circumstances of our cultivation, we may find that our wheat at least does not suffer as much as in some other countries.

Also the enormous prevalence of the two stem attacks caused respectively by the corn sawfly (*Cephus pygmæus*), and by the dipterous fly, the *Chlorops tæniopus* (attacks which far exceed in amount any which have been brought under my notice as caused by these insects), give a hope that the climatal circumstances which usually prevail here will have an effect in checking the attack of the *Cecidomyia destructor*, as well as the above-named crop pests, as we see that all three kinds have been exceptionally thriving in the exceptional heat and drought.

It is unnecessary to point out to your highly informed and thinking readers that the statements now appearing of the *Cecidomyia destructor* having been a corn pest in this country for many years have not the slightest foundation.

ELEANOR A. ORMEROD.

THE BRITISH ASSOCIATION.

MANCHESTER, *Tuesday Evening*.

ABOUT the success of the Manchester meeting there seems to be only one opinion. In mere numbers—the most popular gauge of success—it has by several hundreds surpassed all former meetings; the number of tickets sold very closely approaches 4000. As a natural result, the amount of money collected and available for the purposes of research is unprecedentedly great, as will be seen by the list of grants which have been allotted to the various Committees. The great increase in attendance over all former years is to a considerable extent due to the large number of foreign visitors, who have formed a marked and prominent feature of the present meeting. In the proceedings of nearly every Section the representatives of foreign science have taken an active

part, with the result that the time of the whole meeting has been more intensely scientific than in the case of any previous meeting. This has been especially shown in the case of the important discussions which had been arranged for, and which most of them bore the character of real debate; the only exception, we believe, being the case of electrolysis, in Sections A and B, the "discussion" consisting mostly of the reading of a series of papers. Quite otherwise, however, was it with the discussions on heredity, introduced by Prof. Lankester, and on the cell theory, introduced by Prof. Schäfer, in Section A—discussions in which the subjects were threshed out very thoroughly. To some extent it is generally conceded that the great mixture of foreigners has to some extent solved the problem of an International Scientific Congress, which in any formal way is generally considered impracticable. Their presence here has certainly added a stimulating variety to the meeting, and the honour has been duly appreciated by the Corporation and citizens of Manchester. The foreigners have all been hospitably entertained as guests, and there have been not a few special entertainments got up for their special behoof. At the great dinner to be given to-morrow by the Mayor and Corporation nearly half of the guests will be foreigners. One of the pleasantest gatherings of the meeting was at a little dinner given on Sunday night by a few of the biologists to a select few of their foreign co-workers, especially botanists, at which De Bary delighted everybody present.

The number of papers read at this meeting has been quite comparable with its other exceptional features. Sections that have never split before have been compelled to split now. Biology, though it has thrown off Section H, has this year split into two sub-sections,—Botany and Physiology,—and there is even some fear, perhaps hope, that these divisions may become permanent. On Saturday every Section met except E, and to-morrow the majority will have to sit close up to the General Committee-meeting. Out of all this rush of papers no doubt some good comes, but most of those interested in the welfare of the Association admit that it would be well to moderate it, or perhaps still more completely to organize it. For one thing the custom of reading one paper in several Sections is greatly to be deprecated, and this year it has been carried still further than ever, greatly to the indignation of those Sections which had to submit to hearing the story retold. This came to a crisis in Section E, where an eminent geologist, who condescended to read a paper to some extent already given to his own Section, was told in almost so many words that Section E had no time to listen to geological lectures. Here indeed the battle between the geologists and geographers was fought out, greatly it was thought to the discomfiture of the former, who are loth to think that there is anything worthy of the name of geography outside of their own lines.

In spite of the persistently unfavourable weather, the public lectures have been quite successful. The lecture on the rate of explosions by Prof. H. B. Dixon kept a large audience intensely interested from beginning to end; and nothing could be more striking and instructive than his experiments, some of which were on a gigantic scale. Equally attractive was the lecture to working men on Saturday evening on electricity by Prof. George Forbes. The biggest audience of any, however, assembled in the Free Trade Hall on Monday evening to listen to Sir Francis De Winton's lecture on exploration in Central Africa. The audience was evidently a popular one, and the lecturer had the warmest reception. Unfortunately, the lantern used to show maps and pictures on the screen was rather a failure. Distinctly popular as it was, probably even the specialists were glad to get a convenient summary of recent work in Central Africa, pleasantly conveyed.

The address of the President of the Association, Sir Henry Roscoe, was very numerously attended. Sir Henry was evidently audible all over the place, and his reception, as might have been expected, was enthusiastic.

Manchester is rather badly off for excursion places, and on Saturday, we believe, quite as many people spent the day in Manchester as elsewhere. Indeed, most of the Sections were so busy with work that they had no time to think of play. The little dredging excursion was joined in by about fifty men, who all seemed highly satisfied with the results, in spite of the weather. One of the most popular of the coming excursions will be that to the Isle of Man from Friday to Tuesday, under the guidance of Prof. Boyd Dawkins. There is also some talk of an excursion to the Lakes, but the weather does not encourage holiday enterprise in so notoriously rainy a region.

One popular and distinctly useful feature in connexion with the present meeting has been the Anthropometric Laboratory which has been established in connexion with Section H, under the care of Dr. Garson and Mr. Bloxam. It has been very largely frequented by the members of the Association, who have had themselves weighed, measured, and tested in a variety of ways. The object, we believe, is to obtain data from the most cultured classes to compare with those collected by Mr. Francis Galton, at South Kensington, from all and sundry. The result, it is expected, will be highly interesting.

We have already referred to the very varied exhibition which has been arranged in the galleries around the attractive reading-room. This has received various additions during the week. Prof. Boyd Dawkins shows some very instructive exhibits in various rooms belonging to the Geological Department of Owens College, including William Smith's first geological map and an autograph letter. Another exhibit deserving mention is the collection of wax models, illustrating vertebrate morphology and embryology, shown by Prof. His, of Leipzig, on behalf of Dr. A. Ziegler, of Freiburg. Naturally the model of the Manchester Ship Canal has attracted much attention, but not more than the great variety of interesting anthropological exhibits, which include the collection of casts and photographs from Egyptian monuments contributed by Mr. Flinders Petrie.

Great complaints have been made of the way in which the Press has reported the proceedings of the meeting. This may partly be due, no doubt, to the fact that Parliament is still sitting and takes up much of the space of the papers; but is also to be ascribed to a larger extent to the fact that ordinary reporters are hardly equal to the work of the British Association. When there are printed abstracts the matter is simple enough, but when discussions have to be reported the failure is almost absolute. This is certainly to be regretted, as it could not but be of the greatest service to have such discussions widely circulated. Surely it is quite worth while for the Sections to organize adequate reporting arrangements for their own sake.

The next meeting, at Bath, will be presided over by Sir Frederick Bramwell. In 1889 the Association will meet in Newcastle, and it is expected that an invitation will come from Leeds for 1890.

The following is the list of grants which have been made this year by the Association:—

A.—*Mathematics and Physics.*

Ben Nevis Observatory	£150
Electrical Standards	80
Magnetic Observations	15
Standards of Light	100
Electrolysis	50
Solar Radiation	10
Differential Gravity Meter	10
Uniform Nomenclature in Mechanics	10

B.—*Chemistry.*

Silent Discharge	10
Properties of Solutions	25
Recording Water Analysis Results	10
Influence of Silicon on Steel	20
Methods for Teaching Chemistry	10
Isomeric Naphthalene Derivatives	25
Action of Light on Hydro-acids	20

C.—*Geology.*

Sea Beach near Bridlington	20
Geological Record	50
"Manure" Gravels of Wexford	10
Erosion of Sea Coasts	15
Erratic Blocks	10
Underground Waters	5
Palæontographical Society	50
Volcanic Phenomena of Japan	50
Pliocene Fauna of St. Erth	50
Carboniferous Flora of Lancashire and West Yorkshire	25
Volcanic Phenomena of Vesuvius	20

D.—*Biology.*

Zoology and Botany of the West Indies	100
Flora of Bahamas	100
Development of Fishes, St. Andrews	100
Marine Laboratory, Plymouth	100
Migration of Birds	30
Flora of China	75
Naples Zoological Station	100
Physiology of the Lymphatic System	25
Marine Station at Granton	50
Peradenyia Botanical Station	50
Development of Teleostei	15

E.—*Geography.*

Depth of Frozen Soil	5

F.—*Economic Science and Statistics.*

Precious Metals in Circulation	20
Value of the Monetary Standard	10

G.—*Mechanics.*

Investigation of Estuaries by Models	200

H.—*Anthropology.*

Effect of Occupations on Development	25
North-Western Tribes of Canada	100
Prehistoric Race in the Greek Islands	20
Anthropological Notes and Queries	50
Total	£2025

Captain Sir Douglas Galton (one of the general secretaries) submitted the following report of the Council to the General Committee at the meeting held on the 31st ult. :—

The Council have received reports during the past year from the general treasurer, and his account for the year will be laid before the General Committee this day. Since the meeting at Birmingham the following have been elected corresponding members of the Association : Dr. Finsch, Dr. O. W. Huntington, Dr. A. König, Lieut. R. Kund, Prof. Leeds, Prof. H. Carvill Lewis, Prof. John Trowbridge. The Council have nominated Mr. Oliver Heywood a vice-president of the meeting at Manchester. An invitation for the year 1889 will be presented from Newcastle-upon-Tyne ; but the invitations from Melbourne and Sydney have been withdrawn. The following resolutions were referred by the General Committee to the Council for consideration, and action if desirable :—(*a*) "That the Council be requested to consider the question of rendering the reports and other papers communicated to the Association more readily accessible to the members and others by issuing a limited number of them in separate form or in associated parts, in advance of the annual volume." The Council, after careful consideration of the question, are of opinion that a certain number of copies of the more important reports presented to the Sections of the Association should be kept in stock and sold separately, the number of copies printed and the price of each report to be fixed by the secretaries after communication with the officers of the several Sections. (*b*) "That the Council be requested to consider the advisability of selling publicly the presidential addresses." The Council have considered the question, and are of opinion that it is desirable that printed copies of the addresses of the president and the presidents of Sections should be stitched together and sold ; that a number of copies, not exceeding 1000, should be printed ; and that these should be placed on sale, at the price of one shilling, through agents or otherwise, as may be considered most suitable. (*c*) "That the Council be requested to consider the advisability of calling the attention of the proprietor of Stonehenge to the danger in which several of the stones are at the present time from the burrowing of rabbits, and also to the desirability of removing the wooden props which support the horizontal stone of one of the trilithons ; and in view of the great value of Stonehenge as an ancient national monument, to express the hope of the Association that some steps will be taken to remedy these sources of danger to the stones." The Council have carefully considered the question, and having had the advantage of perusing the detailed report recently prepared by a deputation of the Wilts Archæological and Natural History Society on the condition of the whole of the stones constituting Stonehenge, are of opinion that the proprietor should be approached with the expression of a hope that he will direct such steps to be taken as shall effectually prevent further damage. (*d*) "That the Council be requested to consider whether a memorial should be presented to Her Majesty's Government, urging them to undertake and supervise agricultural experiments, and to procure further and more complete agricultural statistics." The Council have considered the question, and are not prepared to memorialize the Government on the subject. The question of the re-arrangement of the journal has been brought before the Council by Mr. J. B. Martin, and, after careful consideration, the Council are of opinion that it is unnecessary to print in each number of the journal the list of the papers read on the previous day ; also that it would be well to place the list of officers of each Section at the head of the list of papers to be read in that Section. The Council wish to obtain the sanction of the General Committee to these alterations. The Council having considered a letter addressed to them by Mr. R. H. Scott, are of opinion that it should be an instruction to the secretaries of all committees, other than committees of Sections, to send notices of all meetings to each member of a committee, and that the draft report of the committee should first be sent in proof to each member, and then submitted to a meeting of the committee specially called for the purpose. The Corresponding Societies Committee, consisting of Mr. F. Galton (chairman), Prof. A. W. Williamson, Sir Douglas Galton, Prof. Boyd Dawkins, Sir Rawson Rawson, Dr. J. G. Garson, Dr. J. Evans, Mr. J. Hopkinson, Prof. R. Meldola (secretary), Mr. W. Whitaker, Mr. G. J. Symons, and General Pitt-Rivers, having by an oversight not been re-appointed at Birmingham last year, the Council have requested these gentlemen to continue the work of their committee, and now nominate them for re-election, with the addition of the names of Mr. W. Topley, Mr. H. G. Fordham, and Mr. William White. In accordance with the regulations the five retiring members of the Council will be Mr. W. Pengelly, Sir Richard Temple, Dr. De La Rue, Sir F. J. Bramwell, and Mr. J. C. Hawkshaw.

The Council recommend the re-election of the other ordinary members of the Council, with the addition of the gentlemen whose names are distinguished by an asterisk in the following list:—Capt. W. de W. Abney, F.R.S., Sir R. S. Ball, F.R.S., W. H. Barlow, F.R.S., W. T. Blanford, F.R.S., W. Crookes, F.R.S., Prof. G. H. Darwin, F.R.S., Prof. W. Boyd Dawkins, F.R.S., Prof. J. Dewar, F.R.S., *Sir James Douglass, F.R.S., Prof. W. H. Flower, F.R.S., Dr. J. H. Gladstone, F.R.S., Lieutenant Colonel H. H. Godwin-Austen, F.R.S., Prof. O. Henrici, F.R.S., Prof. J. W. Judd, F.R.S., J. B. Martin, F.S.S., Prof. H. McLeod, F.R.S., Prof. H. N. Moseley, F.R.S., Admiral Sir E. Ommanney, C.B., F.R.S., Prof. W. C. Roberts-Austen, F.R.S., *Prof. Schuster, F.R.S., *Prof. H. Sidgwick, *Prof. Schäfer, F.R.S., W. T. Thiselton-Dyer, C.M.G., F.R.S., Prof. T. E. Thorpe, F.R.S., *H. Woodward, F.R.S.

Sir H. E. Roscoe, M.P., moved, and Sir R. W. Rawson seconded, the adoption of the report. The motion was adopted.

The Chairman submitted the treasurer's report, which stated that the receipts for the past year were £5081 6*s.* 3*d.*, including a balance of £1869 brought forward at the Birmingham meeting. The disbursements included the sum of £1186 18*s.* in respect of grants in aid of scientific research. The balance in hand was £1718. The report was adopted.

SECTION B.

CHEMICAL SCIENCE.

OPENING ADDRESS BY EDWARD SCHUNCK, PH.D., F.R.S., F.C.S., PRESIDENT OF THE SECTION.

IT is, I can assure you, with a feeling of extreme diffidence that I take the chair to-day as President of the Chemical Section at this meeting of the British Association. When I look round me and see the many distinguished men who are prepared to take part in our proceedings I cannot but very strongly feel that the Council's choice might have fallen on a worthier representative of chemical science than myself. Having in the course of my career bestowed more time and attention to technical matters than to purely scientific subjects, and having moreover arrived at a time of life when active participation in work of any kind must necessarily be drawing to a close, you must not expect from me the accurate knowledge of the present state of chemical science and the questions that are at this moment presenting themselves for solution such as would naturally be required from anyone occupying the post which I have on this occasion the honour to hold. The marvellously rapid progress of chemistry during the last twenty years has made it difficult for the most industrious cultivator of the science to keep abreast of the knowledge of the day, and for a *dilettante* like myself one may say it is next to impossible. I confess myself painfully conscious of my defects in this respect, and I shall therefore have to claim the indulgence of the Section should questions arise on which I am unable to speak with authority, or even to discuss with advantage.

Considering, however, how efficiently I am supported by the gentlemen with whom I have the honour to be associated, and to whom I am sure in any case of difficulty I may appeal for assistance, I trust to be able to perform the duties of my office without discredit. I will not, however, trouble you with merely personal questions, which are always more or less tedious, but proceed with the few remarks which I wish to make, and which, if not new or instructive, may perhaps serve to entertain you during the time usually devoted to addresses of this kind.

I think you will hardly expect me, even were I fully competent to do so, to review the progress of chemistry during the last half-century, for the time at my disposal would be too short and the result at my hands, I fear, unsatisfactory. I shall prefer to call attention in a few words to the chemistry of other days as I knew it, and the chemistry of the present time as known to us all, and to point out what I consider to be the chief characteristics of each. I shall then, with your permission, point out a few of the directions in which, in my opinion, the chemistry of the future will probably be developed, and in this undertaking I shall perhaps be more successful than in the other; for to discuss the history of science requires exact knowledge; but in speculating on its future the imagination comes into play, and to imagine is easier than to describe.

When I first entered on my studies, exactly fifty years ago, chemistry could hardly be called a science—it was rather a collection of isolated facts unconnected by any consistent theory covering the whole field. Most of the important elements were known, but of the exact proportions in which they combine together we were ignorant. The law of definite proportion had been generally accepted, but so imperfect were the data then at our disposal that we may say the law was rather taken for granted than proved. The atomic theory of Dalton as explaining this law had also been adopted by chemists; but it is not unlikely that this theory, then in its infancy, might by the vigorous onslaught of a man of Berthollet's acumen have been upset, and we should then have been left entirely without a guide through the bewildering labyrinth of facts. Of any connexion between chemistry and physics there was in those days no question; of any but the most superficial notions regarding the effects of heat, light, and electricity on chemical substances we had no conception. The idea that chemistry could have any bearing on or connexion with physiology or pathology would have been ridiculed as absurd. I cannot think of the then state of organic chemistry without feeling amused. The state of this branch of chemistry could hardly perhaps be called chaotic or rudimentary, for, after all, what had been done had been well done and neatly done, but the assemblage of facts of which it consisted was devoid of systematic arrangement; it resembled a cabinet of curiosities, the components of which stand in no recognizable relation to one another, or a miscellaneous collection of books placed in an orderly manner on shelves, but without any attempt at classification. As to the genesis of organic compounds, what would now be called absurd notions prevailed. I distinctly remember eminent chemists maintaining that no strictly speaking organic body, even of the simplest constitution, could possibly be formed without the intervention of the so-called vital force. The fact, then recently discovered by Wöhler, of the artificial formation of urea from inorganic substances, was considered as something almost miraculous—*i.e.* as a phenomenon the like of which would never again recur. Without, however, entering into further details, I think I may, without fear of contradiction, assert that the main distinction between the chemistry of fifty years ago and the chemistry of the present day consists in this, that whereas formerly the science dealt chiefly with qualitative reactions, it now occupies itself principally with quantitative determinations. To have established the fact that every chemical phenomenon may be represented in figures, denoting either number, measure, or weight, such figures, when once accurately determined, remaining constant and unchanged through all time—this seems to me the crowning glory of modern chemistry. It is the firm establishment of this principle that has transformed the face of chemistry and has changed it from a mere descriptive into an exact science.

In justice to our predecessors it should, however, be remembered that this principle, though more fully developed in our own day, was not for the first time set up in quite recent times. The labours of Dalton, conducted on quantitative lines, were performed in this city of Manchester in the early part of this century. At the same time Berzelius was engaged in analyzing the most important inorganic compounds and establishing the fact, not previously recognized, strange as it may now appear, that every well-defined substance has a definite chemical composition. But going still further back, we come to the alchemists. Now alchemy, if it has any logical basis at all, is founded on quantitative notions as regards matter. All metals, the alchemists said, consist of sulphur, salt, and mercury (these terms signifying not so much elements in the modern sense as qualities) in various proportions; hence their convertibility. Take copper, remove from it a certain proportion of its sulphurous constituent, and add more of the mercurial, and you have silver; repeat the process with silver, and gold results. At the time of which I speak, though much important analytical work had been done by Berzelius, Rose, and others in organic chemistry, though the veteran Chevreul had led the way in placing organic chemistry on a quantitative basis, and the composition of the most important organic compounds—thanks to the labours of Liebig and his method of organic analysis—had been ascertained, still quantitative determinations were not considered of such paramount importance as at present. In fact, scientific thought did not run in that direction, but satisfied itself, for the most part, with the study of qualitative reactions. It was still possible to see

memoirs by eminent chemists containing not a single quantitative determination. Strange as it may seem, two able chemists, Boettger and Schoenbein, were living until quite recently who worked and obtained valuable results without resorting to the balance, the instrument which of all others seems the most indispensable to the chemist of to-day. The balance was indeed universally employed in my younger days, but no other instrument, properly so called, was ever seen in the laboratory. The spectroscope was not yet invented; the polariscope had not come into use; volumetric analysis was still in its infancy. Even the thermometer was but seldom used. What a different picture does the laboratory of the present day present, with its instruments of precision and its various appliances for effecting quantitative determinations of all kinds!

Whether the universal prevalence of, and exclusive attention to, quantitative methods in chemistry has been an unmixed good may be doubted. Who has not run with a weary eye over the long array of figures, the never-ending tables of which some modern memoirs seem to consist, and not longed for some mere description—were it only regarding trivial matters—to relieve the monotony and fix the subject treated of on the memory? That quantitative determinations given in quite precise terms may occasionally be entirely futile may be seen on referring to the history of alchemy. One of the later alchemists professes to have converted 5400 parts by weight of copper into 6552 parts of silver by the action of 1 part of a metal-improving substance—philosopher's stone (Kopp, "Die Alchemie"). Here we see the quantitative method applied to a purely chimerical process, elaborated from the depths of the experimenter's inner consciousness, and of no value whatever. Much of what is at the present day carefully worked out and presented to the world in numerical form may, like this statement of the alchemist, pass away and be forgotten. This may possibly be the case with the numerous carefully-made analyses of water which we now meet with, and which we would gladly exchange for a few decided qualitative tests of its hygienic properties. In the case of air and water it is not the minuteness of the noxious matter which causes doubts to arise, but the absence of any decided and undoubted chemical characteristics of the impurities present. It is probable that a refined sense of taste, uncorrupted by the luxurious indulgences which civilization has introduced, would be able to detect differences in drinking-water which might escape the attention of the most consummate analyst.

Whatever objections may, however, be entertained to the application of quantitative methods in natural science, to the exclusion of others, it is certain that important results have flowed from their adoption, insomuch that we seem to have arrived at the conclusion that the expression of quantitative results is the be-all and end-all of science; that all differences are merely quantitative; that there is no such thing as mere quality. The whole philosophy of our age is expressed in this one proposition: All differences within the sphere of our experience are quantitative. It is the basis of Darwinism, if I am not mistaken, and underlies many of our political and social theories. Of course it is a mere assumption if stated generally, for the phenomena that admit of purely quantitative expression are few in number compared with those that do not; but then it is surmised, and with some degree of probability, that the vast region outside the quantitative sphere will in time come to be included within it. The past history of science seems to render this likely in the future. The science of chemistry has so far, however, presented an insuperable barrier to the general adoption of this view, and will continue to do so as long as the so-called elements remain what we now admit them to be—indestructible, immutable, inconvertible. It is possible to denote all the known properties of gold and silver, their atomic weight, specific gravity, hardness, malleability, action towards heat, light, and electricity, in precise numbers with reference in each case to a certain standard; and yet we cannot say that silver minus a little of this, plus a little of that, constitutes gold—the two elements are essentially and radically distinct. Unless we admit with the alchemists that by taking away a little of A and adding a little of B we can convert one metal into another, one element into another, the quantitative method must fall short of its complete development in chemistry. Numerous attempts have, therefore, been made to show the theoretical probability, even if it should not be possible to prove it experimentally, of the so-called elements being really compound bodies, or at least of their containing a basic matter common to all. My predecessor in this chair has endeavoured to show, in the brilliant address delivered to this Section on the occasion of its last meeting, that the barrier hitherto presented to us by the intractability of our present elements may be overcome, and has adduced experimental illustrations in favour of his views of the compound nature of the elements. Mr. Crookes has called to his aid the doctrine of evolution, which has proved so valuable an instrument in the hands of the biologist, maintaining that the elements, like the species of plants and animals, were gradually evolved by some process of condensation from a primordial matter called by him protyle, each step in the process being represented by a distinct element. This is doubtless taking very safe ground, for if the process of evolution was the same in the inorganic as it is supposed to have been in the organic world, the process can never be repeated, and we shall, therefore, never be in a position to illustrate it experimentally. I may, however, have misunderstood what Mr. Crookes meant to convey, and, if so, must apologize for the dulness of my apprehension. Granting, however, the possibility of our resolving our present elements, were it in theory only, into modifications of one basic material out of which they have been evolved, the question would still remain to be answered, What has caused this primordial matter to be split up into groups and forms having distinct and opposite qualities? and when this question is answered, if it can be answered even in a problematical way, then other questions would arise, until by degrees we should arrive at the confines of physical knowledge and find ourselves in the region of metaphysics, where scientific reasoning ceases, and thinking for scientific purposes becomes unprofitable. Excursions into this region would indeed be very useful if on returning to physical regions we could every time bring back with us an instrument as potent and far-reaching as the atomic theory has proved to be, a theory which still remains the basis of all our reasoning in chemistry; but then the atomic theory has been quite an exceptional instance. Metaphysical speculation, such as the *Naturphilosophie* of the Germans has dealt in, has, generally speaking, been utterly barren in natural science.

I will not on the present occasion dwell on the vast addition made to the number of useful and beautiful substances by chemists during the last fifty years. Their number is legion, and their mere description fills volumes, whereas half a century ago a dictionary of moderate size would have sufficed for the purpose. Among these newly-discovered substances none are more remarkable than the metals rubidium, cæsium, thallium, indium, gallium, the existence of which was revealed by the spectroscope, and which, indeed, would have probably remained unknown but for the labours of Bunsen and Kirchoff in perfecting and applying that instrument.

I must not, however, omit all reference to a department of chemistry which has been, one may almost say, created within the time to which I am referring—I mean that of synthesis. When I began to study chemistry we only heard of analysis; of synthesis, so far at least as regards organic bodies, we only dreamt as a remote and unattainable region. The only instance then known of the synthesis of an organic substance was that of urea by Wöhler. Synthesis was, indeed, supposed to be an essentially vital process effected under the influence of the vital force, and quite outside the sphere of the chemist. Since then what marvels have we not seen? Alizarin and purpurin, the colouring-matters of madder, have been prepared artificially by Graebe and Liebermann, indigo by Baeyer, not to mention bodies of simpler constitution obtained by comparatively less complicated processes. We are honoured to-day by the presence of Prof. Ladenburg, who has succeeded in artificially preparing coniin, the alkaloid to which hemlock owes its poisonous properties; the first natural alkaloid, indeed, which has been obtained artificially. Looking back at what has been achieved, I think we may entertain the confident anticipation that all the most important organic bodies, acids, alkaloids, and neutral substances will, in course of time, be obtained in a similar manner, though of one thing we may be pretty sure—viz. that we shall never succeed in forming any really organized matter as distinct from organic. The term organic matter is in fact only employed for the sake of convenience, and as an expression handed down to us from former days, since so-called organic compounds are subject to the same laws with regard to composition as the bodies which we name mineral or inorganic, but organized matter such as we find constituting the vessels of plants and animals is a different thing. The protoplasm contained in the vegetable and animal cell is something very distinct from the same matter after the death of the organism, but the difference

between living and dead matter is not of a chemical nature. In referring to chemical synthesis I cannot refrain from expressing regret that so little has hitherto been done in the artificial production of minerals with a view to elucidating the processes by which they were formed in Nature, but it is possible that more has been done in this direction than I am aware of, since this is a department of chemistry with which I am not familiar. It is certain that inorganic chemistry generally does not now receive the attention which it formerly did. The exclusive devotion to the chemistry of the carbon compounds which we find in most of our laboratories at the present day may, however, be accounted for when we see the brilliant results to which the study of those compounds has led.

After these few remarks on the development of chemistry during the last fifty years, of which I know a little, it may seem presumptuous on my part in the presence of some of the most eminent chemists of our day, whose opinions must be of infinitely more value than mine, to say anything about the future of our science and the direction it will probably take. Nevertheless, trusting to your kind indulgence, I will venture on some speculations in this direction, which, if they do not instruct the younger members of the Section, may serve to amuse their seniors, and at all events will refer to subjects on which some thought is well bestowed.

As regards the future of chemistry, the question has frequently suggested itself to me as it has doubtless done to others —Will chemical science go on expanding and developing during the next few generations as it has done in the course of the last hundred years? Will discovery follow discovery, and fact be added to fact, until the record occupies not a few volumes only, but a whole library? Will systematic chemistry, *i.e.* the history and description of all possible combinations of the elements, have any limits? I am inclined to answer in the negative. All human institutions pass through the same phases; they have their rise, they culminate, and decay; and I do not see why the science of chemistry should form an exception. Moreover, it is a natural law that whatever develops rapidly also declines rapidly, and the development of systematic chemistry since the commencement of this century has been perfectly unprecedented. I think it probable that in the course of time, at the rate at which we are now progressing, nearly all possible compounds will have been prepared, all the most important chemical facts will have been discovered, and pure chemistry will then be practically exhausted, and will be in the same condition as systematic botany and mineralogy now are. New compounds will now and then be discovered, just as new plants and new minerals now are, but nothing further will be brought to light that will affect the theories at which we shall then have arrived, whatever they may be. All the material with which the science has to deal having then been brought together, what will happen? Will chemical science cease? Will chemists, satisfied with past achievements, cease to work, confining themselves to practical questions and the history of the days gone by? I think not. The science will continue to develop, but in other directions than those previously pursued. The exhaustion of systematic botany has not put an end to botanical science, for vegetable physiology has opened a wide field to the botanist, one that will take a long time to explore thoroughly. To indicate the directions which chemical science will take in its various applications to other departments of knowledge, as, for instance, in connexion with the study of the physical properties of matter, or in elucidation of the chemical processes whereby minerals have been formed, or those through which geological strata have passed in bygone ages, would not be within my competency, as I should have to touch on subjects with which I am not familiar; but I may be permitted to refer by a few words to a subject, with which, by reading at least, I have become better acquainted, and which seems to me to offer a wide field to the investigator who shall come well provided with physical and chemical knowledge to its cultivation. I allude to the processes whereby the substances constituting the various organs of plants and their contents are formed, and those again to which the decomposition and decay of vegetable matter are due; a subject as to which our knowledge is quite elementary, but which, it seems to me, admits of an extension and development of which we have at present not the least conception.

De Saussure, it is well known, first discovered the fact that plants under the influence of light absorb carbonic acid and give off oxygen, the inference of course being that the carbonic acid and the water present are decomposed, the carbon of the former and the hydrogen of the latter going to form the various organic constituents of the plant, while the oxygen or a part of it is set at liberty and poured into the atmosphere. The facts as they stand are simply these: what the plant requires for its subsistence is carbonic acid, water, nitrogen in some form (presumably that of a nitrate), certain bases—potash, lime, magnesia, iron oxide, and phosphoric acid. Out of these it constructs the whole of its organic frame, its cells and their contents, rearranging the elements of which its food consists in such a manner as to convert inorganic into organic matter, *i.e.* changing bodies in which the affinities of the atoms are thoroughly satisfied into such as contain them in a state of more or less unstable equilibrium, and therefore liable to alteration when their atoms are allowed to act in accordance with their natural affinities. More than this we do not know; our ignorance of the several steps or stages of the process, if there are any such steps, is complete; all that has been added to the general statement just given is mere speculation. Yet it is impossible to remain satisfied with the present state of our knowledge on the subject. Accordingly numerous attempts have been made to bridge over the chasm which separates the inorganic and organic worlds, not indeed to show that the change does not involve the creation, as was once supposed, of new matter—for this was proved long ago —but to exhibit in its details the hidden mechanism which produces it, but hitherto without success. We know that light is essential to the process of assimilation in plants, since this does not proceed in the dark, but this fact does not help us to an explanation, for light in this case is a mere stimulant, and never produces the same or similar effects outside the vegetable organism. Liebig and others have attempted to show that the process of assimilation in plants commences with the formation of some simply constituted body, such as oxalic or formic acid, with the elimination of oxygen, out of which by condensation and farther separation of oxygen more complex bodies, such as sugar, fats, &c., are formed; but there is not the slightest evidence at present in favour of this view. The first product of assimilation that is distinctly recognized is starch, a highly complex organic, we might almost say an organized body, which appears at once with all its characteristic properties, like Minerva springing fully armed from the head of Jove. If we are to adhere to the facts so far observed, we must conclude that the plant does not proceed as we should do in the laboratory, beginning with the more simply constituted compounds and advancing to the more complicated, but that the reverse process is the one actually adopted, the supposed intermediate products being more probably the results of retrogressive metamorphosis. This conclusion is, however, so much opposed to ordinary chemical views that one cannot feel surprised at the constantly repeated attempts to clear up the question. There can be no doubt indeed that much here remains to be done and to be discovered.

Intimately connected with this subject is that of chlorophyll, the green-colouring matter of leaves, which is always found wherever the process of assimilation in plants is going on, and nowhere else, and is therefore doubtless an essential factor in the process. What part it plays in this process is, in my opinion, still unknown. Its action is probably in part chemical, in part physical, and this adds, it may be, to the difficulty of understanding it. It is generally supposed that it is chlorophyll which by its direct action on the carbonic acid and water with which it comes into contact leads to the formation of organic matter with elimination of oxygen. But this is, I think, a mere assumption—an error due, like many others, to a mistaken use of terms. The chlorophyll of chemists is simply an organic colouring matter, like alizarin or indigo, but being in the vegetable cell intimately associated with other matters, vegetable physiologists have attributed to the action of one, and that the most obvious, constituent what is really due to the complex, perhaps even to some quite other constituent of the complex. It is impossible to conceive that the chlorophyll of chemists can be endowed with the remarkable and exceptional properties attributed to it by physiologists; it is a chemical entity, nothing more. It may indeed be said that chlorophyll only acts as it is stated to do when inclosed within the vegetable cell, but this merely amounts to saying that its action is not merely chemical, but is controlled by the vitality of the cell, which, I suppose, means the action of the protoplasm. If chlorophyll is the agent whereby the decomposition of carbonic acid and water is effected, how, it may be asked, is the agent itself produced? It does not come from without; the plant must be able to form it in the first

instance. We are told by vegetable physiologists that the Coniferæ when raised in total darkness from seeds produce chlorophyll. In light or in darkness I am convinced it is the same; the plant forms chlorophyll as a means to an end. What the end is we know; it is the assimilation of carbon and hydrogen to form organic matter. How does the chlorophyll assist in attaining this end?

In propounding a new theory in reply to this question I venture to claim your indulgence, such as has been accorded to some of my predecessors and others who at these meetings of the British Association have been permitted to make statements and use arguments of a novel or paradoxical character, which, if they effect nothing else, at least afford a relief to the usual routine of scientific reasoning. My experiments on chlorophyll have led me to infer that the constitution of that body is much less simple than it is generally supposed to be. I do not mean by this that chlorophyll is a mixture in the usual sense; everyone who has paid any attention to the subject knows that ordinary chlorophyll consists of several colouring matters, some of which are yellow, not to mention fatty matters which are unessential. What I mean to say is this, that the pure green substance, the chlorophyll *par excellence*, does not belong to the same class of bodies as alizarin or indigo, but contains three elements, each of which is essential to its constitution, one being a basic nitrogenous colouring matter, the second a metal or a metallic oxide, the third an acid, the three together constituting green chlorophyll. The basic colouring matter is a body of very peculiar properties; it is the phyllocyanin of Fremy; the metal may be iron or zinc, the acid I will suppose to be carbonic acid. Now the plant having formed its colouring matter, the metallic oxide being present in some form or other, and the carbonic acid being supplied by the atmosphere, all the necessary conditions co-exist for the formation of chlorophyll. The compound is an unstable one; it easily parts with its carbonic acid, giving it up to the protoplasm or whatever the agent may be that effects its actual decomposition under the influence of light. The advantage of this arrangement would consist in this, that the carbonic acid would be presented in a more condensed state to the agent which effects its decomposition than if it were merely contained in a watery solution, but more loosely combined, and therefore more easily accessible than if it were united to a strong base such as potash or lime. The carbonic acid having been disposed of, the other two constituents would be in a state to take up fresh quantities of carbonic acid, and so on. Chlorophyll would therefore act as a carrier of carbonic acid in the plant, just as hæmoglobin serves to convey oxygen in the animal economy. Numerous objections may of course be raised to the theory of which I give an outline; I only throw it out as a tentative explanation, showing that the function of chlorophyll may be, in part at least, chemical, and that we need not suppose it to be endowed with the marvellous and exceptional powers usually ascribed to it. Other and more probable explanations will doubtless suggest themselves when this difficult subject has been more thoroughly worked out. Eventually, too, it will be found, I imagine, that physical forces as well as chemical affinities play a part in this as in every other process of the vegetable economy. In the case of cholorophyll there can be no doubt that the green colour and the peculiar behaviour towards light have something to do with its action, but on this point it is not necessary for the chemist to pronounce any opinion. I may take this oportunity of mentioning the important experiments of Sachs and Pringsheim on the optical properties of chlorophyll in their relation to assimilation in plants, as they are probably not so well known to chemists as to botanists.

What I have said may serve to show that the very first steps of the process whereby organic or organized matter is formed in plants are hardly understood. We understand still less the further steps leading to the production of the more complex vegetable bodies—acids, alkaloids, fatty matters. Granted that we were able to trace the formation in the plant of a compound of simple constitution, such as oxalic or formic acid, how far should we still be from understanding the building up of such compounds as starch, albumen, or morphia? The syntheses so successfully and ingeniously carried out in our laboratories do not here assist us in the least. We know the steps by which alizarin is artificially produced from anthracene; but can anyone for an instant suppose that the plant commences in the same way with anthracene, converting this into anthraquinone, and having acted on the latter first with acid, then with alkali, arrives at last at alizarin? Indeed the plant never contains ready-formed alizarin at all. What we observe from the commencement is a glucoside a compound of alizarin and glucose, which, so far as we see, is not gradually built up, but springs into existence at once. When we think of the complicated process by which indigo is produced in the laboratory with the various substances and appliances required, and then see how in the minutest seed-leaves of a plant like woad a still more complex substance, indican, is found ready-formed, we stand confounded at the simplicity of the apparatus employed by the plant, and are obliged to confess that we have no conception of the means whereby the end is attained. The same difficulties occur in other cases, and it will therefore probably be conceded that the synthetic process carried on in plants, from the first step to the last, are not in the least understood.

It might be supposed that after all the labour and attention bestowed on the inorganic constituents of plants we should know something of the part played by these constituents in the processes of assimilation and nutrition, but here the obscurity is as great as elsewhere. We know by experiment that certain inorganic matters—potash, lime, magnesia, iron oxide, phosphoric acid—are essential to the growth of plants; but of their mode of action, or of the reason why certain plants require potash salts, others lime, and so on, we know nothing. Phosphoric acid is no doubt an essential constituent of the protoplasm of the plant; but why cellulose, of which the various organs chiefly consist, should require mineral matters, which do not enter into its composition, for its formation and building up is still a mystery.

The department of chemistry which relates to the decomposition of organic and organized matters presents problems almost as difficult of solution as those relating to their formation and building up; that is to say, the phenomena observed do not apparently obey the same laws as those prevailing in the inorganic world. When I began my chemical studies the difference in this respect between mineral and organic compounds was less clearly seen than at present. The conversion of alcohol into acetic acid, the putrefaction of animal and vegetable matter were thought to be simply due to oxidation; they were phenomena, it was supposed, exactly similar to the rusting of iron, the tarnishing of metals, the fading of colours. That a third body was required to initiate and continue the process of decomposition, that organic matter in contact with purified air would remain unchanged for any length of time—was not known nor suspected. I am not quite sure whether spantaneous decomposition—*i.e.* the splitting up of a complex body without the intervention of an external agent—might not at that time have been considered possible. In order to explain the phenomena of fermentation, the decomposition of sugar into alcohol and carbonic acid, for instance, we had only the theory of contact—devised by Berzelius and Mitscherlich, the latter of whom used to expatiate on the subject at great length in his lectures. When this ghost of a theory was laid by Liebig, who suggested an intelligible explanation of the phenomena in accordance with the facts then known, it was felt to be quite a relief, as affording a resting-place—if only a temporary one—for the mind. The brilliant researches of Pasteur, which have thrown so much light on the action of the insoluble organized ferments, I need only refer to, as they are so widely known, even outside scientific circles; and since also investigations such as his cannot be discussed without some reference to biological questions, which cannot be entered on now. I will confine myself therefore to a few remarks on the unorganized or soluble ferments, one of which I had occasion to examine when engaged in investigating madder and its colouring matters. These ferments, the type of which is diastase—a substance found accompanying starch in the seeds of plants—are soluble in water, perfectly neutral, devoid of all definite form, and though apparently inert, able when acting within the sphere in which Nature has placed them to cause changes and decomposition of the most profound character. Their action excludes everything in the shape of vitality, and yet it is as mysterious and unaccountable as anything that the vitality of the organized ferments is capable of effecting. Indeed, in vegetable, and especially animal, organisms they seem expressly intended for the attainment of certain ends necessary for the well-being, or even the existence, of the organism, insomuch that it has been supposed, with some show of reason, that it is to bodies of this class existing within the cells of organized ferments, but not separable by any means at our disposal, that the changes produced by the latter are really due.

A great deal of attention has been paid to the product and

results of fermentation, but very little hitherto to the *modus operandi* of the ferments themselves, and yet this seems to me to offer a wide field for interesting research, especially in the case of those of the soluble class, which are easily prepared, and can be manipulated in the laboratory like any chemical substance without the tedious precautions and preliminary operations necessary in the case of the organized ferments. In what way, it may be asked, do these soluble ferments produce the effects peculiar to them? Is the action essentially chemical, or is it due to physical causes as well? Is the quantity of fermentable matter acted on by a certain quantity of ferment unlimited in amount, or are there limits to that amount somewhere? Does the ferment itself undergo any change during the process of fermentation, or is it the same afterwards as before and capable of acting on fresh quantities of fermentable matter? When a ferment is replaced by a strong mineral acid, the products of decomposition being the same, is the *modus operandi* in both cases alike, or must a different explanation be in each case sought? These questions have never been satisfactorily answered, and await solution. I know of only one attempt to show what actually takes place during a process of fermentation set up by a soluble ferment.

The experiments of Wurtz (*Comptes Rendus*, xci. 787), on papain, the soluble ferment of *Carica Papaya*, led to the conclusion that the fibrin on which it is made to act combines in the first instance with the ferment itself, the latter after the hydration of the fibrin is completed being again set at liberty, and then able to act on fresh quantities of fibrin. Thus, according to Wurtz, the action is found to be the same as that of chemical agents, properly so called, such as sulphuric acid, of which minute quantities may exert a hydrating action in consequence of the transitory formation of compounds which are constantly being produced and again decomposed.

There is another question referring to these soluble ferments to which in the present state of our knowledge it is impossible to frame a probable answer, viz. why does it so frequently happen that each ferment exerts a specific action, an action peculiar to itself, this being in fact, in the absence of any marked chemical characters, the only means by which they can be distinguished one from the other? Why does one ferment act on starch only, while the function of another consists in the hydration of fibrin, that of another in the decomposition of a glucoside, and so on? In accordance with the explanation of Wurtz, we should say that a specific ferment is one capable of combining only with the body on which it is to act, and with no other. I was led to ask this question when engaged in the examination of the colouring matters of *Rubia tinctorum*. The root of this plant, the madder of commerce, contains glucosides, which, though coloured, are quite devoid of tinctorial power. Nature has at the same time placed in the root a peculiar ferment, which, coming into contact with these glucosides at a certain temperature, effects their decomposition, splitting them up into glucose and true colouring matters. Now this ferment is a body *sui generis*, and cannot be replaced by any other ferment that I have tried; its action is specific. Why Nature should have deposited this body in the recesses of the plant for the express purpose of acting on certain glucosides and forming colouring matters, the object of which, so far as the economy of the plant is concerned, can only be guessed at, is difficult to understand. One is inclined in such a case to revert to the old-fashioned doctrine that some natural processes were devised for the use and delectation of man. It is quite certain in the case of madder that had it not been for its peculiar ferment erythrozym, the valuable tinctorial properties of the root, which have for centuries been applied in the production of that splendid dye Turkey red, would have remained unknown perhaps to the present day, since the only efficient substitute for the natural ferment is a strong mineral acid, and such acids and their uses were unknown in former days.

I am inclined to think that some of the younger chemists and physiologists of to-day may live to see the time when all the at present mysterious and unaccountable processes going on in the organisms of plants and animals, including those of fermentation, will be found to obey purely physical and chemical laws. To the biologist it may seem derogatory to the dignity of his science to have the principle of vitality, which has so long reigned supreme, dethroned and replaced by hard, unbending law. Such, however, is not the opinion of that distinguished botanist Sachs, who says, referring to this very point:—"Der Organismus selbst ist nur die aus verschiedenen Theilen bestehende Maschine, die durch weitere Eingriffe äusserer Kräfte in Bewegung gesetzt werden muss: von ihrer Struktur hängt es ab, welchen Effekt diese äusseren Kräfte an ihr bewirken. Es würde einen sehr niedrigen Horizont wissenschaftlicher Bildung verrathen, in diesem Vergleich eine Herabsetzung des Organismus sehen zu wollen, denn in einer Maschine, wenn auch nur von Menschenhänden gemacht, liegt das Resultat tiefsten und sorgfältigsten Nachdenkens und hoher Intelligenz, soweit es ihre Struktur betrifft, und wirksam sind in ihr schliesslich dieselben Naturkräfte, welche in anderer Combination die Lebenskräfte eines Organs darstellen. Die Vergleichung des organischen Lebens mit unorganischen Processen kann nur dann als eine Erniedrigung des ersteren gelten, wenn man so thöricht gewesen ist, die letzteren als etwas Niedriges und Gemeines aufzufassen, während die unbegreifliche Grösse und Durchgeistigung der Natur in beiden Fällen sich gleichartig offenbart" (*Vorlesungen über Pflanzenphysiologie*). The time may be far distant when these views of the great botanist shall be universally accepted; but they will, I think, sooner or later prevail.

The little known territory which separates the domains of chemistry and physiology will, in my opinion, offer a wide and interesting field for research, after that of pure chemistry shall have been exhausted or lost its interest. Most important problems connected with life and its relation to the inorganic world there await solution, and I confess that I am inclined to envy the young investigator who, coming provided with an ample store of chemical and physical knowledge, shall apply himself to the solution of these problems. The pleasures derived from the successful pursuit of such studies belong to the highest and purest that we are able to conceive. I can, however, only repeat what has so often been said before, and what the young man of science should not forget, that a life devoted to research only involves no material rewards; it certainly never secures wealth, sometimes not even honour nor fame. Looked on with indifference or even dislike by the State, the Church, and the public at large, all that the man of science can certainly look forward to at the close of his career is the addition at his hands of a few stones to the vast edifice of truth, and the consciousness of having attained a higher stage of intellectual insight.

You may probably expect me, before I conclude, to make some reference to technological matters, to the various chemical arts and manufactures for which the Manchester district is noted. At the last meeting of the British Association in Manchester a report on the condition at that time of manufacturing chemistry in the South Lancashire district, by Sir Henry Roscoe, the late Dr. Angus Smith, and myself, was laid before the Chemical Section. A similar report showing the progress made in chemical technology since that time would have been interesting. Great changes have taken place during the period that has elapsed, especially as regards the alkali trade, and quite a new branch of industry has been developed, that of the coal-tar colours. A description of these new features of our chemical industry with statistics of production would therefore have been acceptable. The idea of a report had however to be given up on account of the difficulty of obtaining reliable information as to details, and in these matters it is the details only which are interesting, the general features of the subject being well known. It can hardly be a matter for surprise, I think, that our manufacturers, considering the active competition to which they are exposed, and the disadvantages under which they labour in consequence of the exclusiveness of foreign nations, should be loth to furnish information which would benefit their rivals in trade. Several interesting papers on branches of chemical industry by gentlemen well versed in them will, however, be read before the Section, and these will, to a great extent, make up for the want of a general report. In the Chemical Section of our Jubilee Exhibition, too, you will see a very fine collection of chemical products, more extensive and beautiful, perhaps, than any previously brought together, and these will give you a good idea of our industrial activity. It would have been interesting to witness step by step some of the processes employed in the manufacture of these various products, but this, I am sorry to say, must not be expected generally.

To some it may seem that this Jubilee Exhibition shows the manufacturing industry and prosperity of this district at least at their highest state of development; that they are now at their meridian, and in the future are doomed to decline. If this be so—and there are certainly indications which seem to favour this view—it would be well for those whose visits here are only occasional to take especial note of the present state of things so as to be able to compare their impressions when they next visit

us with those now received, since gradual changes in communities, as in individuals, are more patent to casual observers than to those who are always on the watch.

From some points of view the signs of the times are certainly not encouraging. It should not be forgotten that the manufacturing prosperity of this district depends to a great extent on the ample supply of a product which is brought to us at some cost from tropical and semi-tropical countries to be re-exported in the shape of manufactured goods. A political convulsion abroad, and this, unfortunately, is a casualty that may at any time be expected, or even the determination on the part of other nations to starve us out, however short-sighted such a determination might be, might cut off our supplies and disable us permanently as we were partially disabled twenty-five years ago. If to this be added the fact that foreign nations are becoming increasingly hostile and exclusive commercially, we cannot feel surprise at the dismal forebodings entertained and the confident predictions of decline uttered by some who claim to know all the facts. I ought to apologize for alluding to so gloomy a subject on the occasion of this to a great extent festive gathering, but then men of science like to look at a question not only from a hopeful but from every point of view. Fortunately on this question they are not called upon to pronounce any opinion one way or the other.

Should this be the last time that Manchester shall entertain the British Association in the day of its prosperity, I can only say with the German poet—

"'Schliesst den Kreis und leert die Flaschen]
Diese Sommernächte feiernd,
Schlimme Zeiten werden kommen,
Die wir Auch sodann ertragen."

Whether in prosperity or adversity I feel sure that this city will always endeavour to entertain its visitors to the best of its ability. On the present occasion I may with confidence on the part of the chemical world of Manchester offer to the many friends from near and far who honour us with their presence at this meeting a most hearty welcome.

SECTION C.

GEOLOGY.

Opening Address by Henry Woodward, LL.D., F.R.S., V.P.G.S., President of the Section.

Since I received the friendly intimation from Prof. Bonney your distinguished and able President of last year, that the Council of this Association had done me the honour to select me to occupy the Presidential chair of this Section which he had vacated, I have been greatly exercised as to what subject to choose for the brief address with which it has now become customary to open the session. Not that there is any lack of materials ready to hand for the purpose—on the contrary, the subjects embraced by geology are now so varied and extensive that the effort to focus them in a single mind is ever becoming a more difficult task to accomplish, and demands the literary skill of a Lyell or a Geikie to marshal and arrange them from year to year in a manner suitable for presentation to you at our annual gathering.

Foremost in interest must necessarily be that which relates to our home affairs, and in this I have been most kindly favoured by Dr. A. Geikie, the Director-General of the Geological Survey of Great Britain, who sends me a brief notice of the progress of the Survey for 1886, taken partly from his Annual Report as Director-General and partly from information supplied by the Office through the kindness of Mr. William Topley, our Recording Secretary. The following is the statement which I have received :—

The survey of the solid geology of England and Wales was completed at the end of 1883, and the field staff has since been occupied in surveying the drift deposits, making at the same time such revisions of the ordinary (solid) geology as may be necessary. In the north and east of England the drift and solid have been surveyed at the same time. The areas examined in the earlier days of the survey, in the south, centre, and west of England, and in Wales, were done for the solid rocks only.

In order to meet the great need for a general map of England and Wales on a moderate scale, one is being engraved by the Survey on the scale of 4 miles to 1 inch (1 : 253440), and will be issued in fifteen sheets.

A few of the survey memoirs relate to large areas, and give complete descriptions of the formations therein exposed, but most of the memoirs are explanations of special sheets of the map. A series of monographs is now in preparation giving full descriptions of special formations. Mr. Whitaker has charge of that on the Lower Tertiaries; Mr. H. B. Woodward and Mr. C. Fox-Strangways are preparing the Jurassic memoir, the former taking the rocks south of the Humber, and the latter those of Yorkshire; Mr. Jukes-Browne is writing the Cretaceous monograph; and Mr. Clement Reid that on the Pliocene beds.

In Scotland some advance has been made in mapping the important and complicated area of the north-west Highlands. The surveyors there were chiefly engaged between Loch Stack and Ullapool, subsequently completing the area about Durness and Eriboll. The other parts of Scotland now being surveyed are the north-eastern and western side of the Grampians, all south of the latter having been already completed.

Ireland is entirely surveyed with the exception of a small area in Donegal, which will probably be completed this year. This district is of interest from its resemblance to the north-west Highlands, and from the problems which it presents as to the origin of the crystalline schists. The recent discovery of organic remains amongst the Donegal schists adds additional interest to this inquiry.

The publications of the Survey during the past year are as follows: England and Wales, six sheets of the map, two sheets of horizontal sections, three of vertical sections, and six memoirs; Scotland, three maps and one memoir; Ireland, two maps and six memoirs.

The next matter which has arisen since our last meeting relates to our colonies, and comes to us in the shape of a message from the retiring President of the Association, Sir William Dawson, who has embodied his ideas in a letter to the President of the Royal Society (Prof. Stokes), copies of which have been sent also to all the learned Societies. To the former I am indebted for a copy accompanied by a favourable report thereon from the Royal Society of Canada.

As the object of this communication is one in which I am sure we, as Englishmen, must all feel a hearty sympathy, appealing as it does to our patriotism in its widest sense, as well as to our devotion for and interest in the science of geology, I feel I shall not need to apologize for introducing it to your notice here.

We are invited by it to enrol ourselves, as geologists, in a Federal Union, composed of all our brethren at home, in our colonies, and in all the dependencies of the British Crown. Nor are we to stop here, for when this has been satisfactorily accomplished it is suggested that we should invite our English-speaking cousins of the great United States, with whom we are already in such close alliance upon so many objects of common scientific interest, to join our Geological Confederation, and, having thus obtained an overwhelming majority, we are to proceed—without armies or vessels of war—to extend our peaceful conquest over every country on the habitable globe, urging and persuading those countries which have not established geological surveys to do so forthwith, and inviting those which have surveys of their own to join our British Association Geological Union. And when all has been accomplished in this direction our exertions as a confederacy may well be extended to secure the mapping of all those outlying regions of the earth's surface at present imperfectly known or still geologically unexplored.

Suggestions such as these could hardly come at a more fitting and appropriate moment, for are we not now on the eve of the completion of the geological surveys of the British Islands? if such a task can ever be said to be completed which has occupied the attentive study of so many able geologists during the last eighty years or more, and from the very nature of the case must always require additional research and revision.

India, Africa, and our colonies may all hope for future assistance from the many geological students now being trained in our schools and colleges, who may not be required in the near future for home surveys, and must needs go further afield to win their title of admission to the ancient and honourable order of "Knights of the Hammer."

This idea of scientific federation was referred to by Prof. Huxley in his Presidential Address to the Royal Society in 1885, and subsequently by the present President (Prof. G. G. Stokes) in November last.

If we would devise a scheme by which we might from time to

time, recognize in a suitable manner—whether by corresponding membership, or honorary fellowship, or by medals and awards—as Prof. Huxley has suggested, the good scientific work being done by members of the many Societies in our distant colonies of Canada, South Africa, Australia, New Zealand, and elsewhere, that would indeed be a step in the right direction, and would doubtless prove most helpful and encouraging to all our fellow geologists abroad.

The Geological Society of London, no doubt, to some extent covers this ground; but it should be noticed that in the view of this Society, our colonies and other dependencies are not, and I think rightly, recognized as foreigners, that designation being employed for those who are not in any sense subjects of the Queen.

As a consequence, the geologists of our colonies are not looked upon as eligible for honorary connexion with the Geological Society, and though in the distribution of the medals and awards their work is no doubt noticed, yet that is now so important and extensive that it might be desirable to secure for it a more specific and extensive recognition than has hitherto perhaps been possible.

Might we not also through the home influence we could bring to bear by means of this great Section of the British Association succeed in inducing our practical colonial Governments to see the enormous commercial as well as scientific gain that must eventually accrue to themselves if they would, with wise liberality, continue to completion their much-needed geological surveys, instead of (as has too often happened) abandoning the work before its end has been attained, or making its maintenance from year to year contingent on the chance discovery of gold, or the successful boring for coal or water—results not always to be attained within twelve months by a geologist in a new country, however good he may be, unless he have a fairy godmother or a divining-rod at his command?

If by means of our confederation such useful and helpful works can be inaugurated, we shall have fulfilled an object well worthy the initiation of Sir William Dawson, and of all those whose names may be connected with so laudable an undertaking.

Nor need such a development of the work of this Section interfere in any way with the labours of the "International Geological Congress," which occupies a distinct field of its own; for whatever we might accomplish in carrying out the suggestions put forward by Sir William Dawson would really be in effect to second and support—not to hinder—the work of that most useful body of geologists.

Our next topic relates to foreign affairs.

The International Geological Congress, which met in Bologna in 1881, and in Berlin in 1885, will hold its next meeting in London in 1888. This year the Committee of the Congress on Geological Nomenclature will meet during the Association week at Manchester. Prof. Capellini, of Bologna, is the President of this Committee, and Prof. Dewalque, of Liége, is the Secretary. Its object is to discuss various questions respecting the classification and nomenclature of European rocks, and to report thereon to the Congress in London.

It is quite certain that a large number of Continental and American geologists will be present in London next year, and it rests with English geologists to determine whether the meeting shall be as successful as those which have preceded it. The Berlin Congress left the arrangement in the hands of a small Committee of English members (Messrs. Blanford, Geikie, Hughes, and Topley), and advantage will probably be taken of the presence of so many geologists in Manchester to further the organization of the English meeting.

The occasion of the Congress visiting London next year should also be a sufficient reason to enlist new members here, and it is to be hoped that a very cordial reception will be accorded to all those who come from abroad to attend the meeting. It ought to be a great success, and deserves our warmest sympathies and co-operation.

Geology seems, at present, to be passing through what may not inaptly be termed a transitional or metamorphic period in its history, when old-established ideas are rapidly melting away, and under fresh influences are crystallizing out into quite other forms.

"New lights for old" is the popular cry both in science and politics, and, like the Athenians, nothing delights us more than to hear tell of some new thing.

If the proposition lately made by Prof. Judd, the President of the Geological Society in London, in his recently delivered Anniversary Address, holds good, that mineralogy is the father of geology, it seems not improbable that, like Saturn's offspring, our science is in danger of being devoured by its reputed parent; for certainly mineralogy, in the form of petrology, has of late years most largely occupied the geological field, whilst palæontology, once the favourite child of geology, is in its turn threatened with imminent extinction, as a separate study, by biology, which, without any substantial gain, now replaces, *in name only*, the hitherto better known sciences of botany and zoology.

Indeed, could the views so eloquently put forward by Prof. Judd be maintained, mineralogy itself would have to be added to the list of sciences included under biology. But notwithstanding the well-known aphorism of Linnæus—

> "Lapides crescunt; Vegetabilia crescunt et vivunt;
> Animalia crescunt, vivunt, et sentiunt"—

the growth of the first is of a totally different nature from that which takes place in the last.

Minerals, or more properly crystals, increase or grow in size by additions to their *external surfaces* of molecules of matter identical with themselves. They are therefore as a rule homogeneous throughout, almost rigid, and remain under ordinary circumstances unchanged irrespective of time.

Plants and animals, on the contrary, increase by intussusception, or the taking of matter within their tissues. Their bodies are *not homogeneous*, and they exhibit all the various phenomena of growth and decay.

We stand, then, still like "watchers on the threshold," not yet admitted beyond the veil. We are not prepared to include minerals in the study of living beings, nor are we, I submit, any nearer the solution of the problem, What is life? whether we call it "*vitality*" or "*vital force*," nor can we produce it like "electricity" or "electrical force" by the aid of mechanics. That it has existed ever since our planet became habitable by living organisms is beyond doubt; and since life first dawned it seems equally certain that this "vital force" was never at any time extinguished, but, like the sacred flame of Iran, its light has always gladdened our earth with its presence.

I have already referred to the vastness and diversity of the domain which Geology claims as her own; indeed, we might, if so disposed, pursue our subject in its *cosmical aspect*, and, inviting the astronomer and the physicist to our aid, proceed to consider the evolution of our earth and its subsequent history as a part of the solar system.

Or, taking up *geognosy*, we might inquire into the materials of the earth's substance and the chief rocks and minerals of which its crust is built up.

Should *dynamics* charm us, then we may study the various agencies by which rocks have been formed and altered, and the frequent changes in relation to sea and land which the terrestrial surface has undergone in former times.

Does *rock-architecture* attract us? It is ours to inquire how the various materials of the earth came to be arranged as we find them—whether wrought by living agents, or ejected by volcanic forces, or laid down quietly by water.

Or is *chronology* the object of our study? Then our task will be to investigate the well-marked succession of the stratified rocks and the sequence of events which they record.

Again, we might prefer the *physiographical aspect of geology*, embracing the history of the features of the earth and the causes which have brought about its varied conditions of continent and ocean, of mountain and valley, hill and plain, making up that grand diversity of surface which constitutes its scenery.

Yet more, it is within our domain as geologists to investigate *the past life of the globe* through all its successive changes and to trace it from its earliest dawn in pre-Cambrian times down to its grand development at the present day.

One result of the very vastness of this kingdom is that there is a tendency amongst its subjects to form into separate constituencies, and these in an incredibly short time evolve languages of their own, so that, unless this fissiparity can be successfully arrested, we shall speedily repeat the story of "the confusion of tongues," and our geological tower, which once promised by our combined labours to reach grandly heavenwards, may soon cease from building altogether.

This incoherence in our body politic may, I think, be traced to that great development by the microscope in mineralogical geology and petrology, which has no doubt been necessitated by the investigation of those remote pre-Cambrian or Archæan rock-masses in the north-west Highlands, Shropshire, the Malverns,

South Wales, Cornwall, and the west of Ireland, whose fossils, if they ever existed, have been entirely obliterated[1] by the changes which their matrix has undergone, and whose very stratification has been lost by metamorphic action. In such investigations some of our ablest geologists have now been for long occupied with the best possible results, and Bonney, Callaway, Cole, Davies, Geikie, Hicks, Hull, Judd, Lapworth, Peach, Sorby, Teall, and many others have been labouring most zealously on these most ancient sediments, barren though they be of life forms, and often destitute of bedding.

It is refreshing, however, to find Prof. Judd at times abandoning volcanoes, and turning his attention most successfully to lizard-hunting with Prof. Huxley in the Elgin sandstones, or studying the micro-organisms in the cores from the Richmond boring or the valley of the Nile; to see Dr. Hicks leaving his patron St. David far behind, and digging for bones in the pre-Glacial caves at St. Asaph. Prof. Lapworth, too, we see avoiding Cape Wrath, and discoursing on the beauties of Canadian Graptolites and the Cambrian rocks at Nuneaton.

Thus there is still a bond of union connecting stratigraphical geology and palæontology and a common ground of interest whereon all geologists may meet. It should then be our endeavour not to dissociate ourselves or our interest from any subject of geological inquiry, but to maintain the union between all branches of our science and with all workers in whatever field they may labour, adopting for our motto the ancient maxim, "*Vis unita fortior est.*"

Especially should we adhere to the study of palæontology, seeing that it is indissolubly connected with one of the earliest chapters in the history of our science. Indeed, through the evidence afforded by organic remains, William Smith (better known by the title given to him by Prof. Sedgwick, "the father of English geology") was led to those remarkable generalizations as to the identification of strata by means of their contained fossils, which have exercised so great an influence over our own science during the past ninety years, and is still the guiding principle on which our classification of the sedimentary rocks is based. What Wollaston has done for mineralogy and crystallography, William Smith initiated for stratigraphical geology; and we cannot overlook our obligation to Smith whilst we reverence the work of his distinguished contemporary, Wollaston.

Palæontology, or the study of ancient life forms, stands somewhat in relation to geology as the science of archæology does to history, or as zoology and botany to physical geography. But, whereas the investigator of recent living forms deals with entire organisms and can study both their morphological and their physiological history as well as their geographical range, the palæontologist has too often to deal with imperfect remains, many of which have no exact modern representative, and has, in consequence, to look for and seize upon minute characters for his guidance, which the worker on recent forms would probably neglect as too trivial for even specific diagnosis.

The palæontologist, if he would succeed, must in fact be a trained zoologist or botanist, as the case may be, and an accomplished geologist also; such combinations of qualities like those possessed by the earlier race of "naturalists" are less frequently to be met with at the present day. They represent amongst us the same class of men as the "general practitioner" does in medicine; they are the all-round good scientific men, but not "specialists."

Biology, or the study of living things, has now become so vast a field that everyone is compelled to take up some special subject, and in striving to master it he makes his reputation as an authority on this or that group of organisms.

There is much to be said in favour of such a method of working, but I hold that everyone who so elects to spend his life must first of all pass through a thorough grounding in general biology, and should on no account take up special work until he has mastered thoroughly the general principles of scientific classification and the various types of organized beings, otherwise he will be for ever viewing all Nature with distorted vision, seeing, in fact, "men as trees walking." If as a student he shall have been nurtured wholly on the anatomy of the *sole*, all objects will be viewed from the stand-point of that one-sided fish. If the cockroach has engrossed his youthful studies, all nature will swarm with *Periplaneta orientalis*.

We have to guard against the starting of *student-specialists*. They must begin by being "general practitioners" if they are ever to do any good in the world of science, and after serving their time in a museum or elsewhere, then by all means let each follow his own "bent" and devote himself to some particular group, as did Davidson to the *Brachiopoda*, to the exclusion of all else.

It is the absence of "all-roundness" which has retarded more than any other thing the constant interchange of ideas between zoologists, botanists, and palæontologists, without which science languishes. Biologists as a body do not care to look at or study fossils; they see neither form nor beauty in the petrified fragments of a plant or animal such as would induce them to study these more closely, and they turn to the exquisitely perfect specimens of recent objects in their cabinets with a sigh of relief. But *Nemesis* is at hand created by our modern system of extreme biological training. The student of to-day is averse to the systematic work of both zoology and palæontology in our museums, and, technically inclined, craves for nothing so much as to be allowed to embed some interesting embryo in paraffin and cut it into 10,000 slices.

As a consequence our museums will suffer unless we can revive amongst our students a taste for and a love of general natural history; such, we mean, as the *taste for Nature* which excited the enthusiasm of Charles Kingsley and stimulated the zeal of Charles Darwin. We cannot all sail round the world as did Banks and Solander, Darwin, Huxley, Hooker, Wyville Thomson, Moseley, and so many other naturalists, though the mere act of travelling has now become so ridiculously easy that our own Association awoke one morning in Montreal, and may, for aught we know, find itself some day in Sydney or Melbourne! But we can fully appreciate Nature in a dredging expedition or feel her influence on a moor or mountain, in a quarry or down a mine.

What we want for our students in these high-pressure days are less frequent attendance in the examination room and a more frequent examination of Nature in the field. Our professors must take their men more often afield, and show them how to collect specimens and familiarize them with the aspects of natural objects as seen *without microscopes*, and they will return to their studies with far better and keener eyesight after their own *macroscopic* vision has been enlarged by contact with Nature.

Whoever then takes up the study of fossils must also be well acquainted with the structure of living animals and plants; he may also be expected to go on adding to his store of biological knowledge—but as some division of labour is absolutely essential, the man who pursues palæontological research must be prepared to concentrate all his energies to the elucidation of these extinct organisms, studying, but not occupying himself in describing, recent forms.

In order, however, to work satisfactorily at any particular group of extinct organisms, his eyes and his understanding must go through a long and careful training before he will be able to interpret correctly the appearances presented by the specimen before him, and to avoid the fallacies by which he is liable to be misled, arising out of the necessarily imperfect materials and their different modes of preservation in the matrix.

He must learn to distinguish between a suture and a fracture, and to know when a specimen has been distorted by cleavage or other mechanical cause, or altered by mere difference of mineralization. Such deceptive appearances have too often led to the multiplication of species, and even the creation of spurious genera.

Thus occupied in the investigation of ancient life forms, he will in truth be only writing the first chapters on the botany or the zoology of the earth, and whilst his carefully obtained results are of the greatest importance to the speculations and conclusions of the geologist they are equally essential to and a part of biological science.

My friend Dr. Traquair has recently thus expressed, in relation to his own subject, what I have attempted to make more general:—"The man who satisfactorily investigates the structure or determines the systematic position of a fish or reptile *preserved in stone* is as much a zoologist as he who describes a similar creature *preserved in spirits*, though with this difference, that the former task is in some points rather the more difficult, seeing that we have *only the hard parts* to go upon, and these generally in a crushed, fragmentary, or scattered condition. And," he adds, "without a genuine interest in, as well as a thorough knowledge of, recent biology, no one can hope to produce work of any value in palæontology."

[1] Traces of fossils are said to have been met with in Donegal, and I have just received evidence of Trilobites in the Upper Green Llanberis slates at Penrhyn, hitherto considered unfossiliferous!

Of course the value of all palæontological work, as of all zoological or botanical work, must depend entirely upon the care and exactness with which the work is performed.

Time, the great assessor of all human labours, will sit in judgment upon them and pronounce by their durability or instability the comparative value of each.

It appears to me that to the careful palæontological worker, as to the careful archæologist, the greatest merit is due if he succeed correctly in deciphering the too often fragmentary and blurred remains of a bygone age, and giving us in the present an accurate interpretation of a page from the life-history of the past.

Then, too, there is the geological aspect of palæontology. And here I may state that one of the charges made by a brother zoologist against us is "that we use fossils *merely* as counters by which to record the progress of geological time."

As well might exception be taken that the milestones along a turnpike road had been used by a traveller to calculate the length of his journey.

But omitting the word *merely* (for fossils have been made to give up many secrets to the investigator besides their age), I gladly accept the charge as conveying a great and important truth.

Do not the historian and the antiquary use the coins and medals dug from the ruins of the dead and long past dynasties of the world as sure guides in the chronology of the human period?

And may not the geologist also use "the medals of creation" as Dr. Mantell aptly called them—coined in no counterfeit mint, as the best and most trustworthy guides to enable him to establish the chronology of the stratified rocks of the earth?

Great then as is the benefit which zoology has derived from palæontology in enabling the zoologist to learn the earliest appearance in time of each group of organisms, and the modifications in structure, so far as we are enabled to ascertain them, which each may have undergone from the ancient to the modern period—it may be doubted whether even this valuable aid equals the service performed to stratigraphical geology by the careful study of organic remains—in enabling us to write the chronology of the rocks over so large a portion of the habitable globe.

Without fossils stratigraphical geology would be as unsatisfactory as it would certainly be uninteresting; with their aid it becomes, both in the field and in the cabinet, one of the most attractive and delightful of studies.

Owing to the very nature of sedimentary deposits, being of necessity either lacustrine, estuarine, or marine in origin, our knowledge of the ancient land surfaces of the globe is necessarily very limited, but we know much concerning its old marine areas. These are the more constant and widespread, and it is mainly upon these deposits, and not so much upon the more limited evidences of ancient land surfaces, that our chronology has been based.

Of the antiquity of cave-folk and their contemporary Mammalia we may expect to hear the very latest utterances from Prof. Boyd Dawkins and Dr. Hicks. The former is also to be congratulated upon his renewed work on the Mammalia in the Palæontographical Society's volume for 1885 (just issued). Prof. O. C. Marsh has added a further contribution to American palæontology in the shape of a memoir describing and figuring sixteen new species of Mesozoic mammals from the Upper Jurassic rocks in Wyoming, on the western slope of the Rocky Mountains. Mr. Lydekker has just completed Part V. of his most useful and much-needed Catalogue of the Fossil Mammalia in the British Museum, containing the Sirenia, Cetacea, Edentata, Marsupialia, and Monotremata.

The fossil birds remain to be catalogued. In the Reptilia it is refreshing to see Prof. Huxley once more taking up the pen and writing upon *Hyperodapedon* and *Rhynchosaurus* in his old vigorous and earnest style. We can only regret that his health precludes him from continuous labour, to the no small loss of science. Prof. Marsh shortly promises us his memoir on the Sauropoda, the plates of which are progressing rapidly to completion.

Our late veteran chief, Sir Richard Owen, although retired from active official duties, contributes a paper on *Galesaurus planiceps*, a Triassic saurian from South Africa, and a further memoir on *Meiolania* from Lord Howe Island.

Prof. Seeley and M. Louis Dollo are both occupied with Dinosauria, the former from the Cape (whence he has also detected part of a mammalian skeleton in the Triassic rocks), and the latter is adding to our knowledge of Iguanodon and other forms from the Wealden of Bernissart.

In the Amphibia, Prof. Dr. Herman Credner has added a most valuable paper on the development of *Branchiosaurus*, a small Labyrinthodont from the Keuper of Saxony, in which he has been able successfully to trace the development through a long series of individuals of a water-breathing naked larva of the Palæozoic epoch into an air-breathing adult form, clad in a strong coat of mail.

In fossil ichthyology, A. Wettstein has been occupied in the study of the Eocene fishes of the Glarus slates, and in his recent memoir he shows that out of one fish (*Anenchelum*), so constantly distorted by slaty cleavage, Agassiz had made no fewer than six species. The fish is now found to be identical with the living "scabbard-fish," *Lepidopus*; and the author reduces the forty-four species of Glarus fishes to twenty-three, and adds four new ones. Among the latter is the first fossil *Remora* yet met with, named *Echeneis glaronensis*. Its first dorsal is modified as a sucker, exactly as in the living *Remora*.

Baron Zigno, of Padua, has figured and described the first entire *Myliobatis* hitherto discovered in the Eocene of Monte Bolca.

M. Louis Dollo records the occurrence of two skeletons of *Carcharodon heterodon* in the Eocene of Boom, Antwerp, one measuring 7 metres, and the other nearly 9 metres in length. They are now mounted and exhibited in the Brussels Museum.

Mr. J. F. Whiteaves is commencing to publish the detailed descriptions of the Devonian fishes from Scaumenac Bay, Quebec.

Mr. James Wm. Davis, of Halifax, has produced a second monograph for the Royal Dublin Society. The first, which appeared in 1883, was devoted to the teeth and spines of Elasmobranch fishes from the Carboniferous limestone of Ireland; the present monograph, illustrated by twenty-four plates, is devoted to the description of the fishes of the Cretaceous rocks of the Lebanon, and makes us acquainted with a wonderful series of Selachian fishes, representing nine genera and sixteen species, of which two genera and twelve species are new to science. The Ganoids comprise two species of Pycnodonts and two forms related to Amia; there are also a number of Teleostean fishes, amongst which are *Pagellus*, *Beryx*, *Homonotus*, *Platax*, and many other genera. Two species of eel, *Anguilla*, are the first Mesozoic examples recorded. Altogether we have ten genera and sixty-three species of fish recorded as new. The author is to be congratulated upon having contributed to fossil ichthyology one of the most extensive works published in recent years.

Mr. Arthur Smith Woodward (a former student of Owens College, Manchester) has this year also contributed numerous papers on fossil fishes: on *Ptychodus* from the Chalk; *Squaloraja* from the Lias; on the Brazilian genus *Rhacolepis*; on a Maltese *Holocentrum*; "On some Eocene Siluroid Fishes from Bracklesham"; and "On the Canal-System in the Shields of Pteraspidean Fishes."

Mr. E. T. Newton describes a *Semionotus* from the Trias of Warwickshire.

Both Mr. James W. Davis and Dr. R. H. Traquair have given us descriptions of the anatomy of *Chondrosteus acipenseroides* from the Lias of Lyme Regis.

Mr. William Davies describes two species of *Pholidophorus* from the Purbeck beds of Swanage, Dorset.

But the groups which have proved of the greatest service in the chronology of the sedimentary rocks have been the Mollusca, the Brachiopoda, and Crustacea (especially the Trilobita, Phyllopoda, and Ostracoda), the Echinodermata, Corals, Graptolites, Sponges, and Foraminifera.

It would be an interminable task merely to record the workers in the various sections of palæontology, but in glancing at these one cannot prevent many illustrious names arising in one's mind—many who have finished their work, and are reckoned among the fathers of the science, but many also who are still our companions, and from whom we may expect further important help before they lay down their hammer, their lens, and their pen.

In the Cephalopoda the task so lately left by our countryman Dr. Wright, after a long life devoted to palæontological science, has been taken up by Mr. S. S. Buckman, who has already presented one fasciculus of a monograph on the Ammonitidæ of the Inferior Oolite.

The Gasteropoda of the Oolites have an able historian in Mr. W. H. Hudleston, whose contributions on this subject enrich the pages and plates of the *Geological Magazine* and the Proceedings of the Geologists' Association; the Palæozoic forms are in the hands of Dr. Lindström.

The Lamellibranchiata cry for help at present in vain, and we regret more than ever the loss of Stoliczska, who promised such good work had his life been spared.

The Brachiopoda, so long and so well cared for by Dr. Davidson, now also demand a successor to that illustrious name.

The Polyzoa, which suffered so severe a loss in the death of Mr. Busk, have since been well cared for by Mr. Arthur W. Waters and Mr. Vine.

Until quite lately, the oldest fossil insects known were the six fragments of wings of Neuroptera, from the Devonian of New Brunswick, obtained by Mr. C. F. Hartt and described by Mr. S. H. Scudder. More lately the wing of a cockroach has been obtained from rocks of Silurian age in Calvados, France; whilst almost simultaneously fossil scorpions have been met with by Dr. Hunter, of Carluke, in the Upper Silurian of Lanark, and determined by Mr. B. N. Peach, and from the Upper Silurian of Gotland, described by Dr. Lindström.

These discoveries carry back our records of old land surfaces to a far more remote period than that of the Coal-measures, vast as its distance is removed from recent times.

Mr. B. N. Peach is the discover of several scorpions, and I have also recently figured and described three new forms of cockroach and several spined myriapods from the Coal-measures. Another cockroach, also new, which has been kindly sent me for study by Mr. Peach, brings to our knowledge a larval stage of *Blatta* from the Scottish Carboniferous.

Dr. McCoy has just added a genus of spiders, *Atypus*, to our [illegible] beds from the Isle of Wight.

[illegible] Crustacea have found in Mr. B. N. Peach and in Prof. [illegible]rt Jones able and willing historians. Mr. Peach has taken the [illegible] Carboniferous Macrouran Decapods, and Prof. Rupert [illegible]nes the Palæozoic Phyllopoda, aided by myself; Prof. Jones is attacking the Tertiary and Cretaceous as well as the Palæozoic Ostracoda, so that his hands will be full for many years to come.

The Echinodermata have lost Dr. T. Wright, who for years acted as their monographer in the Palæontographical Society's volumes, but they have secured the services of other accomplished naturalists. Mr. Robert Etheridge, Jun., and Dr. P. Herbert Carpenter have produced a grand monograph on the Blastoidea in the British Museum; and no doubt this is but the beginning of good things to come, for although Mr. Etheridge has entered upon a new sphere of work in the Australian Museum, Sydney, Dr. P. Herbert Carpenter hopes to take up the stalked Crinoids before long, and Mr. Percy Sladen, who, with Prof. P. Martin Duncan, has already done so much good work amongst the Indian Echinoderms and elsewhere, promises to take the star-fishes in hand for us later on.

The Corals have many friends, chief amongst whom is Prof. P. Martin Duncan, and Prof. H. A. Nicholson, and various other excellent workers, but they are even a more difficult and a less attractive group than the Echinodermata, and their determination is not so satisfactory owing to their irregular and heteromorphic growth.

The Stromatoporoids have lost an investigator in the field in Arthur Champernowne, whose unexpected and early loss we all deplore. But in Prof. Nicholson they will find a most careful and painstaking monographer, who has already given us one fine instalment of his work in the Palæontographical volume.

In Prof. C. Lapworth we have an exponent of the structures and affinities of the Graptolites as a class and of their stratigraphical position in the rocks unsurpassed by any other worker. With him must be associated the names of Barrande, Carruthers, Hopkinson, Nicholson, and a long list of foreign workers, all of whom, however, look upon Lapworth as the highest authority in this group.

In the Spongida we are especially indebted to Dr. G. J. Hinde, first for an excellent well illustrated quarto catalogue of these organisms in the geological collection of the British Museum, and secondly for the Palæozoic part of a fine monograph of these for the Palæontographical volume just issued.

Nor must we omit to recall the names of Prof. Zittel, of Dr. Carter, of Prof. Sollas, and many other able workers in the fossil sponges.

In the Foraminifera we naturally recall the names of D'Orbigny, D'Archiac, Carpenter, Parker, Brady and Jones, and Sir William Dawson, our illustrious ex-President. Prof. Rupert Jones is still at work on this group, and has recently published a paper on *Nummulites elegans* from the Eocene beds of Hampshire and the Isle of Wight.

Of late years fossil botany, too long neglected, has taken a place of note in all those inquiries concerning the origin of floras, the age of the stratified rocks, the former distribution of land surfaces, and especially in all questions relative to the climate of the globe in past times.

Passing over the earlier period of the present century, when fossil botany was known only by the works of Artis, Witham, Schlotheim, Sternberg, Goeppert, Cotta, Lindley and Hutton, Steinhauer and Adolphe Brongniart, we have to recall the names of other workers who have only passed away in our own time, such as Binney, Bunbury, Corda, Bowerbank, Heer, Unger, Schimper, and Massalongo.

In the period of fifty years, whose completion we have just celebrated, the names of our countrymen Binney, Bowerbank, Williamson, and Hooker, stand prominently forward contemporarily with those of Geinitz, Unger, Rossmasler, and Schimper, in Germany. In 1845 Dawson and Lesquereux entered the field in America, Hooker in England, and one of the ablest writers on fossil plants, Oswald Heer, entered upon his great work in Switzerland. In 1850 Massalongo in Italy, and von Ettingshausen in Austria, were added to the roll of famous palæobotanists, and in 1853 Newberry joined the American field of research. In 1860 the work so long abandoned by Brongniart, in France, was taken up by de Saporta, and it is no small gratification to have him with us here to-day, and to welcome him amongst our distinguished foreign guests.

About the same time my friend and colleague William Carruthers commenced to write on fossil botany, and brought to bear upon the subject that accurate and careful knowledge of living forms without which such investigations must always prove but futile.

It is extremely difficult to estimate the number of species of fossil plants that had been described up to the year 1837, but it probably fell far short of a thousand. In 1828 less than 500 species were known to Brongniart.

In the first edition of "Morris's Catalogue," published in 1843, the number of British fossil plants recorded is 628.

Careful lists were published by Goeppert and by Unger in 1844 and 1845, giving a total of known species from 1600 to 1800.

In 1849 the number had increased, according to Bronn's "Index Palæontologicus" to over 2000, and the following year Unger enumerated 2421 in his "Genera et Species Plantarum," rather more than 500 of which may have been British. In 1852 Morris (2nd edition) gives the number of species as 750. Since then, chiefly through the labours of Heer, Ettingshausen, Lesquereux, Massalongo, Unger, and de Saporta, this number has been more than quadrupled. Mr. Gardner estimates that at least nine thousand species must have been described. This great increase is chiefly due to the more careful exploration of the Tertiary strata, in which the more highly organized and consequently more differentiated plant forms occur.

The number of plant remains described in Great Britain during the whole fifty years has been extremely small, but much has been accomplished in the study of fossil plants generally, and in this task no one has been more earnest than Prof. Williamson, of Owens College, Manchester.

His investigations of the plants of the Coal period have been of the most exhaustive nature, and from his researches into their microscopic structures we are almost as well acquainted with the minute tissues of these ancient denizens of the forests of the Carboniferous epoch as we are with those in the parks around Manchester to-day.

Mr. Carruthers's "Memoirs on the Coniferæ and Cycadeæ, and on the Fruiting Organs of the Lycopodiaceæ" have greatly advanced our knowledge of these interesting types, heretofore but imperfectly known from their fossil remains.

Mr. R. Kidston has devoted himself most earnestly to the investigation of the fossil plants of our British coal-fields, and he has determined not to rest satisfied merely to work out the plants obtained by others in our museums, but he has visited all our coal-fields and searched the shales on the spot for himself. The results of his collectings may now be seen in the valuable additions made to the Coal-measure series of plants in the British Museum (Natural History).

But it is more especially in reference to the Tertiary flora of Britain that progress has been made of late years.

Thanks to the labours of Mr. Starkie Gardner, who has not only obtained abundant materials for an exhaustive monograph

with his own hands from Sheppey, Alum Bay, Bournemouth, Reading, Mull, Antrim, and many other localities, but has already favoured us with several memoirs in the Palæontographical Society's annual volumes and elsewhere on the British Eocene flora, we may hope before long to have a more complete history at this period of our islands than we already possess of the flora of the Carboniferous age.

Nor has any research, favoured by the aid of this Association, brought so large a return in beautiful and instructive specimens to our National Museum of Natural History as have the investigations carried out by Mr. J. S. Gardner.

We must not omit to mention Mr. Clement Reid, who has so diligently traced many of the specimens of our existing flora in the Pleistocene strata of the eastern counties.

"Large numbers of ferns and gymnosperms," says Mr. Gardner, "have been discovered in Mesozoic rocks, but remains of the interesting monocotyledons which must have accompanied them are provokingly scarce. We know that palms, grasses, &c., appear at certain definite horizons, but we are ignorant regarding their ancestry. We know that temperate floras, largely composed of dicotyledons, flourished as far north as man has been able to penetrate, in the Cretaceous and Tertiary periods, but nothing in the least suggesting a transitional form has been found amongst them. Lastly, we have learnt that floras now indigenous to Japan and the Himalayas, to Australia and South America, once inhabited Europe, groups of wholly different plants succeeding and displacing each other in such rapid succession on the same spot as to suggest that the normal condition of floras is one of slow but perpetual migration, and that the term 'indigenous' has no geological significance."

In reference to the question of geographical distribution of organized beings in geological time, the conclusion is strongly forced upon us, from a study of fossil remains, that the great zoological provinces into which the earth's surface and the seas of the globe are now subdivided have been brought about by the limitation of species at no more distant date than the Secondary period, and probably even later than this.

That in Palæozoic times there must have been a great uniformity of marine conditions, and the fauna of each of the primary formations was consequently not only of vast duration but of world-wide extent, is evident.

When, as in Carboniferous times, we are enabled to study the contemporary land conditions of the globe, we find they must also have been very uniform, at least so far as the explored parts of this hemisphere are known, both the fauna and the flora at this epoch being co-extensive with the northern hemisphere, indeed, in all probability far wider, seeing that identical species occur in the Carboniferous series of Australia and North America. Even those well-marked lines which at present follow more or less closely the isotherms of our hemisphere seem not to have exercised the same influence on the fauna and flora as they do at present. Thus in high northern latitudes and within the Arctic Circle we find abundant evidence of life in Palæozoic, Mesozoic, and even down to Tertiary times, unaffected by latitude; so that we are justified in assuming that a far milder temperature extended to much higher northern regions than that which at present exists on the globe, and consequently that a larger portion of the earth's surface (as well as its seas) was then habitable.

How great, then, is the field of research still open to our investigation, and how far distant must that day be ere the last problem shall have been solved, and the last chapter written, in the ancient life-history of our earth!

"We write in sand, our labour grows,
And with the tide the work o'erflows."

With unskilled hand I have struck here and there only a few chords on the many-toned harmonicon of geology. I fear they may not all have vibrated quite in unison as a perfect composition would; but, however crude the performance has been, I trust that it will not be provocative of discord. If some few ideas suggest themselves as worthy of your acceptance, I shall not have spoken altogether idly, nor you have listened so long and so patiently entirely in vain.

NOTES.

DR. EMIL HOLUB has arrived in Europe, after three years of adventurous exploration in Africa, and although he brings with him only a part of his scientific collections—the rest having been plundered by the Mashukulumbe, a tribe to the north of the Zambesi—yet this fragment is certain to prove of great scientific value. It includes over 2000 specimens of birds, 27,000 of insects, and 6500 of plants. Dr. Holub brings with him also many hundred observations in meteorology and measurements of all nations. The collection will be exhibited in Europe; and it is stated that, if circumstances enable them to do so, Dr. and Mrs. Holub will resume their African explorations. They are expected to arrive in Vienna on the 15th or 16th inst., and will be officially received by the Austro-Hungarian Exploration Society.

A SLIGHT shock of earthquake was felt at Bonn and in its vicinity at fifty-two minutes past four on Monday afternoon, accompanied by a dull subterranean rumbling.

THE Government of India has sanctioned the purchase for the Lahore Museum of a zoological collection illustrative of Indian silk culture.

M. PAVIE, who undertook an adventurous journey from Siam into Tonquin, starting from Luang Prabang, has been compelled to return to Siam, having been driven back by bands of Chinese, who are described as ravaging the country.

MR. J. B. LILLIE MACKAY, of the Royal School of Mines, and at present Lecturer in Chemistry at Trinity College, Melbourne University, has been appointed Director of the School of Mines, Sandhurst, Victoria.

MR. OMOND, the Superintendent of the Ben Nevis Observatory, replying in the *Times* to criticisms in Parliament of the work of the Observatory, to which we referred in a note last week points out that the report of the Meteorological Office, on which these criticisms were based, merely states that Mr. Omond's telegrams were useless for a particular purpose, viz. forecasting storms. He then proceeds to state the position of the Observatory in the matter as follows:—"When the Ben Nevis Observatory was opened at the close of 1883, the directors offered to send the Meteorological Office daily weather telegrams—the only satisfactory way in which observations can be used for forecasting. The Office declined this offer, mainly on the ground of expense, but asked that occasional messages might be sent when any unusual or interesting events occurred, adding that these messages might be sent more frequently at first. I had some difficulty in understanding why the Office wished a record of sudden changes to be sent by wire instead of waiting till they got them in the ordinary way by post, but considered that the best way was to follow their instructions literally and send telegrams recording sudden changes or unusual occurrences, with a due regard to economy and with gradually diminishing frequency. A reiterated request for economy at the time of the introduction of sixpenny messages led to the entries sent being usually restricted to two hours' reading of the various instruments—one before the change in question had set in and one after it was fairly established. Though most sudden changes are connected with storms, yet a few of the messages recorded changes from bad to good weather, especially when the occasional great dryness characteristic of high-level stations began suddenly or in any unusual manner. How a Committee, 'composed of men of the very highest scientific standing,' had come to regard these messages, and especially this last sort, as 'storm-warnings' passes my comprehension. The question of the value to meteorological science of the Ben Nevis observations I may safely leave to abler hands—it needs no vindication of mine; but I must protest against the unfairness to me and the other members of the Ben Nevis Observatory staff of first asking for records of changes which have occurred, and then declaring them useless because they are not also forecasts of what is to be. It should be noted, however, that in two cases the record from Ben Nevis arrived at the

Meteorological Office before the warning from the sea-level stations—a fact which proves, all the more conclusively because unintentionally, the great value Ben Nevis would have if properly utilized as a forecasting station by means of daily reports sent to some one able both to interpret their indications and to compare them with similar daily reports from low-level stations all over the country. The former of these conditions can only be fulfilled by those who have studied the Ben Nevis records of the last three years and a half, while the latter in this country necessarily is limited to the Meteorological Office, which alone receives such daily telegrams. It is to this unfortunate hiatus and not to any defect in the position of Ben Nevis that the alleged uselessness for practical forecasting of its observations is due." Mr. Omond's vigorous defence is therefore the same in substance as that of Mr. Buchan which we reproduced last week. It amounts to this: the Observatory is blamed for not doing certain work which it offered to do, but which the Meteorological Office refused to permit it to do; Mr. Omond did what he was asked, and did not do that which the Meteorological Council refused to have from him on the score of expense.

THE Committee of the International Geological Congress dealing with the question of geological nomenclature is holding its meeting in Manchester at the same time as the British Association. The representatives at present in Manchester are Prof. G. Capellini, Rector of the University of Bologna, and representative of Italy (President); Prof. Dewalque, Belgium (Secretary); Prof. O. Torell, Sweden; Prof. Vilanova-y-Piera, Spain; Dr. T. Sterry Hunt, Canada; Dr. W. T. Blanford, India; and Prof. T. McK. Hughes, England. Meetings of the Committee, at which the foregoing were present, were held in the Committee-room of the Geological Section on Tuesday and Wednesday last week, at which Mr. J. E. Marr and Mr. W. Topley, of the English International Committee, were also present. The object of this Committee of the Congress is to discover the nomenclature and classification adopted by different authorities and in different countries, with the view of bringing their views into harmony, and also to lay down rules for the guidance of geological workers in the future. The subject chiefly discussed has been the classification of the Cambrian and Silurian rocks, some reference having also been made to the question of the Archæan rocks. The full Congress will meet in England next year, when these and other similar matters will be considered. Another meeting of the Congress Committee was held on Friday.

THE autumn meetings of the Iron and Steel Institute are announced to take place this year at Manchester. They will be held at Owens College, the use of which has been granted by the Governors for the occasion, and will begin on the morning of September 14. The programme is a very comprehensive one viewed from a metallurgical and manufacturing point of view, while the more strictly holiday features of such a gathering have not been neglected. The President of the Institute (Mr. Daniel Adamson), will give a paper on "Machines for the Testing of Metals," a subject to which he has devoted a great deal of attention. Mr. Thomas Ashbury is down for a paper on "Recent Metallurgical and Mechanical Progress, as illustrated at the Manchester Exhibition," and as that Exhibition, so far as relates to machinery in motion, is probably the best that has ever been held, and illustrates with unusual completeness every department of the engineering art, such a paper can scarcely fail to be an instructive and valuable record. A third paper will be read by Mr. James Johnson, on "The Mechanical Apparatus for Continuous Moulding at the Works of M. Godin, at Guise," the interest of which will be of a two-fold character—first, as regards the processes and apparatus to be described, which are of a novel and improved character; and, secondly, as regards the system of co-operation which has been adopted there on a more complete and successful scale as between employers and employed than probably in any other part of Europe. Dr. Fleming, of University College, London, will contribute a paper on "Electric Light Installations for Works and Factories," a subject with reference to which he has had a very large amount of experience as the consulting engineer for the Edison-Swan and other companies. The manufacture of ordnance, respecting which there has been so much discussion in military and political circles of late, will be brought forward by Capt. Cubillo, who occupies a responsible position at the Royal Arsenal of Trubia, in Spain. Capt. Cubillo has studied the conditions of the manufacture of ordnance at all the leading arsenals both in this country and abroad, and as the Spanish Government has recently exhibited a strong disposition to be abreast of the world in naval and military affairs, this paper is likely not only to show what they have so far achieved, but also to bring to the front comparisons with reference to ordnance that will probably be extremely useful and interesting at the present time. One other paper, that by Mr. Wailes, of the Patent Shaft and Axle-tree Company, at Wednesbury, promises to give to the world some extremely interesting data respecting the recent progress of the basic process for the manufacture of steel on the open hearth. Hitherto, as is well known, the progress of this system, both on the Continent and in England, has been chiefly in the direction of Bessemer working, but recently the open hearth has come into competition with the Bessemer process for this purpose, and Mr. Wailes's paper will probably lead to a discussion as to the comparative merits of the two systems which will be of value both to producers and consumers of steel.

THE *Times* Correspondent in Philadelphia telegraphed on Monday night that an International Medical Congress was formally opened by President Cleveland at Albaugh's Opera House in Washington on Monday. Five thousand physicians are in attendance, including over 2000 delegates sent here from nearly all parts of the globe. One of those present, Dr. Fanny Dickinson, of Chicago, is the first woman who has been admitted to an international medical gathering as delegate. Many of the most distinguished doctors of the day were present. An address of welcome was delivered by Mr. Bayard, Secretary of State. The welcome was acknowledged and responded to by Dr. William Harris Lloyd, Inspector-General R.N., on behalf of Great Britain; Dr. Léon Laforte, of Paris, on behalf of France; Prof. P. G. Unna, of Hamburg, on the part of Germany; Senator M. Semmola, of Naples, for Italy; and Dr. Charles Reyber, of St. Petersburg, representing the Government of Russia.

THE centenary of the first ascent of Mont Blanc was celebrated at Chamounix on the 28th ult., when a monument for which the various Alpine and tourist clubs of the world, as well as the Paris Academy of Sciences, contributed the funds, was at the same time unveiled to De Saussure, who made the ascent on August 28, 1787, with the guide Jacques Balmat. The monument is of bronze on a granite pedestal. The principal feature of the procession with which the proceedings opened was the band of old guides, forty in number, and all over seventy years of age. M. Spuller, French Minister of Education, unveiled the statue, and delivered an oration in honour of De Saussure.

CONSIDERABLE additions have recently been made to our knowledge of those interesting substances, the chemical products of the action of Bacteria upon the animal body, called ptomaines. In the last number of the *Berichte* of the German Chemical Society, p. 2217, Dr. Ladenburg clears up completely all doubt as to the composition of one of the best known of the ptomaines, cadaverin, which he shows is perfectly identical with

artificially prepared penta-methylene-diamine. A short time ago Dr. Bocklisch published (*Ber.* 1887, p. 1441) the results of his researches upon the products of the action of Finkler's bacillus (*Vibrio proteus*) upon sterilized flesh, showing that this bacillus decomposes flesh with formation of the alkaloid cadaverin, $C_5H_{14}N_2$, which is non-poisonous, and ammonia. On repeating his experiments, however, in presence of ordinary putrefaction germs in addition to the Finkler bacilli, he made the remarkable discovery that an entirely different base, methyl-guanidine, of intensely poisonous properties, was the chief product; hence the symptoms of particular diseases may be due to the poisonous alkaloids formed by the joint action of specific bacilli and ordinary putrefaction germs. Bocklisch made several analyses of the cadaverin which he obtained in the first series of experiments, due to the action of pure cultivations of the *Vibrio proteus*, and showed that its hydrochloride forms a crystalline compound with mercuric chloride of the composition $C_5H_{14}N_2$. $2HCl.4HgCl_2$, and as this differed somewhat from the composition formerly assigned to the artificial preparation by Ladenburg, the subject was involved in some doubt. Happily, Ladenburg has made fresh and purer preparations of his penta-methylene-diamine, and finds that its compound with mercuric chloride has precisely the composition assigned to the double chloride of mercury and cadaverin by Bocklisch. Hence cadaverin is conclusively proved to be none other than penta-methylene-diamine, and, consequently, must be added to the list of products of animal life which have been synthesized. The formation of these alkaloids, during disease or after death, has a most important bearing upon the treatment of cases of suspected poisoning, inasmuch as, whether poisonous or not, their reactions differ very little from those of the deadly alkaloids; and in the interests of justice it is to be hoped that our knowledge of this branch of organic chemistry may soon be rendered as complete as possible.

MESSRS. MACMILLAN announce the following scientific works for the forthcoming publishing season:—"Electricity and Magnetism," by Amédée Guillemin, translated and edited, with additions and notes, by Prof. Silvanus P. Thompson; "The Nervous System and the Mind," by Charles Mercier; "Popular Lectures and Addresses on Various Subjects in Physical Science," by Sir William Thomson; "Radiant Light and Heat," by Balfour Stewart, F.R.S. (the last three belonging to the Nature Series); "Kinematics and Dynamics," by J. G. Macgregor; "Geometrical Conics," by A. Cockshott and Rev. F. B. Walters; "A Treatise on Analytical Statics," by I. Todhunter, F.R.S., a new edition, revised by Prof. J. D. Everett, F.R.S.; "A Key to Mr. Todhunter's Conic Sections," by C. W. Bourne; "A Key to some Examples in Messrs. Jones and Cheyne's Algebraical Exercises," by Rev. W. Failes; "A Key to Mr. Lock's 'Arithmetic for Schools,'" by the Rev. R. G. Watson; "A Companion to 'Weekly Problem Papers,'" by the Rev. John J. Milne; "A Key to Dr. Todhunter's Treatise on the Differential Calculus," by H. St. J. Hunter; "A Treatise on Chemistry," by Sir H. E. Roscoe, F.R.S., and C. Schorlemmer, F.R.S. (Vol. IV. Part I.); "Algebra for Schools and Colleges," by Charles Smith; "The Elements of Chemistry," by Ira Remsen; "Absolute Measurements in Electricity and Magnetism," by Andrew Gray; "A Practical Text-Book of Pathology," by D. J. Hamilton; "A Course of Quantitative Mineral Analysis for Students," by W. Noel Hartley, F.R.S.; "School Course of Practical Physics," by Prof. Balfour Stewart, F.R.S., and W. W. Haldane Gee (Part I. "Electricity and Magnetism); "Examples in Physics," by D. E. Jones; "Inorganic and Organic Chemistry," by Sir Henry E. Roscoe, F.R.S., and Prof. C. Schorlemmer, F.R.S. (Vol. III. "Organic Chemistry," Part IV.); "Greenland," by Baron A. E. von Nordenskjöld; and "Corea," by W. A. Carles.

MESSRS. SWAN SONNENSCHEIN AND CO.'s announcements of new books include the following works:—"The Microscope," edited from the work of Profs. Naegeli and Schwendener, by Frank Crisp and J. Mayall, Jun.; "Animal Biology," by Adam Sedgwick; "British Fishes," by F. A. Skuse; "Mammalia," by F. A. Skuse; "Reptiles," by Catherine Hopley; "Ants and Bees," by W. Harcourt Bath (the last four in the Young Collector Series); "The Solomon Islands and their Natives" and "The Geology and Physical Characteristics of the Solomon Islands," with maps, by Dr. H. B. Guppy.

MESSRS. LONGMANS announce the following works of scientific interest:—"The Literary Remains of Fleeming Jenkin, F.R.SS.L. and E., late Professor of Engineering in the University of Edinburgh," edited by Sidney Colvin, with a Memoir by Robert Louis Stevenson; "Picturesque New Guinea," by J. W. Lindt; "A Manual of Operative Surgery, having Special Reference to many of the Newer Procedures," by Arthur E. J. Barker; "A Course of Lectures on Electricity, delivered before the Society of Arts," by George Forbes, F.R.S.

MESSRS. KEGAN PAUL AND CO., London, and Messrs. Appleton and Co., New York, will publish shortly the Hon. Ralph Abercromby's work on "Weather" as a number of the International Science Series. This will be the first book in the English language which deals exclusively with the causes of weather changes from day to day, as distinguished from the climatic or statistical treatment of the subject. There will be ninety-nine charts and diagrams, of which a considerable number will relate to the United States, and others to India and Australia, so as to illustrate the nature of weather on the widest possible basis.

THE additions to the Zoological Society's Gardens during the past week include a Pig-tailed Monkey (*Macacus nemestrinus*) from Sumatra, presented by Mr. B. Lynch; a Fettered Cat (*Felis maniculata*), a Spotted Eagle Owl (*Bubo maculosus*), a Hoary Snake (*Coronella cana*), four Spotted Slowworms (*Acontias meleagris*) from South Africa, presented by Dr. E. Holub, C.M.Z.S.; a Carrion Crow (*Corvus corone*), British, presented by Mrs. MacLochlin; a Martinique Gallinule (*Ionornis martinicus*), captured at sea, presented by Mr. R. Drane; two African Lepidosirens (*Protopterus annectens*) from the River Gambia, West Africa, presented by Mr. H. H. Lee; a Malabar Parrakeet (*Palæornis columboides* ♂) from Southern India, a Malaccan Parrakeet (*Palæornis longicauda* ♂) from Malacca, a Laughing Kingfisher (*Dacelo gigantea*) from Australia, deposited; a Tiger Bittern (*Tigrisoma brasiliense*) from Brazil, purchased; a Red-faced Ouakari (*Brachyurus rubicundus* ♀) from the Upper Amazons, received in exchange; a Collared Fruit Bat (*Cynonycteris collaris*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

BROOKS'S COMET.—Mr. H. V. Egbert, from observations made on August 26, 28, and 30, has computed the following elements for the comet discovered by Mr. Brooks on August 24:—

T = 1887 October 6·48d. G.M.T.

$\pi - \Omega = 63\ 18$
$\Omega = 84\ 33$ } Mean Eq. 1887·0.
$i = 44\ 10$
$\log q = 0\cdot08718$

It will be seen that these elements bear a great resemblance to those of Olbers' comet of 1815. Dr. Holetschek, in a supplemental circular of the *Astronomische Nachrichten*, supplies the subjoined ephemeris for the comet, basing it upon the sweeping ephemeris for Olbers' comet given by Ginzel in the *Astronomische Nachrichten*, No. 2696, the comet's orbit being assumed

to be a parabola for the sake of simplicity, and the following observation made by Herr Palisa at Vienna being used :—

August 27, 15h. 27m. 12s. Vienna M.T.
R.A. = 8h. 42m. 55·71s., Decl. 29° 34′ 24″·7 N.

Ephemeris for Berlin Midnight.

1887.	R.A. h. m.	Decl. ° ′	Log r.	Log Δ.
September 5 ...	9 21·1 ...	30 16 N. ...	0·120 ...	0·311
9 ...	9 39·7 ...	30 20 ...	0·112 ...	0·304
13 ...	9 58·8 ...	30 16 ...	0·105 ...	0·297
17 ...	10 18·4 ...	30 2 N. ...	0·099 ...	0·291

THE MORRISON OBSERVATORY.—The first number of the publications of the Morrison Observatory, Glasgow, Missouri, U.S.A., has just appeared. This Observatory was founded, in 1875, by the liberality of Miss Berenice Morrison, and possesses an equatorial refractor by Alvan Clark, of 12¼ inches aperture, and a transit-circle by Troughton and Simms, with objective of 6 inches aperture and 77 inches focal length, the circles being 24 inches in diameter. In this first volume Prof. C. W. Pritchett, the Director, gives a history and description of the Observatory, with an account of the determination of the longitude and latitude of the meridian pier, besides a selection of such observations and notes made at the Observatory as are likely to be of use to astronomers. These latter include measures of double stars, observations of occultations, of the transit of Mercury, 1878, measures of the diameter of Mars, observations of comets, of Jupiter and Saturn, and of the figure and dimensions of Uranus. Prof. Pritchett's work appears to have been seriously crippled through lack of means, and, considering the excellent use which he has made of the resources at his command, it is to be hoped that he may speedily find himself in a position to carry on the operations of the Observatory on a more extended scale.

NEW OBSERVATORY AT JUVISY.—The current number of *L'Astronomie* contains a description of a new Observatory belonging to M. Camille Flammarion, which has just been completed. An admirer of M. Flammarion had presented him some five years ago with a little chateau and park situated on the road from Paris to Fontainebleau of historic name and interest. The house, which was built in 1730, possessed walls so thick and solid as to serve as a perfectly stable base for the equatorial and dome with which M. Flammarion has surmounted it. The dome is 5 m. in interior diameter, and covers an equatorial by Bardou of 0·24 m. aperture and 3·75 m. focal length, with clockwork by Bréguet, furnished with a Villarceau governor. Two smaller telescopes—one by Secretan of 108 mm. aperture, the other by Foucault of 160 mm., stand on the adjoining terrace. The Observatory, the co-ordinates of which are East longitude from Paris oh. om. 8s., N. latitude 48° 41′ 36″, commands an uninterrupted horizon, and an atmosphere noticeably purer than that of Paris.

THE TOTAL SOLAR ECLIPSE OF AUGUST 19.—We learn from the current number of *Ciel et Terre* that M. Niesten, of the Brussels Observatory, was fairly successful in his observations of the eclipse. It had been his intention to push on as far east as Perm, but a delay in the arrival of his instruments led him to accompany M. Belopolsky to Jurjewitz on the Volga. The sky was cloudy here as at most of the other stations, but cleared a little round the sun at the time of totality, and M. Niesten was able to see the chromosphere and prominences, and the appendices of the corona, and his assistant secured eight photographs, of which six were good. The exposures varied from ·8 seconds to half a minute; the chromosphere and prominences were shown on all, and two gave traces of the corona and also of Regulus, which was near the sun. M. Karinne, a Moscow photographer of the same station, also secured several photographs. A drawing which M. Niesten made of the corona showed a strongly-marked coronal ray, about a degree in length, in the direction of the solar equator.

MINOR PLANET No. 267.—M. Charlois, of Nice, who discovered this object, has named it Tirza.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 SEPTEMBER 11–17.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on September 11

Sun rises, 5h. 30m.; souths, 11h. 56m. 36·1s.; sets, 18h. 24m.; decl. on meridian, 4° 35′ N.: Sidereal Time at Sunset, 17h. 46m.

Moon (New, September 17, 14h.) rises, 22h. 27m.*; souths, 6h. 22m.; sets, 14h. 22m.; decl. on meridian, 19° 12′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Decl. on meridian. ° ′
Mercury ...	5 28 ...	12 1 ...	18 34 ...	5 48 N.
Venus	7 23 ...	12 40 ...	17 57 ...	9 9 S.
Mars	1 43 ...	9 29 ...	17 15 ...	18 55 N.
Jupiter... ...	9 46 ...	14 48 ...	19 50 ...	12 1 S.
Saturn... ...	1 8 ...	9 0 ...	16 52 ...	19 47 N.

* Indicates that the rising is that of the preceding evening.

Sept. h.
14 ... 2 ... Saturn in conjunction with and 1° 39′ north of the Moon.
14 ... 18 ... Mars in conjunction with and 1° 48′ north of the Moon.
17 ... 16 ... Venus in conjunction with and 12° 50′ south of the Moon.
17 ... 22 ... Mercury in conjunction with and 2° 33′ south of the Moon.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
Algol	3 0·8 ...	40 31 N. ...	Sept. 11,	2 19	*m*
			,, 13,	23 8	*m*
λ Tauri...	3 54·4 ...	12 10 N. ...	,, 11,	3 6	*m*
			,, 15,	1 59	*m*
δ Libræ	14 54·9 ...	8 4 S. ...	,, 12,	4 5	*m*
			,, 16,	19 48	*m*
U Coronæ	15 13·6 ...	32 4 N. ...	,, 16,	2 34	*m*
R Draconis	16 32·4 ...	67 0 N. ...	,, 12,		*m*
U Ophiuchi... ...	17 10·8 ...	1 20 N. ...	,, 11,	22 34	*m*
		and at intervals of		20 8	
X Sagittarii... ...	17 40·5 ...	27 47 S. ...	Sept. 14,	23 0	*m*
W Sagittarii ...	17 57·8 ...	29 35 S. ...	,, 12,	4 0	*M*
U Sagittarii... ...	18 25·2 ...	19 12 S. ...	,, 14,	0 0	*m*
R Aquilæ	19 0·9 ...	8 4 N. ...	,, 16,		*M*
η Aquilæ	19 46·7 ...	0 43 N. ...	,, 14,	4 0	*M*
W Cygni	21 31·8 ...	44 52 N. ...	,, 11,		*M*
δ Cephei	22 25·0 ...	57 50 N. ...	,, 19,	2 0	*M*
			,, 15,	20 0	*m*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
Near χ′ Orionis... ...	88° ...	18 N. ...	Very swift; streaks.
,, 50 Aurigæ ...	98 ...	43 N. ...	Very swift; streaks.
,, α Lyræ	282 ...	42 N. ...	Swift; bright; long.
,, γ Piscium... ...	346 ...	0 N. ...	Slow; bright.

SCIENTIFIC SERIALS.

THE most interesting item of information in the *Journal of Botany* for August is the record of an addition to the flowering plants of Great Britain, in the discovery, by Mr. H. C. Hart, of the Arctic *Arabis alpina* in Skye.—Mr. Tokutaro Ito, has an interesting paper on the history of botany in Japan.

Rendiconti del Reale Istituto Lombardo, June 30.—On the normal derivatives of the potential function of surfaces, by G. Morera. This paper forms a supplement to the author's late communication (*Rendiconti*, vol. xx. Part 8) on the derivatives of the potential function of space. The extremely simple analytical method by which he succeeded in determining general conditions for the existence of those derivatives and their effective expressions has also enabled him to solve the analogous question regarding the normal derivatives of the potential function of surfaces.—On the part played by sensuous images on the development and exercise of the reasoning faculty, by Tito Vignoli. In this paper the author investigates the actual form and genesis of perceptions acquired through the senses, from the standpoint of their efficacy in developing and sharpening the intelligence of animals. The subject is treated comparatively, it being impossible to understand any act or fact of human psychology unless studied in connexion with similar manifestations in

other animals. The general conclusion is arrived at that the impression communicated through the senses is the true instrument of intellectual progress, and that in it lies the potentiality of abstract science. Pure geometry, arithmetic, and algebra are merely the last term of abstract simplification reached by the sensuous perception in its intrinsic evolution.

In the *Nuovo Giornale Botanico Italiano* for July, Sig. P. Voglino publishes critical remarks on a number of species of Fungi belonging to the Agaricini; and Prof. Caruel gives his Annual Report of the Botanical Museum of Florence for the year 1885-86.—Prof. Delpino discusses the chemical and physiological equation of the process of alcoholic fermentation, which he considers to be more simple than it has been regarded by recent writers. Succinic acid and glycerin, which are found in the liquid after fermentation, he believes to be only secondary products of the process, which consists in the simple removal from grape-sugar of a portion of its carbon and oxygen, and its consequent reduction to the constitution of alcohol.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, August 29.—M. Hervé Mangon in the chair.—On tornadoes in the United States, by M. H. Faye. Some observations are made in connexion with the popular work on tornadoes recently issued by Mr. Finley, of the United States Signal Service. In reply to that writer, M. Faye maintains that tornadoes are not ascending but descending movements, being whirlwinds with vertical axes due to the different velocities of moderately elevated atmospheric currents, which, like the eddies in running waters, always descend till arrested by the resistance of the ground. They penetrate like a cork-screw through the lower strata, continually contracting and tapering to a point owing to the increasing pressure of these lower strata. Their progressive movement, mainly towards the north-east, is due to the upper currents where they take their rise, and whose mean velocity and direction they retain. Their ravages are due to the violent shock of the descending spirals against the obstructions of the ground, and their fury is not spent or diminished in overcoming these obstacles, because the source of their energy is always in the upper regions, where it is constantly renewed and transmitted to the earth by the descending motion. It is further shown that the United States comply more than any other region of the globe with the conditions most favourable for the development of these destructive cyclones.—Observations of Barnard's comet, May 12, 1887, made at the Observatory of Bordeaux with the 0·38 m. equatorial, by MM. G. Rayet and Flamme.—Determination of the longitude of Haiphong, Tonquin, by telegraphy, by M. F. La Porte. Its longitude was for the first time determined in 1874 by MM. Héraud and Bouillet, who deduced it from that of Saigon. But at the beginning of this year the meridian of Haiphong was connected with that of Hong Kong by means of the submarine cable, and from the observations taken at both extremities a mean was obtained for the cathedral of Hong Kong of 7h. 27m. 20·43s., and for the Observatory of Haiphong, 6h. 57m. 22·63s. east of Paris. For the latter point Héraud's chronometric observations had given 6h. 57m. 19·8s.—Note on a projection saccharimeter, by M. Léon Laurent. The saccharimeters already constructed by the author are of two types: the rotatory polarimeter, requiring monochromatic light; and the compensating saccharimeter, more specially adapted for sugar, and using ordinary light. The present apparatus, of which a sectional view is given, has the advantage of being adapted for use with the electric light now so generally employed in large scientific establishments.—Experiments in agricultural chemistry, by M. J. Raulin. The experiments here described were carried out last year at the agricultural station of the Rhone. Their special object was to ascertain how the disturbing influences due to the varying fertility of the soil may best be obviated. The land being disposed in three equal plots, A, B, C, the extremes A and C are treated identically, while B serves as the point of comparison for the special circumstance under consideration. Normally the fertility increases or diminishes with a certain uniformity from A to C, so that half of the joint yield of A + C would be equal to that of B if the three plots were subjected to the same treatment. The cause of error due to the inequality of the soil being thus for the most part removed, the relation of the yield of $\frac{A + C}{2}$ to that of B will express the actual influence of the circumstance under consideration. In an experiment carried on according to this method, the superphosphate and precipitated phosphate used with Dattel wheat gave a very decided increase, while the result of the application of fossil phosphates and scoriæ was somewhat doubtful.—Note on the waterspout of August 19 on the Lake of Geneva, by M. Ch. Dufour. This waterspout, formed by the collision of the west and the *vaudaire* or south wind, immediately disappeared on reaching the shore half a mile west of the Rivaz railway-station on the Swiss side. From the data supplied by various observers the author calculates its height at about 106 metres. It churned up the surface of the lake, producing an effect somewhat like that of paddle-steamers, but did no damage of any kind on the land.—M. R. Guérin presented a note on a process by means of which the question of the lunar atmosphere might be elucidated. He remarked that the diurnal motion of the moon, owing to its proper motion, is not the same as that of a star. Hence, under certain conditions, a photographic lunette would give to our satellite a sharp edge, and for a star in the neighbourhood of this edge a luminous streak. It therefore seems certain that, however attenuated may be the lunar atmosphere, the photogenic conditions will be changed at the point of contact of the two heavenly bodies, and that the streak made by the image of the star should show some trace of this change.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Animal Alkaloids: Sir W. Aitken (Lewis).—City and Guilds of London Institute Programme of Technological Examinations, Session 1887-88.—First Steps in Geometry: R. A. Proctor (Longmans).—Easy Lessons in the Differential Calculus: R. A. Proctor (Longmans).—Australian Museum: Report of Trustees.—A Treatise on the Animal Alkaloids: A. M. Brown (Baillière).—Hydrophobia: R. Suzor (Chatto and Windus).—The Glasgow and West of Scotland Technical College Calendar for the year 1887-88 (Anderson, Glasgow).—Elementos de Calculo de los Cuaterniones: V. Balbin (Buenos Aires).—Les Plantes des Champs et des Bois: G. Bonnier (Baillière, Paris).—Bench Book for Test-Tube Work in Chemistry: H. T. Lilley (Hamilton).—Notes upon the History of Floods in the River Darling: H. C. Russell.—Notes upon Floods in Lake George: H. C. Russell.—Results of Rain and River Observations made in New South Wales and part of Queensland, 1886: H. C. Russell (Sydney).—Journal of Chemical Society, September (Gurney and Jackson).

CONTENTS.

	PAGE
Hydrophobia	43
Popular Books on Birds. By **Prof. R. Bowdler Sharpe**	435
Our Book Shelf:—	
Colenso: "First Lessons in Science"	436
Letters to the Editor:—	
On the Constant P in Observations of Terrestrial Magnetism.—**William Ellis**	436
The Svastika on English Walls.—The Solar Eclipse of August 19.—**H. G. Madan**	437
Large Meteor.—**W. F. Denning**	437
Colliery Explosions and Atmospheric Pressure.—**Hy. Harries**	437
Measurement of Specific Heat. By **George N. Huntly.** (*Illustrated*)	438
The Hessian Fly. By **Miss Eleanor A. Ormerod**	439
The British Association	439
Section B.—Chemical Science.—Opening Address by **Edward Schunck, Ph.D., F.R.S., F.C.S.**, President of the Section	
Section C.—Geology.—Opening Address by **Henry Woodward, LL.D., F.R.S., V.P.G.S.**, President of the Section	447
Notes	452
Our Astronomical Column:—	
Brooks's Comet	454
The Morrison Observatory	455
New Observatory at Juvisy	455
The Total Solar Eclipse of August 19	455
Minor Planet No. 267	455
Astronomical Phenomena for the Week 1887 September 11-17	455
Scientific Serials	455
Societies and Academies	456
Books, Pamphlets, and Serials Received	456

THURSDAY, OCTOBER 6, 1887.

ALPHITA.

Alphita. A Medico-Botanical Glossary from the Bodleian Manuscript Selden B 35. Edited by J. L. G. Mowat, M.A., Fellow of Pembroke College. [*Anecdota Oxoniensia.* Mediæval and Modern Series. Vol. I. Part 2.] (Oxford: Clarendon Press, 1887.)

THIS interesting vocabulary, which Mr. Mowat has transcribed and edited from a manuscript in the Bodleian Library, is offered by him as a contribution to the study of English plant-names. To explain why it has this and also other claims to attention, we must say a word about the class of literature to which it belongs.

When ancient Greek science was first brought to the knowledge of mediæval Europe it was by means of Latin versions of the Greek writers, made not directly, but at second hand from Arabic versions written or brought into Europe by the Moors. The earliest known Latin translations of certain works of Galen, Hippocrates, and other medical writers, with probably some of Aristotle, originated in this way. On the basis of these versions, which began in the eleventh century with the writings of Constantine the African, a medical literature grew up, containing many Greek words corrupted by passing through an Arabic channel, as well as Arabic and some Latin terms hardly less strange to the mediæval reader. It is clear that these hard words presented great difficulties, not as a matter of language only, but of practical use, since it was difficult for the reader to identify the diseases spoken of and the drugs recommended for their cure.

To remedy the uncertainties and dangers thus arising, a new class of literature sprang up—that of the writers whom we may perhaps call the synonymists or glossarists—who compiled lists of the hard words occurring in medical works of the Arabian school, with explanations in Latin. The most celebrated though not the earliest of these was Simon of Genoa, whose list of medical synonyms, the "Clavus Sanationis," was largely borrowed from by subsequent writers. Several others might be named, but there are also anonymous collections of the same kind, one of which is the vocabulary or glossary known as "Alphita."

The anonymous character of this production is not merely a matter of accident. In its present form it is clearly not the work of one writer. A vocabulary or glossary originally intended to explain some work of practical medicine (possibly the "Antidotarium Nicolai," as Mr. Mowat suggests) was expanded by matter introduced from many sources, and by the work of many hands, till at length it could only be regarded as a sort of joint-stock compilation to which no one man's name could be attached.

The title under which it goes is not explained (so far as we can see) by Mr. Mowat, and therefore we may say that it is merely the first word of one of the glosses or definitions, "Alphita, farina ordei idem," or "ἄλφιτον—the same thing as barley-meal."

This definition happening to come first in an older form of the glossary was taken as its title. It has been reprinted under this title in the "Collectio Salernitana" of De Renzi, and another Bodleian manuscript (Ashmole, 1398) giving what appears to be an abridged form of that now published is also thus headed.

But what has all this to do with English plant-names? Merely this, that when scholars or scribes in Northern Europe copied or edited these glossaries (most if not all of which were produced in Italy) they often added the French or English vernacular names of plants. Hence these glossaries form a supplement to the earlier lists of names published by Prof. Earle in his valuable "English Plant Names." There is no reason to suppose that these names were contemporary with the original composition; we may rather assign them to the date of the manuscript, which Mr. Mowat refers to the fifteenth century. They thus form a connecting-link between the Anglo-Saxon and Old English names of the earlier lists, and those which we find in Gerarde and the printed "Herbals" which preceded his. Some of these are very interesting, and have been elucidated with much skill in Mr. Mowat's most laborious and valuable notes; the corrupt and barbarous forms of the Greek and Latin words making them often difficult of recognition.

The interest of the work then lies in the preservation of a number of plant-names; and it is worth inquiring first of all in what way the English names have been identified with their classical equivalents. Sometimes the modern name is a mere corruption of the ancient, as in rose, bugloss, tansy, and numbers more. Sometimes the one is a translation of the other; hound's tongue, coltsfoot, cranesbill, are familiar instances. But when a name was thus altered or translated it did not follow that the plant was identified. A curious instance of the confusion which may arise is the following.

Eleutropia, or elitropia, evidently represents the Greek ἡλιοτρόπιον = heliotropium, and the Latin equivalents *Solseqium* and *Sponsa solis* have the same meaning, viz. a flower which turns to the sun; and an Anglo-Saxon glossarist (quoted by Earle) boldly translates the Latin as *Sigel hweorfa* (turning to the sun). It might still, however, remain doubtful what flower was meant; but when we find Calendula used as a synonym of Solseqium, and when we read in "Alphita" (p. 88), "*Kalendula, sponsa solis, incuba* idem, *Anglice* goldwurt vel rodes," we see that the marigold, a common garden flower in the Middle Ages, and known as golde, gold wort, rode-wort, ruddes, marigolds, mary gowles, &c., is meant (though by the bye it had only borrowed from the marsh marigold—*Caltha palustris*—a name which originally belonged to the latter). The Latin name points clearly to the Calendula folding its flowers when the sun goes down.

But the synonym *incuba* in the above line betrays a confusion with intybum, chicory, as shown again in the gloss (p. 53): "Eleutropia, incuba, sponsa solis vel mira solis solseqium cicoria idem, anglice et gallice cicoree;" or in the line from "Sinonoma Bartolomei" which seems meant for a hexameter: "Incuba, solseqium, cicoreaque sponsaque solis." Gerarde also gives sponsa solis as a name of chicory. So that both marigold and chicory are made synonyms of heliotropium. The curious thing is that the glossary gives definitions quoted from Dioscorides of a larger and lesser "Eliotropium," neither

of which can be identified with either of the plants above mentioned.

The glossarist can hardly have supposed that marigold and chicory meant the same thing, but he was evidently hazy as to the meaning of incubus, which occurs again in the following gloss (p. 39): "Cicuta, celena, incubus, coniza vel conium, herba benedicta idem. Gallice chanele vel chanelire; angl. hemelok vel hornwistel."

Gerarde has preserved the name Herb Bennet; the other synonyms we must leave Mr. Mowat to explain. He suggests that the strange name hornwistel may be derived from the offensive smell of the plant. Very likely he is right, but, without any pretensions to philological learning, we may suggest that a hemlock stem is easily converted into a *whistle*.

At p. 156 we have the true etymology of the deceptive name meadow-sweet, "Reginela, Regina Prati, medewort," the English name meaning a plant used for flavouring mead, and altered into meadow-sweet possibly, as Dr. Prior suggests, through some confusion with Regina Prati, queen of the meadow, which name, again, is preserved in the French "Reine des Prés."

Several glosses give the old form of primrose, primerole, a diminutive of Italian *prima vera*, the first flower of spring; and show, moreover, that this name was originally assigned to the daisy, called also *Consolida minor*, of which the German "Ortus Sanitatis" gives an unmistakable figure. The reason evidently was that our primrose is a rare flower in Italy, where the daisy is the herald of spring, but the northern botanists found the name better suited to the flower which now bears it, or to the cowslip, *herba Sancti Petri*.

It is still more startling to find Ligustrum (or modern privet) glossed in some lists (though not in this) as primrose or cowslip. But whatever plant may have been originally meant by Ligustrum, the name privet, or primet, was, as shown by Dr. Prior, originally identical in meaning and almost in etymology with primrose, being derived from French Prime-printemps = Primprint, primet, or prim. Why the Latin name was at one time applied to the flower, at another to the shrub now thus called, is not quite clear.

A curious relic of ancient medicine is preserved in the gloss (p. 5): "Allium domesticum, tyriaca rusticorum, gall. angl. garleke." Here *tyriaca* = θηριακή = *theriaca* (treacle), a once celebrated antidote against snakes and venomous animals. A plant supposed to be the garlick was called by Galen a name rendered in Latin *Theriaca rusticorum*, and so became "poor man's treacle," a name which garlick still bears, though the modern transference of the word treacle to molasses makes it appear absurd.

The medical terms in "Alphita" are extremely interesting, but space forbids entering upon the subject. One curious instance may, however, be quoted, which shows that "there is nothing new under the sun." Only last year Prof. Liebreich, of Berlin, introduced to the medical world, under the name of "lanoline," a new fatty substance for ointments, derived from wool, which has proved a most successful novelty. Now, we find in our glossary the following: "Ysopus cerotis vel Ysopum cerotum est succus lane succide per decoctionem extractus. Qualiter efficitur quere in Dyascorides" (p. 198). *I.e.* "the cerate (or ointment) Ysopum is a 'juice' extracted by boiling from uncleaned wool. For the mode of preparation consult Dioscorides." This is, in fact, οἴσυπος, or *æsopus*, mentioned by Dioscorides and Pliny as a fat extracted from the fleeces of sheep, and is practically identical with Liebreich's lanoline.

While thanking Mr. Mowat for this valuable contribution to the history of mediæval science, and the Clarendon Press for their spirited endeavour to make the treasures of the Bodleian common property, we may suggest that there are other scientific relics equally worthy of attention: such, for instance, as some remarkable illustrated manuscripts of anatomy and natural history, or the works of John Arderne, the English surgeon, a relic at least equal in historical value to those already published, and of far greater national significance.

J. F. PAYNE.

OUR BOOK SHELF.

Fresh Woods and Pastures New. By the Author of "An Amateur Angler's Days in Dove Dale." (London: Sampson Low, 1887.)

IN this delightful little volume the amateur angler, who discoursed so pleasantly on the beauties of the streams and fields of Dove Dale a few years ago, recounts his subsequent experiences of country life and amongst country scenes. Angling plays but an inconsiderable part in the present book, but the spirit of the angler is over every chapter—the spirit, namely, which finds placid enjoyment in all the sights and sounds of Nature, and something new and interesting everywhere. His motto is, that the old simplicity of the country "though hid in grey, Doth look more gay Than foppery in plush and scarlet clad." Of this capacity for finding amusement everywhere the chapter on turkeys and peacocks is an example. A battle between two flocks of turkeys is described with much humour; the method in which these birds fight is perhaps new even to persons who think they know a good deal about turkeys; it certainly will be to others. Again, the description of a peacock going to roost is full of quiet fun; few persons, even of those who live in the country, have ever seen a peacock perform the feat of flying into a tree for the night. Yet it is a feat to which great importance is attached by the bird himself; it is only to be done with great circumspection, hesitation, and show of indifference. A score of other topics connected with the country are treated with a like charm. The little book, both in subjects and mode of treatment, is a gem.

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 British Museum and American Museums.

I VERY much regret to learn that my friend Prof. Flower thinks I have done great injustice to the British Museum of Natural History in my article on "American Museums," which has appeared in the September number of the *Fortnightly Review*. The article was sent to England last February, and I had no opportunity of correcting the proofs, as some very bad misprints will sufficiently indicate. Nothing was farther from my mind than to make any reflections on the management or arrangement of the Museum by

Prof. Flower and the able heads of departments, for all of whom I have the greatest respect; and I am further convinced that much credit is due to them for doing the very utmost that is possible under the circumstances of the case. My strictures on the Museum were intended to apply solely and exclusively to the fundamental *principle* underlying its arrangement, which principle is embodied in the new building as in the old one. I contrasted strongly the principle of moderate-sized rooms as compared with large galleries,—the principle of exhibiting, to the public, on the one hand, strictly limited typical collections; on the other, almost complete series of species,—the principle of making a geographical arrangement the main feature of a museum, as compared with that in which almost no provision at all is made for such an arrangement.

I had always understood that for this fundamental system of arrangement neither the present Director nor the heads of departments of the Museum were in any way responsible, and that in criticising it frankly I should not be considered to reflect on them. So clear was I in my own mind that I was discussing this general system only, that I used some expressions which I now see, with much regret, were capable of being misunderstood. After referring to some of the improvements in the New British Museum, I say, "but the great bulk of the collection still consists of the old specimens exhibited in the old way in an interminable series of overcrowded wall-cases, while all attempt at any effective presentation of the various aspects and problems of natural history as now understood is as far off as ever." To the latter part of this sentence, Prof. Flower objects, as not recognizing the many improvements recently made and still making; but I intended it to apply, as I think the whole context of my article shows, to the *system* and the *building*, which themselves, from the point of view I have taken throughout the article, render any attempt at an "effective" presentation of these aspects and problems impossible. Again, at the end of my article I speak of Prof. Agassiz having said that he intended his museum "to illustrate the history of creation as far as the present state of scientific knowledge reveals that history," and then go on: "It is surely an anomaly that the naturalist who was most opposed to the theory of evolution should be the first to arrange his museum in such a way as best to illustrate that theory, while in the land of Darwin no step has been taken to escape from the monotonous routine of one great systematic series of crowded specimens arranged in lofty halls and palatial galleries, which may excite wonder, but which are calculated to teach no definite lesson." Here I was referring to the fact that the new Museum at South Kensington was constructed and arranged substantially on the same lines as the old one at Bloomsbury, and regretting that the only effective step towards inaugurating a new system of arrangement was not then taken. Prof. Flower, I find, thinks that I imply that no steps are being taken now to render the Museum more instructive and generally interesting. This was very far from my meaning, and I am exceedingly sorry that such an interpretation of my words should have been possible. I visited the Museum several times last summer before leaving for America, and I noted many improvements that were being introduced in all departments; but I could not fail to see that the main principle of the arrangement, both of the building itself and of the collections in it, had not been changed, and it was to this that all my criticisms were directed.

Godalming, September 22. ALFRED R. WALLACE.

The Law of Error.

MR. F. Y. EDGEWORTH has, in NATURE of September 22 (p. 482), replied to Dr. Venn's letter from the mathematical standpoint; perhaps a few words from the meteorological side may not be out of place. The gist of Dr. Venn's remarks lies in his statement that the law of error applies to cases where there are "equal and opposite independent disturbing causes" (September 1, p. 412). Now, the excess and defect of barometrical pressure from the average, depend mainly on anticyclones and cyclones respectively, which though in many respects opposite in character are by no means equal, the latter being much more intense than the former; and there is no reason in the nature of the case why they should be equal, as many of their characteristics are so dissimilar.

As regards the second instance given by Dr. Venn, the chief factor in the variations of temperature at different times of the year is the varying declination of the sun, the rate of change of declination passing through two minima yearly—namely, at the solstices, so named for this very reason. One would naturally expect that about these times the temperature should remain more nearly the same than about the equinoxes; Dr. Venn's curve would consequently give two maxima. The deviations of the temperature of each *day* from the average would not be unlikely to conform to the law of error, but it is evident that a curve formed from the temperatures for the whole year would be of a totally different kind. T. W. BACKHOUSE.

Sunderland, September 26.

Lunar Rainbows.

ON Sunday night, August 28, a lunar rainbow was visible here. As the occurrence seems to be uncommon, some particulars may interest your readers.

We had a very heavy shower before 11 o'clock, with a south-west wind. The rain left off suddenly, as it began, a few minutes past 11; and as the heavy cloud moved away to the north-east it left a gloriously clear sky behind, with the moon, then a little past its first quarter, shining brightly a few degrees above a heavy bank of cloud which lay on the horizon. Looking out of a window on the opposite side of the house, I had the satisfaction of seeing a complete pale white bow in the black cloud to the north-east, which lasted very clear and distinct for about five minutes, when it quickly grew faint as the bank of clouds on the horizon began to rise and obscure the falling moon. The outer edge of the bow was well defined against the intense black of the cloud beyond; the inner edge was much less distinct, and the area within was covered with a slight suffused light, which, however, appeared to diminish as the distance from the bow increased.

The drops of rain were unusually large, and the downpour, while it lasted, was extraordinarily heavy.

A. F. GRIFFITH.

15 Buckingham Place, Brighton, September 22.

A LUNAR rainbow was visible here shortly after 11 o'clock last night. It extended without break through three-quarters of a semicircle, the top of the arch being about 60° high. In colour the bow resembled a moonbeam shining between two clouds, and its brightness was sufficient to cause it to be immediately detected by a casual glance, in spite of the presence of numerous white clouds occupying its centre. The sky just outside the bow appeared darkest, probably by contrast with these clouds. Ten minutes elapsed before the rainbow faded.

Rock Ferry, September 27. S. J. H.

The Perception of Colour.

IS Mr. Stromeyer sure that the observations he made (see NATURE, July 14, p. 246) prove any difference in the rapidity of perception of colour, and that they do not rather show a difference in perception of brightness? It is well known that faint objects are not so quickly perceived as bright ones (see Webb's "Celestial Objects," p. 368 of the 4th edition, under ε Pegasi); and as the violet end of the spectrum is much fainter than the rest, the effect described would be produced by the difference in brightness apart from the difference in colour. I have tried Mr. Stromeyer's experiment of rotating the spectrum, and it appears to me that the red as well as the violet end lags behind the middle; though as the red is so much shorter, this is more difficult to see. T. W. BACKHOUSE.

Sunderland, September 15.

Tertiary Outliers on the North Downs.

IN August of last year (NATURE, vol. xxxiv. p. 341), I ventured to draw a distinction between the *unfossiliferous sands* found at certain places on the North Downs and the fossiliferous deposits at Lenham. For reasons assigned, I suggested a certain degree of probability of their being of Bagshot age, and indicating a former extension by overlap of the higher beds of that important Eocene formation. This summer I have had opportunities of examining all the principal outliers referred to; and I must say that I am strongly impressed with the Bagshot character of these unfossiliferous sands, and of the well-rolled flint pebbles associated with them, in some cases (as at Headley) in great quantity. I speak only of those which can be identified with

some degree of certainty as Tertiary beds *in situ*. The sands at Netley Heath and at Chipstead have a remarkable *Upper* Bagshot facies. Those at Headley do not present such a strong character in this respect, but I have no hesitation in referring them on lithological grounds to the Bagshot series.

Wellington College, Berks, September 27. A. IRVING.

MODERN VIEWS OF ELECTRICITY.[1]

PART I.

I.

IT is often said that we do not know what electricity is, and there is a considerable amount of truth in the statement. It is not so true, however, as it was some twenty years ago. Some things are beginning to be known about it; and though modern views are tentative, and may well require modification, nevertheless some progress has been made. I shall endeavour in this lecture to set forth as best I may the position of thinkers on electrical subjects at the present time.

It will at once strike you that the whole subject of electricity as at present known is too gigantic for anyone to make an attempt to compass it in a single lecture, even though he assume on the part of his audience a perfect acquaintance with all the ordinary phenomena; and you will admit that it is much better to limit one's self definitely at the beginning to some one branch than by attempting too broad and discursive a survey to risk slurring the whole and becoming totally unintelligible.

I begin by saying that the whole subject of electricity is divisible for purposes of classification into four great branches.

(1) Electricity at rest, or static electricity: wherein are studied all the phenomena belonging to stresses and strains in insulating or dielectric media brought about by the neighbourhood of electric charges or electrified bodies at rest immersed therein; together with the modes of exciting such electric charges and the laws of their interactions.

(2) Electricity in locomotion, or current electricity: wherein are discussed all the phenomena set up in metallic conductors, in chemical compounds, and in dielectric media, by the passage of electricity through them; together with the modes of setting electricity in continuous motion and the laws of its flow.

(3) Electricity in rotation, or magnetism: wherein are discussed the phenomena belonging to electricity in whirling or vortex motion, the modes of exciting such whirls, the stresses and strains produced by them, and the laws of their interaction.

(4) Electricity in vibration, or radiation: wherein are discussed the propagation of periodic or undulatory disturbances through various kinds of media, the laws regulating wave velocity, wave-length, reflection, interference, dispersion, polarization, and a multitude of phenomena studied for a long time under the heading "Light." Although this is the most abstruse and difficult portion of electrical science, a certain fraction of it has been known to us longer than any other branch, and has been studied under special advantages, because of our happening to possess a special sense-organ for its appreciation.

Now, with some qualms of regret I have decided to refrain from speaking to you about any one of these great and comprehensive groups except the first. It is hopeless to attempt more; and even the small portion of that on which I shall touch will tax the time at our disposal to the utmost, and I must assume acquaintance with the elementary facts in order to proceed to their elucidation.

The great names in connexion with our progress in knowledge as to the real nature of electricity, irrespective of a mere study and extension of its known facts, are

FRANKLIN, CAVENDISH, FARADAY, MAXWELL.

To these, indeed, you may feel impelled to add the tremendous name of THOMSON; but one has some delicacy in attempting to estimate the work of living philosophers, and as Maxwell has been very explicit in acknowledging his indebtedness to his illustrious contemporary, whose work will in the course of nature have to be criticised and appraised by far abler hands than mine and by the philosophers of generations yet unborn, we may well afford to abstain from minute considerations and accept for the present the name of Maxwell as representative of the great English school of mathematical physicists, under whose influence, Cambridge, in the pride of having reared them, is awaking to new and energetic scientific life, and whose splendid achievements will shine out in the future as the glory of this century.

The views concerning electrification which I shall try to explain are in some sense a development of those originally propounded by that most remarkable man, Benjamin Franklin. The accurate and acute experimenting of Cavendish laid the foundation for the modern theory of electricity; but, as he worked for himself rather than for the race, and as moreover he was in this matter far in advance of his time, Faraday had to go over the same ground again, with extensions and additions peculiar to himself and corresponding to the greater field of information at his disposal three-quarters of a century later. Both these men, and especially Faraday, so lived among phenomena that they yielded up their hidden secrets to them in a way unintelligible to ordinary workers; but while they themselves arrived at truth by processes that savour of intuition, they were unable always to express themselves intelligibly to their contemporaries and to make the inner meaning of their facts and speculations understood. Then comes Maxwell, with his keen penetration and great grasp of thought combined with mathematical subtlety and power of expression; he assimilates the facts, sympathizes with the philosophic but untutored modes of expression invented by Faraday, links the theorems of Green and Stokes and Thomson to the facts of Faraday, and from the union there arises the young modern science of electricity, whose infancy at the present time is so vigorous and so promising that we are all looking forward to the near future in eager hope and expectation of some greater and still more magnificent generalization.

You know well that there have been fluid or material theories of electricity for the past century; you know, moreover, that there has been a reaction against them. There was even a tendency a few years back to deny the material nature of electricity and assert its position as a form of energy. This was doubtless due to an analogical and natural, though unjustifiable, feeling that just as sound and heat and light had shown themselves to be forms of energy so in due time would electricity also. If such were the expectation, it has not been justified by the event. Electricity may possibly be a form of matter—it is not a form of energy. It is quite true that electricity *under pressure* or *in motion* represents energy, but the same thing is true of water or air, and we do not therefore deny them to be forms of matter. Understand the sense in which I use the word electricity. *Electrification* is a result of work done, and is most certainly a form of energy; it can be created and destroyed by an act of work. But electricity—none is ever created or destroyed, it is simply moved and strained like matter. No one ever exhibited a trace of positive electricity without there being somewhere in its immediate neighbourhood an equal quantity of negative.

This is the first great law, expressible in a variety of ways: as, for instance, by saying that total algebraic pro-

[1] Expansion of a lecture delivered by Dr. Oliver Lodge, partly at the London Institution on January 1, 1885, and partly at the Midland Institute, Birmingham, November 13, 1886, but not hitherto published.

duction of electricity is always zero ; that you cannot produce positive electrification without an equal quantity of negative also ; that what one body gains of electricity some other body must lose.

Now, whenever we perceive that a thing is produced in precisely equal and opposite amounts, so that what one body gains another loses, it is convenient and most simple to consider the thing not as generated in the one body and destroyed in the other, but as simply *transferred. Electricity in this respect behaves just like a substance.* This is what Franklin perceived.

The second great law is that electricity always, under all circumstance, flows in a closed circuit, the same quantity crossing every section of that circuit, so that it is not possible to exhaust it from one region of space and condense it in another.

Another way of expressing this fact is to say that no charge resides in the interior of a hollow conductor.

Another is to say that total induced charge is always equal and opposite to inducing charges.

[This is illustrated by the well-known experiment of insulating and charging a parrot-cage with a sensitive electroscope inside connected to its wires ; also by the ice-pail experiment.]

When we thus find that it is impossible to charge a body absolutely with electricity, that though you can move it from place to place it always and instantly refills the body from which you take it, so that no portion of space can be more or less filled with it than it already is, it is natural to express the phenomenon by saying that electricity behaves itself like a perfectly *incompressible* substance or fluid, of which all space is completely full. That is to say, it behaves like a perfect and all-permeating *liquid.* Understand, I by no means assert that electricity *is* such a fluid or liquid ; I only assert the undoubted fact that it behaves like one, *i.e.* it obeys the same laws.

It may be advisable carefully to guard one's self against becoming too strongly imbued with the notion that because electricity obeys the laws of a liquid therefore it is one. One must always be keenly on the look-out for any discrepancy between the behaviour of the two things, and a single certain discrepancy will be sufficient to overthrow the fancy that they may perhaps be really identical. Till such a discrepancy turns up, however, we are justified in pursuing the analogy—more than justified, we are impelled. And if we resist the help of an analogy like this there are only two courses open to us : either we must become first-rate mathematicians, able to live wholly among symbols and dispensing with pictorial images and such adventitious aid ; or we must remain in hazy ignorance of the stages which have been reached, and of the present knowledge of electricity so far as it goes. I need hardly say that by "modern views" I do not mean *ultimate* views ; nor do I mean that I can give an account of all the speculations and ideas floating in the minds of some two or three of our most advanced thinkers. All I attempt is to give an account of the stage which has certainly been attained, and to ask you to take for granted that at the next quarter of a century will see as great advances made upon these views as they are superior to the doctrines inculcated by the ordinary run of text-books.

Imagine now that we live immersed in an infinite ocean of incompressible and inexpansible all-permeating perfect liquid, like fish live in the sea, and how can we become cognizant of its existence? Not by its weight, for we can remove it from no portion of space in order to try whether it has weight.

We can weigh air, truly, but that is simply because we can compress it and rarefy it. An exhausting or condensing pump of some kind was needed before even air could be weighed or its pressure estimated.

But if air had been incompressible and inexpansible, if it had been a vacuum-less perfect liquid, pumps would have been useless for the purpose, and we should necessarily be completely ignorant of the weight and pressure of the atmosphere.

How then should we become cognizant of its existence? In four ways :—

(1) By being able to pump it out of one elastic bag into another [not out of one bucket into another : if you lived at the bottom of the sea you would never think about filling or emptying buckets—the idea would be absurd ; but you could fill or empty elastic bags], and by noticing the strain phenomena exhibited by the bags and their tendency to burst when over full. [Water (or air) was here pumped out of one elastic bag into another, and the analogy with an electrical machine charging two conductors oppositely was pointed out.]

(2) By winds or currents ; by watching the effect of moving masses of the fluid as it flows along pipes or through spongy bodies, and by the effects of its inertia and momentum. [A hanging vane in a tube deflected by a stream of water was here likened roughly to a galvanometer ; also the effect of suddenly stopping a stream of water, as in a water ram, was mentioned as analogous to self-induction.]

(3) By making vortices and whirls in the fluid, and by observing the mutual action of these vortices, their attractions and repulsions. [Whirlwinds, sand-storms, waterspouts, cyclones, whirlpools.]

(4) By setting up undulations in the medium : *i.e.* by the phenomena which in ordinary media excite in us through our ears the sensation called "*sound.*"

In all these ways we have become acquainted with electricity, and in no others that I am aware of. They correspond to the four great divisions of the subject which I made above. But there are differences, very important differences, between the behaviour of a material liquid ocean such as we have contemplated and the behaviour of electricity. First it is doubtful whether electricity by itself and disconnected from matter has any inertia. It is by no means certain that it has not : the experiments made by Maxwell with a negative result need only prove either that its speed of flow is very small, or that an electric current consists of equal opposite streams of equal momentum. The laws of electric flow in conductors are such as indicate no inertia, and this fact would be conclusive were it not that a recent brilliant paper by Prof. Poynting explains the reason of it completely otherwise, and leaves the question of inertia quite open ; on the other hand, the facts of magnetism seem definitely to require inertia, or something corresponding to it. Leaving this therefore as an open question, there can be no doubt but that when in connexion with insulating or dielectric matter *the combination* most certainly possesses inertia.

A more serious and certain difference between the behaviour of electricity and that of an incompressible fluid comes out in the fourth category—that concerned with wave-motion. In an incompressible fluid the velocity and length of waves would both be infinite, and none of the phenomena connected with the gradual propagation of waves through it could exist. Such a medium therefore would be incapable of sound vibrations in any ordinary sense. On the other hand, it is quite certain that the disturbances concerned in light radiation take place at right angles to the direction of propagation—they are tranverse disturbances—and such disturbances as these no body with the entire properties of a fluid can possibly transmit. Remember, however, that the medium which transmits light is the ether and not simply electricity. We have nowhere asserted that electricity and the ether are identical. If they are, we are bound to admit that ether, though fluid in the sense of enabling masses to move freely through it, has a certain amount of rigidity for enormously rapid and minute oscillatory disturbances. If they are not identical, we can more vaguely say that ether contains electricity as a jelly contains water, but that the rigidity concerned in the transverse vibrations

belongs not to the water in the jelly but to the mode in which it is entangled in its meshes. However all this is a great and difficult question into which we shall be able to enter with more satisfaction twenty years hence.

Provisionally we will accept as a working hypothesis the idea of the ether consisting of electricity in a state of entanglement similar to that of water in jelly; and we are driven to this view by the exigencies of mode 1, the electrostatic or strain method of examining the properties of electricity, because otherwise the properties of insulators are hard to conceive. If it turn out that space is a conductor, which seems to me highly improbable, then we must fall back upon the other view that it is rigid only for infinitesimal vibrations, and fluid for steady forces.

Return now to the consideration of electrostatics. We are to regard ourselves as living immersed in an infinite all-permeating ocean of perfect incompressible fluid (or liquid), as fish live in the sea; but this is not all, for if that were our actual state we should have no more notion of the existence of the liquid than deep-sea fish have of the medium they swim in. If matter were all perfectly conducting, it would be our state: in a perfectly free ocean there is no insulation—no obstruction to flow of liquid: it is the fact of insulation that renders electrostatics possible. We could obstruct the flow and store up definite quantities of a fluid in which we were totally submerged by the use of closed vessels of course. But how could we pump liquid from one into another so as to charge one positively and another negatively? Only by having the walls elastic: by the use of elastic bags, and elastic partitions across pipes. And so we can represent a continuous insulating medium (like the atmosphere or space) by the analogy of a jelly, through which liquid can only flow by reason of cracks and channels and cavities.

Modify the idea of an infinite ocean of liquid into that of an infinite jelly or elastic substance in which the liquid is entangled, and through which it cannot penetrate without violence and disruption; and you have here a model of the general insulating atmosphere. Our ocean of fluid is not free and mobile like water, it is stiff and entangled like jelly.

Nevertheless bodies can move through it freely. Yes *bodies* can, it is the *liquid* itself only which is entangled. How we are to picture freely and naturally the motion of ordinary matter through the insulating medium of space it is not easy to say. It is a difficulty not fatal but sensible, and due to an imperfection in our analogy.

Insulators being like elastic partitions or impervious but yielding masses, conductors are like cavities, porous or spongy bodies perfectly pervious though with more or less frictional resistance to the flow of liquids through them. Thus, whereas bodies easily penetrable by matter are impervious to electricity, bodies like metals which resist entirely the passage of matter, are quite permeable to electricity. It is this inversion of ordinary ideas of penetrability that constitutes a small difficulty at the beginning of the subject.

However, supposing it overcome, let us think of these insulated spheres and cylinders on the table connected by copper wire as so many cavities and tubes in an otherwise continuous elastic impervious medium which surrounds them and us, and extends throughout space wherever conductors are not. All, however, cavities as well as the rest of the medium, are completely full of the universal fluid. The fluid which is entangled in insulators is free to move in conductors; whence it follows that its pressure or potential is the same in every part of a conductor in which it is not flowing along. For if there were any excess of pressure at any point, a flow would immediately occur until it was equalized. In an insulator this is by no means the case. Differences of pressure are exceedingly common in insulators, and are naturally accompanied by a strain of the medium.

[Here certain electrostatic experiments were shown as evidence of the strain existing at the ends of a long insulated wire connected to a Voss machine.]

There have been, as you know, two ancient fluid theories of electricity—the one-fluid theory of Franklin, and the two-fluid theory of Symmer and others. A great deal is to be said for both of them within a certain range. There are certainly points, many points, on which they are hopelessly wrong and misleading, *but it is their foundation upon ideas of action at a distance that condemns them, it is not the fluidity.* They concentrate attention upon the conductors; whereas Faraday taught us to concentrate attention on the insulating medium surrounding the conductors—the "*dielectric*" as he termed it. This is the seat of all the phenomena: conductors are mere breaks in it—interrupters of its continuity.

To Faraday the space round conductors was full of what he called lines of force; and it is his main achievement in electrostatics to have diverted our attention from the obvious and apparent to the intrinsic and essential phenomena. Let us try and seize his point of view before going further. It is certainly true as far as it goes, and is devoid of hypothesis.

Take the old fundamental electric experiment of rubbing two bodies together, separating them, and exhibiting the attraction and repulsion of a pith ball, say, and how should we now describe it? Something this way.

Take two insulated disks of different material, one metal, say, and one silk, touch them together, the contact effects a transfer of electricity from the metal to the silk; rub slightly to assist the transfer, since silk is a non-conductor, then separate. As you separate the disks the medium between them is thrown into a state of strain, the direction of which is mapped out by drawing a set of lines, called lines of force, from one disk to the other, coincident with the direction of strain at every point. As Faraday remarked, the strain is as if these lines were stretched elastic threads endowed with the property of repelling each other as well as of shortening themselves; in other words, there is a tension along the lines of force and a pressure at right angles to them. When the disks are near, and the lines short, they are mainly straight, Fig. 1,



FIG. 1.—Rough diagram of the state of the medium near two oppositely charged disks when close together.

but as the distance increases they become curved, bulging away from the common axis of the two disks, and some even curling round to the back of the disk (Fig. 2), until when the disks are infinitely distant as many lines spring from the back of each as from its face; and we have a charged body to all intents existing in space by itself.

The state of tension existing in the medium between the disks results in a tendency to bring them together again, just as if they were connected by so many elastic threads of no length when unstretched. The ends of the lines are the so-called electrifications or charges, and the lines perpetually try and shorten and shut up, so that their ends may coincide and the strain be relieved. If one of the disks touch another conducting body, some of its lines instantly leave it and go to the body; in other words, the charge is capable of transference, and the new body is urged toward the other disk, just as the disk was from which it received the lines. If this new body *completely surrounds* the disk, it receives the whole of its lines, and the disk can be withdrawn perfectly free and inert. [Faraday's "ice pail" experiment.]

Now take the two charged disks, facing one another,

and let, say, a suspended gilt pith ball hang between them. Being a conductor there is no strain inside it, and so it acts partially as a bridge, and several of the lines pass through it—or, rather, they end at one side of it and begin at the other : thus it has opposite charges on its two faces—it is under induction (Fig. 3). Let it now be moved so as to touch one of the disks, the lines between it and the disk on that side have shut up, and it remains with those only which go to the other disk. In other words, it has received some lines from the touched disk. These will pull it over to the far disk and there shut

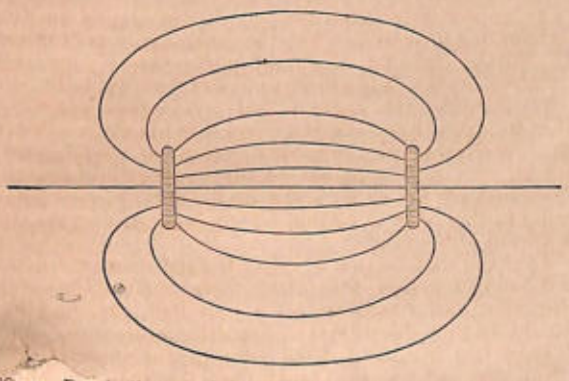


FIG. 2.—Rough diagram of the state of the medium near two oppositely charged disks when separated.

themselves up. From that disk it receives more, and travels with their ends back to the first disk, and so on (Fig. 4), perpetually receiving lines and shutting them up until they are all gone and the disks are discharged.

This mode of stating the facts involves no hypothesis whatever—it is the simple truth. But the "lines of force" have no more and no less existence than have "rays of light." Both are convenient modes of expression.

But so long as we adhere to this mode of expression we cannot form a complete mental picture of the actually

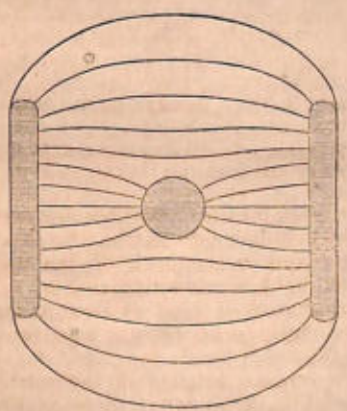


FIG. 3.—Rough diagram of the medium between two disks disturbed by the presence of an uncharged metal sphere. The two halves of the sphere are oppositely charged "by induction."

occurring operations. In optics it is usual to abandon rays at a certain stage and attend to the waves, which we know are of the essence of the phenomenon, though we do not know yet very much about their true nature.

Similarly in electricity, at a certain point we are led to abandon lines of force and potential theories, and to try to conceive the actual stuff undergoing its strains and motions. It is then we get urged towards ideas similar to those which are useful in treating of the behaviour of an incompressible fluid.

In an utterly modified sense, we have still a fluid theory of electricity, and a portion of the ideas of the old theories belong to it also.

Thus Franklin's view that positive charge was excess and negative charge was a deficit in a certain standard quantity of the fluid which all bodies naturally possessed in their neutral state, remains practically true. His view that the fluid was never manufactured, but was taken from one body to give to another, so that one gained what the other lost—no more and no less—remains practically true. Part also—a less part—of the two-fluid theory likewise remains true, in my present opinion ; but this is not a branch of the subject on which I shall enter in the present discourse. It will suffice for the present to fix our attention on one fluid only.

You are to think of an electric machine as a pump which, being attached to two bodies respectively, drives some electricity from the one into the other, conferring upon one a positive and upon the other a precisely equal negative charge. One of the two bodies may be the earth, in which case the charge makes little or no difference to it.

But, as has been objected before, if electricity is like an incompressible and inextensible fluid, how is it possible to

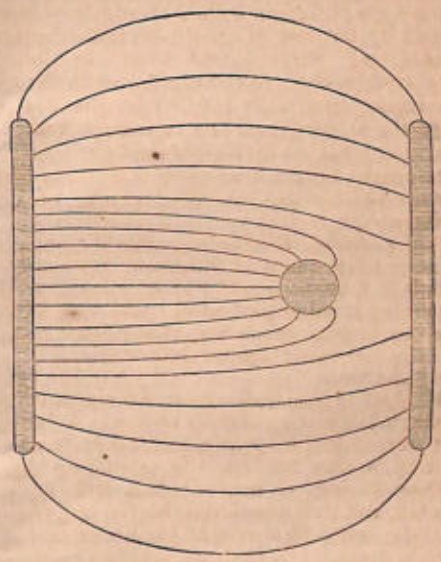


FIG. 4.—Rough diagram of the medium near two oppositely charged disks between which a metal carrier ball is oscillating, having just touched the right-hand disk. (Discharge by "alternate contact.")

withdraw any of it from one body and give it to another ? With rigid bodies it is not possible, but with elastic bodies it is easy.

The act of charging this sphere is therefore analogous to pumping water into this elastic bag, or rather into a cavity in the midst of an elastic medium, whose thick walls, extending in all directions and needing a great pressure to strain them, better represent the true state of the case than does the thin boundary of a bag like this.

Draw a couple of such cavities and consider fluid pumped from one into the other, and you will see that the charge (*i.e.* the excess or defect of fluid) resides on the outside. You may also show that when both are similarly charged the medium is so strained that they tend to be forced apart ; whereas when one is distended and the other contracted they tend to approach.

Further you may consider two cavities side by side, pump fluid into (or out of) one only, and watch the effect on the other. You will thus see the phenomena of induction, the near side of the second cavity becoming

oppositely charged (*i.e.* the walls encroaching on the cavity), the far side similarly charged (the cavity encroaching on the walls), and the pressure on the fluid in the cavity being increased or diminished in correspondence with the change of pressure in the charged or inducing cavity. In other words, conductors rise in potential when brought near a positively charged body.

The actual changes in volume necessary to the strain of these cavities are a defect in the analogy. To avoid this objection, one will have to accept a dual view of electricity—a sort of two-fluid theory, which many phenomena urge one to accept, but about which I will say nothing to-night. It is sufficient at first to grasp the one-fluid ideas.

Return Circuit.—Sometimes a difficulty is felt about electricity flowing in a closed circuit—as, for instance, in signalling to America and using the earth as a return circuit: the question arises, How does the electricity find its way back?

The difficulty is no more real than if a tube were laid to America with its two ends connected to the sea and already quite full. If now a little more sea-water were pumped in at one end, an equal quantity would leave the other end, and the disturbed level of the ocean would readjust itself. Not the same identical water would return, but an equal quantity would return. That is all one says in electricity. One cannot label and identify electricity.

To imitate the inductive retardation of cables, the tube should have slightly elastic walls; to imitate the speed of signalling, the water must be supposed quite incompressible, not elastic as it really is, or each pulse would take three-quarters of an hour to go.

Condensers.—Returning to the subject of charging bodies electrically, how is one to consider the fact that bringing an earth-plate near a conductor increases its capacity so greatly, enabling the same pressure to force in a much larger quantity of fluid? how is one to think of a condenser, or Leyden jar?

In the easiest possible way, by observing that the bringing near an earth-connected conductor is really *thinning down the dielectric* on all sides of the body.

The thin-walled elastic medium of course takes less force to distend it a given amount than a thick mass of the same stuff took. A Leyden jar is like a cavity with quite thin walls—in other words, it is like an elastic bag.

But if you thin it too far, or strain it too much, the elastic membrane may burst: exactly, and this is the disruptive discharge of a jar, and is accompanied by a spark. Sometimes it is the solid dielectric which breaks down permanently. Ordinarily it is merely the air; and, since a fluid insulator constitutes a self-mending partition, it is instantaneously as good as new again.

There are many things of interest and importance to study about a Leyden jar. There is the fact that if insulated, it will not charge: the potential of both inner and outer coatings rises equally; that, in order to charge it, for every positive spark you give to the interior an equal positive spark must be taken from the exterior. There is the charging and the discharging of it by alternate contacts, as by an oscillating ball; and there are the phenomena of the spark-discharge itself.

But, as you know, *all* charging is really a case of a Leyden jar. The outer coat must always be somewhere—the walls of the room, or the earth, or something—you always have a layer of dielectric between two charges—the so-called induced and the inducing charge. You cannot charge one body alone.

To illustrate the phenomena of charge, I will now call your attention to these diagrams—which less completely but more simply than hydraulic illustrations, serve to make the nature of the phenomena manifest.

(*To be continued.*)

ON THE TEACHING OF CHEMISTRY.[1]

THE question is being often asked, Why does chemistry progress so slowly in this country? Different answers, all more or less true, may be given; one answer that has not, I think, been sufficiently insisted on is: Because chemistry is so little taught.

Classes, nominally in chemistry, are conducted in many schools, and in almost all the colleges, of the country; but I assert that very little of what is taught is really chemistry. For what is it that is taught? On the one hand, catalogues of so-called facts detached from reasoning and from generalizations; on the other hand, definitions and generalizations and speculations detached from the facts on which they rest. But neither detached facts, however accurately stated, nor definitions, in however sharply-cut words they are contained, nor speculations, however interesting they may be, are science; and chemistry really is a branch of natural science.

It is admitted by all that hydrogen is a colourless, odourless gas, 14·435 times lighter than air, produced by the interaction of dilute sulphuric acid and zinc, combustible, condensable to a liquid at very low temperatures. These statements are facts; but when the student of chemistry is required to read, and if possible to remember, such facts as these about each of the elements and its compounds, the statements cease to be facts to him, and become false, inasmuch as they cover up and hide the really important facts regarding the interactions of elements and compounds, and regarding the connexions between changes of composition and changes of properties, which form the subject-matter of chemistry.

I have known students have at their finger-ends the properties of all the elements—as these properties are detailed in the ordinary text-books—and yet be almost wholly ignorant of chemistry.

And I have also known students ready at a moment's notice to repeat the orthodox definitions of atomic and molecular weights, or to draw structural formulæ of complex minerals, or to speak fluently about double bonds and unsaturated units of affinity, and yet be quite innocent of any knowledge of chemistry.

A fatal distinction is too often drawn by chemical teachers between the facts on which chemical science rests, and reasoning and generalizing on these facts: the former, that is statements of detached facts, is called chemistry; the latter, that is reasoning on facts and generalizing to principles, is called chemical philosophy. I believe strongly that there are not two chemistries, but one chemistry. If chemical teachers were quite decided as to what they ought to teach, we might hope for marked advances in our science.

My own experience in teaching chemistry convinces me that it is a very difficult subject both to teach and to learn. Of course there is no great difficulty in restating to a class what is printed in the text-book, and occasionally enlivening the routine by a few experiments; nor does it require high mental capacities and training to tell the laboratory student that bottle A contains a double salt, to be analyzed by the help of the tables on page so-and-so of the book. But do not let us call this kind of thing teaching chemistry. To teach chemistry well requires experience and an educated mind. It is not easy to hit the golden mean. If the teacher despises facts his reasoning becomes absurd, because it is based on nothing; his principles become only speculations; and his laws merely phrases. If he disregards principles, generalizations, and theories, his facts become false, and when facts are false (as they often are) they are deadly, and kill those that trust in them.

Chemistry is a branch of natural science; it deals with one class of natural occurrences, it observes and experi-

[1] A Paper read before Section B of the British Association at Manchester, by M. M. Pattison Muir, M.A., Fellow of Gonville and Caius College, Cambridge.

ments on these occurrences, it classifies and generalizes, and tries to ascend from empirical generalizations to natural laws. The business of the teacher of chemistry I take to be to make his pupil understand the methods of chemistry; to select typical facts and put these before the learner so that he may to some extent see their relative importance in the general scheme of well-built knowledge; to show the student how complex are the phenomena chemistry investigates, and how she simplifies that she may explain; to imbue him with some of that fine enthusiasm without which no great work is possible, by presenting to him glimpses of the greatness of the subject he is studying, and the importance of the prosecution of the study; and thus to build up in the student a scientific spirit, until at last the teacher and the learner are merged in their common investigation of nature.

In teaching chemistry the all-important things appear to me to be chiefly four: (1) to teach so that the student shall acquire real knowledge; (2) to carefully select both the facts and the reasoning to be set before the student; (3) to impress the learner with the importance and value of what he is learning as a part of that orderly and methodized study of nature which we call science; (4) to teach without fear of the examiner.

Real knowledge of chemistry can only be acquired by connecting the experimental work in the laboratory with chemical reasoning and with the principles of the science. This is rarely done in our chemical schools. The student generally hears lectures on chemistry, or at least on the materials from which chemical science is constructed; he sees experiments performed that have some connexion with what the lecturer teaches; then he goes into the laboratory and day after day performs qualitative analyses, for the most part by rule of thumb. The learner, especially the beginner, cannot connect what he is taught in lectures, and told to read in books, with what he does in the laboratory. The introduction of a well-arranged and properly graduated system of practical chemistry, is, in my opinion, one of the things which will do much to hasten the advance of chemistry among us. The work done in the laboratory must be directly connected with the lecture-work and the reading of the student; it must be progressive, beginning with easy experiments and leading the student onwards until he is able to investigate chemical occurrences for himself; and it must be arranged so that as the experimental work becomes more difficult the reasoning on the results becomes more close and accurate.

Such a course of practical chemistry can be arranged without any complicated laboratory appliances. (I may say parenthetically that in my opinion the building of luxuriously fitted laboratories has not been an unmixed gain to chemical science.)

In the course of laboratory work which seems to me to be called for, the student would begin with easy experiments on chemical and physical change, on the distinction between elements and not-elements (this would involve quantitative measurements), and on the classification of not-elements into mixtures and compounds. He would then proceed to classify compounds into acids, bases, and salts, working through well-chosen examples of each class. He would learn by his own experiments what is meant by "the replaceable hydrogen of acids," and by the classification of acids in accordance with their "basicity." He would study classes of elements, and so get a real grasp of the reasons for placing certain elements in one class, and of the principles of chemical classification. After hearing in the lecture-room about the properties of the members of a class of elements—say the iron class—he would at once go into the laboratory and himself prepare typical similar compounds of these elements. He would then turn to the conditions of chemical action; he would practically learn what an ordinary chemical equation teaches, and he would be impressed, by the results of his own experiments, with the importance of determining the conditions under which chemical changes occur, and with the many and varied facts regarding even every-day chemical changes which are not expressed in our chemical notation. The study of the conditions of chemical change would lead on to the study of affinity, of dissociation, and of allied subjects.

Turning again to the study of composition, the student would make determinations of the equivalent weights of a series of similar compounds, and also of several elements; he would determine the molecular weights of a few gaseous bodies, and the atomic weights of one or two elements. He would then proceed to study experimentally some of the methods whereby light is thrown on the constitutions of compounds. For instance, he would determine the specific volumes of a series of carbon compounds, the rates of etherification of a series of alcohols, and the nature of the products of the reaction of such a compound as phosphorus pentachloride with carbon compounds belonging to different classes but showing similarities of composition. Thus the molecular and atomic theory would become a reality to him. Finally, he would be required to repeat an investigation before undertaking himself to advance into the realm of the unknown.

In such a course as this the student would study a series of carefully selected facts; his reading and laboratory work would go hand in hand, each would react upon the other, and so he would be saved from the danger of attempting to draw a distinction between two things which are truly one—chemistry and chemical philosophy.

In selecting the facts and reasoning to be placed before the student of chemistry I think we should now finally abandon the method of treating the elements individually, and rather consider them in groups or classes. If this is done the student soon acquires a fair knowledge of chemical facts; he learns the compositions and properties of groups of similar compounds, he traces some of the connexions between changes of composition and of properties in analogous compounds. By considering the elements in groups the artificial difference between rare and common elements disappears, and this, I think, is a decided gain. The learner also begins to recognize that there are reasons for classing certain elements and compounds together; he sees that order and law pervade the vast domain of chemistry; he connects the constant *atomic weight* with the position of each element in the scheme of classification; and so he gains a basis on which he may rest the superstructure of facts as they are presented to him. This method of treatment inspires the learner with the hope that it is possible to get a firm hold of the subject he is studying. The method is progressive: principles are seen to arise out of the classes of facts considered; each event examined appears as at once the cause and the consequence of other events; generalizing on facts accompanies the acquisition of the facts themselves.

But if the student is expected to learn the properties of each element and its compounds, proceeding from element to element, he generally completely fails to grasp the connexion between similar elements; indeed, he usually and not unnaturally inquires why he should be burdened by these details, which seem to him only unmeaning facts: if he knows and can repeat the properties of half-a-dozen elements, what the better is he for knowing and being able to repeat the properties of a dozen more? The additional facts do not seem to help him to a knowledge of chemistry. And so he either despairs of finding any guiding light in the maze of facts, or he falls into the error of supposing that the maze in which he is wandering without a clue is chemistry. It is the old school method again, which teaches the uselessness of knowledge. When we look back on our school days do we not regret the hours wasted on learning rules

and exceptions to rules? Do we not remember how hopeless it all was? how little we advanced? how we spent a year on this subject and a year on that, and failed to gain a grip of any? Did we not sometimes believe that every branch of knowledge was merely a collection of rules? Do not then let us teach science as we learned grammar; else the burden we attempt to bind on the shoulders of our pupils will be more grievous even than that which we bore ourselves in our youth.

The third point on which I would insist is that chemistry should be taught so as to impress the student with the importance of the subject, and with the fact that chemistry is a living and growing part of the scientific study of nature. A teacher of any branch of natural science must thoroughly believe in his subject; and he must have a vivid and active imagination. The subject he has to teach is so immense that, unless his imagination is clear and active, he forms blurred and inaccurate images of the natural occurrences which he wishes to put before his pupils, and so presents them with poor distorted pictures in place of the vivid and vivifying realities of nature.

I see no way of impressing chemical students with the greatness of the subject of their study other than that of constantly keeping before them the many-sidedness of the problems they are trying to solve; of constantly showing how even the smallest, and apparently quite detached, fact is really in living connexion with the whole science; and from time to time of reminding them that the subject of their study is but one part of natural science and is closely connected with many other branches of scientific investigation. But these things can be done only by the teachers and the taught constantly working side by side in the laboratory at some of those apparently simple chemical problems which branch off in many directions, and thus together gaining a real grasp of the many-sidedness of the subject they are studying. The student is thus convinced that, although "there are no boundaries in nature," yet it is necessary for us to draw boundary lines; he also learns the importance of those very facts which, when separated from each other and from the principles that bind them together, retain only a negative value. Science is more than knowledge, and we must make our students realize this. To be in touch with nature is what we aim at; and knowledge alone will not gain this end. We are striving for a real, living, imaginative, acquaintance with the laws of the universe, in order that our lives may thus become "rich and full and satisfying through realities and not through dreams."

Finally, I think that fear of the examiner acts very prejudicially on much of the chemical teaching of this country, more especially on the teaching in schools. For after all—I speak as an examiner—even the youngest examiner is fallible. He is not so very terrible a person as some teachers seem to suppose. Not unfrequently he is a foolish person who knows but little chemistry, and has recourse to text-books for good tips.

If it is really chemistry that is taught, a good examiner will soon find out that the students have learnt the real thing, and a bad examiner will perhaps be incited to leave his rules and definitions, and attempt to gain some real knowledge of the subject in which he examines.

I think that much more care ought to be exercised in selecting those who are to examine the results of the chemical teaching given in schools. Even the seats of the higher learning are scarcely yet impressed with the really tremendous importance of sound scientific teaching; and they still too often wish to get examiners who will set papers for schools in half-a-dozen subjects, instead of selecting men who have made special study of special branches of science, and asking them to examine in their own subjects.

I have not directly insisted much on the importance of research. Of course the aim of all scientific teaching must be to train up men competent to investigate nature for themselves. But unless the men are properly trained, and are taught by the examples as well as by the precepts of their teachers what true scientific research is, they will only add a few more facts to that vast gathering of these "brute beasts of the intellectual domain" which is so often but so falsely called chemistry; and they will persuade themselves that in doing this they are advancing the scientific study of nature.

I would sum up what I have tried to say in a few words. The teaching of chemistry is still too much under the trammels of the old scholastic methods; it stands too much in fear of books, and rules, and definitions. The teacher who is in earnest about his work must break through rules, he must "swallow all formulæ," he must go constantly to nature and take his pupils with him; and his reward will be great.

BOTANY OF THE RIUKIU (LOOCHOO) ISLANDS.

WITHIN the last score of years much light has been thrown upon our knowledge of the flora of Japan. We still, however, know little about the flora of the groups of islands which lie scattered off the coast of her southern boundary. It is true that some botanical collections made during the last few years have shown a certain insight into the flora of some of the archipelago known as the Riukiu or Loochoo Islands, but it is equally true that most of the islands remain as yet absolutely uninvestigated. Since careful studies of the materials, both literature and specimens, scanty as they are, have shown that the flora of the Riukiu Islands form obviously the connecting link between that of Japan, on the one hand, and, on the other, those of South-Eastern China and the Indo-Malayan region through the islands of Hong Kong and Formosa, it seems necessary to take a clear view of the flora of the Riukiu Islands where the boundary lines of those of the two above main regions overlap. Hence it may be worth while to offer a brief summary of our present knowledge of the flora of the Riukiu Islands, taken not only from the materials already presented to the scientific world, but also from those works which have been brought out by the hands of native botanists of Eastern Asia, which have not yet been accessible to most of the Western men of science.

We observe that the earliest records of the plants of the Riukiu Islands are found in that section of the work entitled "Chuzan Denshinroku," or "Records of the Riukiu Islands," which deals with the natural products of that archipelago. These records are detailed in an extended form in another work, the "Riukiu Sambutsushi," or, "The Natural Products of Riukiu." But, as to the works which treat exclusively of the botany of the Riukiu Islands, reference should be made to the "Riukiu Sōmoku Shin Dsu" ("Illustrations of the Plants of Riukiu") and to "Shitsumon Honzō" ("Queries on the Botany of the Riukiu Islands"). The latter, on account of the excellence of its illustrations of plants, and also on account of some interesting facts connected with the preparation of the work, demands here special mention. The author, Go Keishi, a physician of the Island of Okinawa, collected during many years not only the plants of the island in which he lived, but also specimens from the Isles of Takara and Yokō, both of which are situated near 29° N. lat. and 129° E. long. He drew figures, and gave brief descriptions, of these plants, accompanied by dried specimens, forming a collection of about seventy or eighty species of plants at each time, and sent them annually, between 1781–85, to China, in order to acquire further information about those plants which he considered to be doubtful. No fewer than forty-five Chinese representative physicians and herbalists of that time, in various parts of the country,

who studied these collections, added information; they also suggested queries, many of which, however, were vague and perplexing. The results thus accumulated formed, in 1789, four volumes, exclusive of a supplementary one. These were divided into sections, "Nai-hen" and "Gwai-hen," referring to those plants used in medicine for internal and external purposes respectively. Each of these two sections is again subdivided into four parts. The work was afterwards published, about 1835, by the order of the then Prince of Satsuma, of the province of Kiusiu, but appears to me to have been printed in Yédo (Tokio). The excellence of the illustrations throughout the work makes it easy for botanists to recognize the characters of plants which are represented in the work. To take an instance, it is interesting to learn in this work, that *Epimedium macranthum*, with its violaceous variety, is found in the Riukiu Islands.[1] There is additional interest in the illustration of a species of *Balanophora*, a genus of the order now known to occur from tropical parts of Asia to nearly 34° N. lat. of Japan, through Hong Kong and the Riukiu Islands; which I have lately treated elsewhere (Journ. Linnean Society, Bot., vol. xxiv.).

Now, to examine the Western literature of the flora before us.[2] Though no small number of plants, recorded as indigenous to the Riukiu Islands, are mentioned in botanical literature,[2] the first compact exposition of the flora, drawn up under the careful examination of a number of specialists, is a memoir which appeared in the fourth and fifth volumes of *Engler's Botanische Jahrbücher*, published in 1883 and 1885 with the title "Beiträge zur Flora des südlichen Japan und der Liukiu-Inseln." This memoir, based principally upon the specimens collected by Döderlein and Tashiro in Ohshima, enumerates ninety-five species of plants from that part of the Riukiu Islands. Of these, three species are shown to be new, one of which, *Asplenium Döderleinii*, belongs to Vascular Cryptogams, while the remaining two, *Sceleria Döderleinii* and *Cinnamomum Döderleinii*, represent respectively Mono- and Di-cotyledons. Among these ninety-five species, sixty were previously known to occur in Japan, and also in other parts of Eastern Asia, the remaining thirty-five being unknown in Japan. Of these thirty-five species, sixteen are known to occur in China and the Indian Archipelago, seven in Australia, and the rest in Malacca, Ceylon, Himalaya, and other places. It must, however, be remembered that a Leguminous plant, *Lotus australis*, collected in the Riukiu Islands, though previously known in Australia, has not yet been discovered in any of the transitional regions.

Early in 1886, Maximowicz published, in the *Bullétin* of the Academy of Science of St. Petersburg, the sixth part of his work on the plants of Eastern Asia, which added no less than fifty species of Riukiu plants to those contained in *Engler's Jahrbücher*. The main portion of these plants were collected by Tashiro, who, working among other groups of the islands, made a preliminary contribution to our knowledge of the flora of Miyako-sima, a small isle lying in 45° N. lat. and 125° E. long. The new species here described from Riukiu Islands are eight, viz. *Euonymus Tashiroi*, *Galactia Tashiroi*, *Erythræa japonica*, *Anœctochilus Tashiroi*, *Premna staminea*, *P. glabra*, *Rhododendron Tashiroi*, and *Webera retusa*. The first five species are endemic to the Riukiu Islands, while the last three are found as well in the adjacent isles of Kiusiu, and in the Ogasawara Islands.[3] I may here remark that a new genus of Rubiaceous plant, established by Ahlburg (in *Bot. Zeitg.* 1878, p. 113), under the name *Aucubæphyllum Liukiuense*, which has remained doubtful, will probably be identified with *Psycotria elliptica*.

Lastly, Forbes and Hemsley's "Index Floræ Sinensis," which is intended to include all plants known in China proper, Corea, and their adjacent islands, but excluding Japan, does nevertheless embrace the flora of Riukiu Islands within its scope. As only the first two parts of this important work have made their appearance, it will perhaps be better not to draw any conclusion at present; still it may be of some interest to point out that we are now practically furnished with our first knowledge of the specimens of plants collected in the Riukiu Islands by Charles Wright and a few other botanists, and deposited in those two great botanical store-houses, the Kew Herbarium and the British (Natural History) Museum.

We cannot but feel how imperfect is our knowledge of the flora of the Riukiu Islands. We should attach much importance to its further investigation. For careful examination of the southern group of the Riukiu Islands will probably bring out, to some extent, the relations between the floras of China and Japan, and between those of the latter and the Philippine Islands, the Indian Archipelago, and perhaps even that of Australia. It will be understood that this southern group of islands, known as Yayeyama, which is situated about 24° N. lat. and between 123° and 124° E. long., remains as yet absolutely uninvestigated. The Yayeyama group consists of nine isles—namely, Ishigaki, Iriomoté, Taketomi, Kobama, Hatoma, Kuro-shima, Arakusuku, Hateruma, and Yonakuni; besides which there are three smaller ones—Uchi-Hanaré, Soto-Hanaré, and Kayama-shima, the two latter being the only uninhabited isles in the group. Hateruma, which is situated in 24° 4′ N. lat., is known as the southern extremity of the Japanese Empire. We rejoice to be able to state that our friend, Y. Tashiro, who had previously made some important collections of the plants of the Riukiu Islands, has, by the commission of the Japanese Government, lately extended his researches to this Yayeyama group, where he resided during 1885 and 1886. We hope that the collection resulting from his incessant labours will be intrusted to competent hands, and that an adequate account of it will soon be published.

Cambridge. TOKUTARO ITO.

[1] Up to the time of the publication of my paper on the "Berberidaceæ of Japan" (Journ. Linn. Soc. 1887, vol. xxii.), I found that no one had collected specimens of this plant in the Riukiu Islands.

[2] Hooker and Arnott's "Botany of Beechy's Voyage" contains some account of the plants collected in the Riukiu Islands.

[3] Generally known as the Bonin Islands, which is probably a corruption of *Munin-jima*, *i.e.* destitute of man, another Japanese name for the islands.

NOTES.

An interesting collection of specimens has just been received at the Natural History Branch of the British Museum, Cromwell Road, from Emin Pasha. They were despatched from Wadelai in November last, *vid* Zanzibar, through the kind assistance of Mr. Mackay, of the Church Missionary Society in Uganda, and have arrived at their destination in good condition. The collection consists of skins of birds and mammals, butterflies, and some anthropological objects, and, when worked out by the officers of the Museum, will be described in detail at one of the meetings of the Zoological Society during the ensuing session. In a letter received a few days ago by Prof. Flower, dated Wadelai, April 15, Emin Pasha speaks of a further consignment of specimens (chiefly ethnological), as being ready for despatch to the Museum on the first opportunity.

In 1884 Mr. John Ball, F.R.S., published a paper in the Journal of the Linnean Society (vol. xviii. pp. 203–240) giving the first comprehensive account of the flora of North Patagonia. This was based on a collection obtained from him during his travels in South America from M. Georges Claraz, a Swiss gentleman who had passed several years chiefly at Bahia Blanca. Mr. J. L. Williams-Andrews has now sent to Kew a beautifully-preserved collection made by him in the same region during the years 1881–85. The excellence of the specimens is the more remarkable as the majority of them have travelled more than six hundred miles on horseback. Mr. Williams-Andrews writes

to Kew :—"The Indians are certainly a very fast-decreasing race, and at the present day cannot exceed two thousand in number. The combs, or rather brushes, mentioned [by Mr. Ball in his paper, p. 225] are formed of a species of very tough grass, not of roots. The use of vegetable dyes is also rapidly dying out amongst them, though they still make a considerable quantity of textile fabrics, of which I have numerous examples ; the same is to be said of their silver ornaments." Vice-Consul Goodhall, of Bahia Blanca, has taken much trouble to obtain information about the plants used by the Indians for dyeing purposes. He has unfortunately failed in obtaining any trustworthy specimens, as "the Indians are most jealous about affording any information on the subject." Mr. Ronald Bridgett, Consul at Buenos Ayres, has sent to Kew some articles dyed by the Indians, in which the greens and yellows are native dyes made from roots and plants. These have been sent to the Chemistry and Dyeing Department of the Bradford Technical College. A number of Indian ornaments, also obtained by Mr. Consul Bridgett, have been forwarded to the Ethnographical Department of the British Museum.

A HIGHLY ingenious modification of Cowper's writing telegraph has been shown at the American Exhibition, by Mr. J. H. Robertson, an American electrician. The movement of a pen at the sending station varies the resistance of two electric circuits along which two currents are flowing. These varying currents act upon two coils at the receiving station so as to impart motion in two directions to a pen filled with ink, so that the resultant motion of this pen exactly reproduces the movement of the writing pen at the sending station. Mr. Robertson has replaced Mr. Cowper's resistance coils by a series of thin carbon disks, which vary their resistance with variation of pressure, as was discovered by Edison and utilized in his carbon telephone transmitter. He has also improved the receiving portion, and has made the apparatus very practical. It is being commercially worked out in the United States, and we shall watch its progress with much interest. It forms a really beautiful system of written messages, and is decidedly simpler than any previous system of facsimile telegraphy. It is very doubtful whether there is a demand for such a system, for the operation is necessarily slow.

THE session of the International Hygienic Congress at Vienna was closed on Sunday, when it was finally decided that the next session should be held in London in 1891. The meetings were remarkably successful, and did much to enlighten the public as to the nature of the questions which are now being discussed by students of hygienic laws. On Wednesday, September 28, interest was centred chiefly in the Third Section, where the circumstances under which cholera is disseminated were considered. Prof. Max Gruber, of the Vienna University, who gave an account of the incidents of cholera in Austria during the years 1885 and 1886, stated that he could find no evidence of water having played any part in disseminating the disease during that period. He believed that cholera was disseminated by human intercourse, and this experience, he said, coincided with that of English observers. On the other hand, Dr. Spattuzzi, of Naples, attributed the absolute immunity from cholera enjoyed by Naples during 1885 and 1886, and the comparatively small extent of the disease during the present year, to the excellent water supply provided in 1884. Prof. Pettenkofer made some interesting statements on the influences which, in his opinion, locality and season have on the spread of cholera. In support of his views he referred to experiences in India, where each province has its own time of year when the disease is more prevalent, but he also freely admitted the effects of pilgrimages and fairs in spreading the disease. In the course of the debate Prof. Pettenkofer again took occasion to pay a high tribute to England for the measures adopted for the prevention of cholera ; and M. Proust, of Paris, expressed himself in the same sense. Thursday was devoted to excursions and the visiting of public institutions in Vienna. On Friday Sir Douglas Galton, who presided over the First Section, offered some valuable remarks on the treatment of infectious fevers. He showed that in London much had been done by the system of isolating small-pox and scarlet-fever patients quickly, by taking them to a ship-hospital, or to hospitals remote from dwelling-houses. "But it is most undesirable," he added, "that in these isolated hospitals too many patients should be concentrated in one ward. The principle should be smaller wards, of four to six patients at most, and great simplicity of construction with ample aëration." Sir Douglas expressed the opinion that the bodies of patients who die of infectious fevers should be burned, and in this view he was supported by Sir Spencer Wells, who said that the good done by giving the people pure air and water, wholesome food, and proper dwellings must to a large extent be counteracted by the continual presence of thousands of putrefying bodies in and around centres of population. On Saturday much interest was excited by the proceedings of the Third Section, when the question of preventive inoculation against rabies was discussed. At the final meeting on Sunday morning the usual votes of thanks were passed, and Prof. Ludwig, the President, said that all the objects of the Congress had been attained. Dr. Roth, of London, expressed a hope that the "protectorate" of the next Congress would be undertaken by the Prince of Wales.

SIR SPENCER WELLS was entertained at a grand banquet on Friday evening last by some of the leading surgeons and professors of Austria-Hungary and by a deputation of the younger surgeons and students. Only one toast was given—that of "The Guest of the Evening." It was proposed by Prof. Breisky, who said that he and many others present were indebted to the teaching of Sir Spencer Wells for much of their success. Replying to this toast, Sir Spencer Wells spoke of the changes which have taken place in surgery during the last thirty-five years, and of the great results accomplished by wise sanitary legislation. "If," he said, "we had in England full power, a competent Minister of Public Health, and an efficient staff of health officers and engineers, the present death-rate of London—that is, 19 in a thousand—might certainly be reduced to 14, or probably to 12."

MR. F. A. BATHER AND MR. G. W. GREGORY have been appointed assistants in the Department of Geology of the British Museum (Natural History), to fill the vacancies caused by the resignations of Mr. William Davies and Mr. Robert Etheridge, Junior.

ON Saturday last the foundation-stone of the Walker Engineering Laboratories in connexion with University College, Liverpool, was laid by Sir James Poole, Mayor of the city. Sir A. B. Walker's original gift to the institution was a sum of £15,000, but Sir James Poole was able to announce that Sir Andrew had generously increased the amount to £20,000.

BY his will, dated August 5, 1887, Mr. Richard Quain, F.R.S., who died on September 15, has bequeathed almost the whole of his fortune, amounting to about £75,000, to University College, London, subject to certain annuities to family connexions. The College will at once benefit to the extent of about £60,000. The annual income is to be applied to "the promotion and encouragement in connexion with University College, London, of general education in modern languages (especially the English language and composition in that language) and in natural science." The trustees, Lord Justice Fry, Sir William Jenner, Mr. George Brodrick, the Warden of Merton, and Sir George Young, are authorized to carry out the testator's wishes either by salaries "or other payments to those

who teach, as by endowing professorships, or by pecuniary aid to those who are being taught, or by endowing scholarships, or fellowships, or in any other manner which the trustees may in their absolute discretion think proper," and they are requested to place themselves in communication with the Council of University College with a view to preparing a scheme for carrying out the objects of the bequest. The testator desires that in any statement of the foregoing bequest the name of his brother, the late Sir John Richard Quain, shall be associated with his name.

A SEVERE shock of earthquake was felt throughout Greece at 1 o'clock on Tuesday morning, the 4th inst., the strongest disturbance being on the northern and southern shores of the Gulf of Corinth.

MR. D. NUTT will publish shortly a new and thoroughly revised edition of Brenicker's "Logarithms, with Supplementary Tables of Natural Functions and Circular Measures of Angles to each Minute of Arc," by Prof. Alfred Lodge, of Cooper's Hill College.

MESSRS. MARCUS WARD AND CO. will issue in a few days "Teneriffe and its Six Satellites," a new work of travel in two volumes by Mrs. Olivia M. Stone, author of "Norway in June." Together with a narrative of wanderings through the seven inhabited islands, Mrs. Stone gives an historical account of the past race of inhabitants, and she draws attention to the value of the archipelago as a health resort. She has also something to say about the flora, and tables of temperature are appended.

THE third scientific voyage of Prince Albert of Monaco, in his schooner, the *Hirondelle* (200 tons), terminated at the end of August, when the vessel came back to Lorient. The Prince was accompanied by Prof. Pouchet, who made a special study of currents; and by M. Guerne, whose work was zoological. Leaving early in June, they went to the Azores, where three weeks were spent in dredging, &c. A newly-captured sperm-whale was examined. The fauna of the lakes at the bottom of craters was studied by M. Guerne. The Gulf Stream was then crossed, and a thousand of the Prince's floats were thrown out. At St. John's, Newfoundland, researches were continued. The vessel was then directed northward along the coast, but bad weather put an end to the project. In returning to Europe the party encountered a violent storm, in which grave damage was averted by the use of oil. The voyage is considered a great success. A noteworthy feature of it is the carrying on of productive and difficult dredging operations entirely without steam.

EXPERIMENTS have been recently made on the Seine, in the stoppage of steamers in motion, by means of a "cable-anchor" invented by M. Pagan. This is a cable having on it a series of canvas cones, which open out by the action of the water, and close again when drawn in the opposite way. The steamer *Corsaire*, running 13 knots, was stopped each time by the apparatus in seven or eight seconds, and in a space of 26 to 30 feet at the most. For comparison, the steamer, running full speed, was stopped in the usual way, by reversal of the engines. This took at least thirty-four seconds, and the space was 350 to 360 feet. It would thus appear that M. Pagan's apparatus effects the result in less than a tenth of the space, and a fourth of the time, of the ordinary method.

IN clinical practice it is often desirable to be able to measure in a simple and direct way the speed with which nerve-impressions are conveyed. An electric chronometer for this purpose by Dr. D'Arsonval, is described in a recent number of *La Lumière Électrique*. An axis with small terminal plate is driven round uniformly by clockwork, making one turn per second. Opposite the plate is another plate connected with the axis of a pointer on a dial. These axes are independent while a current passes through a small electro-magnet holding the second plate; but when this current ceases, a spring brings the latter in contact with the moving plate and the pointer is carried round till the current flows again. The patient to be examined having shut his eyes, the doctor applies to a part of the body a simple instrument which by this application breaks circuit, so that the pointer begins to travel. The patient is required, immediately on feeling the pressure, to press a button, which makes the circuit again, and the pointer stops. The interval can then be determined in hundredths of a second. The velocity of sensations in different parts of the body can thus be compared very rapidly. It is proved that different sensations (pressure, heat, pricking, cold, electricity, &c.) are transmitted with different velocity, and some diseases abolish some while exalting others, &c.

PROF. MUSHKETOFF'S account of his explorations in the Caspian steppes contains some interesting remarks on the work done by marmots (*Spermophylus eversmanii*) in the modification of the surface of the steppe. They made their appearance in the region only a few years ago, but their heaps of earth already cover hundreds of square miles. Like earthworms, they must therefore be regarded as a factor of some importance in modifying the surface of the soil. Their heaps of earth have an average length of 3½ metres, and a width of 2½ metres, with an average height of from 30 to 50 centimetres, and it was found that on each 2 square metres there were no less than five, seven, or even eight heaps, each of which represented at least 2 cubic metres of earth removed. It may be safely asserted that on each square kilometre of surface no less than 30,000 cubic metres of earth have been brought to the surface owing to their activity. Their influence on vegetation is also well worthy of notice.

AT a recent meeting of the Paris Biological Society, M. Mégnin gave an account of a peculiar disease which is very prevalent at present among hares in Alsace. It is a parasitic disease, a sort of pulmonary tuberculosis, caused by the presence, in the lungs, of *Strongylus commutatus* (*Filaria pulmonalis* of Frölich). The same disease was noticed in Thuringia in 1864.

AN introductory lecture at St. Mary's Hospital was delivered on Monday last by Mr. A. Critchett, Ophthalmic Surgeon to and Lecturer at St. Mary's Hospital. Speaking of the studies in which he himself is chiefly interested, Mr. Critchett said he was old enough to remember the introduction of the ophthalmoscope by that great teacher and thinker Helmholtz, who, he rejoiced to say, yet lives to witness the priceless boon which his discovery has conferred upon the human race. It was difficult for those who are now familiar with its use to conceive the wondering eagerness with which the original workers sought, by the aid of their new weapon, to bring to light those numerous hidden diseases of the eye which had till then been only partially and most imperfectly recognized. Numerous modifications of the instrument have since been introduced, and among the most recent and most useful improvements has been the ingenious adaptation of the electric light to the ophthalmoscope by Mr. Juler. After alluding to the labours of his late father, of the venerated Nestor of English ophthalmic surgery, Sir William Bowman, and of the much lamented Von Graefe, the lecturer reminded his hearers of the colossal work which had been achieved by Prof. Donders, who had opened out a new world for thought and investigation, and had elevated the study of practical optics to the dignity of a science.

A REMARKABLE series of experiments upon the synthesis of water by weight is described by Dr. E. H. Keiser in the current number of the *Berichte* of the German Chemical Society, in which not only has a known weight of oxygen in the form of copper oxide been employed, but has also been made to combine with an actually weighed quantity of hydrogen. In the

well-known experiments of Dumas it will be remembered that an indefinite quantity of hydrogen was employed, the loss of oxygen by the copper oxide and the weight of water formed furnishing the only data obtainable by the then possible experimental methods. But Dr. Keiser has succeeded in weighing his hydrogen by taking advantage of its peculiar property of being occluded by the metal palladium; it is shown that a piece of palladium 100 grammes in weight will readily take up between 0·6 and 0·7 grammes of hydrogen, when heated in a stream of the gas to about 150°. The palladium-hydrogen compound formed is perfectly stable at ordinary temperatures, and may be preserved unchanged in a vessel filled with hydrogen gas; but on heating this *quasi*-alloy the gas is slowly driven out again, and by weighing before and after the heating, the weight of hydrogen expelled may be accurately determined. This weighed quantity of hydrogen gas was then passed over heated copper oxide, and the weight of water formed determined in the usual manner. But Dr. Keiser has gone further than merely synthesize water by the most direct means possible, he has refined the process so far as to be enabled to employ it as a direct means of determining the atomic weight of oxygen. The minutest precautions were taken against error, and the purification of the hydrogen carried out in a most thorough manner, with the unexpected result that the atomic weight of oxygen is most probably slightly lower than 15·96 and more nearly 15·87. It is interesting to be reminded that Dumas states in his memoir:—"Of all analyses, that of water involves the most uncertainty. It is true that one part of hydrogen combines with eight parts of oxygen to form water, and nothing could be more exact than the analysis of water, if one were able to weigh the hydrogen and the water formed by its combustion. But the experiment is not possible in this form." However, owing to the ingenuity of Dr. Keiser, this happy result has now been achieved.

THE *Times* of September 22 contained an interesting account of the Troglodyte remains in Southern Morocco. The difficulties placed in the way of exploration in Morocco have prevented an examination hitherto of these remains. The Troglodyte caves are situated near Ain Tarsil, a village some three days' ride to the south-west of the city of Morocco, near the borders of the province of Haha. At the village the scenery undergoes a complete change. Instead of the dreary plains one comes across curious hills divided by great ravines, not unlike the cañons of California, in one of which is situated the strange city of the Troglodytes. The gorge is a narrow one, the cliffs on either side rising almost perpendicularly from the bottom of the deep valley; after progressing some little way along this valley the first caves are sighted. They are cut in the solid rock at a great height from the ground, and are in some places single, in others in tiers of two or three, one above the other. The entrances are small, varying from 3½ to 4½ feet in height and about 3 feet in breadth. In places where the rock has fallen away the entrances are faced with masonry of a neat and orderly type, and in one or two cases where the natural formation of the rock necessitated exceptionally large entrances this masonry served also the purpose of dividing the door into two parts, one of which no doubt served as a window. As to the means of access to these caves, the writer discusses three theories, all possible, but one alone of which is probable. The first is that the face of the cliff has since the formation of the caves been worn away by wind and water, and so crags and projecting pieces of rock that once rendered access possible have disappeared; secondly, the Troglodytes may have been so active as to be able to get up perpendicular smooth rock; the third, that they used ropes and ladders. He dismisses the first as unjustified by anything we know of Morocco, and inconsistent with the exi-tence of various tiers of cliffs; and as to the second, although it is stated that these cave-dwellers were swift of foot as a horse, the shape of the cliff renders climbing an impossibility. The walls of rock are perfectly smooth from the bottom to the height of the caves, which is 200 to 300 feet in many cases, and it is only in a few places that there are ledges sufficiently wide for birds of prey. In favour of the ladder theory there is the circumstance that in the doorways of many of the caves there still remains, a few inches above the floor, and crossing from lintel to lintel, a bar of wood some six inches in thickness, which must have acted as a roller for ladders. The freshness of the wood is surprising, but in the Karli Caves in the Western Ghauts there is a roof which is considered by experts to be coeval with the caves themselves. The writer was able to enter the caves in only one or two cases, a landslip having built a pile of earth and stones that rendered access possible. Passing through the low, narrow doorway, the visitors found themselves in an oblong room 15 feet by 7, leading from which at either end were smaller chambers. The wall of the larger room adjacent to the cliff was pierced with three windows; each of the smaller ones also possessed a window. The walls were rough, but the floor and ceiling were well smoothed and cut. The height of each was about 5 feet 2 inches. In no cases were bones discovered, only a little broken pottery and one or two doubtful flint-heads alone being found. No doubt much greater success will await the explorer who manages to overcome Moorish prejudice and bigotry, and by means of ropes and ladders enters the upper tiers of caves which have lain in their present state since the old race died out or took to more rational abodes. The Troglodytes cannot have been such savages as they are usually considered: their excavations are hollowed out skilfully, and their abodes show signs of great labour and some idea of care and comfort.

THE additions to the Zoological Society's Gardens during the past week include two Macaque Monkeys (*Macacus cynomolgus*) from India, presented by Miss Barker; two Arctic Foxes (*Canis lagopus*) from the Faroe Islands, presented by Mr. T. Nordenfelt; a Macaque Monkey (*Macacus cynomolgus*) from India, presented by Mr. R. Taylor; a Corn-crake (*Crex pratensis*), British, presented by Mr. Howard Bunn; a Ring Dove (*Columba palumbus*), British, deposited; a Sumatran Wild Dog (*Canis javanicus*?) from Sumatra, purchased; a Wedge-tailed Eagle (*Aquila audax*) from Australia, received in exchange; a Mule Deer (*Cariacus macrotis*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

FLAMSTEED'S STARS "OBSERVED BUT NOT EXISTING."—Prof. C. H. F. Peters, in a paper published in vol. iii. of the Memoirs of the National Academy of Sciences of Washington, has discussed Flamsteed's observations of the twenty-two stars which, it will be remembered, Baily, in his "Account of the Rev. John Flamsteed," entered in a list with the above heading, which, as he explains, means stars "of which the observations appear to be accurately recorded, but which still cannot now be found in the heavens." As the disappearance of so many stars in comparatively so short an interval of time appeared to Prof. Peters to be rather improbable, it seemed to him to be desirable that these cases should be scrutinized with the additional means of identification provided by the *Durchmusterung*, which of course was not available at the time of Baily's publication. Acting, then, on the assumption that some otherwise plausible error in Flamsteed's entry of his observation which leads to a modern star-place is to be held much more reasonable than the vague acquiescence in a supposed disappearance of the star, Prof. Peters considers that (making allowance for the probable error of a position in Flamsteed's Catalogue) he has succeeded in finding for every case at least a probable explanation. One of the observations in question—that of the object No. 1647 in Baily's "Flamsteed"—turns out to have been an observation of the planet Uranus, an explanation which was suggested by Argelander but rejected by Baily. The agreement, however, of the position of the object observed by Flamsteed with the place of Uranus computed from Newcomb's Tables is so close as to leave no doubt of its identity.

CORRIGENDA IN VARIOUS STAR-CATALOGUES.—The paper following the above in vol. iii. of the Memoirs of the National Academy of Sciences is likewise by Prof. Peters, and contains a very extensive list of corrigenda to various star-catalogues, the great majority of which have hitherto been unknown. Following Argelander's example, Prof. Peters has not been content with simply detecting an error, but has in most cases turned to the original observations, when accessible, to discover the source of error. The catalogues in which the corrections, nearly 700 in all, have been made, are: Oeltzen's Catalogue of Argelander's southern zones, Vol. vi. of the Bonn observations, Weisse's Catalogue of Bessel's zones, between Decl. – 15° and + 15°, and between + 15° and + 45°, Rümker's Catalogue of 12,000 stars (original catalogue and the new series), Schjellerup's Catalogue, Baily's Lalande, Yarnall's Catalogue (second edition), the Glasgow Catalogue of 6415 stars, Moesta's observations at Santiago de Chile, and the Geneva observations, 1842-49. The corrigenda which Prof. Peters has discovered for the Harvard zones have been published in the Annals of the Harvard College Observatory, vol. xiii. pp. 188-208, and are not given here; those for the Washington zones have been communicated to Prof. Holden, and those for Lamont's publications to the astronomers of the Munich Observatory. Prof. Peters certainly deserves the hearty thanks of all astronomers for the very essential service he has rendered in the detection and publication of these errors in their standards.

Besides these lists of errata, Prof. Peters also gives in the same memoir a list of 191 "Anonymous" stars in Yarnall's Catalogue, which he has identified with stars in other catalogues.

THE "SATELLITE" OF VENUS.—One of the standing enigmas of astronomical history has been the number of observations, the great majority of which were made in the years 1761 and 1764, by astronomers of reputation, of some small object close to Venus which was supposed to be a satellite of the planet. The fact, however, that the observations were consistent with no possible orbit, and that no trace of the body has been seen for more than a century, were conclusive proofs that it was not a real satellite, and many theories were started to account for the observations, but all of them were open to one or more fatal objections. The problem, however, seems now to have been fairly solved at last. M. Stroobant, in a paper which he has recently communicated to the Académie Royale de Belgique, gives evidence to show that in a large number of instances the object supposed to be a satellite was actually a star. He was led to this conclusion from the result of an inquiry into the observation of Rœdkiœr and Boserup, August 4, 1761, in which a star, as well as the "satellite," was mentioned as being near the planet. Curious to find out what star this was, he reduced its place and found it to be χ_3 Orionis. It then immediately struck him that the so-called satellite occupied the place of χ_4 Orionis. An attentive examination of the other observations showed that in many cases these also were probably of stars; that of Horrebow's on January 3, 1768, was unquestionably an observation of θ Libræ. Not only did the star occupy the precise place indicated for the "satellite," but the motion of Venus was such as to produce just the apparent motion ascribed to it. Several other observations can be almost as clearly referred to some star or other; the chief objection to such identification—that some of the stars in question are too faint to have been seen near the planet—being overruled by M. Stroobant's own observations, he having found that with a telescope of 6 inches aperture a star of the eighth or even of the ninth magnitude could be seen in the immediate neighbourhood of Venus. In order to present the whole question as fully as possible in one view, M. Stroobant has exhibited all the thirty-three observations in tabular form, has given all the various particulars required referring to Venus, together with an abstract of all the different theories hitherto broached respecting the true nature of the "satellite," and has reprinted the original observations themselves, whilst his own identifications with stars are illustrated by a neat series of little star-maps. Only one series, those of March 1764 (printed by a curious typographical error as Mars 1861), by Rœdkiœr remain without at least a plausible explanation, and it is possible that in this case it may be found that one of the brighter minor planets was sufficiently near to Venus to be seen in the same field with her. At all events, for the great proportion of the observations M. Stroobant has fairly cleared up the mystery which has perplexed astronomers so long.

THE LEANDER MCCORMICK OBSERVATORY.—From Prof. Ormond Stone's Report for the year ending June 1, 1887, recently received, we learn that the 26-inch refractor has been chiefly employed as heretofore in observations of nebulæ. 351 observations of miscellaneous nebulæ have been made during the year, as well as a large number of sketches, and 270 nebulæ have been discovered, which are supposed not to have been hitherto detected. Efforts have also been made to determine with as much accuracy as possible the positions of a select list of nebulæ in order that materials may be accumulated for the determination of their proper motions. Special attention has been paid to the nebula in Orion. Holding that it is not to photography but to photometry that we must look for the earliest possible evidence of change in this object, Prof. Stone has repeatedly examined the brighter portion of the nebula for the purpose of determining the relative brightness of the various condensations of which it is composed. The region "A" preceding the trapezium has been especially observed, and the brightness of its condensations compared with one another and with portions of the "Huyghenian" region. Estimates have also been made of the relative brightness of the stars in the brighter portion of the nebula in order to trace if possible any connexion which may exist between them and the nebula.

THE NICE OBSERVATORY.—M. Perrotin has just published the second volume of the "Annals of the Nice Observatory"; the first, which will contain a description of the Observatory and of its instruments, has not yet appeared, but is in preparation. The present work contains six sections, of which the first is devoted to the determination of the difference of longitude between Paris and Nice, and between Nice and Milan, which M. Perrotin set about making immediately on the foundation of the Observatory and his appointment to the Directorate. In this work he was joined by Commandant Bassot, who observed at the Observatory of the Dépôt de la Guerre, Montsouris, during the first part of the operations, and then exchanged places and instruments with M. Perrotin, the better to eliminate personal errors; M. Celoria, of the Brera Observatory, Milan, co-operating with M. Perrotin in the determination of the Nice-Milan longitude. The observations were made in the autumn of 1881, and the final results agreed very closely with the Milan-Paris longitude which had been determined in the preceding July and August by Colonel Perrier and M. Celoria, as the following figures will show:—

	h. m. s.	h. m. s.
Paris-Nice ...	0 19 51·513 ± 0·01	0 27 25·325
Nice-Milan ...	0 7 33·812 ± 0·01	
Paris-Milan (direct)		0 27 25·315

The Montsouris instrument being 0·288s. to the west of the meridian of Paris, the longitude of the pillar upon which the Nice meridian instrument was mounted is 0h. 19m. 51·225s. east of Paris.

The second section contains the determination of the provisional latitude of the Observatory, which was found to be 43° 43′ 16″·9; the pillar of the small meridian instrument being still, as for the longitude, the place from whence the observations were made. The third section contains a fine series of micrometric measures of double stars, made by M. Perrotin with an equatorial, by Eichens and Gautier, of 0·38 metre aperture and 7 metres focal length; the observations of comets and planets, which were published as made in the *Comptes rendus* of the Paris Academy, and which occupy the fourth section, being made with the same instrument. Some important notes on solar spectroscopy by M. Thollon follow in the fifth section, and include several remarkable observations of solar storms, a correspondence with M. Faye on the interpretation to be attached to the displacements and contortions of the spectral lines, and a study of the B and D groups in the solar spectrum. The concluding section contains notes by MM. Thollon and Puiseaux on the total solar eclipse of May 17, 1882; on the transit of Venus, 1882, by M. Thollon; on the remarkable crepuscular glows and "coronæ" of 1883-84, by MM. Perrotin and Thollon; and elements of Comet 1885 II. (Barnard) and of Minor Planet No. 252 (Clementina), by M. Charlois. The volume is illustrated by seven beautifully finished plates; one of which, viz. that to illustrate M. Thollon's paper on the group, affords an example of the fullness of information and beauty of execution of M. Thollon's drawings of the solar spectrum which are now being engraved for publication in the forthcoming third volume of the "Annales de l'Observatoire de Nice."

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 OCTOBER 9-15.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on October 9

Sun rises, 6h. 15m.; souths, 11h. 47m. 20·0s.; sets, 17h. 19m.; decl. on meridian, 6° 15′ S.: Sidereal Time at Sunset, 18h. 31m.

Moon (at Last Quarter October 10, 5h.) rises, 21h. 6m.*; souths, 5h. 6m.; sets, 13h. 9m.; decl. on meridian, 19° 54′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Decl. on meridian. ° ′
Mercury ...	8 10 ...	12 58 ...	17 46 ...	14 30 S.
Venus	4 9 ...	10 5 ...	16 1 ...	1 34 S.
Mars	1 32 ...	8 48 ...	16 4 ...	13 49 N.
Jupiter... ...	8 27 ...	13 19 ...	18 11 ...	13 52 S.
Saturn... ...	23 31* ...	7 20 ...	15 9 ...	19 16 N.

* Indicates that the rising is that of the preceding evening.

Occultations of Stars by the Moon (visible at Greenwich).

Oct.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. ° °
11 ...	ζ Cancri	4½ ...	3 48 ...	4 16 ...	343 299
12 ...	π′ Cancri	6½ ...	5 57	near approach	323 —
13 ...	Regulus	1½ ...	4 44 ...	5 50 ...	34 225
14 ...	χ Leonis	5 ...	5 15	near approach	305 —

Oct.	h.	
10 ...	13 ...	Venus stationary.
11 ...	15 ...	Saturn in conjunction with and 1° 20′ north of the Moon.
13 ...	8 ...	Mars in conjunction with and 0° 19′ north of the Moon.
14 ...	8 ...	Mercury in conjunction with and 2° 58′ south of Jupiter.
14 ...	14 ...	Venus in conjunction with and 7° 52′ south of the Moon.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·3 ...	81 16 N. ...	Oct. 13,	4 32	*m*
ζ Geminorum ...	6 57·4 ...	20 44 N. ...	,, 9,	21 0	*m*
			,, 14,	21 0	*M*
S Canis Minoris ...	7 26·6 ...	8 34 N. ...	,, 13,		*M*
S Herculis	16 46·8 ...	15 8 N. ...	,, 14,		*M*
U Ophiuchi... ...	17 10·8 ...	1 20 N. ...	,, 12,	3 11	*m*
		and at intervals of		20 8	
X Sagittarii... ...	17 40·5 ...	27 47 S. ..	Oct. 13,	0 0	*m*
			,, 15,	21 0	*M*
U Sagittarii... ...	18 25·2 ...	19 12 S. ...	,, 11,	0 0	*m*
			,, 13,	23 0	*M*
π Aquilæ	19 46·7 ...	0 43 N. ...	,, 12,	21 0	*m*
S Sagittæ	19 50·9 ...	16 20 N. ...	,, 11,	22 0	*m*
			,, 14,	22 0	*M*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A. °	Decl. °	
Near γ Persei	44 ...	55 N. ...	Slow.
,, 41 Arietis ...	45 ...	26 N. ...	Swift.
,, τ Geminorum ...	103 ...	33 N. ...	Swift; streaks.
	135 ...	80 N. ...	Swift; streaks.

GEOGRAPHICAL NOTES.

M. SERRANO'S recent expedition, and the second expedition which was organized by the Chilian Government in order to determine the watershed between the east and west coast of South America, have settled the most interesting fact that the high chain of the Andes in these regions does not form the watershed between the Atlantic and Pacific Oceans, but that it lies somewhat further east of it, on a plain about 500 metres high. The rivers which rise here and flow towards the Pacific have their source in small lakes, and pass through the Cordilleras in narrow gorges very difficult to penetrate. The land from the eastern slope to the watershed which forms, according to Chilian reckoning, the boundary between Chili and the Argentine Republic, is pampa, and well adapted for cattle-breeding.

CONSUL PLUMACHER, of Maracaibo, in his last report says that the peninsula of Goajira, which forms the extreme north-western part of Venezuela, is chiefly remarkable for its entire abandonment into the hands of the Indians of the same name, who have succeeded up to the present day in preserving their absolute independence, recognizing no authority except that of their own chiefs. They are divided into different clans, or tribes, all, however, being of the same race, with similar language and customs, and the different divisions now existing are developments of individual families of the same general stock. The Venezuelan Government has contented itself with placing a military post on the frontier for the protection of the whites who, attracted by the fine grazing country, have established cattle-farms and small settlements in the neighbourhood. In spite of this precaution, the Indians at times combine in numbers of several hundreds, and make a raid into the civilized territory, retreating to their own domain with the plunder. The Indians know but little of agriculture, but engage largely in the breeding of cattle. Maize and vegetables are cultivated on a small scale, and cotton, which grows wild in some localities, gives exceptional returns when any attention is paid to its culture. The customs of the Goajiras are singular and interesting, and it is noticeable that their laws and usages have remained the same from time immemorial. One of their most striking customs is a complicated system of what is called by them "payment of tears and blood," and this is the principal cause of conflict between the clans. Among all savages revenge is a sacred duty, and as, according to Goajira ethics, an entire tribe is supposed to be responsible in the aggregate and individually for the acts of one of its own members, a trifling affair in the beginning may produce grave consequences ultimately. This is one of the reasons why it is dangerous for white men to enter the Goajira territory, as the Indians make no distinction of nationality, but consider all who are not of themselves as belonging to one great family, all the members of which are responsible for a real or fancied outrage committed by an individual, and any of whom are to be considered to a certain extent as a hostage for the conduct of the rest. By the payment of the compensation of tears and blood, any injury inflicted may be condoned, it being noticed that it is not the aggrieved individual who demands this payment, but his relatives, especially those on his mother's side, who are supposed to be of closer relationship than the family of his father. If an Indian accidentally wounds himself, breaks a limb, or meets with any similar accident, his mother's family immediately demand of him the "payment of blood," on the theory that, as his blood is also their own, he has no right to shed it without compensation. The relatives of the father also claim the payment of their tears, which is of less value. Even the friends who may have witnessed the accident are entitled to compensation for the grief into which they are plunged at seeing their companion suffer. To such an extreme is this system carried out, that should a child die in the absence of one of its parents, the one can demand from the other payment for the tears supposed to be shed over the occurrence.

MUCH attention has been attracted in Australia by the results achieved by Mr. Theodore Bevan in his recent exploring expedition in New Guinea (see NATURE, August 11, p. 351). From a letter addressed to the *Times* by Mr. Thomas Bevan (September 27) we learn that the New South Wales Government have placed at Mr. Theodore Bevan's disposal a suitable steam-launch for further investigation, while the Queensland Government have allowed him the services of a thoroughly competent surveyor, and have offered the use of the steamship *Albatross* to tow the launch over to New Guinea waters. An influential committee has been formed at Sydney for the purpose of promoting Mr. Bevan's work. It was expected that the new Expedition would start in the course of September. Mr. Bevan will carry on his investigations between 200 and 300 miles to the north-west of Port Moresby, and at a still greater distance from the site of the explorations now being made on the Owen Stanley Range by the Victorian branch of the Royal Geographical Society.

ANOTHER advance has been made by Australia towards the fitting out of an Antarctic Expedition. The Agent-General for

Victoria, Sir Graham Berry, has addressed a letter to Sir Erasmus Ommanney, informing him that, in accordance with instructions, he has asked Her Majesty's Government if they would contribute the sum of £5000 towards an Antarctic exploring expedition, provided the Australian colonies contributed a similar sum. Sir Graham has received (September 2) a letter from the Colonial Office, stating that the subject is now under the consideration of Her Majesty's Government. Not only for the sake of promoting science, but also the good feeling and bond of union which should exist between mother-country and colonies; let us hope the answer will be favourable. Here at least is a common work, for the benefit and honour of both. If the reply is favourable, the Agent-General is instructed to communicate with Sir Allen Young, with the view of ascertaining on what terms he would take the command of such an Expedition. If there is any obstacle in the way of a money grant, why should not a suitable vessel be placed at the disposal of Australia?

LIEUTENANT VAN GELE has started for Bangala Station, under instructions from the head-quarters of the Congo Free State at Brussels, for the purpose of solving the problem as to the connexion, if any, which exists between the Wellé and the Mobangi. It is clear that Mr. Stanley does not mean to face this problem, as it was hoped he would do.

METEOROLOGICAL NOTES.

THE new Chief Signal Officer of the United States is making some sweeping changes in the meteorological service. We regret that the series of simultaneous meteorological observations taken at noon, Greenwich time, which began in 1875, at the instigation of the Vienna Meteorological Congress, is to be given up at the close of the present year, from lack of funds. This service has developed from a comparatively limited work to one of great magnitude, covering almost the whole of the northern, and part of the southern, hemisphere. For some time the observations were reduced, and published in the form of daily bulletins and maps, but the continued reduction of the amount at the disposal of the Chief Signal Officer rendered it necessary to give up this great and useful publication, and to limit the work to the issue of a monthly "Summary and Review of International Meteorological Observations," containing the monthly means of all the observations, with explanatory text and maps of the average isobars, isotherms, winds, and tracks of areas of low pressure. This valuable publication will be continued up to December 1887, to complete the data for ten consecutive years in a shape convenient for further research. General Greely states that it is further intended to publish charts of the average monthly pressure and temperature for each month of the year, based on ten years' international observations.

FOR some years Prof. Cleveland Abbe has been engaged, under the superintendence of the Chief Signal Officer of the United States, in the preparation of a general bibliography of meteorology, which has been very largely contributed to by Mr. Symons, by Dr. Hellmann of Berlin, and others; the number of books and pamphlets now catalogued amounts to about 52,000. Prof. Abbe stated, at the recent meeting of the British Association, that the work is now practically complete, and ready for publication. The General Committee of the Association fully recognized the high importance of the work, and expressed a hope that its publication by the Signal Office would speedily render it accessible to all nations.

THE last number of the *Annuaire de la Société Météorologique de France* for April and May contains two interesting papers. (1) On the distance of the arc of the aurora borealis from the ground, deduced from the variation of its angular velocity, or from its breadth, by M. Carlheim-Gyllenskjöld. The author states that the observations made during the Swedish expedition to Cape Thorsden prove that the angular velocity of the movement of the arc increases according to a regular law as the arc rises from the horizon towards the zenith, and that its more or less rapid change depends chiefly on the vertical elevation of the arc above the ground. The formula employed in the calculation is fully explained, and the result arrived at is that the mean height of the aurora borealis is from 30 to 45 miles above the earth, which agrees very closely with the results obtained at Ice Fjord by the Swedish Expedition. (2) A paper by M. G. Guilbert on the prediction of clouds and their succession throughout the day. The author finds that the first arrival of clouds, their movement over us, and their disappearance below the horizon are not left to chance, but on the contrary follow a regular order which renders prediction possible. Several examples are given of the connexion between the succession of the clouds and barometric depressions. The same journal also contains a communication by M. G. Tissandier on an extraordinary decrease of temperature observed in a captive balloon, on January 15 last, near the Champ-de-Mars. The wind was very strong from north-east, and the temperature at the ground was 24°·8 F. at 1h. 30m. p.m., while at about 330 feet it fell to 20°·3. At 1h. 50m. a second ascent of nearly 600 feet was made, where the temperature was 19°·1, showing an unusual diminution in the upper regions, especially as the weather at the time was very cloudy.

THE *Annuaire de l'Observatoire de Montsouris*, near Paris, for the year 1887, has been somewhat late in publication, apparently owing to recent changes in the management of the Observatory. M. Marié-Davy, who had charge of it since 1873, has retired, and from January 1 last the Observatory has ceased to be a Government establishment, and has been taken over by the Municipal Council of Paris. The work of the Observatory is, as before, divided under three heads: (1) Meteorology properly so called, and its application to agriculture and hygiene, together with magnetism and electricity; (2) chemical analysis of the air and of the rain-water collected at Montsouris; (3) microscopic study of the organic dust held in suspension in the air and water, each of these services being intrusted to a separate scientific man under the supervision of a special Commission. The *Annuaire* contains elaborate discussions under each of these heads; the temperature ob ervations date from 1699, and rainfall observations extend from 1689 to 1886; those prior to 1873 were taken at the Paris Observatory. The highest shade temperature last year was 91°·0 on July 21, and the lowest 18°·1 on January 24; the mean for the year was 52°·0. The thermometer screen is an open stand sheltered at top and sides, unlike those used in this country, and the year dates from October or December, being what is called the agricultural or meteorological year; this want of uniformity renders it difficult to compare the observations with others. The greatest monthly rainfall was in June, being 4·57 inches, and the least in February, 0·71 inches. The apparatus used in the different investigations is clearly illustrated.

PROF. HUGO MEYER discusses, in the *Nachrichten der k. Gesellschaft d. Wissenschaften* of Göttingen (No. 9, 1887), the thunder-storms at that place during the years 1857-80. The discussions of thunder-storms have hitherto mostly been for large areas, hence the results of a long series of observations referring to a single place have a special interest. The observations now in question were carefully made by M. Listing, and are preserved in the Physical Institute at Göttingen. They show, with regard to the yearly period, two principal maxima: the first occurring about the beginning of July, being later than at many other places—for instance at Prague and Munich, which have their second maximum about that time; the second maximum at Göttingen being about the middle of August. These observations also show two secondary maxima of thunder-storm frequency, one in the spring (April 1-10) and another in the autumn (September 28 to October 7): the first being a period of unusually rapid increase of temperature; and the second, one of a relatively slight fall of temperature; such a late autumn maximum being of rare occurrence. With regard to the daily period, two maxima occur in all months, one at the warmest part of the day, and one at midnight. In the winter half-year both the maxima occur some hours earlier than in the summer half-year, and the afternoon maximum in winter is divided into two parts. The occurrence of these double maxima, both in the yearly and daily periods, has been previously pointed out by Prof. von Bezold with regard to the thunder-storms in Bavaria. The tables show that thunder-storms at Göttingen only come from between N.W., through N., and round to S.E. in the warm daily and yearly periods, which tends to prove that they are heat thunder-storms. The cyclonic thunder-storms come almost exclusively from a westerly and south-westerly direction. The yearly march of thunder-storm frequency at Göttingen and various other places for the eight principal points of the compass is clearly shown by graphical representations, in the form of wind-roses; the mean direction of motion of all the storms at Göttingen is nearly from S. 68° W.

THE *American Meteorological Journal* for August contains an important article by Prof. W. Ferrel, on the relation of the

pressure to the velocity of the wind. He points out that the formula generally used by English and American engineers and meteorologists, and which seems to have come down from a preceding century, is undoubtedly very erroneous. The formula, viz. $p = 0{\cdot}005\ v^2$, is used at all altitudes and for all temperatures, without regard to the varying densities of the air. The true theoretical formula—that is, one that would hold good in case of no viscosity of the air—is given at p. 302 of his "Recent Advances in Meteorology" (NATURE, July 14, p. 255). For an average temperature of, say, 15° C., and air of the standard pressure of 760 millimetres, this formula becomes $p = 0{\cdot}00255\ v^2$, which gives the ratio 1 : 1·96 between the two constants, from which it follows that the velocities usually deduced from pressures should be very considerably increased. The author also objects to the use of the constant 3 which is employed in the reductions of wind-velocities obtained from a Robinson's anemometer of the Kew pattern, and which is about one-fourth too large, except for low velocities, as is shown by recent experiments by Stokes and Whipple in this country, and by others abroad. The same journal also contains interesting articles on the comparison of rain-gauges, by F. Pike, and on tornadoes, by H. Allen. The latter recommends the adoption of the term "low area," or "helicone," instead of cyclone, which he thinks should be applied to West Indian storms only.

THE results of rain and river observations made in New South Wales and part of Queensland during 1886, published by the Government Astronomer for New South Wales, contain a large quantity of valuable statistics on the distribution of rain, the heights of rivers, and evaporation. The number of stations in New South Wales has increased from 641 in 1885 to 772 in 1886, yet there are many parts of the colony still unrepresented. The Report is accompanied by a map, showing very plainly by means of black spots of various sizes the increase in the amount of rainfall as we go northwards into tropical regions, until Innishowen, in Queensland, caps the list with 176 inches. The greatest average rainfall in New South Wales is only 64 inches, at Antony, just under a very high mountain range, and next to this Port Macquarie, 60 inches. The mean rainfall for the whole colony amounted to 26·04 inches in 1886, being 11 per cent. more than the average for the past twelve years.

THE Meteorological Council have issued a new edition of their "Fishery Barometer Manual." The first edition of this work was published by Admiral FitzRoy about thirty years ago, and was freely distributed by the Board of Trade to small ports and fishing-stations supplied with public barometers. This useful practice of supplying barometers to fishing-stations has been continued to the present time, nearly 170 barometers having been erected, in addition to those issued by the Royal National Lifeboat Institution. The present Manual contains much additional elementary information likely to be of use to the fishermen, and refers briefly to the recent advances in the development of weather prediction, especially by means of daily charts. Reference is also made to the telegrams now received daily from America, and to the warnings issued by the *New York Herald* Service. The Manual also contains a table showing the distribution of gales on our coasts during fifteen years, from which it appears that November is generally more stormy than December, and that the maximum storminess in March, which is especially marked in North-East England, entirely disappears in South-West Ireland and South-West England.

WE had occasion recently (NATURE, June 23, p. 184) to refer to the active steps taken by Mr. Clement L. Wragge in promoting the meteorological service in Queensland, and we have now to record a further development by the publication of daily weather charts for Australasia. The charts are drawn for 8 a.m. daily, giving isobars, wind direction and force, and the temperature and humidity of the air. Rainfall is represented by dots of various sizes, while other phenomena, such as dust-storms, fog, hail, &c., are shown by appropriate symbols, and there is also a synopsis of the existing weather. The charts will be of great utility in the study of the weather of the Australian colonies.

WE are pleased to notify the publication of a Monthly Weather Record for the Mauritius, the first issue of which, for January last, has been received. The Record, which is after the style of the United States Weather Review, but without plates, contains the results of observations taken at the Royal Alfred Observatory, together with the means and extremes of temperature at four other stations, rainfall observations taken at fifty-five stations, observations taken at Rodrigues and the Seychelles, and observations taken on board ships in the Indian Ocean. The Observatory of Mauritius stands on a plain near Port Louis, three miles from the west coast, 179 feet above the sea-level. From west-south-west through west to north there is an uninterrupted view of the sea, and from north through east to south-east the ground generally slopes to the summit of the Piton, four miles distant, and 917 feet above the sea. Between south-east and south-west there is a chain of mountains, the highest peak of which bears nearly six miles due south, and has an altitude of 2874 feet above the sea. Among the miscellaneous observations it is noted that the tail of a comet (supposed at first to be Barnard's comet) was seen on January 20, and three subsequent evenings from various parts of the island.

THE BRITISH ASSOCIATION.

SECTION A—MATHEMATICAL AND PHYSICAL SCIENCE.

On the Magnetization of Iron in Strong Fields, by Prof. Ewing, F.R.S., and Mr. W. Low. Read by Prof. Ewing.—In the experiments described iron was subjected to very intense magnetization by placing a narrow neck between two massive pole-pieces. In this way values of magnetic induction higher than those previously reached had been attained. Through the kindness of Prof. Tait the large electro-magnet of the Edinburgh University had been transferred to University College, Dundee, and by its means the induction was pushed up to the value of 38,000 C.G.S. units. There seemed, indeed, to be no limit to the value attainable, and so the neck was then turned down to about one-sixth of its previous diameter, and the induction was forced up to 45,000. By turning the neck still further and annealing it, the highest value of 45,350 was reached. An attempt was made to determine the strength of the magnetic field in the immediate neighbourhood of the neck. The quantity $\frac{B - \text{field}}{4\pi}$, where B was the magnetic induction, was found to change from 1680 in an experiment where B was 24,700, to 1420 in the case of the highest value of B attained. This would favour the idea that the intensity of magnetization has a limit. But it is difficult to be quite sure that the field in the immediate neighbourhood of the neck is the same as in the neck itself.

In order to overcome this difficulty the field in the air round the neck was explored by means of three or four coils wound one on top of the other. This will show if the field is varying fast near the iron. If not, it would be natural to assume that the field is much the same as in the iron, because in the median plane there is no surface magnetism.

On Some Points in Electrolysis and Electric Conduction, by Prof. G. Wiedemann.—Before proceeding to the discussion of electrolysis the author wished to congratulate the Association on the appointment of a Committee to investigate this important subject, and further to congratulate the Committee on having Prof. Lodge to direct their labours. He had read with great interest the able report on electrolysis which had been some time ago presented to them by Prof. Armstrong. His own communication would contain much that was old, and something that was new. There was a difficulty in the definition of an electrolyte. Some people say an electrolyte is a salt. Some say it is a binary compound. But what is a binary compound? It is something which can be decomposed into two parts. But water-free hydrochloric acid does not conduct. Nevertheless it can be decomposed into two parts. Whether the water plays a part in decomposition is still an open question, although Kohlrausch thinks he has shown that in very dilute solutions the water does take part. The resistance of an electrolyte is measured by the work done in the wandering of the ions. It had been said that his view was that the viscosity is proportional to the resistance. This is not quite correctly stated. There are to be considered (1) friction of the ions in the liquid, (2) friction of the salt in the liquid, (3) friction of the whole liquid on the walls of the vessel. (3) may be avoided, and therefore we can omit it. The main thing considered has been the friction of the ions in the liquid. Kohlrausch has lately taken very dilute solutions, and can only find the friction of the salts and not that of the ions present here, which agrees with his theory. A difficulty in this connexion is, that in very dilute solutions the impurities of the

water conduct better than the salt. There may be double decompositions between those impurities and the salt. Further we know that many salts decompose in water, *e.g.* magnesium salts. Again HCl separates from a solution of ammonium chloride, and here we have acid and base separate in the solution. He did not know how to avoid this difficulty, and must content himself with pointing out the existence of it. Then another question is, What is it that is decomposed in the decomposition of salts? Is the process a simple molecular decomposition? We may ask further how hydrides are decomposed. But it is generally assumed that in the liquid it is only the salt which is decomposed. His son had sent a paper on solutions of chloride of copper. He observed that there is a change of colour in very dilute solutions; and we may be sure that in those solutions the salt has combined with the water. We cannot say whether a salt in solution is alone electrolyzed, or the salt in combination with the water. A relation between conductivity and chemical constitution had recently been obtained by Mr. Hartwig in his laboratory. He found that with rising concentration the conductivity of solutions of acids attains a maximum earlier the more carbon they contain. In regard to the friction of the salt in the liquid there is no doubt that the undecomposed salt in the liquid has a certain influence, and work must be done to produce motion of the salt in the liquid.

Prof. Quincke said that he agreed with the views of Prof. Wiedemann as to the influence of secondary decompositions in the liquids. It was difficult to distinguish between secondary and primary decompositions.

Prof. Fitzgerald, F.R.S., read a paper by Mr. F. Trouton and himself *On the Accuracy of Ohm's Law in Electrolysis*.—To avoid the difficulties due to heating of the liquid by the current the method which Chrystal and Maxwell used for solid conductors was employed, but the alternation had to be more rapid. The amount of accuracy obtained was approximately 1/2000 per cent.; and up to this Ohm's law was verified.

On the general question of electrolysis Prof. Fitzgerald said that the usual reasoning was that if the *atoms* are to be dragged asunder this requires finite E.M.F. On the whole, however, no work is done; and therefore, he contended, whatever theory may be adopted, it cannot require a finite E.M.F. to detach atoms. He believed that by this the whole Williamson-Clausius hypothesis was swept away. There were no separate atoms in the liquid. If in the case of HCl there were separate atoms of hydrogen in the liquid, surely some of them would escape from the surface of the liquid.

Prof. S. P. Thompson read a communication from Prof. von Helmholtz on *Further Researches concerning the Electrolysis of Water*.—Prof. Helmholtz has been working at the question, whether, when you electrolyze water at different pressures, it needs different electromotive forces. He found that in water which was originally free from gases the smallest E.M.F. will send a current through. He likens the difficulty which there is in getting gas to develop in an electrolytic cell originally quite free from gas to the difficulty which is experienced in getting a perfectly clean liquid to boil.

His apparatus consists of a U-tube, bent over at one end, and there blown into two bulbs, which contain the electrolyte. The electrodes are fused into the glass. One limb of the U-tube is open. From the other, which is in connexion with the bulbs containing the electrolyte, there comes off a side tube through which mercury poured into the open limb can escape, and so exhaust the space over the electrolyte to any required extent. An air-bubble is left in the large bulb above the electrolyte.

In another apparatus there was no air, and the mixed gases collected. With 1·79 volts at atmospheric pressure a balance was obtained, and the mixed gases did not increase. He finally fixes upon the superior limit of the E.M.F. with atmospheric pressure at 1·775 volt.

Experiments on the possible Electrolytic Decomposition of Alloys, by Prof. Roberts-Austen, F.R.S.—Experiments were made on gold-lead and silver-lead alloys. The results are absolutely negative. No electrolytic action whatever could be found, although cupellation would certainly have detected a variation of 1/100 per cent. in the composition.

Dr. Gladstone and Prof. Wiedemann were able to confirm the result from experiments performed by other methods in their own laboratories.

Sir W. Thomson said it was a most important discovery.

Experiments on the Speeds of Ions, by Prof. Lodge, F.R.S.—These experiments are still going on. The object is to determine directly the speeds of the ions in a liquid. The current is sent through a tube of liquid which contains some detecting substance.

At first something to give a precipitate was used, the advance of which could be timed. But this has the disadvantage of removing the substance from the tube, because the current does not affect it when it becomes solid. Now he uses fluid detectors—such as some of the aniline bodies—to detect the advance of acidity or alkalinity. Thus, for example, he may have the tube filled with solution of sodium chloride, with a trace of caustic soda, and a body which is coloured in alkaline solution, but which loses its colour when the alkalinity disappears. If now, in the course of the electrolysis, ions from the substance being electrolyzed which will unite with the Na of the caustic soda travel along the tube, they will cause the alkalinity to disappear, and the rate at which this change travels can be measured.

The composition of the liquid, however, does not remain constant, and therefore we get a broken slope of potential in the tube, because the bad-conducting alkali is turned into the good-conducting acid. This difficulty is got over by making the principal ingredient in the measuring tube the same as the product of the action for any given case. A small addition to its amount is therefore of no consequence.

The theory of Kohlrausch with regard to the speed of ions was shown to be in accordance with the results.

On Chemical Action in a Magnetic Field, by Prof. H. A. Rowland.—It had been observed by his colleague, Prof. Remsen, that if a thin plate of iron be placed between the poles of an electro-magnet and then acted on by $CuSO_4$, the copper was deposited in lines very similar to the equipotential lines. Around each pole was a clear space where the iron was not acted on at all. This part of the field is of course the part where the rate of variation of the square of the magnetic field is greatest; and it occurred to Prof. Rowland that the want of action of the sulphate of copper in this position was due to the attraction of the magnet on the iron. With the help of Mr. L. Bell he had carried out experiments on the point.

Between the poles of a powerful electro-magnet was placed a glass beaker containing the liquid whose action upon iron it was desired to test. Nitric acid generally acted very well; so did sulphate of copper, and almost any salt which would deposit metal on iron. In the liquid were immersed two pieces of iron, one of which was pointed. The greater part of each piece was covered with wax, and what was exposed to the liquid was a point in the one case and a plane surface in the other. They were connected through a galvanometer, and a current was obtained which was not reversed on reversing the direction of the current of the electro-magnet. This indicated that the point was protected from the action of the liquid.

On the Electro-deposition of Alloys, by Prof. S. P. Thompson.—In a mixture of metals which is electrolyzed, the most negative metal comes down first. Prof. Thompson made a series of experiments on solutions of zinc and copper in cyanide of potash solution of different strengths. The electromotive force was measured for each strength of the cyanide of potash solution. The curves representing the E.M.F. for copper and zinc were found to cut at a certain strength of the KCN solution. Beyond this strength copper became positive to zinc. In the ordinary brassing solution he found that it depended on the temperature whether zinc was positive to copper or copper to zinc.

On the Action of the Solvent in Electrolytic Conduction, by T. C. Fitzpatrick. This paper was communicated by Mr. W. N. Shaw.—Mr. Fitzpatrick found that although methyl alcohol has greater conductivity than water, yet a solution of calcium chloride in the former liquid is a worse conductor than an aqueous solution. He found similar results for calcium nitrate, lithium chloride, and lithium nitrate solutions. Solutions in ethylic alcohol were also used. He was much impressed with the idea that electrolysis is the electrolysis of molecular aggregates.

The next paper was by Prof. S. P. Thompson, on the *Industrial Electro-deposition of Platinum*. He exhibited specimens illustrating a new process.

The Princeton Eclipse Expedition, by Prof. C. A. Young.—The expedition had its origin in his desire to repeat observations

made by him seventeen years ago, as objection had been taken, not to the accuracy of the observations themselves, but to the conclusions he had drawn from them. Prof. Libby was to do the photography. Photographs of the corona were to be taken; and they were anxious to determine whether there are true dark lines in the corona or not. The place selected for the observations lay 150 or 160 miles to the north-west of Moscow. It is needless to say that on the morning of the eclipse it rained, and hardly anything could be done. They made an attempt to determine the end of totality by the amount of light. The diminution of the light was gradual, but after totality there was a sudden burst.

Prof. L. Weber, of Breslau, described photo-metric measurements made during the eclipse at Breslau; and then read a paper on *Observations of Atmospheric Electricity*. Prof. Weber said that the increase of potential seemed to be a linear function of the height; but the presence of dust in the air disturbed this relation. The earth represents a surface of equipotential, and the other surfaces of equipotential are parallel, but come closer together above the mountain-tops.

Prof. Schuster said that, granting that the earth has a given potential at any moment, the convection-currents in the air would tend to reduce this, or to equalize the potential within the earth itself.

Prof. Everett remarked that wherever electricity is carried down by raindrops, an inequality of potential will be caused; and evaporation would also cause inequalities.

Prof. Rowland said that observations had been made during the last four years at his laboratory by the U.S. Signal Service. He did not see how the raindrops could disturb the distribution of potential much. If the earth is electrified most of the electricity would be on the outside of the atmosphere. He therefore looks for some other theory, and has given one in the *Phil. Mag.*, viz. that the earth would naturally be uniformly electrified if it were not for currents of air in the upper atmosphere, which will carry the electricity of the atmosphere towards the poles, making auroras there. At the equator, therefore, a space must be left which has to be filled up with electricity, and this takes place by thunderstorms. Accordingly there is a circulation of electricity. In this connexion it is to be remembered that thunderstorms are most common about the equator.

The Hygrometry of Ben Nevis, by Mr. H. N. Dickson.—This paper gives an account of observations which were undertaken for the purpose of testing the applicability at high-level stations of existing tables and formulæ for calculating the dew-point and humidity from the readings of wet- and dry-bulb thermometers. The construction of the direct hygrometer used, that of Prof. Chrystal, is described, and the action of the wet and dry bulbs under different meteorological conditions is examined in considerable detail; the results showing that for investigations of this kind a great range of humidity is necessary, the indications of the wet and dry bulbs being very uncertain when the difference between them is small.

The reduction of the observations is performed, in the first place, by a graphic method, from which the following expression is reduced: $f' - f'' = (t - t')k$, f' being the vapour-pressure at the temperature t' of the wet bulb, f'' that at the temperature of the dew-point, and t the air temperature. The truth of the above equation being assumed, the values of the quantity k are next found by direct calculation from the observations. A sudden large change takes place in its value at the freezing-point, and a similar, though much smaller, discontinuity is shown to occur when the wet bulb stands between 39° and 40°.

The Different Varieties of Thunderstorms, and a Scheme for their Systematic Observation in Great Britain, by the Hon. R. Abercromby.—The writer said that there were three well-defined types of thunderstorms in this country: (1) squall thunderstorms, *i.e.* simply a squall associated with thunder and lightning; (2) a very common form which occurs in secondary cyclones: the nature of this class needed investigation; (3) far the most curious class was that which might be called line-thunderstorms, because their shape was a long narrow belt sometimes 200 or 300 miles long and only 4 or 5 broad. They move broadside on, and are usually preceded by a squall of extreme violence. He explained a scheme for the future systematic study of thunderstorms, and invited the co-operation of volunteer observers.

Sir W. Thomson said that the natural history of thunderstorms was less known than any other part of meteorology, and that Mr. Abercromby's scheme would be likely to give much information on the subject.

On the Magnetization of Hadfield's Manganese Steel in Strong Fields, by Prof. J. A. Ewing, F.R.S., and William Low.—Messrs. Hadfield, of Sheffield, manufacture a steel containing about 12 per cent. of manganese and 0·8 per cent. of carbon, which possesses many remarkable qualities. Prominent amongst these, as the experiments of Hopkinson, Bottomley, and Barrett have shown, is a singular absence of magnetic susceptibility. Hopkinson, by applying a magnetic force, $\mathfrak{H}$, of 244 C.G.S. units to a specimen of this metal produced a magnetic induction, $\mathfrak{B}$, of only 310 C.G.S. units; in other words, the permeability μ was 1·27, and the intensity of magnetization $\mathfrak{I}$ was a little over 5 units.

The experiments made it clear that even under magnetic forces extending to 10,000 C.G.S. units the resistance which this manganese steel offers to being magnetized suffers no break-down in any way comparable to that which occurs in wrought iron, cast iron, or ordinary steel at a very early stage in the magnetizing process. On the contrary, the permeability is approximately constant under large and small forces.

The conclusion has some practical interest. It has been suggested that this steel should be used for the bed-plates of dynamos and in other situations where a metal is wanted that will not divert the lines of induction from neighbouring spaces. In such cases the magnetic forces to which manganese steel would be subjected would certainly lie below the limit to which the force has been raised in these experiments. We may therefore conclude that in these uses of the material it may be counted upon to exhibit a magnetic permeability only fractionally greater than that of copper, or brass, or air.

On the Influence of a Plane of Transverse Section on the Magnetic Permeability of an Iron Bar, by Prof. J. A. Ewing, F.R.S., and William Low.—It has been remarked by Prof. J. J. Thomson and Mr. H. F. Newall that when an iron bar is cut across and the cut ends are brought into contact, the magnetic permeability is notably reduced (Cambridge Phil. Soc. Proc., February 1887). The attention of the authors was directed to the matter by finding the same phenomenon present itself in experiments on the magnetization by the "isthmus" method; and they proceeded to examine the effect by an application of the method Hopkinson has used to measure magnetic permeability ("Magnetization of Iron," Phil. Trans., Part 2, 1885). A round bar, nearly half a square centimetre in section, and 13 centimetres long, had its ends united by a massive wrought-iron yoke to reduce it to a condition approximating to endlessness, and its magnetization by various magnetic forces was examined, both when free from stress and when compressed by a load of 226 kilos. per square centimetre. It was then cut in the lathe and the halves placed in contact, and the magnetization again examined with and without load. It was next cut into four parts, and finally into eight parts, and magnetized in each case. Every new plane of section caused a notable loss of permeability. The following are the maximum values of the permeability in each case:—

Solid bar	... ...	1220
Bar cut in two	...	980
Bar cut in four	...	640
Bar cut in eight	...	400

Next another bar was tested, first when solid, next with one cut finished in the lathe, and finally with the cut surfaces faced true by scraping and comparing them with a Whitworth plane. So long as the bar was not compressed its magnetic permeability was nearly the same, whether the ends were left roughly finished or were faced true. But when load was applied the effect of facing the ends was remarkable: the faced bar then behaved as a solid bar would, while the bar with rough cut ends still showed a decided defect of permeability as compared with a solid bar.

This made it seem highly probable that the whole effect was due to a film of air between the cut faces. Applying Hopkinson's method to calculate the thickness this film would need to have in order to account for the observed increase of magnetic resistance, the authors find its thickness is only about 1/35 of a millimetre when the magnetic force is 10 C.G.S. units, and diminishes to about 1/70 of a millimetre when the force is 50 C.G.S. In the case of the bar cut into four or eight parts, each cut has an effect equivalent to the introduction of a film of

this thickness. The authors conclude that in all probability the whole phenomenon is due to the surfaces being separated by these short distances.

On the Magnetic Properties of Gases, by Prof. Quincke, Ph.D.—A few years ago he invented what he called a magnetic manometer. It consists of a bent tube, of which one limb is much wider than the other. In the wide limb is the gas to be experimented on. The narrow limb and the connecting horizontal piece contain liquid, and the difference of level of the liquid in the narrow limb produced by the magnetic field is what is measured.

The magnetic pressure per unit of area is given by the formula—

$$p = \frac{R}{8\pi} H_1^2.$$

If h be the difference of level of the liquid in the two limbs, *i.e.* the hydrostatic pressure, we have—

$$h\sigma = \frac{R - R_1}{8\pi} H_1^2.$$

The smallest diamagnetic constant for the gases experimented on was found to be that of hydrogen. Oxygen had the highest. He compares his results with Faraday's, and finds that they agree substantially, the differences being probably due to impurities.

Final Value of the B.A. Unit of Electrical Resistance as determined by the American Committee, by Prof. H. A. Rowland.—His determination in 1876 gave 1 B.A. unit = ·9878 ohm. For his present determination the apparatus was on a very large scale. He employed both the Kirchoff and the Lorenz method. By the former method he got a final value of ·98646 ± 40, by the latter a value of ·9864 ± 18; so that the latter method has a probable error of less than a half that of the former. His value for the resistance of 100 cubic centimetres of mercury came out ·95349 B.A. units.

Lord Rayleigh said that the results showed that the absolute determination of the B.A. unit by various experiments agreed much better than the comparison with the mercury standard. This was exactly the opposite of what he would have expected. Prof. Rowland had suggested that one cause of the difference between their determinations of the mercury standard might be that in the American experiments the tubes had been mechanically wiped, so that there was no chance of dust remaining in them. He hardly thought, however, that this was likely. The want of uniformity in the diameter of the tube might possibly have an effect.

On Induction between Wires and Wires, by W. H. Preece, F.R.S.—A continuation of a subject brought before the Association last year, when it was shown that electro-magnetic disturbances extended to distances much greater than was imagined, and that effects were observed across many miles of country. Experiments were made on the banks of the Severn and Mersey, on the Portcawl Sands of South Wales, in the fields in the neighbourhood of Cardiff, on the roads and railways of Oxfordshire, Worcestershire, and Shropshire, in the air and under water, in the corridor of the General Post Office in London; and the law was formulated that the distance depended directly on the strength of the currents inducing the disturbance and on the length of the wires opposed to each other, and inversely on the square of the distance separating them, and on the electrical resistance of the disturbed wire.

The influence of 1 mile of wire carrying 1 ampere of current can apparently extend to a distance of 1·9 mile. The law is given by the following formula:—

$$C_2 = M\frac{C_1 l}{d^2 r_2},$$

where C_1 is the primary current, C_2 the secondary, l the length of the wires opposed to each other, d the distance separating them, r_2 the resistance of the secondary circuit. When these quantities are represented in C.G.S. units, M equals ·005. The current induced by 1 mile, of 1 ampere at 1 mile distant is $1{\cdot}3 \times 10^{-13}$ ampere. A current is still perceptible at 1·9 mile distant; hence we can calculate that a Bell telephone requires six ten-thousand-millionths of a milliampere, or, in figures, ·0000000006 milliampere, to be audible.

One curious result of these inquiries is that the disturbances are transmitted equally well through water and the earth as through air, and hence our cables are disturbed as well as our land wires. Communication with coal-pits is possible, though nothing but the earth intervenes.

On the Effect of Continental Lands in altering the Level of the adjoining Oceans, by Prof. Edward Hull, F.R.S., Director of the Geological Survey of Ireland.—The effect of the attraction of continental lands upon the oceanic waters adjoining seems to have been very much overlooked by British physical geographers. That some slight effect arises in the direction of elevating the surface of the ocean in proximity to the coast is generally admitted, but the amount of rise is considered to be small, perhaps insignificant. The prevalence of these views was attributed by the author to the widespread influence of Lyell's hypothesis of the uniformity of the ocean-surface all over the globe.

The author proceeded to discuss the effect of continental lands, showing that this was in the first instance divisible under two principal heads: The effect (1) of the unsubmerged, and (2) of the submerged masses. In the former case, where the mass rose above the surface, one component of the attraction acted in a more or less vertical direction; in the second case, all in a lateral direction; but both had the effect of elevating the surface of the ocean. The horizontal distance to which the vertical effect extended owing to the curvature of the earth's surface was then considered: and it was shown that, where continental lands rise from a deep ocean, the effect of the lateral attraction far exceeds that of the vertical attraction of the unsubmerged mass. Prof. Stokes has furnished the author with a hypothetical case, in which the elevation of the ocean was estimated to reach 400 feet above the mean geodetic surface of the earth.

For the purposes of illustration three cases were selected, viz.:—

(1) The table-land of Mexico, between lats. 18° and 26° N.
(2) The table-land of Bolivia, ,, 19° and 26° S.
(3) The Andes of Chili, ,, 26° and 35° S.

The mean elevations, distances from the ocean, and extent having been determined, and the mean density of the crust being taken at 2·6 for emergent, and 1·6 for unsubmerged land, the results of the attraction of the mountain masses in each case were as follows:—

(1) Mexico, 780 feet; (2) Bolivia, 2160 feet; (3) Chili, 1580 feet.

The total calculated rise of the ocean-waters at a distance of 900 miles from the coast in lat. 10° S. would amount to 2568 feet.

The above results, which are probably rather under than over estimates, fall considerably short of those to be drawn from Suess and Fischer's formula, but are probably much in excess of the views held by British physical geographers generally; and the conclusion was drawn that if the same processes of reasoning and calculation were applied to all parts of the world, it would be found that the ocean waters were piled up to a greater or less extent all along our continental coasts, producing very important alterations in the terrestrial configuration as compared with an imaginary ellipsoidal, or geodetic, surface, to which all these changes of level must necessarily be referred.

On a Standard Lamp, by Prof. A. A. Vernon Harcourt, F.R.S.—At one of the meetings of this Section last year a lamp devised by the author for producing a constant amount of light was shown and described by Mr. W. S. Rawson. The lamp now exhibited serves the same purpose, but is simpler in principle, more easily adjusted, and less affected by draughts. It consists of a glass reservoir with tubulure and stopper of the form and size of a large spirit-lamp, mounted on a metal stand provided with levelling screws. The wick can be turned up and down in the normal manner within a long tube attached to the body of the lamp. Round this tube is a wider tube 100 × 25 mm., and the two being joined together above and below by flat plates constitute the burner of the lamp. When the burner becomes warm by conduction of heat from the flame of the lamp, the pentane in the wick volatilizes and burns at a considerable distance above the point to which the wick is turned down. Thus the size, or texture, or quality of the wick does not affect the flame. Around the burner and the lower part of the flame is another cylinder open at both ends and contracted above the burner to a tube 21 mm. in diameter. A similar tube forms the lower part of an upper chimney, which is enlarged above to a diameter of 25 mm. The upper part of the

flame is concealed by this chimney, excepting where a narrow slit, 10 × 3 mm., on each side shows the tip of the flame and enables its height to be regulated. Through the interval between the two chimneys the flame shines, and the light which it gives is the same whenever the tip of the flame is visible through the slit, whether towards the lower or the upper end. The two chimneys are attached together by two curved metal bands sufficiently removed from the flame on either side not to affect it. The attachment of the bands to the lower chimney are adjustable, so that the opening through which the central parts of the flame are seen may be made larger or smaller. By means of small cylindrical blocks, whose thickness is accurately gauged, the width of the opening may be set either to that at which the light emitted is one candle, or, if a greater or smaller light is desired, a candle and a half or half a candle. The liquid with which the lamp is fed is pentane, obtained in a manner already described from American petroleum.

Mr. W. N. Shaw read a paper by Mr. J. T. Bottomley on *Expansion by Heat of Wires under Pulling Stress*. The wires were two fine copper wires. One of them carried about half its breaking weight and the other about a tenth of its breaking weight. The wires were suspended in a tube, a scale being attached to one, and a pointer moving over the scale to the other. Thermometers were inserted into the tube at various points, and the wires were heated by passing steam into the tube. It was found that the more heavily weighted wire extended much more than the lightly-weighted one. An amount of permanent elongation remained, but more in the heavily-strained wire. Each time the heating was done there was more and more permanent elongation, and ultimately one of the wires was broken under less than its breaking load in the normal state. Further experiments were made with wires which had been hardened, and the final result is that the coefficient of expansibility for heat of copper wire strained by a certain weight is greater than that of similar wire less heavily weighted.

Experiments on Electrolysis and Electrolytic Polarization, by W. W. Haldane Gee, Henry Holden, and Charles H. Lees.—This is a preliminary notice of experiments that are in progress in the Owens College Physical Laboratory. The experiments fall under four heads: (*A*) electrolysis under pressure; (*B*) time-rate of fall of polarization in closed circuit; (*C*) irreciprocal conduction; (*D*) the production of an oily fluid in electrolysis with palladium electrodes.

A. Numerous experiments have been made in order to determine the variation of the resistance of polarization of a sealed voltameter in which dilute sulphuric acid was electrolyzed between platinum wire electrodes, it being thus subjected to the pressure (up to 200 atmospheres) of the evolved gases. It was found that the resistance markedly decreased, and the polarization decreased slightly. These changes may, however, it is thought, be due to change of temperature, the influence of which would appear from later experiments not to have been fully eliminated. In two cases no change whatever was perceived: (1) when two platinum *plates* were used as electrodes, and (2) when two voltameters were connected together forming a sealed vessel, one voltameter being used to increase the pressure, while observations were made on the other. As it has not been possible to obtain glass tubes sufficiently strong for the high pressures desired, an apparatus of gun-metal has been constructed. This apparatus, which is fitted with a Bourdon's gauge recording to six tons on the square inch, may also be arranged for pressure experiments in general by attaching to it, by means of a strong metal tube, a suitable receiver. In two of the experiments, when the pressure had reached between 200 and 300 atmospheres, the evolved oxygen and hydrogen gases combined with explosion, although precautions had been used to prevent the gases from coming into contact with the platinum, except in the liquid.

B. The object of this research was to try to learn the parts played by the various portions of the evolved gases: (1) that occluded by the electrodes; (2) that deposited on them; and (3) that contained in the liquid in influencing polarization. The method employed was to vary the conditions under control, *e.g.* time of changing, density of current, &c., and to observe the time-rate of the fall of the polarization thus produced in closed circuit. It was found to be very difficult to apply this method, because though the conditions under control were kept as constant as possible, yet the time-rates of fall in two successive observations were often different. This was thought to be due to the insufficient cleaning of the electrodes between each experiment, and various methods were tried to remedy it, with the general result that the more perfect the cleaning became the more regular did the curves giving the time-rate of fall of the polarization become, but still the inconsistencies were not wholly removed. Heating the electrodes by the electrical current seemed preferable to the other methods of heating.

C. Whilst electrolyzing strong sulphuric acid between platinum electrodes, it was noticed that when the current density at the anode had exceeded a certain value decomposition apparently ceased. The value of the anode current-density necessary to produce this phenomenon is increased by diminishing the concentration or increasing the temperature of the acid (thus diminishing the viscosity), and is diminished by cleaning the electrodes. It was found that this great diminution of the current was not caused by the formation of an opposing E.M.F., but by a sudden increase of from 500 to 50,000 ohms in the resistance of the circuit. That the insulating condition occurs at the anode is shown by successively replacing the kathode and the anode by clean plates; in the first case the stoppage of the current persists, in the second case the current is readily conducted. The cause seems to be a sheath of oxygen bubbles which firmly adhere to the anode when the insulating condition is formed. The film is removed by momentarily breaking the circuit, or short-circuiting the voltameter, or reversing the current, or by replacing the anode by a clean plate.

D. During the electrolysis of various liquids between palladium electrodes it has been observed that a dense-looking liquid streams from *one* of the electrodes (the anode in dilute sulphuric acid, the kathode in caustic soda) *after a reversal of the current*. The liquid seems to be a compound of oxygen and hydrogen, presumably hydroxyl.

On the Vortex-Theory of the Luminiferous Ether, by Prof. Sir W. Thomson, F.R.S.—"In endeavouring to investigate turbulent motion of water for my communication on that subject to this Section, I have found a solution (many times tried for within the last twenty years) of the problem—to construct, by giving vortex-motion to an incompressible viscid fluid, a medium which shall transmit waves of laminar motion as the luminiferous ether transmits waves of light. Let *xav*, *xzav*, *xyzav* denote space-averages, linear, surface, and solid, through infinitely great spaces." After defining and illustrating this method of averages by examples, and remarking in passing that a general property of it is that

$$xav\,\frac{dQ}{dx} = 0,$$

where Q is any quantity which is finite for infinitely great values of x, he proceeded thus:—

Suppose now the motion to be homogeneously distributed through all space. This implies that the centres of inertia of all great volumes of the fluid have equal parallel motions, if any motions at all. Conveniently, therefore, we take our reference-lines, OX, OY, OZ, as fixed relatively to the centres of inertia of three (and therefore of all) centres of inertia of large volumes; in other words, we assume no translatory motion of the fluid as a whole. This makes zero of every large average of u, and of v, and of w; u, v, and w being the velocity-components; and we may write as the general expression for nullity of translational movements in large volumes—

$$0 = \text{ave. } u = \text{ave. } v = \text{ave. } w,$$

where ave. denotes the average through any great length of straight or curved line, or area of plane or curved surface, or through any great volume of space. In terms of this generalized notation of averages, homogeneousness implies—

$$\text{ave. } u^2 = U^2,\ \text{ave. } v^2 = V^2,\ \text{ave. } w^2 = W^2,$$
$$\text{ave. } vw = A^2,\ \text{ave. } wu = B^2,\ \text{ave. } uv = C^2,$$

where U, V, W, A, B, C are six velocities independent of the positions of the spaces in which the averages are taken. These equations are, however, infinitely short of implying, though implied by, homogeneousness.

Suppose now the distribution of motion to be isotropic. This implies, but is infinitely more than is implied by, the following equations in terms of the above notation, with further notation, R, to denote what we shall call *the average velocity* of the turbulent motion—

$$U^2 = V^2 = W^2 = \tfrac{1}{3}R^2,$$
$$0 = A = B = C.$$

Large questions now present themselves as to transformations which a distribution of turbulent motion would experience in an infinite liquid left to itself with any distribution given to it initially. If the initial distribution be homogeneous through all large volumes of space, except a certain large finite space, S, through which there is initially either no motion or turbulent motion, homogeneous or not, but not homogeneous with the motion through the surrounding space, will the fluid which at any time is within S acquire more and more nearly as time advances the same homogeneous distribution of motion as that of the surrounding space, till ultimately the motion is homogeneous throughout? Probably, I think I may say certainly, yes—at all events for a large class of cases.

But can it be that this equalization comes to pass through smaller and smaller spaces as time advances? In other words, will any given distribution, homogeneous on a large enough scale, become more and more *fine-grained* as time advances? Probably *yes* for some initial distributions; probably *no* for others. Probably *yes*, for vortex-motion given continuously through all of one large portion of the fluid while all the rest is irrotational. Probably *no* for the initial motion given in the shape of equal and similar Helmholtz rings, of proportions suitable for stability, and each of overall diameter considerably smaller than the average distance from nearest neighbour. Probably also *no*, though the rings be of very different volumes and vorticities. But probably *yes* if the diameters of the rings or of many of them, be not small in comparison with distances from neighbours, or if the individual rings, each an endless slender filament, be entangled or nearly entangled among one another.

Again a question: If the initial distribution be *homogeneous and eolotropic*, will it become more and more isotropic as time advances, and *ultimately quite isotropic?* Probably *yes* for any random initial distribution, whether of continuous rotationally-moving fluid or of separate finite vortex-rings. Possibly *no* for some symmetrical initial distribution of vortex-rings, conceivably stable; though it does not seem probable that there is any such stability.

If the initial distribution be homogeneous and isotropic (and therefore utterly *random* in respect to direction) will it remain so? Certainly *yes*.

We shall now suppose the initial motion to consist of a laminar motion $[f(y), 0, 0]$ superimposed on a homogeneous and isotropic distribution (u_0, v_0, w_0); so that we have—

$$\text{when } t = 0,\ u = f(y) + u_0,\ v = v_0,\ w = w_0;$$

and we shall endeavour to find such a function, $f(y, t)$, that at any time, t, the velocity-components shall be—

$$f(y, t) + u, v, w,$$

where u, v, w are quantities of each of which every large enough average is zero.

With this assumption the equations of motion yield the following—

$$\frac{df(y, t)}{dt} = -\,xzav\frac{d(uv)}{dy}.$$

It is to be remarked that this result involves no isotropy, no homogeneousness in respect to y; and only homogeneousness of *régime* with respect to y and z, with no translational motion.

The translational component of the motion is wholly represented by $f(y, t)$, and, so far as our establishment of the above equation is concerned, may be of any magnitude, great or small relatively to velocity-components of the turbulent motion. It is a fundamental formula in the theory of the turbulent motion of water between two planes; and I had found it in endeavouring to treat mathematically my brother Prof. James Thomson's theory of the "Flow of Water in Uniform *Régime* in Rivers and other Open Channels" (Proceedings of the Royal Society, August 15, 1878). In endeavouring to advance a step towards the law of distribution of the laminar motion at different depths, I was surprised to discover the law of propagation as of distortional waves in an elastic solid, which constitutes the conclusion of my present communication—

$$\frac{d}{dt}\,xzav\,(uv = -\,\tfrac{1}{3}R^2\frac{df(y, t)}{dy}.$$

Eliminating the first member from this equation, by the former, we find—

$$\frac{d^2f}{dt^2} = \tfrac{1}{3}R^2\frac{d^2f}{dt^2}.$$

Thus we have the very remarkable result that laminar disturbance is propagated according to the well-known mode of waves of distortion in a homogeneous elastic solid; and that the velocity of propagation is $\frac{\sqrt{2}}{3}$ R, or about ·47 of the average velocity of the turbulent motion of the fluid. This might seem to go far towards giving probability to the vortex-theory of the luminiferous ether.

But a difficulty remains unsolved: a possible rearrangement of vortices within each wave, giving rise to dissipation of the wave-energy.

The mathematical investigation appears in full in the October number of the *Philosophical Magazine*, with some slight farther considerations regarding this virtual viscosity, and the question of what, if any, distribution of vortices can either have no tendency to the vitiating rearrangement, or can, with the requisite fine-grainedness, be slow enough in the vitiating rearrangement to allow the propagation of waves of light to go on through a hundred million million miles of space, or a million times the earth's distance from the sun.

The Committee of the Section reported that at a meeting of the Committee it had been resolved, on the motion of Prof. Gustav Wiedemann, of Leipzig, seconded by Sir William Thomson:—"That this Committee of the Mathematical and Physical Science Section of the British Association hereby convey to Dr. Joule their sense of the great loss sustained by the Section in consequence of his inability to take part in this meeting of the British Association in his native city, and express their sincere regret at the cause of this loss, and their hearty sympathy with him in his illness. The Committee take this opportunity of recording their appreciation of the splendid work of this most painstaking and conscientious seeker after truth, who, with his discoveries, has led the way in the greatest advance in knowledge made in this age, and, by his life, has conferred on mankind a precious example for their admiration and imitation."

SCIENTIFIC SERIALS.

American Journal of Science, August.—History of the changes in the Mount Loa craters (continued), by James D. Dana. In this paper the history of Kilauea is continued from January 1840 to the end of 1886, during which period sufficient facts were accumulated for a widened and apparently final explanation of the method of filling the pit. The eruptions of 1849, 1855, 1868, and 1886 are fully described, and the whole subject is illustrated with maps of the burning mountain at various dates during the period under consideration.—On some phenomena of binocular vision (continued), by Joseph Le Conte. In this paper, the twelfth of the series, the author deals with certain peculiarities of the phantom images formed by binocular combination of regular figures. The phenomena here described, none of which have hitherto been satisfactorily accounted for, are all explained by the law of corresponding points, justly regarded as the most fundamental law of binocular vision.—Chemical integration, by T. Sterry Hunt. In this paper the author deals more fully with several points connected with chemical metamorphosis, which were more briefly noticed in his recently published work, entitled "A New Basis for Chemistry."—Studies in the mica group, by F. W. Clarke. In this paper the author deals with specimens of muscovite from Alexander County, North Carolina; of lepidomelane from Baltimore and Litchfield, Maine; of iron biotite from Auburn, Maine; and of iron mica from near Pike's Peak.

SOCIETIES AND ACADEMIES.

LONDON.

Institution of Mechanical Engineers, September 30.—Mr. E. H. Carbutt, President, in the chair.—A supplementary paper by Major Thomas English, R.E., on the initial condensation in a steam cylinder, was read and discussed in connexion with the paper by the same author on the distribution of heat in a stationary steam-engine, read at the spring meeting on May 17, an abstract of which has already appeared in NATURE (vol. xxxvi. p. 115). The supplementary experiments were carried out in a portable engine of ordinary type, the cylinder of which was jacketed on the cylindrical portion but not at the ends. The steam was admitted directly from the boiler into the steam chest, and the quantity required for each experiment being small compared with the capacity of the boiler, no question of priming or condensation before admission can arise. The con-

that lies spread below him, can for a moment gainsay the fact, whatever may be his opinion as to the conclusions drawn from it: by nearly all geologists they are accepted as the only reasonable explanation. But we need not go to the Highlands to find instances of old table-lands, which have been trenched by sub-aërial denudation; and perhaps some examples on a smaller scale may be more easily appreciated by beginners. An admirable case is found in the south-west of England. Anyone who walks up from Bristol on to Clifton and Durdham Downs cannot fail to be struck by the remarkable evenness of their surface; after a long pull up a succession of steep inclines you find yourself all at once on a plateau as flat as an alluvial meadow. By dropping down into the gorge of the Avon, an equally good instance of a river-trench cut through the plateau is supplied, and at the same time proof is furnished that the present surface, so suggestive of level-bedded strata underneath, really cuts sheer across beds tilted at high angles, broken by faults, and bent into complicated folds. The flat top runs on along the line of limestone hills that connect Bristol with Clevedon; it is specially noticeable around Clevedon; and after we have crossed a broad depression occupied by softer Coal Measures and Secondary rocks, we find it reappearing in the flat-topped Mendips. Nor is it confined to the exposed portions of the limestone area. Where that rock is covered by Secondary strata, shafts sunk in making tunnels and other excavations show that the newer beds rest on an almost level surface of Palæozoic rocks. Over the whole country there can be traced, whether exposed or hidden, the clearest remnants of an old pre-Triassic table-land.

We also find in the Avon gorge that the limestone has undergone disturbances smaller in amount, but identical in kind, with the gigantic displacements of Sutherland and Ross-shire, of which a short account is given in the present volume, and which are most graphically depicted in a longitudinal section by Mr. Peach. Anyone who wishes to understand what "thrust-planes" are, will find here very good miniature examples. An announcement of the greatest interest is made by the author while touching on this part of his subject. He states that the general assemblage of the organisms in the Durness Limestone recalls none of the Lower Silurian formation of Wales, but rather some of the still older groups of the Lower Palæozoic series of Canada. This reminds us that Lapworth finds the best palæontological parallel to his Moffat series in North America. To speculate on the geographical distribution of animals at so distant a time is risky work, but we may be tempted to conjecture that one great life province included both Scotland and North America, while Wales and Central Europe formed parts of another. Maybe the buried Archæan ridge, the tops of which stick up through the Secondary rocks of the centre of England, formed a portion of the barrier between the two.

But to return to the book before us. Having made good his contention that there was a time when the whole of what is now the Highlands was a broad undulating table-land, and that all its manifold diversity of feature has been carved out by sub-aërial denudation, the author takes us to the hills, the valleys, and the lakes, and enforces his conclusion with a wealth of illustration and a series of word-pictures of the most vivid character, which can be appreciated only by a study of the work itself. The Southern Uplands he treats in a similar manner, and incidentally he puts in a strong plea for a district which possesses much beauty of an unobtrusive kind, but which is apt to be condemned as bare and monotonous.

The Midland Valley is next brought before our notice. How delightful is even a railway ride on a bright sunny day over this charming country! It is in the main a land of broad rolling hills and wide valleys, well wooded and well watered, pleasant to the eye from its brightness and its richness. But, if this were all, there would be, it must be confessed, somewhat of a sameness about it that might be accounted tiresome. But it is redeemed from any risk of monotony by numerous ranges or groups of hills, of moderate elevation, but high enough to tower well above the general level, of ruggeder and wilder forms than the flowing contours of the body of the country, and many of them peaky and mountainous in outline. These are formed of the products of the old volcanoes which were once sprinkled so thickly over the district: their rocks, being harder than the sedimentary beds among which they occur, have been able better to hold out against denudation and have therefore not been worn down to so low a level. A very similar little tract is found in West Shropshire, where the Wrekin, Caer Caradoc, the Cardington Hills, and some other hill-groups—none very high, but all mountains in miniature—introduce a delicious diversity in the rich pastoral country of the Severn valley.

So far we have looked at the book solely from a scientific point of view, and, if no more were said, a suspicion might arise in the minds of the readers of this notice that perhaps the work was a trifle dry. But a very slight acquaintance with the book itself will dispel any misgiving on this head. No one has done more than the author to elucidate the geology of Scotland, but he knows and loves his fatherland too well to look upon it merely as a field for geological research. Legend and history, old ballad and modern poetry, have all been pressed into his service, and he interweaves into his narrative allusion and quotation in a way that enlivens even the most technical parts of the volume. The chapter on "The Influence of the Physical Features of Scotland upon the People" shows well what a vast amount of human interest attaches even to so special a science as geology.

If the intending tourist in Scotland will before he starts read enough of this book to enable him to comprehend its general drift and line of argument, and if he will then take the book with him and study on the ground such of its illustrations and examples as lie on his road, he will ever after thank the author for having supplied him with a new pleasure. One great charm of travel in Scotland is that it is ever leading us through scenes rich in historic associations. The enjoyment derived from such a source may be vastly enhanced by the aid of this book; for he who has mastered its contents will feel an interest not in the events of the human epoch only, but will see in every peak, hill-side, valley, and lake that he passes the monuments of a history which carries him back through that long vista of the ages which geology has opened out to us.

A. H. GREEN.

OUR BOOK SHELF.

Longmans' Shilling Geography. (Longmans, 1887.)

THIS book is dated 1887. It would have disgraced 1862. On the first three pages a volcano is defined as "a mountain from which smoke, flames, ashes, and lava are thrown," the words "continent" and "hemisphere" are treated as synonymous, and the unqualified statement is made that Yorkshire is the basin of the River Ouse. On p. 94 the tributary States of India are styled independent, and on p. 137 the warm water which drifts towards the Pole is placed "low" (deep?). The mass of the book is of the old vicious type—composed of lists of names and disconnected remarks; and what remains is a disorderly compilation of statements intended to be scientific, but from which the essential point is often omitted. The maps are numerous, but not of a high order. On some of them dials are inserted, showing relative time; but why this should be done for European countries where the difference of time from Greenwich time is counted in minutes, and yet not for the United States, India, or Australia, is not quite obvious. The book is careless and ignorant, and its plan radically bad. We hope that teachers will not be deceived by the title, and imagine that they have here a shortened form of "Longmans' School Geography" by Chisholm. H. J. M.

--es Plantes des Champs et des Bois. Par Gaston Bonnier. (Paris, 1887.)

IT is an accusation which has been justly brought against the botanists of this country that they habitually write in an austere style, which will repel rather than attract the general public; it would be difficult to point to any among the younger men representative of the science who have taken the trouble to please or interest the laity. It is true Miss Plues and Mrs. Gatty have made the attempt, but theirs are books which date many years back. The French are much less open to this charge, having a peculiar and in some cases even a dangerous aptitude for dressing science in popular colours. The "Vegetable World" of Figuier, well known to us from its English translation, has done good service in the past, and now Prof. Bonnier has produced a popular book, made attractive by numerous illustrations, and written in a style which will be readily followed by those who as yet know nothing of the science of botany.

The plan of the book is well adapted to the object before its author: an introduction of some 50 pages suffices for the definition of terms, and of the fundamental points in organography, together with a brief sketch of classification. Armed with this limited but sufficient knowledge, the reader may enter upon his studies in the field. The author divides these into four parts, according to the season of the year, and starting with spring. The description of the plants likely to be found is so arranged as to form a series of progressive lessons, and when autumn is reached the attentive reader will have acquired a fair knowledge of the external form and relationships of many common plants, both Phanerogamic and Cryptogamic. The book is not, and does not pretend to be, any contribution to the sum of knowledge; nevertheless, by means of the easy text and suitable illustrations, the effort of its author to make the rudiments of his science acceptable to the eye, as well as to the understanding, of the general public, should meet with the success it well deserves. F. O. B.

The Hand-book of Jamaica for 1887-88. By A. C. Sinclair and Laurence R. Fyfe. (London: Edward Stanford, 1887.)

THE compilers of this "Hand-book" have brought together a great mass of trustworthy and useful information about Jamaica. A good description of the island is followed by an historical sketch, after which comes a chronological history, brought down to June 30 last. Then we find all the necessary facts about the political constitution, the revenue, and expenditure, the various departments of the public service, and many other subjects. The articles which appeared in previous editions have been revised, and a good deal of new matter has been introduced. Most persons who may have occasion to refer to the volume will be glad to find in it an account (reprinted from the *Jamaica Gazette*) of the cyclone of last year, by Mr. Maxwell Hall, the list of medicinal and economic plants of the colony prepared by Mr. Fawcett, and the list of sugar-canes prepared by Messrs. Fawcett and Morris.

LETTERS TO THE EDITOR.

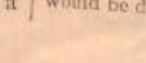
[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

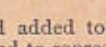
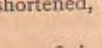
[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

The Natural History of the Roman Numerals.

THAT the Roman numerals, in their primitive forms, articulately symbolized a quinary notation based on the hand sign  , is the view which the following observations are intended to explain.

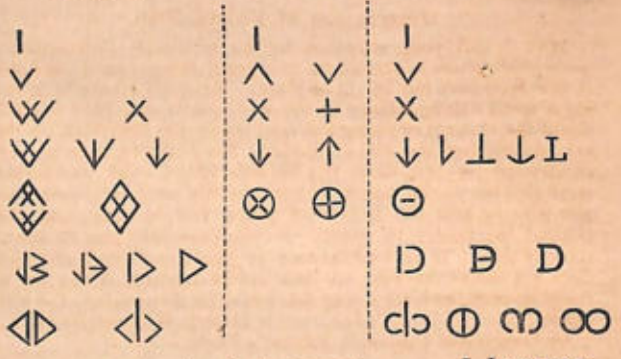
A system of enumeration which arose naturally in the progress of the race would be moulded by the kind of expedient adopted in learning to count, by the methods employed in communicating numbers, and by the difficulty of retaining in the memory more than a very few similar signs or sounds repeated in succession. It is not generally doubted that primitive man learned to count, like the child, on his fingers, first on the one hand, and then on the other. The first stage of numeration was thus reached at five, the second at five and five. Numbers were thought of as represented by fingers and hands. From mental helps these bodily members naturally passed into communicative signs: the uplifted finger or fingers, the outspread hand or hands. This would be followed by the use of numerical language. At first only three numbers would have names, there would be a name for one, for five, and for double five. In communicating numbers four times would seem to have been the limit within which the same sign or sound could be repeated in succession without risk of confusion. If this influence alone had been at work, a new name and sign would have been reached at five, at five times five, and at five times five times five, and a perfect and consistent quinary scale would have been the result. But to the primitive man the two hands together would as naturally represent in thought and in communication two fives as the single hand would five ones; so that after double five the next stage would be five double fives, or five times the outspread hands. For this a fresh sound and a new sign would have to be found. It would be vain to conjecture the name, but surely not unreasonable to suppose that this sign would be made by some manner of placing the hand between the feet.

Having reached this point, and in so doing exhausted the simple bodily signs which would naturally be made use of, recourse would be had to marks drawn with the finger upon soft earth or sand. The first written symbols would almost certainly be numerals: nor is it unlikely that from their use arose the idea of an alphabet, and from their shapes the first forms which letters assumed. And these shapes could be nothing else than imitations of the gesture signs. The finger sign would give the stroke , the five fingers in the unity of the hand would be represented by five strokes converging together,  ; this, again, would be doubled and conjoined,  , to resemble the

combined hands, and tripled,  , to imitate the sign for five times double five—the hand added to the outspread feet. The instinct in the primitive mind to represent concretely in the symbol all that was contained in the idea would sooner or later have to give way to the desire after facility and clearness. All that was found unnecessary to the distinctness of the figure would be done away with, strokes would be dropped or shortened, and complex forms would be made simple. In process of time, therefore,  became  ; first  and then  ; and  , after coalescing in  , was simplified into  . Before this last change took place, however, it would appear that a double-fifty form was employed,  , unifying in  , and rounding into  the Etruscan hundred. It was probably because this double-fifty form was used that no need was felt for a name or sign to represent five times fifty, and that a new form only arose when five times double fifty was reached. By this time the simplified fifty,  , had become contracted into  , and it was plain that a simpler figure could be got by representing double-five times fifty  than by any way of symbolizing five times double fifty. This figure, , would naturally change into  , then be altered into , and become joined into one in  . By the simple process of doubling this symbol, the next higher form,  or , was attained. This figure does not stand for a thousand, but for five hundred and five hundred. If  had been, as is generally supposed, a unity meaning 1000, the doubled length of the stroke would have been unintelligible, and the multiple forms would have been  ,  , and so on, and not ,  , and so on.

With the double five hundred,  , we have probably reached the limit of what may be called the primitive numerical notation—the notation of a people who spoke of, counted by, and had symbols for ones, fives, double fives, fifties, double fifties, double-five times fifty, and twice double-five times fifty. And it was in the main with this cumbrous system of enumeration that the Romans were content during their whole existence as a people, hardly making any advance beyond substituting a hundred form, C, for the double fifty, and a thousand form, M, for the double five hundred.

TABULAR VIEW.



Glasgow University, September 23. J. LYMBURN.

The "Sky-Coloured Clouds."

THE last distinct display of these that I have seen this season was during the night of July 29–30, and there was a very slight display, if really one, on August 9. They seem only to be visible for a short period before and after the summer solstice. I have looked up all the recorded dates to be found in NATURE and elsewhere when these clouds have been seen, either by myself or others, and the following are the first and last noted dates each year: 1885, June 8, July 7; 1886, May 28, August 12; 1887, June 18, July 30—but a suspected display of the phenomenon was observed four days earlier (June 14), and an equally doubtful one (mentioned before) as late as August 9.

It does not appear to me possible to attribute their luminosity to anything but direct solar illumination. Mr. Rowan in his letter (NATURE, vol. xxxvi. p. 245) thinks otherwise, though in an earlier letter (vol. xxxiv. p. 192) he seems to express a similar opinion to mine. With regard to Prof. C. Piazzi Smyth's letter on the subject (vol. xxxiv. p. 311) I have had a conversation with him since that letter was written, and he said he did not intend to convey the idea (which it had done to Mr. Rowan as well as to myself) that the auroral line appertained to the spectrum of the clouds; and my observations with the spectroscope quite agree with his belief that they are not self-luminous, for I have failed to see any bright line. Prof. Smyth says the spectrum of these clouds is purely that of twilight.

The earliest observations on these clouds would appear to be Mr. Rowan's. Some authoritative assertion of when they were first seen would be very interesting in considering their cause. I do not know that any one else perceived them before 1885, but Mr. Rowan, writing in 1886, says they had occupied his attention during the previous *two or three* years—if two years, the date, 1884, would be after the Krakatáo eruption, and would add to the plausibility of their suggested connexion therewith; but if three years, it would disprove such connexion.

Sunderland, September 30. T. W. BACKHOUSE.

A Light Fog.

AT Blowing Rock, Watauga Co., N.C., a part of the main chain of the Blue Ridge Mountains, at this point possessing an elevation of about 4000 feet above the sea, on the night of the 6th inst., while the writer was crossing a causeway through a mill-pond a light fog, obscuring objects at a hundred yards, covered the water. The moon, a little past the full, produced upon the bank of fog a very distinct bow. The bow was luminous white, without any trace of colour, about 2° in breadth. The ends apparently rested on the water, the entire arc being reflected in the water. The segment of fog within the bow was faintly lit up, the lighting up being distinctly seen by contrast with the fog outside the bow. At the same time on turning and looking at the moon it was seen surrounded by a corona about 2° in diameter (four times the moon's diameter), the colours—fairly bright—being in order, going from the moon's limb,

yellow, orange, red. A little later, the same night, on the brow of the ridge, in the faint mist which rose in masses, the bow was again seen vividly, against a background of trees; the bow being within 40 paces of the observer. W. G. BROWN.

Washington and Lee University, Lexington, Va.
September 23.

Destruction of Young Fish.

MAY I call your attention to the wholesale destruction of young fish which is carried on to a great extent round our coast? A few facts may not be out of place. Recently I have been visiting a small fishing village on the east coast, and have carefully noted the amount of young fish rejected by the fishermen on their return from trawling and shrimping. For example, from 4¼ pecks of shrimps no less than 793 flat-fish (dabs, soles, and turbots) were thrown on the beach useless; to this must be added about 200 whiting and an amount of young cod, herring, and skate beyond my power to count. Surely something can be done to remedy this! It is well known to the fishermen that the net does not injure the fish; so that before landing, if the net was roughly examined, all young fish could be thrown into the water again. DAVID WILSON-BARKER.

66 Gloucester Crescent, Regent's Park, N.W.

ON HAMILTON'S NUMBERS.

FOLLOWING in the footsteps of Hamilton in his Report to the British Association, contained in the Proceedings for the year 1836, we may arrive at a solution, in a certain sense the simplest, of a problem in algebra the origin of which reaches back to Tschirnhausen, born 1651, deceased 1708. Every tyro knows how a quadratic equation, and all equations of a superior degree thereto, may be transformed into another in which the second term is wanting. Tschirnhausen showed that a cubic equation, and all equations superior in degree to the cubic, might be deprived of their second and third terms by solving linear and quadratic equations. Then over a century later Bring, of the University of Lund, in 1786 showed that every equation of the 5th, or any higher degree, might be deprived of its first three terms by means of solving certain cubic, quadratic, and linear equations.[1] What, then, it may be asked, is the law of the progression of which the three first terms are 2, 3, 5? What is the lowest degree an equation can have in order that it may admit of being deprived of four consecutive terms by aid of equations of the 1st, 2nd, 3rd, and 4th degrees, or more generally of i consecutive terms by aid of equations of the 1st, 2nd, 3rd, . . . and ith degrees, *i.e.* by equations none of a higher degree than the ith?[2]

In the 100th volume of *Crelle's Journal* (1886) I have shown that the progression continued as far as the case of eight terms being abolished is as follows—

2, 3, 5, 10, 44, 905, 409181, 83762797734.

These, with the exception of the three first, are not exactly what I call Hamilton's numbers, but serve to lead up to them.

Hamilton's numbers are—

2, 3, 5, 11, 47, 923, 409619, 83763206255, . . .

I will endeavour to explain wherein the difference consists between the two series.

Whilst it is true that four terms may be abolished in an equation of the 10th degree without solving equations beyond the 4th degree, there is this difference in favour of equations of the 11th or any higher degree, viz. that *fewer* biquadratics will be required for them than in the case of an equation limited to the 10th degree. And so in general whether we take, as our inferior limit to the degree of the equation to be transformed, the ith number in the upper series or the ith number in the lower one—whilst in neither case it will be necessary to solve any equations of a degree exceeding i—the total system in the latter case will be of a simpler character than in the former.

The numbers which I have named in honour of Hamilton may be obtained by a process exhibited in the table below—

1	0	0	0	0	0	0	0 ...
	1	1	1	1	1	1	1 ...
		2	3	4	5	6	7 ...
		1	5	9	14	20	27 ...
			6	15	29	49	76 ...
			5	21	50	99	175 ...
			4	26	76	175	350 ...
			3	30	106	281	631 ...
			2	33	139	420	1051 ...
			1	35	174	594	1645 ...
				36	210	804	2449 ...
				&c.	&c.	&c.	&c. ...

We may now isolate the greatest figure which occurs in each column, and in this may we obtain the numbers 1, 1, 2, 6, 36 . . . which I call hypotenusal numbers; then adding these numbers together and increasing each sum so obtained by unity we arrive at the so-called Hamilton's numbers, viz. 2, 3, 5, 11, 47. . . . Now the question arises as to how they may be calculated; for obviously the crude method above given will be impossible to carry out in practice beyond the first few numbers in the scale. The method of generating functions—of which the idea occurred first to my coadjutor Mr. James Hammond, which certainly ought not to, and probably in the long run could not, have escaped me—leads to a wonderfully beautiful law, by means of which these numbers may be derived successively each from those that go before, just as is the case with Bernouilli's numbers.

The simplest and best mode of proceeding is as

[1] In a letter to Leibnitz (1677), which I have not seen, and the *Acta Eruditorum* for 1683.

[2] How the elevation of the degree of the equation to be transformed makes it possible to abolish a greater number (μ) of terms by an auxiliary system of equations of degrees none exceeding μ will be understood if we consider the cases of a quintic and quartic.

Supposing $(x, 1)^5$ to be a given quintic, on writing

$$\alpha x^4 + \beta x^3 + \gamma x^2 + \delta x + \epsilon = 0$$

we obtain, by elimination of x, $(\alpha, \beta, \gamma, \delta, \epsilon)^5 = 0$, and any solution of this equation will enable us, by a well-known process, to find x by a linear equation.

If we select any letter, α, of the five we may equate it to a linear function of y, β, γ, δ, ϵ, so as to obtain

$$y^5 + (\beta, \gamma, \delta, \epsilon)^2 y^3 + (\beta, \gamma, \delta, \epsilon)^3 y^2 + (\beta, \gamma, \delta, \epsilon)^4 y + (\beta, \gamma, \delta, \epsilon)^5 = 0.$$

If in this equation we can find any system of ratios $\beta : \gamma : \delta : \epsilon$ such that $(\beta, \gamma, \delta, \epsilon)^2 = 0$, and $(\beta, \gamma, \delta, \epsilon)^3 = 0$, we can find y by solving a trinomial quintic, and therefore a system of admissible ratios $\alpha : \beta : \gamma : \delta : \epsilon$ becomes known.

All that is requisite therefore is to be able to obtain any point whatever of intersection of two given quadratic and cubic surfaces represented by $(\beta, \gamma, \delta, \epsilon)^2$ and $(\beta, \gamma, \delta, \epsilon)^3$ which obviously may be done by first finding a point (any point) in the quadratic surface (which only necessitates solving some quadratic equation or other); second, at this point drawing a right line (either one of a pair) lying on the surface, which may be effected by a well-known method involving only the solution of a quadratic; and third, finding any one of the three intersections of such line with the cubic surface.

Thus, then, by solving quadratic and cubic equations a quintic may be deprived of three consecutive terms. But not so a quartic: for in the case of a quartic we could not (with any real advantage) use a subsidiary equation of a higher degree than the 3rd, we should thus have only three letters, β, γ, δ, instead of four in the equation in y, and to make $(\beta, \gamma, \delta)^2 = 0$, $(\beta, \gamma, \delta)^3 = 0$, simultaneously, is the problem of finding an intersection of a quadratic and a cubic curve, which necessitates the solution of an equation of the 6th degree.

In the case of the quintic it may be well to notice that the ratios $\alpha : \beta : \gamma : \delta : \epsilon$ will not be all real, and consequently the trinomial quintic into which the original one has been transformed will not have its coefficients real, unless the quadric surface is a hyperboloid of one sheet (since it is only that species of quadric surfaces which contains real straight lines); and I have shown in my paper in Crelle that this is the case then, and then only, when the original quintic has four imaginary roots.

[3] If the number of equations of degrees i, $i-1$, $i-2$, ... to be solved in the one case reckoned in DESCENDING order are a, b, c, ... l, ... and in the other a', b', c', ... l' . . . . respectively, if l, l' are the two first corresponding numbers which are not identical l' will be less than l.

follows:—Look at the successive lines of figures in the table and write

$$\begin{aligned}
1 + 0x + 0x^2 + 0x^3 + 0x^4 + 0x^5 + \ldots\ldots &= F_0\\
x + x^2 + x^3 + x^4 + x^5 + \ldots\ldots &= {}^1F_0 = F_1\\
2x^2 + 3x^3 + 4x^4 + 5x^5 + \ldots\ldots &= {}^1F_1 = F_2\\
x^2 + 5x^3 + 9x^4 + 14x^5 + \ldots\ldots &= {}^1F_2\\
6x^3 + 15x^4 + 29x^5 + \ldots\ldots &= {}^2F_2 = F_3\\
5x^3 + 21x^4 + 50x^5 + \ldots\ldots &= {}^1F_3\\
4x^3 + 26x^4 + 76x^5 + \ldots\ldots &= {}^2F_3\\
3x^3 + 30x^4 + 106x^5 + \ldots\ldots &= {}^3F_3\\
2x^3 + 33x^4 + 139x^5 + \ldots\ldots &= {}^4F_3\\
x^3 + 35x^4 + 174x^5 + \ldots\ldots &= {}^5F_3\\
36x^4 + 210x^5 + \ldots\ldots &= {}^6F_3 = F_4
\end{aligned}$$

and so on.

Then evidently

$${}^1F_i = (1 - x)^{-1} F_i - x^i$$

and in general

$${}^{j+1}F_i = (1 - x)^{-1^j} F_i - x^i$$

and consequently calling the ith number in the hypotenusal series 1, 1, 2, 6, 36, ... a_i, and bearing in mind that ${}^{a_i}F_i = F_{i+1}$, we shall have

$$F_{i+1} - (1 - x)^{-a_i} F_i$$

$$= -x^i\{1 + (1 - x)^{-1} + (1 - x)^{-2} + \ldots + (1 - x)^{-a_i + i}\}$$

$$= x^{i-1}\{(1 - x) - (1 - x)^{-a_i + 1}\}$$

which obviously regarded as an equation in differences of the 1st order in F_i, gives the means of expressing F_{i+1} as a function of x and $a_i, a_{i-1} \ldots a_0$, and consequently must enable us to express all the coefficients in F_{i+1}, of which the first is the hypotenusal number a_{i+1}, in terms of all the hypotenusal numbers of lower order. But what is surprising and unexpected is that, as we shall in a moment see, the relation obtained is expressed by an immediate equation between the sums of the hypotenusals 1, 1, 2, 6, 36, ..., each increased by 2, *i.e.* by an equation between the *ipsissimi* numbers of Hamilton augmented by unity.

In fact, multiplying each side of the equation by $(1-x)^{S_i + 1}$, where

$$S_{i+1} = a_0 + a_1 + a_2 + \ldots + a_i,$$

(so that $S_1 = 1$, $S_2 = 2$, $S_3 = 4$, $S_4 = 10$, $S_5 = 46$, ...), it becomes

$$(1 - x)^{S_i + 1} F_{i+1} - (1 - x)^{S_i} F_i$$

$$= x^{i-1}(1 - x)\{(1 - x)^{S_i + 1} - (1 - x)^{S_i}\}$$

which equation, it may be noticed, proved for all values of i down to 1 may be extended also to $i = 0$, provided we make $S_0 = 0$.

Accordingly, giving i all values down to 0 inclusive, we shall easily obtain by addition

$$(1 - x)^{S_i} F_i = 1 + x^{i-2}(1 - x)^{S_i + 1} - x^{-1}(1 - x)$$

$$+ x^{i-3}(1 - x)^{S_{i-1} + 2} + x^{i-4}(1 - x)^{S_{i-2} + 2} + \ldots$$

$$+ x^{-1}(1 - x)^{S_1 + 2},$$

or which is the same thing

$$(1 - x)^{S_i} F_i - 2 + x^{-1} - x^{i-1}(1 - x)^{S_i + 1}$$

$$= x^{i-2}(1 - x)^{S_i + 2} + x^{i-3}(1 - x)^{S_{i-1} + 2} + \ldots$$

$$+ x^{-1}(1 - x)^{S_1 + 2}.$$

Hence, equating the coefficients of x^i and using $\beta^k q$ in general to signify $\frac{q(q-1)\ldots(q-k+1)}{1.2\ldots k}$, if we call $1 + S_i = H_i$ we obtain

$$a_i + S_i + 1 = H_{i+1}$$

$$= \beta^2(H_i + 1) - \beta^3(H_{i-1} + 1) + \beta^4(H_{i-2} + 1) - \ldots.$$

$$+ (-)^{i+1}\beta^{i+1}(H_1 + 1)$$

And on calling $1 + H_i = E_i$ this equation becomes

$$1 - \beta E_{i+1} + \beta^2 E_i - \beta^3 E_{i-1} + \beta^4 E_{i-2} - \ldots.$$

$$+ (-)^{i+1}\beta^{i+1} E_1 = 0.$$

This relation between the sharpened Hamiltonian numbers (*i.e.* these numbers increased by a unit) is in a slightly different form from the result obtained by Hammond. By aid of this formula the values of the successive numbers can be calculated with wonderful facility. The series of them commencing with 1, which although not properly speaking a Hamiltonian number, belongs to the class $S_i + 1$, have been found to be

1, 2, 3, 5, 11, 47, 923, 409619, 83763206255,
3508125906290858798171,
6153473687096578758448522809275077520433167, ...

Thus *ex. gr.* having found $H_6 = 923$ and all the Hamiltonian numbers inferior to it, we have

$$H_7 = \frac{924.923}{1.2} - \frac{48.47.46}{1.2.3} + \frac{12.11.10.9}{1.2.3.4} - \frac{6.5.4.3.2}{1.2.3.4.5}$$

$$= 426426 - 17296 + 495 - 6$$

$$= 409619.$$

I have alluded to Bernouilli's numbers as a parallel case to that of Hamilton's in so far as they too are subject to a scale of relation by which each can be expressed in terms of those of a lower order than itself.

If we use

$$B_1, B_2, B_3, \ldots.$$

to signify as usual the Bernouillian numbers

$$\frac{1}{6}, \frac{1}{30}, \frac{1}{42}, \ldots.$$

and write

$$G_0 = -1, G_1 = -1, G_2 = (-4)B_1, G_3 = 0,$$
$$G_4 = (-4)^2 B_2, G_5 = 0, G_6 = (-4)^3 B_3,$$

and so on, the well-known scale of relation for Bernouilli's numbers may (provided only that n be odd) be written under the form

$$\Sigma_{k=0}^{k=n}(-)^k \beta^k n . G_{n-k} = 0.$$

If in this formula we suppress the n which intervenes between the operative symbol β^k and G_{n-k}, so that the former is brought into juxtaposition with and acts on the latter, it becomes identical with that which we have found for the sharpened Hamiltonian numbers.

Those who wish to pursue the subject further may consult my memoir "On the so-called Tschirnhausen Transformation" (*Crelle*, vol. c. pp. 465–86), another "On Hamilton's Numbers," by Mr. Hammond and myself conjointly, just published in the Philosophical Transactions, and an addition thereto about to be presented to the Royal Society, in which a large generalization of the theory discussed by Hamilton in his Report to the British Association for 1836, but not brought by him to perfection, is resolved with a completeness which leaves nothing to be desired.

J. J. SYLVESTER.

New College, Oxford, October 1.

MODERN VIEWS OF ELECTRICITY.[1]

PART I.

II.

FIRST you have an inextensible endless cord circulating over pulleys; this is to represent electricity flowing in a closed circuit. Electromotive forces are forces capable of moving the cord, and you may consider them applied either by a winch, or by a weight on the hook W. A battery cell corresponds to a small weight; an electric machine to a slow but powerful winch. Clamping the cord with the screw S corresponds to making the resistance of the circuit infinite. Instead of the cord, clamp, and driving

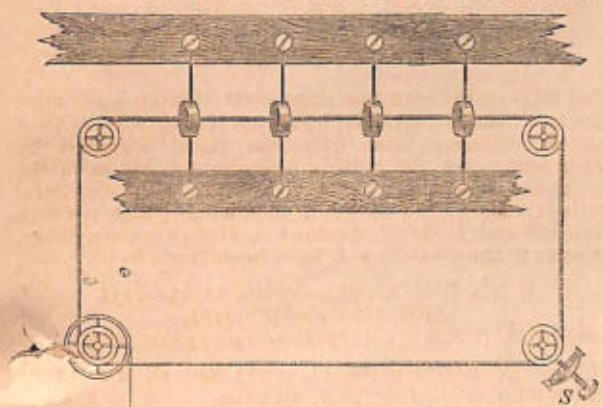


FIG. 5.—Mechanical analogy of a *metallic* circuit.

pulley, one might consider an endless pipe full of liquid with a stop-cock and a pump on it, but for many purposes the cord is sufficient and more simple. In Fig. 5, the only resistance to the motion is friction, and there is no tendency to spring back. Fixed beads are shown on the cord to typify atoms of matter, and they may be more or less rough to represent different specific resistances. If the cord be moved, heat is the only result.

Now pass to Fig. 6. Here the cord is the same as before

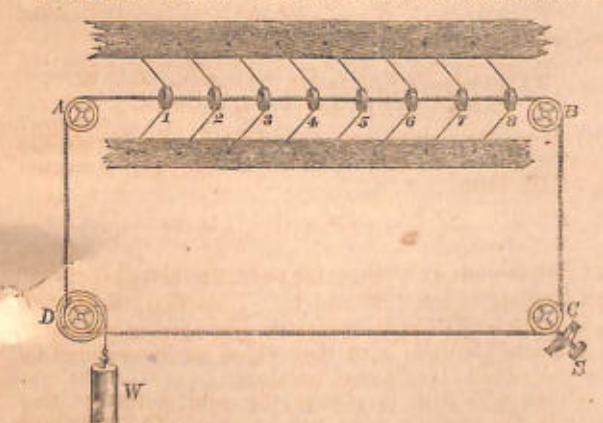


FIG. 6.—Mechanical analogy of a circuit partly *dielectric*: for instance, of a charged condenser. A is its positive coat, B its negative.

but the beads are firmly attached to it, so that if it moves they must move with it. They represent, therefore, the particles of an insulating substance. Nevertheless, their supports are not rigid—they do not prevent the cord moving at all; they allow what is called electric "*displacement*," not conduction; they can be displaced a little from their natural position, but they spring back again when the disturbing E.M.F. is removed. The beads in this figure are supposed to be supported by elastic threads: if the cord were replaced by a closed pipe full of water they would be replaced by elastic partitions.

Apply a given E.M.F. to this cord and a definite displacement is produced. One side gets more cord than usual—it is positively charged; the other gets less—it is negatively charged. If the applied E.M.F. exceeds a certain limit the strain is too great. The elastics break, and you have disruptive discharge with a spark. But even when the strain is only moderate some of the supports may yield viscously, or be imperfectly elastic and permit a gradual extra displacement of the cord, known to telegraphists as "soaking in."

When discharge is now allowed, it will not at once be complete; a large portion of the displacement will be at once recovered, but the rest will gradually "soak out" and cause residual discharges.

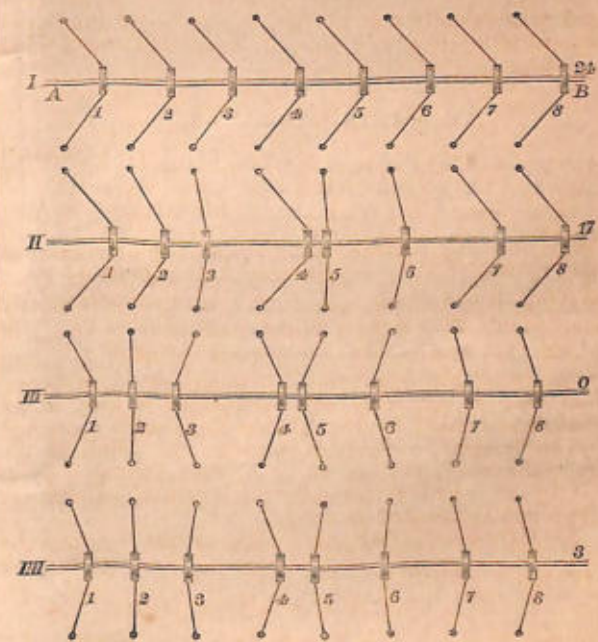


FIG. 7.—Stages in the discharge of a stratified condenser; showing one way in which the phenomena of "residual charge," "internal charge," and "soaking out" are produced.

If the dielectric is at all stratified in structure, so that some of the beads allow cord to slip through them—or yield more than others—then this residual charge effect will become very prominent.

Fig. 7 illustrates the various stages of a stratified dielectric, with layers of imperfect insulating power. I. represents a recent charge, of E.M.F. 24. II. represents the same after lapse of time, reduced to 17 by partial internal leakage; and shows internal charge. The circuit itself is supposed to have been perfectly insulating all the time. III. shows the first discharge; and IIII. shows the state attained after again waiting, viz. a residual charge with an E.M.F. 3 in the old direction.

Return, however, to the simple discharge, and see how it occurs. Will it take place as a simple sliding back of the beads to their old position? Yes, if the resistance of the circuit is great, but not otherwise. If the cord is fairly free the beads will fly past their mean position, overshooting their mark, then rebound, and so, after many quick oscillations, will finally settle down in their

[1] Expansion of a lecture delivered by Dr. Oliver Lodge, partly at the London Institution on January 1, 1885, and partly at the Midland Institute, Birmingham, November 15, 1886, but not hitherto published. Continued from p. 536.

natural position. Thus is represented the fact that the discharge of a Leyden jar is in general oscillatory; the apparently single and momentary spark, when analyzed in a very rapidly rotating mirror, turning out to really consist of a series of alternating flashes rapidly succeeding one another, and all over in the hundred-thousandth of a second or thereabouts. These oscillatory currents were predicted and calculated beforehand by Sir William Thomson; they were first observed experimentally by Feddersen. The oscillations continue until the energy stored up in the strained medium has all rubbed itself down into heat.

Fig. 8 shows part of an actual model of the kind.

To make the model represent *charge by induction* all that has to be done is to immerse a conductor into the polarized dielectric—in other words, to make one or more

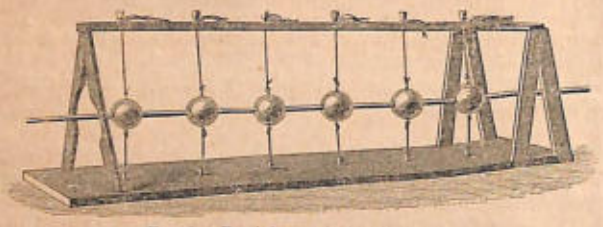


FIG. 8.—Partial model of a dielectric.

of the beads of the fixed and slippery conducting kind, the other beads on the cord being of the elastic and adhesive or insulating kind. Then when the displacement occurs it is plain that a deficiency of cord will exist on one side of the metallic layer and a surplus on the other, as shown in Fig. 9. This state of things corresponds exactly to the equal opposite induced charges on a conductor under induction, as in Fig. 3. If the strain on one side be relieved by letting the beads on that side slip back on the cord: that corresponds to touching the conductor to earth, as in Fig. 4. The other side has now to withstand the whole E.M.F., consequently the strain there and the charge there will have increased. Remove now the applied E.M.F., and the negative charge appears on both sides of the metal partition, either equally, or more markedly on that side which has fewest beads, *i.e.* which is nearest to other conductors.

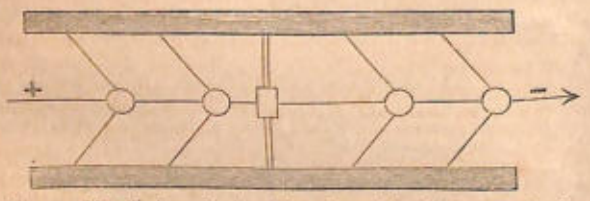


FIG. 9.—Metallic layer in the midst of a polarized dielectric, showing opposite charges "induced" on its surfaces. (Compare Fig. 3.)

Hydraulic Model of a Leyden Jar.—So much for the cord model, but I will now describe and explain an hydraulic model which illustrates the same sort of facts: some of them more plainly and directly than the cord model. Moreover, since all charging is essentially analogous to that of a Leyden jar, let us take a Leyden jar and make its hydrostatic analogue at once.

The form of jar most convenient to think of is one supported horizontally on an insulating stand, with pith ball electroscopes supplied to both inner and outer coatings.

To construct its hydraulic model, procure a thin india-rubber bag, such as are distended with gas at toy-shops; tie it over the mouth of a tube with a stop-cock, A, and insert the tube by means of a cork into a three-necked globular glass vessel or "receiver," as shown in the diagram, Fig. 10.

One of the other openings is to have another stop-cock tube, B; and the third opening is to be plugged with a cork as soon as the whole vessel, both inside and outside the bag, is completely full of water without air-bubbles.

This is the insulated Leyden jar: the bag represents the dielectric, and its inner and outer coatings are the spaces full of water.

Open gauge-tubes, *a* and *b*, must now be inserted in tubes A and B, to correspond to the electroscopes supplied to the jar; and a third bent tube, C, connecting the inner and outer coatings, will correspond to a discharger. Ordinarily, however, of course C will be shut.

A water-pump screwed on to A will represent an electric machine connected to inner coating; and the outer coating, B, should open into a tank, to represent the earth. The pump will naturally draw its supply of water from the same tank.

The bag being undistended, and the whole filled with water free from air, the level of the water in the two gauge-tubes will correspond with that in the tank; and this means that everything is at zero potential, *i.e.* the potential of the earth.

Now, C being shut, shut also B, open A, and work the pump. Instantly the level in the two gauges rises greatly and equally: you are trying to charge an insulated jar. Turn an electric machine connected to a real jar, and its two pith balls will similarly and equally rise.

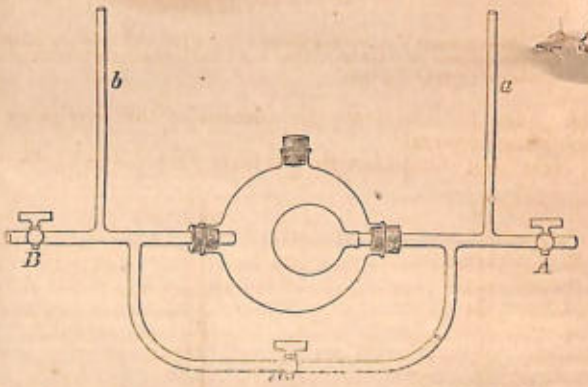


FIG. 10.—Skeleton diagram of hydraulic model of a Leyden jar.

Now open B for an instant, the pressure is relieved, and both gauges at once fall, apparently both to zero. Repeat the whole operation several times however, and it will be found that whereas *b* always falls to zero, *a* falls short of zero each time by a larger amount, and the bag is gradually becoming distended. This is *charge by alternate contact*. It may be repeated exactly with the real jar: a spark put in to the inner coating, and an equal spark withdrawn from the outer coating each time; and unless this outer spark is so withdrawn, the jar declines to charge: water (and electricity) being incompressible.

If B is left permanently open, the pump can be steadily worked, so as to distend the bag and raise the gauge *a* to its full height, *b* remaining at zero all the time, save for oscillatory disturbances.

Having got the jar charged, shut A, and remove the pump, connecting the end of A with the tank directly.

Now of course by the use of the discharger C the fluid can be transferred from inner to outer coat, the strain relieved, and the gauges equalized. But if this operation be performed while the jar is insulated, *i.e.* while A and B are both shut, the common level of the gauges after discharge is not zero, but a half-way level; and the effect of this is very noticeable if you touch an insulated Leyden jar after it has been discharged.

Instead of using the discharger C, however, we can proceed to discharge by alternate contact, and the operation is very instructive.

Start with the gauge *b* at zero, and the gauge *a* at high pressure. Open stop cock A; some water is squeezed out of inner coating, and the *a* gauge falls to zero, but the suck of the contracting bag on the outer coat pulls down

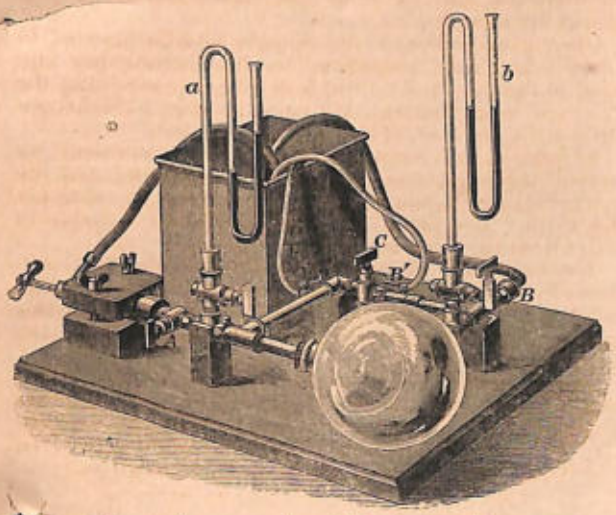


FIG. 11.—First actually constructed model Leyden jar, with mercury gauges for electrometers; the whole rigged up with things purchasable at a plumber's, except the pump.

the gauge *b* *below* zero, the descent of the two gauges being nearly equal.

Next shut A and open B; a little water flows in from

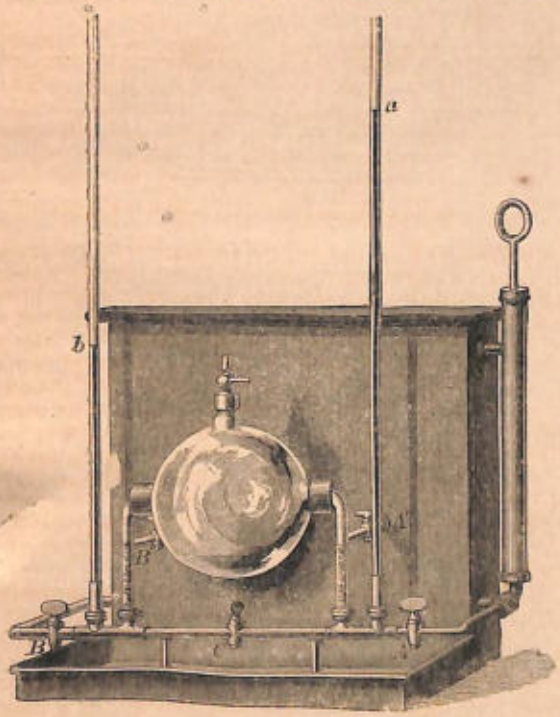


FIG. 12.—Latest form of hydraulic model of a Leyden jar with water gauges, the whole arranged vertically to be more conspicuous. The pump is a force-pump with a communication between top of barrel and tank to get rid of stray water.

the tank to still further relieve the strain of the bag, and both gauges rise; *b* to zero, *a* to something just short of its old position.

Now shut B and open A again: again the two gauges descend. Reverse the taps, and again they both rise; and so on until the bag has recovered its normal size. This is discharge by alternate contact, and exactly imitates the behaviour of an insulated charged Leyden jar whose inner and outer coats are alternately touched to earth. Its pith balls alternately rise with positive and with negative electricity, indicating potentials above and below zero.

Figs. 11 and 12 are taken from photographs of apparatus I have made to use as just described. The glass globe with the partially distended bag inside it, the pump, the tank, the gauges *a* and *b*, the stop-cocks A B C, will be easily recognized. Two extra stop-cocks, A' and B', leading direct to tank, are extra, and are to save having to disconnect pump and connect A direct, when exhibiting the effect of "discharge by alternate contact." But the tank is not sufficiently tall in Fig. 12; I have doubled its height since. The full height of the gauge-tubes is barely shown.

In any form of apparatus it is essential to fill the whole with water—pipes, globe, everything—before commencing to draw any moral from its behaviour. It is rather difficult to get rid of a large bubble of air from the top of the globe of Fig. 11, and though it is not of very much consequence in this place, the stop-cock in Fig. 12 is added to make its removal easy. The gauges in Fig. 11 may be replaced by others arranged as a lantern-slide, and connected by flexible tubing full of air.

I have explained thus fully the hydraulic illustration of Leyden jar phenomena, because these constitute the key to a great part of electrostatics. The illustration is not indeed a complete or perfect one by any means, but by combining with it a consideration of the endless cord models, and of what I have endeavoured to explain concerning conduction and insulation in general, a distinct step may be gained.

Think of electrical phenomena as produced by an all-permeating liquid embedded in a jelly; think of conductors as holes and pipes in this jelly, of an electrical machine as a pump, of charge as excess or defect, of attraction as due to strain, of discharge as bursting, of the discharge of a Leyden jar as a springing back or recoil oscillating till its energy has gone.

By thus thinking you will get a more real grasp of the subject and insight into the actual processes occurring in Nature—unknown though these may still strictly be—than if you employed the old ideas of action at a distance, or contented yourselves with no theory at all on which to link the facts. You will have made a step in the direction of the truth, but I must beg you to understand that it is only a step, that what modifications and additions will have to be made to it before it becomes a complete theory of electricity I am wholly unable to tell you. I am convinced they will be many, but I am also convinced that it is unwise to drift along among a host of complicated phenomena without guide other than that afforded by hard and rigid mathematical equations.

The mathematical theory of potential and the like has insured safe and certain progress, and enables mathematicians to dispense for the time being with theories of electricity and with mental imagery. Few, however, are the minds strong enough thus to dispense with all but the most formal and severe of mental aids: and none, I believe, to whom some mental picture of the actual processes would not be a help if it were safely available.

Such a representation I have endeavoured partially to lay before you to-night, and I hope, if I have succeeded in making myself at all intelligible, that those students of electricity who may be present will find it of some use and service.

O. J. LODGE.

(*To be continued.*)

NEW FORM OF CONSTRUCTION OF OBJECT-GLASSES INTENDED FOR STELLAR PHOTOGRAPHY.

THE interest now generally taken in stellar photography will probably make it desirable that the object-glasses of telescopes ordinarily used for visual purposes should be so constructed as to be readily adapted to photographic use. As now commonly made, the correction for chromatic aberration by means of the flint-glass lens of a refracting telescope is too great to give satisfactory photographic images. The method of adapting such a telescope to photographic purposes which was employed by Mr. Rutherford, and more recently in the case of the great telescope of the Lick Observatory, is to provide an additional convex lens of long focus which may be mounted when photographs are to be taken, and removed when direct observation is desired. The objections to this method are the expense of the additional lens and the introduction of two more surfaces.

Another way of removing the excessive correction for chromatic aberration is to separate the flint- and crown-glass lenses, and to place the flint nearer the eye-piece. But when this is done, while the correction for colour becomes satisfactory, the spherical aberration is only partially corrected, the focal length of the central part of the object-glass being greater than that of the surrounding portions. This difficulty, however, may be surmounted by giving different curves to the two surfaces of the crown-glass lens, and reversing it when the lenses are separated.

In an object-glass of this construction, when used for direct observation, the two lenses are in contact, with the more convex side of the crown lens next the flint. For photographic use, the lenses are separated, and the more convex side of the crown-glass lens is turned outwards. In order to determine whether this principle of construction should be adopted in the case of a large object-glass to be made for experimental work undertaken with the aid of the Boyden Fund, recently transferred to the Observatory of Harvard College, for the purpose of obtaining astronomical observations at elevated stations, a small object-glass was made upon the new plan by Messrs. Alvan Clark and Sons. As this object-glass proved to be well adapted to its purpose, the larger object-glass above mentioned has been similarly constructed by the same makers; its aperture is 13 inches, and its focal length about 180 inches. Upon trial, it is found that the images formed by this object-glass are excellent when the instrument is used for direct observation, and that the photographic images are equally good when the lenses are separated 3 inches and the crown-glass reversed.

The curvature given to the surfaces of such an object-glass will depend upon the quality of the glass employed; in this particular instrument the radius of curvature for the less convex side of the crown-glass is 86 inches; for the more convex side, 77 inches; for the concave side of the flint, 73·8 inches; the side of the flint-glass which is turned towards the eye-piece is convex, and its radius of curvature is 1020 inches.

EDWARD C. PICKERING.
Harvard College Observatory, Cambridge, U.S.,
September 29.

WILLIAM S. SYMONDS.

WE have already announced the death, on September 15, at Cheltenham, of the Rev. William S. Symonds, Rector of Pendock, F.G.S., and a J.P. for the county of Worcestershire. Mr. Symonds, the eldest son of Mr. William Symonds, of Elsdon, Hereford, was born in Hereford in 1818, took his degree at Christ's College, Cambridge, in 1842, and in 1868 was presented to the Rectory of Pendock.

For several years past Mr. Symonds had suffered from heart-disease, and was compelled to withdraw from his parish duties, as well as from participation in those scientific pursuits in which he had long been actively occupied.

The intimate friend of Murchison, Lyell, and of other pioneers in geology, and frequently their companion in their excursions, he enjoyed exceptional opportunities for observing the facts relating to this science, and has contributed numerous papers, chiefly on the rocks and fossils of the west of England, to the scientific periodicals. Yet he sometimes engaged in discussions of a more general character. In the question of the reptiliferous sandstone of Elgin he took much interest. Before the meeting of the British Association at Aberdeen, in 1859, he visited the Elgin area, and, having worked over it with great care, was led to accept the views of Sir C. Lyell in opposition to those of Murchison; and in the discussions at the meeting strongly insisted on the view that the reptiliferous sandstones are of the New Red or Triassic, and not of the Old Red Sandstone age.

Mr. Symonds did not devote as much attention to palæontology as to physical geology, although even in this branch of the science he has left his mark, and has made numerous and valuable contributions from his collections of fossils to several local museums.

During the latter part of his life his interest was much concentrated on the phenomena of the glacial drifts and the question of the antiquity of prehistoric man. In 1871 he communicated to the *Geological Magazine* (p. 433) a paper on the great Doward Caves, and in his "Severn Straits, or, Notes on Glacial Drifts," he gives his conclusions on this subject.

But it is perhaps chiefly as an earnest and eloquent expounder of the facts and principles of geology and cognate subjects that he will be remembered. He had a remarkable power of infusing his own spirit of research and love of Nature into his writings. Few can read his "Records of the Rocks," "Old Stones," and "Old Bones," or his delightful romances of "Malvern Chase" and "Hornby Castle"—tales of the Wars of the Roses and the Parliament—without recognizing this. In the formation and management of many of the field clubs of the west of England Mr. Symonds's ability was especially conspicuous. He was engaged, with the late Mr. Scobie, of Hereford, in the formation of the Woolhope Field Club, and was elected its President in 1854. He was the frequent companion of the late Sir William Govin in his travels, and took an active part in the management of the Cotteswold Club, of which Sir William was President.

By those who had the privilege of an intimate acquaintance with Mr. Symonds, he will ever be thought of and respected as an earnest and most uncompromising seeker after truth for its own sake. He considered that man is performing a most religious duty when he sits as a humble student at the feet of Nature to learn, as far as may be, the lessons which Nature's Creator would teach him. It was a faith, too, which, throughout the trying and painful complaint that clouded the last days of his life, never failed him, and which enabled him to look forward to, and speak of, the great change which was before him, with cheerful hope.

The number of papers which Mr. Symonds has from time to time contributed to various scientific journals appears to have been over forty.

NOTES.

WE reprint to-day from the *Times* an admirable letter by Mr. Samuel Smith on education in Germany. Mr. Smith gives a clear account of some important elements of the educational system which the Germans are gradually bringing to perfection;

and no one who reads what he has to say will be surprised that in many departments of industry our manufacturers are being surpassed by their Teutonic rivals. The letter contains nothing new; but the essential facts connected with this vital subject cannot be too often pressed on the attention of the public.

WE regret to announce the death of Mr. Joseph Baxendell, F.R.S., of the Observatory, Birkdale, Southport. He died at the age of seventy-two.

ACCORDING to a report which appears in the *Mouvement Géographique*, Kilimanjaro has at last been ascended to its summit. This has been done by a German traveller, M. Meyer, of Leipzig. We know that the mountain presents two great peaks:—Kimawenzi, 4973 metres in height, and Kibo, estimated by Mr. H. H. Johnston at 5745 metres. Mr. Johnston succeeded in ascending the latter to a height of 4950 metres. M. Meyer, it is stated, has succeeded in reaching the crater summit of Kibo, which he estimates at 6000 metres. Before giving absolute credence to these statements, it will be well to await the publication of details by M. Meyer.

IN an interesting article in the *Times* on Monday, on "The British Race-Types of To-day," it was stated that "for a generation or more the advocates of the view that the English are almost unmixed Teutons pressed their ideas upon the scientific and literary world with a persistence and learning which went far to produce conviction." Prof. Huxley, writing to the *Times* about this statement, maintains that, whatever may have been the case with "the literary world," scientific anthropologists were never misled by the "persistence and learning" of those who contended that the English are of almost purely Teutonic origin. "A score of years ago," says Prof. Huxley, "this question was hotly debated; and I do not think that, at that time, any of my anthropological colleagues would have found much fault with the propositions laid down in a paper 'On some Fixed Points of British Ethnology,' which was published in 1871, which propositions are in substance corroborated by the writer of your article."

CAPT. DICKINSON, who has been in charge of the Royal Society's boring at Zagazig, in the delta of the Nile, reports that a depth of 308 feet 6 inches has been reached. The soil from 190 feet has been sand, varying in coarseness, and sometimes mixed with stones. At 308 feet, 4 inches of blue clay was passed through.

THE *Nation* of September 22 prints a letter from Miss Mabel Loomis Todd on the Eclipse Expedition in Japan. Miss Todd's letter is dated Shirakawa, Japan, August 20. We regret to find that the weather was not more favourable for observers in Japan than for observers in Russia and Germany.

WE have received the fourth volume of the Proceedings and Transactions of the Royal Society of Canada. It contains the Proceedings and Transactions during the year 1886. Most of the papers are in English, but some are in French. Among the more important papers we may note "The Right Hand and Left-handedness," and "The Lost Atlantis," by Dr. Daniel Wilson; "The Genetic History of Crystalline Rocks," and "Supplement to 'A Natural System in Mineralogy,' &c.," by Dr. T. Sterry Hunt; "Some Points in which American Geological Science is indebted to Canada," and "On the Fossil Plants of the Laramie Formation of Canada," by Sir J. William Dawson; and "On certain Borings in Manitoba and the North-West Territory," by Dr. Geo. M. Dawson.

The October Bulletin of Miscellaneous Information, issued from the Royal Gardens, Kew, presents the results of a careful inquiry made by Mr. Arthur Shipley, under the auspices of the Royal Gardens, into a disease affecting the onion crop at the Bermudas. Mr. Shipley shows that the disease is caused by a fungus, *Peronospora Schleideniana*, which lives parasitically upon the leaf of the onion plant; and that the atmospheric conditions which favour the progress of the disease are heavy dews or rains, followed by warm, moist, calm weather, and the absence of direct sunshine and cold winds. In favourable weather the progress of the disease is very rapid. The fungus lives in the tissues of the leaf, choking up the air-passages and absorbing the nutritive fluid formed in the cells. Its stem protrudes through the stomata of the leaf into the air. Its branches bear spores at their tips. The reproduction of the fungus is effected by means of these spores, which float about through the air, and also by means of certain special cells formed by the fungus and known as resting-spores. These pass the winter in the earth, and are capable of retaining the power of germination for two or three years. It is by their means that the disease is carried on from one season to another. Mr. Shipley offers various practical suggestions, which will no doubt be of considerable service to cultivators.

DR. SCHWEINFURTH, in co-operation with Prof. Ratzel, is about to publish through Brockhaus, of Leipzig, a collection of all the letters and other writings of Emin Pasha. Dr. Junker also, in association with M. Richard Buchta, who visited the Upper Nile region some years ago, is about to issue a work dealing with the events of recent years in the Soudan. The volume will be illustrated with two maps, and have portraits of Gordon, Emin, Gessi, Lupton, and Slattin. Dr. Junker has just finished his work of preparing at Gotha the great map of his explorations in the basin of the Wellé and the region of the Upper Nile. It has been drawn by Dr. Hassenstein in four sheets, which will appear in the beginning of next year in a supplemental part of *Petermann's Mitteilungen*.

AN elaborate work on palæontology ("Handbuch der Palæontologie") is being issued at Munich and Leipzig by R. Oldenbourg. The editor is Prof. Karl A. Zittel, of Munich, who is aided by Dr. A. Schenck, of Leipzig, and Mr. S. H. Scudder, of Boston. The work is divided into two parts, in the first of which palæontology is dealt with; in the second, palæophytology. The first part, when completed, will occupy three volumes; the second, one. The first two volumes have been published, and we have just received the first "Lieferung" of the third volume. The subject of this "Lieferung" (illustrated by 266 woodcuts) is "Vertebrata: Pisces." The subjects of the later "Lieferungen" of the same volume will be "Vertebrata: Amphibia, Reptilia, Aves, Mammalia." We may mention that a French translation, by Dr. Charles Barrois, is issued simultaneously with the original German work.

A WORK on "The Cultivated Oranges and Lemons, &c., of India and Ceylon," by Brigade-Surgeon E. Bonavia, M.D., of the Indian Medical Department, is about to be issued. The writer's object is mainly practical and economical, but he deals also with some questions of purely scientific interest. His researches have brought him into contact with every variety of Citrus in India and Ceylon, and he claims that he has been able to dispose of, or at any rate to throw new light on, certain disputed points, both botanical and historical, in connexion with this genus. The book will be accompanied by an atlas in foolscap size, consisting of 259 plates of outline drawings of all the varieties of Citrus to be found in India and Ceylon.

MESSRS. DULAU AND CO. have now ready for publication "A Chapter in the History of Meteorites," by the late Dr. Walter Flight, F.R.S.

A THIRD edition of "Weather Charts and Storm Warnings," by Mr. R. H. Scott, F.R.S., has just been issued. The writer's object in preparing the work was to convey some idea of the state of weather knowledge, so that readers might understand what was to be learned from a careful study of the weather charts in the newspapers and of the remarks appended to them.

The second edition has long been out of print. In the present edition the entire text has been revised, and some new chapters have been added, mainly relating to changes and improvements in the Meteorological Office work since the year 1876.

MESSRS. DE LA RUE AND CO. will publish shortly the second volume of "A Treatise on Electricity and Magnetism (Methods of Measurement and Applications)," by E. Mascart, Professor in the Collège de France, and Director of the Central Meteorological Bureau, and J. Joubert, Professor in the Collège Rollin. The work is translated by Dr. E. Atkinson, Professor of Experimental Science in the Staff College.

MESSRS. MACMILLAN will shortly publish a new edition of Huxley and Martin's well-known "Manual of Elementary Biology," considerably revised and extended by Messrs. Howes and Scott, Assistant Professors in the Normal School of Science and Royal School of Mines. To the new volume Prof. Huxley will contribute a preface.

A NEW way of utilizing dynamite has been lately devised by a French military engineer, M. Bonnetond. He uses the expansive force to drive out, for a brief period, the water from portions of wet ground in which foundations are to be made. The method is now in practice in the construction of a fortified *enceinte* at Lyons. A hole is first bored 10 or 12 feet deep, and about 1½ inch wide, in the wet ground. Into this is passed a string of cartridges of dynamite, which is then exploded. The water is thus driven far out beyond the sides of the cavity, over a yard wide, which is produced, and it does not reappear till after half an hour at least. The workmen thus have time to clear the cavity and introduce quickly-setting concrete. When the water returns it cannot injure the foundation. A rapid rate of progress is realized by this method.

A RECENT number of *La Nature* (September 17) has an illustrated account of a steam tricycle contrived by MM. Roger de Montais and L'Héritier, which will go 16 to 18 kilometres an hour with one person, and 14 to 16 with two. In front is a small boiler heated by petroleum, which gives off, it is said, no smoke nor smell, nor unpleasant heat. Under the seat is the petroleum reservoir, holding ten litres, enough to last ten hours, and behind is a water reservoir which holds thirty-four litres, allowing a two and a half hours' run without fresh supply. This water reservoir has one compartment for cold water, and another for water constantly heated by escape of steam; the latter feeding the vertical engine behind, and the former having steam turned into it at will.

IN a new galvanic battery, described by Herr Friedrichs in *Wiedemann's Annalen* (No. 9), a series of inverted bottle-shaped vessels have their necks connected by means of a horizontal tube, into which the exciting liquid (say dilute sulphuric acid and bichromate of potash) flows through a flexible tube from the lower part of a jar, which is raised or lowered to fill or empty the vessels. The liquid can also be let off through a cock at the further end of the connecting tube.

THE various Transatlantic and British fish hatched out this year by the National Fish Culture Association have prospered remarkably well at the establishment at Delaford Park. Some of the Californian rainbow-trout have grown to the extent of 4 inches, and the *S. fontinalis* produced from ova taken from two-year-old fish in the ponds have thriven in a similar manner. The landlocked salmon (*S. sebago*), despite their partiality to deep waters, are also doing well. The whitefish (*Coregonus albus*) have attained a length of 4 inches, and appear to be thriving better this year in consequence of their habitat having been deepened to suit their natural necessities. The cultivation of coarse fish is proceeding, a large quantity of fry bred from German carp being now in the ponds, besides other species.

AT the request of the Austrian Minister for Agriculture, Herr Putik has recently investigated the hydrography of Central Carniola, and given special attention to the Zirknitzer Lake. The phenomena of the periodical emptying and filling of this lake are very extraordinary; a gigantic cave, called Gilovca or Karlovca by the natives, and situated at the north-west corner of the lake, near Niederndorf, forms an outlet for the overflow. This cave lies at the foot of perpendicular rocks, and leads to a number of subterranean lakes, five of which Herr Putik has crossed. He is convinced that there are a great many more of these lakes.

IN the spring of 1885 a Society was founded in Hamburg for the establishment of plantations in Cameroon. Some 30,000 cocoa shrubs were planted, which are now 3½ to 4 feet high, and this year 40,000 more have been added.

ON Monday evenings in Michaelmas Term a course of lectures on "Climate and Weather" will be delivered by Mr. A. W. Clayden, at the City of London College, White Street, Moorfields, E.C. The lectures will be delivered in connection with the London Society for the Extension of University Teaching, and under the auspices of the Mitchell (City of London) Trustees.

A CORRESPONDENT of the *Indian Forester* communicates the following account of "a real weeping tree" to that periodical:—"On my way to and from the Mussoorie Library I have noticed for some days a small pool of water in the middle of the road just above 'Auchnagie.' It struck me as being something singular; and to-day when passing I noticed several drops of water fall into it; on looking up I saw that it was the sap from a branch high up on a tree that was falling into it; the drops were large and were falling at the rate of one a second. I afterwards noticed several other trees of the same kind on the roadside dropping sap from their branches in the same way. The tree is a large one, called by the natives Kágashi (*Cornus macrophylla?*). In the spring, if the bark of this tree is wounded by an axe, the sap runs out of the wound in a great stream; some of it solidifies into a thick mucilage of a bright orange colour; it was from a broken branch that the sap was coming, broken most likely by the heavy fall of snow we had at the end of January. These trees are just bursting into leaf, but they have been weeping for the last ten days at least."

THE additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus sinicus* ♀) from India, presented by Mr. H. R. Sherren; a Leopard (*Felis pardus*) from Ceylon, presented by Mr. W. Mauger; a Guilding's Amazon (*Chrysotis guildingi*) from St. Vincent, West Indies, presented by Mrs. Ellice; a Pheasant (*Phasianus colchicus*), British, presented by Mr. A. L. Sawer; a Common Toad (*Bufo vulgaris*) from Gloucestershire, presented by Mr. John Scovell; a Chinchilla (*Chinchilla lanigera*), born in the Gardens.

OUR ASTRONOMICAL COLUMN.

PROPER MOTION OF LL. 26481.—Mr. John Tebbutt (*Observatory*, 1887, October, p. 360), having found a considerable discordance between the places given for this star by Lalande and Lamont, asked Mr. Lenehan, the acting Government Astronomer at Sydney, to observe the star with the transit-circle of that Observatory. The result of his observations, as will be seen, shows that the star has a large proper motion in both elements:—

	Mean R.A. 1886·0. h. m. s.	Mean N.P.D. 1886·0. ° ′ ″
Lalande 1800 ...	14 25 1·06 ...	105 6 33·4
Lamont 1850 ...	14 25 2·17 ...	105 6 56·6
Lenehan 1887 ...	14 25 2·67 ...	105 7 14·2

THE WASHBURN OBSERVATORY.—The fifth volume of the Publications of the Washburn Observatory of the University of

Wisconsin, containing the record of the work done during the year and quarter ending April 1887, has recently been published. Pending the appointment of a successor to Prof. Holden, who resigned his position as Director of the Observatory in the winter of 1885, to assume the direction of the Lick Observatory, Dr. Davies, Professor of Physics in the University of Wisconsin, has had general charge of the Observatory, to which of course he has been able to devote only a limited amount of time and attention. It is no doubt owing to this circumstance that no very definite programme of work has been carried out since the completion of the observations and reductions necessary for clearing off the list of 303 fundamental stars undertaken by Prof. Holden for the *Astronomische Gesellschaft*. The assistants, Mr. Updegraff and Miss Lamb, have been principally employed in observations of fundamental stars for determination of latitude and discussion of the instrumental errors of the meridian-circle and of refraction. The large equatorial has been used for measures of double stars, observations of Sappho and of Comet *b* 1887 (Brooks). A considerable portion of the volume is devoted to an index of the stars occurring in Airy's Greenwich Catalogues, not found in Flamsteed, which has been prepared by Miss Lamb. This index, which will be very useful to practical astronomers, has been prepared according to the general plan suggested by Argelander in his review of the Greenwich second seven-year Catalogue (*Vierteljahrsschrift*, Bd. vi. Heft 2), and contains some 3000 entries. The computer is thus saved the labour of looking through all six catalogues when searching for a star which is tolerably sure to be in one of them, and as the catalogues are reduced to six different epochs, the labour attending such a search would be considerable. The star places in Miss Lamb's index are reduced to 1875·0.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 OCTOBER 16-22.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on October 16

Sun rises, 6h. 27m. ; souths, 11h. 45m. 39·0s. ; sets, 17h. 4m. ; decl. on meridian, 8° 52′ S. : Sidereal Time at Sunset, 18h. 44m.

Moon (New on October 16, 23h.) rises, 5h. 22m. ; souths, 11h. 27m. ; sets, 17h. 21m. ; decl. on meridian, 2° 20′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Decl. on meridian.
Mercury ...	8 41 ...	13 7 ...	17 33 ...	18° 17′ S.
Venus	3 37 ...	9 40 ...	15 43 ...	0 9 S.
Mars	1 28 ...	8 36 ...	15 44 ...	12 24 N.
Jupiter... ...	8 8 ...	12 57 ...	17 46 ...	14 21 S.
Saturn... ...	23 6* ...	6 54 ...	14 42 ...	19 10 N.

* Indicates that the rising is that of the preceding evening.

Oct. h.
18 ... 1 ... Jupiter in conjunction with and 4° 19′ south of the Moon.
18 ... 9 ... Mercury in conjunction with and 7° 39′ south of the Moon.

Variable Stars.

Star.	R.A. h. m. s.	Decl. ° ′		h.	m.	
U Cephei	0 52·3	81 16 N.	Oct. 18,	4	11	*m*
Algol	3 0·8	40 31 N.	,, 21,	5	43	*m*
ζ Geminorum ...	6 57·4	20 44 N.	,, 20,	0	0	*m*
U Monocerotis ...	7 25·4	9 33 S.	,, 16,			*m*
U Coronæ	15 13·6	32 4 N.	,, 17,	4	15	*m*
R Scorpii	16 10·9	22 40 S.	,, 20,			*M*
U Ophiuchi	17 10·8	1 20 N.	,, 17,	3	57	*m*
		and at intervals of		20	8	
X Sagittarii... ...	17 40·5	27 47 S.	Oct. 20,	0	0	*m*
W Sagittarii ...	17 57·8	29 35 S.	,, 17,	0	0	*m*
T Herculis	18 4·8	31 0 N.	,, 19,			*m*
β Lyræ...	18 45·9	33 14 N.	,, 21,	0	0	m_2
R Lyræ	18 51·9	43 48 N.	,, 16,			*m*
η Aquilæ	19 46·7	0 43 N.	,, 20,	1	0	*m*
R Sagittæ	20 8·9	16 23 N.	,, 16,			*m*
S Delphini	20 37·9	16 41 N.	,, 17,			*m*
T Aquarii	20 44·0	5 34 S.	,, 17,			*m*
δ Cephei	22 25·0	57 50 N.	,, 18,	1	0	*m*

M signifies maximum ; *m* minimum ; m_2 secondary minimum.

Meteor-Showers.

	R.A.	Decl.	
Near ξ Ceti	31° ...	8° N. ...	Slow ; with trains.
,, ν Orionis... ...	90 ...	15 N. ...	Swift ; streaks.
,, δ Geminorum ...	108 ...	23 N. ...	Swift ; streaks.
,, κ Cephei	308 ...	78 N. ...	Slow ; faint.

METEOROLOGICAL NOTES.

WE have just received the "Results of Meteorological and Magnetical Observations" made at Stonyhurst College during 1886, which maintains well the high character of previous issues. Pressure, at a height of 381 feet, fell, on December 8, to 27·350 inches, being absolutely the lowest hitherto noted at Stonyhurst. On the other hand, no excessively high readings occurred during the year, the maximum of each month being in each case topped by higher readings in previous years. For eight of the months temperature was under the normal, the deficiency being, in February, 5°·2 ; December, 4°·4 ; January, 3°·4 ; and March, 2°·3 ; and for the year, 1°·5. In October, temperature was 2°·9 above the average of the month. The annual rainfall was 5·18 inches above the mean, a result largely due to the heavy rains of January and May ; but in February and August the fall was much less than usual. The actual sunshine for the year was 30 per cent. of the possible sunshine, the highest monthly percentage being 40 in April, and the lowest 20 in January. The maximum sunshine for the day occurs from 10 a.m. to 3 p.m., the absolutely highest hour being from 1 to 2 p.m. Among the more special meteorological work undertaken by the College are observations of cirrus clouds, upper glows, and trees, shrubs, and flowering plants. For the fourth year an appendix is given, exhibiting with some fullness the results of the observations made at St. Ignatius's College, Malta, by the Rev. J. Scholes.

THE Meteorological Commission of Cape Colony have published their Report for the year 1886, containing summaries of observations taken at 28 stations, and rainfall observations at 268 stations, including 3 in Basutoland and 11 in Orange Free State. At the Royal Observatory the absolute maximum temperature in a Glaisher stand was 104°·0 in January, and the minimum 34°·5 in August. In a Stevenson screen the maximum was 101°·0, and the minimum 35°·0 (in July), plainly showing the effect of radiation on the former readings. The total rainfall measured on 77 days was 27·79 inches, of which 7·68 inches fell in June. Self-recording anemometers are erected at the Royal Observatory, Port Elizabeth, and East London, but no arrangements are made for reducing the curves. The Commission draw special attention to the interesting series of thirteen rainfall maps appended to the Report. These have been reduced from maps exhibited by Mr. Gamble at the Indian and Colonial Exhibition, and represent the average rainfall of the colony for each month and for the whole year.

IN the *Annalen der Hydrographie und maritimen Meteorologie* for September, Capt. D. Ruete gives the results of eighteen years' experience of the typhoons of the China seas, accompanied by rules for manœuvring in different cases, and by charts of the tracks of the typhoons in various seasons. The distinguishing features of the typhoon are its small diameter and its sharply-defined central calm, as compared with ordinary rotatory storms. They occur most frequently in August and September, less frequently in July and October, and more rarely still in May, June, and November. They do not extend into the higher regions of the atmosphere ; Knipping assumed a height of four nautical miles for the severe September typhoon of 1878. Their form is generally oval, but dependent on the contour of the coasts. The author finds that they travel slower when south of 30° N. than they do when north of that position, and that the faster they travel and the greater the area of the storm field, the more severe the typhoons are, and that the duration of the fall of the barometer gives no clue to the magnitude of the area of the storm, and further, that an approaching typhoon is shown less by the amount of the fall than by the general behaviour of the instrument. The regions most frequented by the typhoons extend from lat. 10° N. to 38° N. in the China seas, and to 50° N. in the Japan seas, and may be divided into four different seasons and districts, which are separately discussed in the paper.

At the meeting of the International Meteorological Committee at Paris in 1885, Dr. H. Wild, of St. Petersburg, and Prof. E. Mascart, of Paris, were requested to take the initiative in the publication of a series of international meteorological tables, to allow of a uniform method of reduction of the observations being everywhere carried out, and a specimen has now been printed and issued for criticisms. The work contains (1) a description of the tables and of their use, in English, French, and German; (2) equivalents of various units, and general tables used by different countries for meteorology, magnetism and electricity. The volume will occupy nearly 440 pages quarto, and will cost about 30*s*. The Committee request each country to contribute to the expense of publication by subscribing for a certain number of copies.

We have received the results of meteorological observations made in New South Wales during 1885, under the direction of H. C. Russell, B.A., F.R.S., the Government Astronomer, forming a compact octavo volume of 360 pages, with plates of meteorological curves for each month. The great increase in the number of rainfall and river stations has made it necessary to publish these observations in a separate volume. Some important additions have been made to the information given in past years, and the barometric curves have been so arranged as to show the various changes through all the stations at one view. The tables show (1) the most important data for the whole colony; (2) the data for each station separately; (3) the barometric curves; and (4) the monthly abstracts published during the year. Considerable attention has been paid to the amount of evaporation in various parts of the colony, and the quantities for some districts are found to be much below what has often been stated, depending very much upon the state of the soil. The year 1885 was one of serious drought, although the total rainfall for the year was only 10 per cent. below the average, owing to heavy rainstorms at the end of January. The total rainfall at Sydney was 39·78 inches measured on 145 days. The greatest fall was in June, and the least in August. The mean temperature for the year was 63°·9. The maximum in the shade was 97°·6 on November 5, and the minimum 40°·6 on June 30. The mean temperature of the whole colony for 1885 was 63°·0, or 1°·7 in excess of the mean for 15 years. The absence of cloud, and the drought, have had a decided effect upon the temperature of the whole colony, showing apparently a direct relation between rainfall and temperature.

THE PROBLEM OF THE HOP PLANT LOUSE (PHORODON HUMULI, Schrank) *IN EUROPE AND AMERICA.*[1]

The author has been for several years carrying on investigations with a view of ascertaining the full annual life-history of *Phorodon humuli*, and especially with a view of settling the hitherto moot question as to its winter life. The importance of the inquiry, both from the economic and the scientific sides, is self-evident. The hop crop, in all parts of Europe where it is grown, and especially in England, annually suffers more or less from the ravages of this its worst insect enemy, and in some years is a total failure. The same is true in North America, at least east of the Rocky Mountains, and last year the injuries of this Phorodon in the hop-growing regions of the State of New York were so great that many hop yards were abandoned and have since been ploughed up; while but 10 per cent. of an average crop was harvested. From the purely scientific side, entomologists, notwithstanding the great interest attaching to the subject, have been divided in opinion as to the identity, or specific relationship, of the hop Phorodon and one that occurs on Prunus, while the complete annual cycle of the insect's life has remained a mystery. After full and satisfactory investigations I have satisfied myself that, contrary to the prevailing impression among hop growers and previous investigators, the hop plant louse does not hibernate underground on the roots of the hop, nor in, on, or about anything in the hop yard; but that, upon the advent of the first severe frosts, the hop plant and the hop yards are entirely cleared of the species in any form. I find that all statements to the contrary in America are based on misapprehension, or mistaken identity of species, and I believe (though admitting the possibility of variation in this respect in milder climates) that the same will be found to hold true in England, where hibernation on the hop root has been accepted by high authority. The positive statements made about eggs being laid in autumn, whether on roots or upon the vines left in cutting, or which are carted away, are based on conjecture, and have been blindly copied without credit by one writer from another, a practice too common among second-hand writers on economic entomology.

The conjectures of some of the best students of aphidology that *Phorodon humuli* had a form (*malaheb*, Fonsc.) living on Prunus, and that there was a consequent migration from one plant to the other, I have positively proved to be correct, by direct colonizing from Prunus to Humulus, and by continuous rearing from the original stem-mother hatched from the winter egg.

The observations have been made on growing plants and in vivaria at Washington, and checked by others made simultaneously in hop yards at Richfield Springs, N.Y. An incident may here be recorded as illustrating the effect of meteorological extremes upon Aphides. The extreme heat (over 100° F.) and dryness of July 17 and 18 killed every one of the insects under observation at Washington, entirely clearing the plants. The economic bearing of such exceptional phenomena, as also of the biological observations made, is readily understood.

The more important conclusions from the studies so far made are thus summed up in a paper which I had the honour to read before the American Association at its recent meeting in New York:—

(1) *Phorodon humuli* hibernates in the winter egg state, all the egg being fastened to the twigs (generally the previous season's growth) of different varieties and species of Prunus, both wild and cultivated. The egg is difficult to detect, because it is covered with particles which resemble the bark in colour and appearance. It is usually laid singly, and when freed of disguising particles is seen to be ovoid and 0·04 mm. long.

(2) The annual life-cycle is begun upon Prunus by the stem-mother, which hatches from this winter egg. The stem-mother is stouter than the individuals of any of the other generations, with the legs, antennæ, and honey-tubes relatively shorter, while the cornicles between the antennæ are sub-obsolete. The colour is uniform pale green, with bright red eyes and faintly dusky tarsi.

(3) Three parthenogenetic generations are produced upon Prunus, the second at once distinguished by its more elongate form, much longer members, distinct cornicles and markings of darker green; while the third (or typical *malaheb* form) becomes winged, and instinctively abandons the plum and migrates to Humulus. The habit of moving from plant to plant after giving birth to an individual, and thus scattering the germs of infection on Humulus, is well marked in this winged generation.

(4) During the development of the three plum-feeding generations, the hop is always free, and subsequently, until the return migration, the plum becomes more or less fully free from infection by this species.

(5) A number of parthenogenetic wingless generations are produced on the hop (seven, or the tenth from the stem-mother on plum, having been traced up to August 5, and advices of the eleventh, up to August 19, have been received since my arrival in England); and, finally, there is a return migration of winged females to the plum in autumn. The wingless hop generations are not only incapable of migrating to the plum, but do not thrive upon it when artificially transferred thereto.

(6) Exact observations are not yet complete as to the full number of generations produced upon the hop before the winged return migrant appears, and another month's careful watching and experiment is needed to fill this hiatus in the annual cycle, as also to ascertain the exact number of generations produced in autumn on the plum. From knowledge extant and previous general observation, the facts will probably prove to be as follows:—

(7) The eleventh or twelfth generation will produce winged females (from the middle to the end of August), which will deposit their young upon the plum, and these will become the only sexed individuals of the year, the male winged and the female wingless, the latter after coition consigning a few impregnated or winter eggs to the twigs.

(8) Up to August 5 the first females on hop were still alive and breeding, having existed two months. There is, consequently, an increasing admixture of generations from the first

[1] Abstract of a Paper read before Section D of the British Association for the Advancement of Science, at Manchester, by Prof. C. V. Riley, on September 3.

on hop until frost overtakes the species in all conditions and sweeps from the hop yards all individuals alike, perpetuating in the egg state those only which reached the sexual condition on the plum.

(9) Each parthenogenetic female is capable of producing on an average 100 young (the stem-mother probably being more prolific) at the rate of one to six, or an average of three per day, under favourable conditions. Each generation begins to breed about the eighth day after birth, so that the issue from a single individual easily runs up, in the course of the summer, to trillions. The number of leaves (700 hills, each with two poles and two vines) to an acre of hops, as grown in the United States, will not, on the average, exceed a million before the period of blooming or burring; so that the issue from a single stem-mother may under favouring circumstances blight hundreds of acres in the course of two or three months.

(10) While meteorological conditions may materially affect the increase and power for injury of the species, these are far more truly predetermined and influenced by its natural enemies, many of which have been studied and will be described.

(11) The slight colorational differences, as also the structural differences, including the variation in the cornicles on head and basal joints of antennæ, whether upon plum or hop, are peculiarities of brood, and have no specific importance whatever.

(12) The exact knowledge thus gained simplifies the protection of the hop plant from Phorodon attack. Preventive measures should consist in destroying the insect on plum in early spring where the cultivation of this fruit is desired, and the extermination of the wild trees in the woods wherever the hop interest is paramount; also in avoiding the introduction of the pest into new hop countries in the egg state upon plum cuttings or scions. Direct treatment is simplified by the fact that the careful grower is independent of slovenly neighbours, infection from one hop yard to another not taking place.

Experiments still under way have shown that there are many effective remedies, and that the ordinary kerosene emulsion diluted with twenty-five parts of water and sprayed with the cyclone nozzle, or a soap made by boiling one pound of pure potash in three pints of fish oil and three gallons of water, and this dissolved in eight gallons of water, and sprayed in the same way, are thoroughly effectual remedies, and leave the plant uninjured. The former costs 75 cents, the latter 30 cents, per acre, plus the time of two men for three hours, plus appliances. The object of further experimentation now being carried on is to simplify and reduce the cost of these last to a minimum. As they consist, however, essentially of a portable tank and a force-pump with hose and spraying attachment, which, together, need not involve a greater first outlay than 5 dollars to 10 dollars, and as every American hop grower can afford to expend the larger sum for the protection of a single acre, there is no longer any excuse for allowing this pest to get the better of us.

It is not my purpose, however, to enter into aphidicide details in this communication, which I will conclude by brief reference to the bearings of these discoveries in America on the problem in Great Britain. The most comprehensive and satisfactory review of the knowledge possessed on the subject in England that has come to my notice, is that by my esteemed friend and correspondent, Miss Eleanor A. Ormerod, Consulting Entomologist of your Royal Agricultural Society, in her "Report of Observations of Injurious Insects," &c., made in 1885. So far as her own careful observations are concerned, they fully accord with the facts here set forth; but on the authority of others, and especially on the evidence of Mr. C. Whitehead, who reported finding young lice and large viviparous females on hop shoots as early as March 29, and that of Mr. A. Ward, who experimented with surface dressings near Hereford, Miss Ormerod concludes that attack on the hop begins in spring from wingless females which come up from the hop hills, and, as a corollary, that dressings to prevent such ascent are strongly to be recommended. It is quite within the range of possibility, and what is known of aphid life, that where the winters are mild, with scarcely any frost, this Phorodon may continue on the hop from one year to another in the parthenogenetic condition. If such is ever the case in England, you have a somewhat different set of facts to deal with here from what we have in America. But for the reasons already stated in abstract, from many other detailed observations which it would be tedious to record here, as well as from the ease with which erroneous conclusions are arrived at in entomological matters of this kind, where not checked and proved by the most competent and careful study, I shall be inclined to believe that the facts in England are essentially the same as I have found them in America, until convincing and trustworthy evidence to the contrary be forthcoming. Mr. Whitehead may have had another species under observation, and Mr. Ward's surface dressings may have acted by repelling the winged female migrating from Prunus, in the same way that buckwheat sown among the hops is believed to do with us.

EDUCATION IN GERMANY.

THE following letter appeared in the *Times* on the 10th inst.:—

SIR,—I should be glad if you could find space in your columns for some remarks on the state of education in Germany. I have just completed a short tour in this country, mainly to inquire into its educational system, especially with reference to primary and technical schools. England has at last roused herself to the necessity for technical education.

The Bill, which was unhappily crowded out last session, will be reproduced next year, and, I trust, expanded to larger dimensions. It will contain, I hope, a clause for the establishment of evening continuation schools, for which object I gave notice of an amendment last session. My trip to Germany has been chiefly taken to learn what is doing here in this direction, and what is the drift of educated German opinion. With your permission I will briefly summarize my impressions. I premise by observing that each State of the German Empire manages its own education, and that the laws and regulations differ somewhat, so that general statements referring to all Germany cannot be made without qualifications. I will not weary your readers, however, by going into details respecting each State, but place broadly before them the general features of German education.

The salient fact which strikes all observers is the universality of good education in this country. There is no such thing as an uneducated class; there are no such things, speaking broadly, as neglected and uncared-for children. All classes of the community are better educated than the corresponding ones in our country; and this applies quite as much to primary as to secondary education. Nothing struck me more than the general intelligence of the humbler working classes. Waiters, porters, guides, &c., have a knowledge of history, geography, and other subjects far beyond that possessed by corresponding classes in England, and the reason is not far to seek. The whole population has long been passed through a thorough and comprehensive system of instruction obligatory by law and far more extended than is given in our elementary schools. I went through several of these schools and observed the method of teaching, which was simply admirable. The children are not crammed, but are taught to reason from the earliest stages. The first object of the teacher is to make his pupils comprehend the meaning of everything they learn, and to carry them from stage to stage so as to keep up an eager interest.

I saw no signs of weariness or apathy among either teachers or scholars. The teaching was all *vivâ voce*, the teacher always standing beside the blackboard and illustrating his subject by object-lessons. The instruction was through the eye and hand as well as the ear, and question and answer succeeded so sharply as to keep the whole class on the *qui vive*. The teachers are, as a body, much better trained than in England, and seem to be enthusiasts in their calling, and the school holds a far higher position in the social economy of the country than it does with us. What I am saying here applies equally to Switzerland as to Germany, and for educational purposes Zurich will compare with any part of this empire. The main advantage, however, that primary education has in Germany over England lies in the regularity of attendance and the longer period of school life. There is none of the difficulty of getting children to school that exists in England; the laws are very rigid and permit no frivolous excuses, and, what is even more important, the people entirely acquiesce in the laws, and are inclined rather to increase than relax their rigour. It is well known that in London and all our great cities a large part of the population seek to avoid school attendance by every means in their power, and consequently the attendance is most irregular. There is very little of this in Germany; at least I have not found it so. Then in our country a great portion of our children are withdrawn altogether from school, after passing the fourth or fifth standard, at the age of eleven or twelve, whereas in Germany almost

everywhere attendance is compulsory until fourteen for boys, though in some places girls are allowed to leave at thirteen.

This last point is the one I wish to emphasize. The great defect—I might almost call it the fatal defect—of our system is that it stops just at a time when real education should begin. It allows a child to leave school at an age when its learning is soon forgotten and its discipline effaced. It is hardly too much to say that the two years' additional training the German child receives in the elementary school, doubles its chance in life as compared with the English child.

But this is not all. The Germans are rapidly developing a system of evening continuation classes which carry on education for two or three years longer. In Saxony the boys who leave the primary school, if they do not go to the higher schools must attend for three years longer—say, until they are seventeen—continuation classes for at least five hours per week. But teaching is provided for them, and they are encouraged to attend, twelve hours per week. So complete is this system that even the waiters at the hotels up to the age of seventeen attend afternoon classes, and are taught one or two foreign languages. I take Saxony as one of the most advanced States; but the law is much the same in Würtemburg and Baden, and the system is found to work so well that it is in contemplation to extend it to all the States in the German Empire, and Austria will probably follow suit. This is confidently expected to happen in the course of 1888. I may state as an undoubted fact that in Germany and Switzerland, and I believe in some other Continental countries, the opinion is ripening into a conviction that the education, even of the poorest class, should be continued in some form or another to the age of sixteen or seventeen. They find by experience that wherever this is adopted it gives an enormous advantage to the people in the competition of life, and, above all, trains them to habits of industry and mental application. I believe it is owing to this system of thorough education that Germany has almost extinguished the pauper and semi-pauper class, which is the bane and disgrace of our country.

Wherever I have gone I have inquired how they deal with the ragged and squalid class of children, and I have been told in every city I visited—in Zurich, Stuttgart, Nuremberg, Chemnitz, Dresden, and Berlin—that such a class practically does not exist. I do not mean that there is not poverty and plenty of it in Germany. Wages are much lower than in England, and many have a hard struggle to live. But there does not seem to exist to any extent that mass of sunken, degraded beings who with us cast their children upon the streets, or throw them on the rates, or leave them to charity. Some half-a-million of children in the United Kingdom are dependent, more or less, on the alms or the rates of the community, and probably another half-million are miserably under-fed and under-clad. Nothing to correspond with this exists in Germany. The poorest people here would be ashamed to treat their children as multitudes do with us. Indeed, I have not seen since I left home a single case of a ragged or begging child. I repeat that the great cause of this both in Germany and Switzerland is the far greater care they have taken of the education of the children for at least two or three generations, whereas we have only taken the matter up seriously since 1870, when Mr. Forster's great Act was passed.

Let us contrast the general condition of our London children, for instance, at the age of fifteen or sixteen, with that of the same class in Berlin or Dresden or Chemnitz. With us nine-tenths of the children have long since left school, and a too large proportion of them are receiving no training but the coarse and brutalizing education of the streets. Most of them retain little of what they have learnt at school, except the power to read the "penny dreadful," which stuffs their minds with everything a child should not know. They are to a very large extent adepts in profane and obscene language, and are frequenters of the public-house, the "penny gaff," and such like amusements; a great many of them are learning no useful trade or calling, but are drifting helplessly into the class of wretched, ill-paid, casual labourers. Very many of them marry before they are twenty, and are soon the parents of a numerous progeny, half-starved and stunted both in body and mind. Compare, or rather contrast, this with Germany. At fifteen or sixteen a great part of the children are still under excellent instruction. Exceedingly few are to be found roaming about the streets. They are prohibited, at least in some parts of Germany, from entering the public-houses (except with their parents) until the age of seventeen, and I am told are everywhere prohibited from smoking until sixteen. In fact, there are, both by law and public sentiment, barriers placed against the corruption of the young which do not exist in England.

No country has ever suffered more from the abuse of the idea of individual liberty than England has done. Owing to this overstrained idea we did not get compulsory education until long after the advanced nations of the Continent, and still we are far behind them in the care we take of our children. It is intolerable that this state of things should continue longer. Democratic government everywhere insists upon good education, and expects each citizen to fulfil his duties to the State.

Public opinion in our country will certainly insist, and that before long, that we shall not be for ever disgraced with a residuum of the most drunken, demoralized, and utterly incapable population to be found in any modern State. It will insist that some time be spared for the solution of this vital question from the wrangles of party politics and the party recriminations of party leaders. When one sees what a poor country like Germany has done to raise its people in spite of the conscription and three years' compulsory military service, in spite of frequent and exhausting wars from which our island home has been free, one has grave doubts whether our system of party government is not a failure.

Certainly we waste on barren conflicts and wordy strife far more time than other nations do in the conduct of their affairs. They direct their energies with business-like precision to supply the exact needs of the people, we fritter away our enormous political energy in fruitless party contests which every year degrade Parliament lower and lower, and make it less and less fit for the practical work of governing the nation.

One thing seems certain. Unless we can give more attention to the vital questions which concern the welfare of the masses our country must go down in the scale of nations. No honest observer can doubt that in many respects the Germans are already ahead of us, and they are making far more rapid progress than we are. They are applying technical science to every department of industry in a way that Englishmen have little idea of. Their polytechnics and their practical technical schools are far ahead of anything we possess in England, the leaders of industry are far better trained, the workmen are better educated and far more temperate and thrifty than ours are. Wherever the Germans and English are coming into competition upon equal terms the Germans are beating us. This is not because the Germans have greater natural power. I believe the British race is the more vigorous naturally. But they are organized, disciplined, and trained far better than we are. They bring science to bear upon every department of the national life, whereas we, up till lately, resented all State interference, and so exaggerated the doctrines of freedom as almost to glory in our abuses.

There is much more that I might say if space permitted; but it will not do to trespass further on your indulgence. I will only add in conclusion that England must wake up, and that immediately, to the necessity of a far more thorough and practical system of education, else she will lose the great place she has hitherto held in the world's history.

I am, Sir, yours faithfully,

Berlin, October 4. SAMUEL SMITH.

THE BRITISH ASSOCIATION.

SECTION B—CHEMICAL SCIENCE.

The Atomic Weight of Gold, by J. W. Mallet, F.R.S.—Attention is called to the importance of correct determinations of atomic weights by different experimenters, and especially the elimination of "constant errors." Considering the desirability that all such values should be connected as directly as possible with hydrogen, a method is described by which this may be done in the case of gold. A known weight of zinc is dissolved in dilute sulphuric acid, and the hydrogen evolved is measured. A solution of bromide or chloride of gold is then treated with zinc more than sufficient to precipitate the whole of the gold, the residual zinc being determined by the hydrogen evolved on treatment with sulphuric acid. The difference in volume of hydrogen obtained gives a direct means of calculating the atomic weight of gold. The author described various experimental precautions that had been taken in measuring the gas.

The Atomic Weight of Zirconium, by Dr. G. H. Bailey.—The previous determinations of the atomic weight of this element

were made by Berzelius (89·25), Hermann (88·8), Marignac (90·54). The earlier results were doubtless vitiated by the presence of iron and of the cerite earths, whilst Marignac's determination is open to objection from the character of the salt (potassium zirconium fluoride) which he used. In the present determination zirconia was prepared from North Carolina zircons by three independent methods. It was dissolved in sulphuric acid, and the sulphate was crystallized out. This salt becomes normal and constant in weight by heating some hours at 400°, the temperature at which it begins to decompose being 470°. The relation of zirconium sulphate to zirconia gives a ratio from which the atomic weight is calculated, and, though the work is not complete enough to state the result with accuracy, the value obtained agrees more nearly with that of Marignac. The author proposes to make further determinations, using the tetrabromide.

Torsion Balances, by Dr. A. Springer.—Light frames are made and stiffened by wires or flat bands being tensioned over them. The beam is then firmly clamped to the bands in such a manner that its centre of gravity is above its point of support; this tends to tip the beam, thus equilibrating the torsional resistance of the fulcrums. We thus have the torsional resistance exerted to keep the beam horizontal, and the high centre of gravity tends to tip it out of the horizontal. The adjustment of the position of the centre of gravity is most easily made by having an adjustable poise placed immediately above the centre of the torsional wire. In order to do away with the necessity of alignment of support, a secondary beam is attached to the first in such a manner that both beams tending to tip in the same direction remain stationary owing to their having opposite and equal moments. On this principle scales are constructed which can be used on rolling ships, or in buildings where there is considerable jarring. In all the "torsion balances" there is permanence of adjustment, consequently repeated weighings will give like results. Various "torsion balances" were shown illustrating the principle involved, as well as to show how equal sensitiveness can be obtained with any load.

Integral Weights in Chemistry, by T. Sterry Hunt.—The author maintains the necessity for taking hydrogen as standard of specific gravity, not only for gases, but also for liquids and solids, and thinks that such considerations lead to a comprehension of the physical properties and chemical constitution of chemical substances.

Action of Light on the Hydracids of the Halogens in presence of Oxygen, by Dr. A. Richardson.—HCl and O were mixed in different proportions in bulbs, and exposed forty to sixty days in sunlight, at the end of which time their contents were examined. Free Cl was only noticeable when large excess of O was present, and when the gases had not been dried. Similar experiments were made in the cases of HBr and HI.

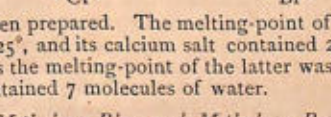
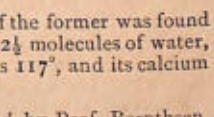
The Present Position of the Alkali Trade, by A. E. Fletcher.—The author, in presenting this report, remarked that no report on this subject had been presented to the Association since that of his predecessor in office, Dr. Angus Smith, in 1861. Tracing the general history of the alkali manufacture, he noticed the various improvements which have been carried out in the mechanical details and in the chemistry of the processes, with special reference to the recovery of by-products. The rapid growth of the alkali trade in Germany was shown by a reference to the exports and imports of the product. In Germany this advance is largely due to the adoption of the ammonia-soda process, a process which in this country has been taken up thus far by only three firms. It was pointed out, however, that the Leblanc process, with certain modifications, was not by any means a forlorn hope; and the improvements now being worked out by Messrs. Chance, Messrs. Parnell, and Messrs. Gaskell, Deacon, and Hurter were referred to.

On the Constituents of the Light Oils of Blast-Furnace Coal Tar from the Garthsherrie Works, by Watson Smith.—These tars are recovered from the condensation of the gases evolved from a blast-furnace, condensed by means of the process of Messrs. Alexander and McCosh. The various constituents are described, amongst others toluene and xylene, the latter consisting of about 70 per cent. metaxylene. Considerable interest was manifested in a specimen of flannel dyed by xylidine scarlet prepared from this metaxylene, being the first dye product ever prepared from the by-products of the blast-furnace.

A New Apparatus for condensing Gases by contact with Liquids, by Prof. Lunge.—The apparatus described is of a construction of the "plate column" style, and consists, as the name implies, of a series of perforated plates arranged in column, and made of stoneware. The gases as they rise are thus brought into immediate contact with an extensive plane surface of the absorbing liquid. The cooling is not so rapid as with coke towers and similar arrangements, but it is to be borne in mind that the gases pass more slowly owing to the completeness of the contact with liquid. Comparative results were given showing the working relation between this apparatus and others.

The Extent to which Calico Printing and the Tinctorial Arts have been affected by the Introduction of Modern Colours, by Charles O'Neill.—The author reviewed the state of things in 1856, at which time Perkin's aniline mauve was discovered, and showed the effect of the introduction of alizarine colours. It was shown that the demand had not kept pace with the production, and this had much to do with the unremunerative condition of calico printing.

The Chemistry of Cotton Fibre, by F. H. Bowman.—It is shown that cotton, in common with all vegetable substances, has for its base cellulose. The opinion is here expressed that, judging from the results of combustion, it consists of a series of bodies not agreeing in all respects with one another. The mineral matter of cotton amounts to about 1 per cent., and this is considered to form an essential constituent.

Isomeric Change in the Phenol Series, by O. R. Ling.—The isomers  and  were stated to have been prepared. The melting-point of the former was found to be 125°, and its calcium salt contained 2½ molecules of water, whereas the melting-point of the latter was 117°, and its calcium salt contained 7 molecules of water.

On Methylene Blue and Methylene Red, by Prof. Bernthsen.—In this paper it was shown how from thiodiphenylamine, by respective nitration, reduction, and oxidation, Lauth's violet (thionine) was obtained. Methylene blue was described as tetramethylthioninechloride.

On the Constitution of Azimido Compounds, by Drs. Noelting and Abt.—The formulæ given by Griess and by Kekulé for the azimido compounds were explained and discussed, and it was shown that the evidence was in favour of the formula of Kekulé.

Velocity of Formation of Acetic Ether by Prof. Menschutkin.—In these experiments acetic anhydride was used instead of the acid by which in his previous work the process of etherification had been effected. It was found that the velocity of the reaction was greater with the primary than with the secondary alcohols, and that it was much slower in the higher members of an homologous series than in the lower. A distinct and marked difference occurs between isomers as shown by the examination of butyl and isobutyl alcohols. By working in solution the reaction proceeds more regularly, and the results agree better amongst themselves. The author finds that the nature of the solvent affects the course of the reaction in such a manner as to throw some doubts on the hypothesis of Guldberg and Waage.

The Relation of Geometrical Structures to Chemical Properties, by Prof. Wislicenus.—This memoir—probably the most important contribution that has yet been made to the question of the constitution of unsaturated organic compounds—cannot readily be understood without the accompanying diagrams. The results have already been published in the Transactions of the Royal Saxon Academy.

Note on Valency, by Prof. Armstrong.—An attempt to explain the phenomena of valency according to the views expressed by Helmholtz as to the unit charge of the elements and its distribution.

The Solubility of Isomeric Organic Compounds, by Prof. Carnelley.

(1) For any series of isomeric organic compounds the order of solubility is the same as the order of fusibility, *i.e.* the most fusible compound is also the most soluble. This is shown to hold true in a very large number of cases, whilst there are very few exceptions, and those of a doubtful character.

(2) The order of solubility of two or more isomeric compounds is independent of the nature of the solvent. This has been experimentally proved, more particularly in the case of meta- and para-nitraniline, the solubility of each of which in thirteen different solvents has been determined. Also by a considerable number of cases taken from literature.

(3) The ratio of the solubilities of two isomers in a given solvent is constant, and is therefore independent of the nature of the solvent. So that—

$$\frac{\text{Solubility of A in any solvent}}{\text{Solubility of B in the same solvent}} = \text{constant.}$$

This has been proved for meta- and para-nitraniline in respect of thirteen different and very varied solvents.

Alcohol and Water Combinations, by Prof. Mendeléef.—The author looks upon solution as implying a definite chemical combination liable to alteration and to decomposition at ordinary temperatures. He has also examined the phenomena at low temperatures where cryohydrates are formed, and finds the same agreement with his adopted formula. At low temperatures he has obtained solid compounds of alcohol and water containing 17·56, 46·04, and 88·46 per cent. of alcohol respectively. The investigations have been extended to various mineral acids and salts without discovering any exception to the generalized expression given by him.

On the Constitution of Atropine, by Prof. Ladenburg.—This body is shown to belong to the class of bodies known as alkines. The reactions of tropine and its transformations show that it stands in a direct and simple relation to atropine. It has not yet been possible, however, to synthesize tropine.

The Reduction-products of the Nitro-paraffins and Alkyl Nitrites, by Prof. Dunstan and T. S. Dymond.—The authors have studied the reduction of ethyl nitrite, using ferrous hydroxide as the reducing agent. More than two-thirds of the nitrogen of the ethyl nitrite is liberated in the form of gas, either nitrous oxide or nitrogen; the remainder appears partly as ammonia and partly as ethylamine. If potassium hydroxide is mixed with the ferrous compound a considerable quantity of potassium hyponitrite is formed. It is also probable that ethyl hyponitrite, a compound that has not yet been prepared, may also be formed as an intermediate compound. The authors are further investigating the change with the object of isolating this compound and of discovering the mode of formation of the ethylamine. The reducing action of ferrous hydroxide on the nitro-paraffins was also partially investigated with interesting results.

On a Partial Separation of the Constituents of a Solution during Expansion by Rise of Temperature, by J. W. Mallet, F.R.S.—When solutions of colloid substances—some alcoholic, some aqueous—are exposed to cold, and afterwards to a gradually increasing temperature, the expansion in the liquid has been noticed to take place with a partial or entire separation of the substance dissolved in the upper layer, without any deposition of solid matter.

A New Method for Determining Micro-Organisms in Air, by Prof. Carnelley and Thos. Wilson, University College, Dundee.—This is a modification of Hesse's well-known process. It consists essentially in the substitution of a flat-bottomed conical flask for a Hesse's tube. Its chief advantages are: (1) much smaller cost of flask and fittings as compared with Hesse's tubes; (2) very much fewer breakages during sterilization; (3) great economy in jelly; (4) freedom from leakage during sterilization; (5) results not vitiated by aërial currents.

The Absorption-Spectra of Rare Earths, by Dr. G. H. Bailey.—This paper is an examination of the conditions of observation of absorption-spectra with a special view to the recent announcement of the twenty new elements of Krüss and Nilson. The author finds that the strengths of the absorption-bands do not diminish equally in all parts of the spectrum when the liquid is diluted. The presence of nitric acid also effects not only a displacement of the bands, but also an alteration in their relative intensity. It is further pointed out that a record of the strength of the bands in mixtures containing, in some cases, large quantities of samaria and erbia, and in others none, cannot be used as a means of comparison, and deductions drawn from variations of intensity are untrustworthy. Whilst acknowledging that, with due allowance for such factors, some assistance may be gained towards the course of fractionation, the author considers the announcement of new elements quite premature, and only calculated to throw further confusion into this already difficult field of work.

The Absorption-Spectra of the Haloid Salts of Didymium, by Dr. G. H. Bailey.—Bunsen has described certain variations that occur in the absorption-spectra given by crystals of the didymium salts. In this paper are detailed the variations produced in the absorption-spectra of crystalline salts of didymium when examined in polarized light. A comparison of the chloride, bromide, and iodide of didymium has also been made, from which it appears that in the bromide the bands are situated 5λ further towards the red end of the spectrum than in the chloride, whilst the displacement for the iodide is 14λ towards the violet. In the solution of the chloride (or nitrate) the bands have almost the same position as in the crystals of the iodide, whilst the addition of nitric acid causes a displacement of 12λ towards the red. It is proposed to determine how far this displacement of bands is due to the dispersion equivalent of the menstruum, and whether it gives evidence of dissociation in the liquid.

On Solution, by W. Durham.—The theory of solution adopted by the author of this paper was published by him before the Royal Society of Edinburgh in 1878. According to these views solution is due to the chemical affinity of the elements of the substance dissolved for the elements of the solvent.

Thermal Phenomena of Neutralization, and their bearing on the Nature of Solution, by W. W. J. Nicol.—A consideration of the general relationship existing between the heats of formation of various salts in dilute aqueous solutions.

Notes on some peculiar Voltaic Combinations, by C. R. A. Wright, F.R.S., and C. Thompson.—The combinations consist of a class of cells in which one or both of the "plates" is a film of gas condensed on the surface of an electrically conducting solid, which is itself not appreciably acted on. In Grove's gas battery both plates are gas films. Mercury and silver in dilute sulphuric acid opposed to aëration plates of platinum sponge form sulphates, whilst gold dissolves in potassium cyanide under the same conditions. If an acid and alkaline solution be united by means of a wick a considerable current is producible for a short time in the external circuit, but this falls off rapidly in consequence of the development of free hydrogen on the surface of the plate in the acid fluid and of oxygen on the other plate. No direct quantitative proof of this has, however, been given, and there are given in this paper a number of experiments showing that for every milligramme equivalent of silver deposited 11·2 cc. of hydrogen are evolved.

The Present Aspect of the Question of the Sources of Nitrogen in Vegetation, by Sir J. Lawes, F.R.S., and Dr. Gilbert, F.R.S.—An outline of the work of various experimenters in this line of research was given, indicating that, whereas in accordance with the original observations of Boussingault the authors had found that nitrogen was assimilated in part from the atmosphere directly, the results of Berthelot, Frank, and others derived all nitrogen from the soil. These results were criticised, and it was pointed out that the methods pursued failed in that they did not represent normal conditions of growth of vegetation.

Dispersion Equivalents and Constitutional Formulæ, by Dr. J. H. Gladstone.—The difference between the refraction equivalent, as observed for the line "A" and for the line "H" in the spectrum, is called the dispersion equivalent. The refraction equivalent of a compound is in general the sum of the refraction equivalents of the elements of which it is composed, but varies according to the constitution. The same holds for the dispersion equivalent, and it is shown that in some cases differences are observable in this respect where they are not indicated by constitutional formulæ, as, for instance, in the allyl compounds, and in naphthalene derivatives.

On Organic Vanadates, by J. A. Hall.—A series of organic vanadates are described, which can be distilled under reduced pressure without decomposition. They are obtained by the action of an alkyl bromide on silver orthovanadate.

On some New Cinnamic Acids, by Dr. Cohen and Prof. Perkin.—A description of a method of obtaining certain substituted cinnamic acids from aromatic aldehydes in cases where, from the nature of the aldehyde, Perkin's reaction cannot be employed. The aldehydo-salicylic acids are converted into ethereal salts, and these are heated with sodium alcoholate, and finally with sodium acetate and acetic anhydride.

The Antiseptic Properties of Metallic Salts, by Prof. Carnelley.—This paper consisted of a description of experiments performed by the author illustrative of further relations between the chemical composition of a substance and its antiseptic properties.

Antiseptic Properties of some Fluorine Compounds, by W. Thomson.—The author describes in this paper the antiseptic properties of sodium fluosilicate. The body is not poisonous, and possesses no smell. Its superiority over chloride of mercury for surgical purposes is claimed.

On the Composition of Water by Volume, by A. Scott.—From various preliminary experiments the author was led to conclude that the relation of oxygen to hydrogen in water was not accurately represented by the numbers 1 : 2. Subsequent experiments gave the ratio 1 : 1·997 with small variation.

On some Vapour Densities at High Temperature, by A. Scott.—A number of vapour densities of inorganic salts of certain elements have been determined by means of the well-known method of Victor Meyer, at a temperature above the melting-point of cast iron. A breaking up of the molecule is shown to occur in the case of iodine, cadmium iodide, mercuric sulphide, mercurous chloride, and mercuric chloride.

On the Estimation of the Halogens and Sulphur in Organic Compounds, by R. T. Plimpton.—The method suggested as a substitute for those already in use is that of introducing the substance into the flame of a Bunsen burner, or into a jet of hydrogen, the products of the combustion being aspirated through an absorbent liquid. In this solution the amount of the halogens or of sulphur can afterwards be estimated.

On the Derivatives and Constitution of the Pyrocresols, by W. Bott and Prof. Schwarz.—The authors describe some derivatives of these bodies, and are led to conclude that the α-pyrocresol and its isomers are anhydrides similar in constitution to diphenyl ether.

Section C—Geology.

On the Mineralogical Constitution of Calcareous Organisms, by Vaughan Cornish and Percy F. Kendall.—In Dr. Sorby's presidential address to the Geological Society in 1879 it was stated that both calcite and aragonite occur in organic structures, and that aragonite fossils are less stable than those of calcite. It appeared probable that carbonic acid has been the agent which effected the removal of the aragonite, but there are no published experimental data to show that it would remove aragonite more readily than calcite. The authors gave an account of the experimental evidence obtained as to the cause of the inferior stability of aragonite fossils as compared with those formed of calcite, with observations on the geological conditions favourable to the removal of aragonite fossils. They then described the work done in following out these observations, and in the examination of certain organisms belonging to groups not yet classified according to their mineralogical constitution.

The Matrix of the Diamond, by Prof. H. Carvill Lewis.—A microscopical study of the remarkable porphyritic peridotite which contains the diamonds in South Africa demonstrates several interesting and peculiar features which are described in detail. It is one of the most basic rocks known, and has a composition which by calculation would belong to a rock composed of equal parts of olivine and serpentine, impregnated by calcite. It is a volcanic breccia, but not an ash or tuff, the peculiar structure being apparently due to successive paroxysmal eruptions. A similar structure is known in *meteorites*, with which bodies this rock has several analogies. The microscopical examination supports the geological data in testifying to the igneous and eruptive character of the peridotite, which lies in the neck or vent of an old volcano. While belonging to the family of peridotites, this rock is quite distinct in structure and composition from any member of that group heretofore named. It is more basic than the picrite porphyrites, and is not holocrystalline like dunite or saxonite. It is clearly a new rock-type, worthy of a distinctive name. The name *Kimberlite*, from the famous locality where it was first observed, is therefore proposed. Kimberlite probably occurs in several places in Europe, certain garnetiferous serpentines belonging here. It is already known at two places in the United States: at Elliott County, Kentucky, and at Syracuse, New York; at both of which places it is eruptive and post-Carboniferous, similar in structure and composition to the Kimberley rock. At the diamond localities in other parts of the world diamonds are found either in diluvial gravels or in conglomerates of secondary origin, and the original matrix is difficult to discover. Thus, in India and Brazil the diamonds lie in a conglomerate with other pebbles, and their matrix has not been discovered. Recent observations in Brazil have proved that it is a mistake to suppose that diamonds occur in itacolumite, specimens supposed to show this association being artificially manufactured. But at other diamond localities, where the geology of the region is better known than in India or Brazil, the matrix of the diamond may be inferred with some degree of certainty. Thus, in Borneo, diamonds and platinum occur only in those rivers which drain a serpentine district, and in Timor Laut they also lie in serpentine. In New South Wales, near each locality where diamonds occur, serpentine also occurs, and is sometimes in contact with carboniferous shales. Platinum, also derived from eruptive serpentine, occurs here with the diamonds. In the Urals, diamonds have been reported from four widely separated localities, and at each of these, as shown on Murchison's map, serpentine occurs. At one of the localities the serpentine has been shown to be an altered peridotite. A diamond has been found in Bohemia in a sand containing pyropes, and these pyropes are now known to have been derived from a serpentine altered from a peridotite. In North Carolina a number of diamonds and some platinum have been found in river sands, and that State is distinguished from all others in Eastern America by its great beds of peridotite and its abundant serpentine. Finally, in Northern California, where diamonds occur plentifully and are associated with platinum, there are great outbursts of post-Carboniferous eruptive serpentine, the serpentine being more abundant than elsewhere in North America. At all the localities mentioned chromic and titanic iron-ore occur in the diamond-bearing sand, and both of these minerals are characteristic constituents of serpentine. All the facts thus far collected indicate *serpentine*, in the form of a decomposed eruptive peridotite, as the original matrix of the diamond.

On the Discovery of Carboniferous Fossils in a Conglomerate at Moughton Fell, near Settle, Yorkshire, by Robert Law and James Horsfall.—After briefly noting the various exposures of the conglomerate, its unconformability with the Silurian rocks, its nature, probable age, and the circumstances which led to the discovery of fossils in it; the authors described the following section exhibited on the south-west side of the Moughton Fell.

		Feet.
a.	Scar limestone, of light grey colour and well jointed; layers very distinct in lower parts and almost horizontal, the genus *Bellerophon* being the commonest fossil in the lowest bed of this rock	300 to 500
b.	Conglomerate.—Of a bluish-grey colour when newly fractured, and becoming reddish on exposure to the air. The fragments consist of slate, grit, flagstone, and vein-quartz, all apparently derived from Silurian rocks. Fossil shells and corals are common throughout the bed. *Bellerophon*, *Euomphalus*, *Syringopora*, and *Lithostrotion* are the prevailing genera...	1 to 12
c.	Lower Silurian slates, of great thickness, having a north-east strike, and a dip of about 65°. The dip and cleavage appear to be on the same plane in this locality.	

The nature and the origin of the stones in the conglomerate were next pointed out; also it was shown that the portion of the bed in which fossils had been found was not more than 200 yards in length, and that it was thickest in the middle, thinning out to the east and west, and at one point could be seen merging into the overlying limestone. Twenty species of fossils were collected from the conglomerate.

Places of Geological Interest on the Banks of the Saskatchewan by Prof. J. Hoyes Panton.—The writer, after referring to some of the marked geological features which characterize the three great prairie steppes of the North-west of Canada, proceeds to describe two localities more particularly, viz. the vicinity of Medicine Hat, situated on the banks of the Saskatchewan, 660 miles west of Winnipeg, and a locality near Irvine Station, 20 miles east of Medicine Hat.

The History and Cause of the Subsidences at Northwich and its Neighbourhood, in the Salt District of Cheshire, by Thos. Ward.—The frequent occurrence of subsidences in the neighbourhood of Northwich makes it desirable to learn their history and cause. Northwich overlies extensive beds of salt. These occupy about three square miles. The first, or "top" rock-salt, lies at a depth of about 50 yards from the surface, and is covered by Keuper marls, and these by the drift-sands and marls. Between the two beds of salt there are 30 feet of indurated Keuper marl. The second, or "bottom" rock-salt, is over 30 yards in thickness. The subsidences which are so destructive in the town of Northwich and the neighbourhood are entirely caused by the pumping of brine for the manufacture of white salt. It was only about 1770, or shortly afterwards, that the first sinking was noticed; since that date, subsidence has gone on very rapidly, and much destruction of property has resulted. Large lakes or "flashes," one of more than 100 acres in area, and of all depths up to 45 feet, have been and are being formed. This peculiar phenomenon of subsidence in the salt-districts is worthy of more consideration than it has hitherto received from scientific men.

The Sonora Earthquake of May 3, 1887, by Dr. T. Sterry Hunt, F.R.S., and James Douglas.—On the afternoon of May 3, 1887, at 2.12 Pacific time (= 120° W. of Greenwich), the first of a series of earthquake movements was felt in the State of Sonora and the adjacent parts of Mexico and the United States, over an area extending from El Paso in Texas on the east to the River Colorado and the Gulf of California on the west, and from the State of Sinoloa on the south as far north as Albuquerque in New Mexico; the extremes in both directions being over 500 miles. It was the fortune of the writers to be at the time at the great copper-mining camp of Bisbee in Arizona, in a narrow gorge of the Mule Pass Mountains, about 5300 feet above the sea, and near the border of Sonora. A violent tremor of the earth, including two sharp shocks, and lasting over ninety seconds, was succeeded at frequent intervals by many minor movements in the next three days, and, less frequently, at least up to May 29. In this part of Arizona solid house-walls, of *adobe* or unburned brick, were cracked or overturned, while huge rocks in the steep mountain gorges rolled down, causing much damage. Fires, perhaps kindled by these in their course, appeared immediately afterwards in various wooded regions in Sonora and Arizona, giving rise to many false rumours of volcanic eruptions. The movement here seemed from south to north. The small town of Bavispe in the Sierra Madre in Sonora was nearly destroyed, many people being wounded and forty-two killed. Opoto suffered in a similar way, and Fronteras to a less extent. The district chiefly affected by the earthquake is, however, for the most part a desert, with some cattle ranches and mining stations. Interesting studies were made by the authors in the valleys, or *mesas*, between the parallel mountain ridges in this region, both in the San Pedro and Sulphur Spring Valleys. The latter, to the east of Bisbee, and stretching north and south about 100 miles, is often 8 or 10 miles wide, and has its lower portion in Sonora. Though without a visible watercourse, water is there generally found at depths of from 10 to 40 feet in the numerous wells sunk at intervals to supply the needs of large herds of cattle. As described by many observers, the surface of this plain was visibly agitated by the first earthquake shock, so that persons were in some places thrown down by the heaving of the soil, which burst open with discharges of water, while the wells overflowed and were partially filled with sand. In the southern part of this valley, for about 7 miles south from the Mexican frontier, the authors found the results of the undulatory movement of the soil apparent in great numbers of cracks and dislocations. For distances of several hundred feet, along some lines with a generally north and south course, vertical downthrows on one side of from 1 foot to 2 feet and more were seen, the depressed portion rising either gradually or by a vertical step to the original level. Branching, and in some cases intersecting, cracks were observed. These depressions were evidently connected with outbursts of sand and water, which, along cracks, marked by depressions on both sides, sometimes covered areas of many hundred square feet with layers a foot or more in depth, marked here and there by craters 2 feet or more in diameter, through which water had risen during the outburst of these mud volcanoes. While the earthquake movements in the adjacent hills of Palæozoic strata had left no marks, the dislocations over many square miles in the valley would have sufficed to throw down buildings and to cause great loss of life in an inhabited region. There are believed to be no evidences of previous earthquake disturbances in this region since its discovery by the Spaniards centuries ago. From the published reports of commissioners named by the State of Sonora, it appears that the regions injured by the earthquake are in two nearly parallel north and south valleys in the district of Moctezuma, along the River Bavispe, a tributary of the Yaqui. In both regions are noticed the opening, in the arable lands, of numerous fissures, generally north or north-east in direction, from many of which water flowed abundantly. The river thus supplied in a time of excessive drought swelled to the volume usual in the rainy season of summer, a condition which lasted up to the time of the report of Señor Liborio Vasquez, dated at Bavispe, May 29, 1887. The fields had become green and the air moist with prevailing fogs. A report concerning the region of Opoto mentions not less than seven volcanoes in the vicinity, which were seen burning for two days, but without any flow of lava; while that for the Bavispe region declares that no volcano had there been discovered. The authors incline to the belief that, as was the case in the San José Mountains, and others examined by them along the borders of Arizona, the appearances of volcanoes near Opoto were due to forest fires.

The Disaster at Zug on July 5, 1887, by the Rev. E. Hill.—On July 5, 1887, at the town of Zug, in Switzerland, a portion of the shore gave way and sank into the lake. About three hours later another much larger adjacent area also suddenly subsided, so that in all an area considerably over two acres, with half of one of the principal streets, was submerged to a depth of about 20 feet. It can be seen that the subsoil consists of coarse gravel and sand, followed after a few feet by soft wet sand and fine mud. According to Prof. Heim, this fine mud or sludge reaches to a depth of nearly 200 feet, and the disaster is shown to be due to a flowing out into the lake of this mobile sludge from under the superincumbent weight of buildings and firmer ground. The buildings collapsed as they sank. The catastrophe must have been long impending; the exact cause which precipitated it is indeterminate, but a low level of the lake and tremors from pile-driving for new quays are suggested as contributories. On the English coast the incessant changes of pressure from tides probably render impossible such instability of equilibrium.

The Triassic Rocks of West Somerset, by W. A. E. Ussher.—This paper is the result of investigations made in the years 1878 and 1879. The constitution, extent, and general relations of the Lower, Middle, and Upper Triassic rocks of the area are briefly described, with the following general results:—The Lower Trias consists of breccia and breccio-conglomerate upon sands and brecciated sand and loam; it is faulted against Keuper basement beds, and is conformably overlapped by Middle Trias marls upon the margin of the older rocks. The Middle Trias consists of marls with sandstones in places at their base; it is faulted against the successive divisions of the Keuper on the east, and terminates northward in the angle made by converging faults at Bicknoller. The Middle Trias marls rest on the older rocks near Vellow Wood Farm, and finally disappear near Orchard Wyndham, south of Williton, under Keuper breccias. The Keuper beds consist of marls, sandstones, and a locally varying series of conglomerates, gravels, and breccia in descending sequence. The sandstones are very calcareous south of Crowcombe; they form marginal deposits in places near Dunster. The coarser beds of the Keuper develop at the expense of the sandstones in the area west of Williton. It is very probable that the Keuper basement beds of the Porlock valley may be marginal deposits formed during a progressive subsidence, and therefore may belong to a higher horizon than the Lower Keuper beds south of Williton.

The Devonian Rocks of West Somerset on the Borders of the Trias, by W. A. E. Ussher.—The composition of the Quantocks is first briefly described, and the faulted relations of Middle Devonian grits, slates, and limestones of which they consist alluded to. From the constitution of the Palæozoic districts on the east and west of the Triassic rocks of Crowcombe and Stogumber, the author considered the beds eroded in the intervening valley would amply account for the variability of the Triassic strata derived from them. From Withycombe to Porlock the faulted relations of the Middle and Lower Devonian grits are then briefly described. The author considered that the elevation of the Quantocks, the Brendon, and the Dunkery ranges was pr

Triassic, accompanied by faulting on an extensive scale; that many lesser faults were produced in post-Triassic times, and that further movements took place along the old lines of fracture. He did not believe that the Devonian highlands were ever covered by Secondary sediments, but was of opinion that the Triassic rocks never extended far beyond their present boundaries, except in old valleys from which they had subsequently been almost entirely removed by denuding agencies.

Observations on the Rounding of Pebbles by Alpine Rivers, with a Note on their Bearing upon the Origin of Bunter Conglomerate, by Prof. T. G. Bonney, F.R.S.—The author describes the result of his observations of the rounding of pebbles in various torrents and rivers in the Tyrol and Dauphiné, and of the gravels of the Piedmontese and Lombard plains. These lead to the following conclusions, among others: (*a*) that pebbles are rounded with comparative rapidity when the descent of the stream is rapid, and they are dashed down rocky slopes by a roaring torrent capable of sweeping along blocks of much greater volume; (*b*) that pebbles are rounded with comparative slowness when the descent is gentle and the average pace of the river is about adequate to push them along its bed. The rocks observed were in some cases limestone and not very hard grits; in others various crystalline rocks, such as granite, gneiss, or mica-schist. Hence, as the majority of the pebbles in the Bunter are of harder material, and are generally better rounded than those which the author observed, he concludes that it is impossible to suppose them mainly derived from any tract of land which, in Triassic times, can have existed in either Central or Eastern England, for they must have been formed by rivers no less important, with courses either longer or steeper than those of Central Europe. Thus these observations are very favourable to the view which ascribes to them a Scotch origin, where alone rocks exactly like them are known to occur.

The Terminal Moraines of the Great Glaciers of England, by Prof. H. Carvill Lewis.—The investigation here recorded is based upon the important principle that *every glacier at the time of its greatest extension is bounded and limited by a terminal moraine*. The great ice-sheet which once covered Northern England was found to be composed of a number of glaciers, each of which was bounded by its own lateral and terminal moraines. These glaciers were studied in detail, beginning with the east of England; the North Sea glacier, the Wensleydale glacier, the Stainmoor glacier, the Aire glacier, the Irish Sea glacier, and the separate Welsh glaciers were each found to be distinguished by characteristic boulders, and to be defined by well-marked moraines. The terminal moraine of the North Sea glacier, filled with Norwegian boulders, may be seen in Holderness, extending from the mouth of the Humber to Flamborough Head, and consists of a series of conical hills inclosing meres. The Irish Sea glacier, the most important glacier of England, came down from Scotland, and, being reinforced by local ice-streams, and flowing southward until it abutted against the mountains of Wales, it was divided into two tongues, one of which flowed to Wellington and Shrewsbury, while the other went south-west across Anglesey into the Irish Sea. This great glacier and its branches are all outlined by terminal moraines, described in detail. A small tongue from it, the Aire glacier, was forced eastward at Skipton, and has its own distinctive moraine. In the neighbourhood of Manchester the great moraine of this Irish Sea glacier may be followed through Bacup, Hey, Staleybridge, Stockport, and Macclesfield, being as finely developed as the moraines of Switzerland and America. South of Manchester it contains flints and shell-fragments, brought by the glacier from the sea-bottom over which it passed. At Manchester the ice was at least 1400 feet thick, being as thick as the Rhone glacier. The great terminal moraine now described of the united glaciers of England, is a very sinuous line, 550 miles in length, extending from the mouth of the Humber to the farthest extremity of Carnarvonshire, and, except where it separates the Welsh glaciers from the North Sea glacier, it everywhere marks the extreme limit of glaciation in England, and is an important feature which might well hereafter be marked on the geological map of England.

In a separate paper, read at a subsequent meeting, the author described more in detail the moraine near Manchester.

On some Important Extra-Morainic Lakes in Central England, North America, and elsewhere, during the Period of Maximum Glaciation, and on the Origin of Extra-Morainic Boulder-Clay, by Prof. H. Carvill Lewis.—The lakes so characteristic of all glaciated regions are due to several causes. Some few are due to an actual glacier scooping-out of the rock-floor, many to an irregular deposition of the drift by which former watercourses are obstructed, and still others to the terminal moraine or to the glacier itself. These latter, known as *morainic lakes*, may be divided into *inter-morainic lakes*, *moraine meres*, and *extra-morainic lakes*, according to their position—back of, in, or outside of the moraine. Extra-morainic lakes, if dammed up by the ice front, are temporary in character, disappearing with the retreat of the glacier; but, as they may be of enormous extent if the glacier is large, they may produce deposits of much geological importance. Instances of such lakes occur in Switzerland, and ancient examples occur as well in Northern Germany, Asia, North America, and Central England. They are to be expected wherever a glacier advances against or across the drainage of a country. Mr. Belt supposed that Northern Asia was covered by a lake of this character, caused by the Polar glacier obstructing the rivers flowing north. In North America, where the terminal moraine has been accurately mapped for thousands of miles, deposits of boulder-clay and erratics occur outside of the moraine, and have been supposed to be due to an older glacier in the first Glacial epoch. But the entire absence of striæ or of glacial erosion or moraines in this district prove that a glacier was not the agent of deposition. Nor are there any traces of marine life in the deposits. This extra-morainic boulder-clay is narrow in Pennsylvania, where the author had called it "the Fringe," but west of the Missouri it is 70 miles wide; and in British America, between the great moraine called the "Missouri Coteau" and the Rocky Mountains, it is 450 miles wide and over 1000 miles long. It only occurs where rivers had flowed *towards* the glacier, and is explained as the deposit of great temporary fresh-water lakes dammed up by the ice front, the erratics having been dropped by icebergs. Similar deposits occur in England outside of the terminal moraine, and have been the subject of much discussion; being held by some to be a proof of marine submergence, by others to be the ground-moraine of a glacier. The "great chalky boulder-clay" is the best known of these deposits. There are serious objections to the two theories heretofore advanced to explain this, whilst the hypothesis of extra-morainic fresh-water lakes, dammed up by the glaciers, is sustained by all observed facts. The most important of these lakes was one caused by the obstruction of the mouth of the Humber by the North Sea glacier, whose terminal moraine crosses that river at its mouth. This large lake reached up to the 400-feet contour line, and extended southward nearly to London, and westward in finger-like projections into the many valleys of the Pennine Chain. It deposited the "great chalky boulder-clay," and erratics were floated in all directions by icebergs. The conclusion that the glacial phenomena of England are due neither to a universal ice-cap nor to a marine submergence, but to a number of glaciers bordered by temporary fresh-water lakes, is in accordance with all the observations of the author in England and elsewhere.

On the Extension of the Scandinavian Ice to Eastern England in the Glacial Period, by Prof. Otto Torell.—The author described the glacial deposits of Eastern England, particularly those of Holderness and Cromer, the latter having been examined by him on several occasions during the last twenty years. Applying his experience gained during winters spent within the Arctic Circle, the author showed that the boulder-clay of Holderness and Cromer is a true ground moraine formed near the southerly limit of the Scandinavian ice-sheet. The Cromer tills were formed by an extension of the Baltic ice; Swedish boulders brought by this ice can be traced across the German plain, and are found at Cromer. The till of Holderness was formed by an extension of the Skagerack, as is proved by characteristic Norwegian boulders. The Baltic ice retreated first, and the Skagerack ice, still moving onward, ploughed into and contorted the Cromer till. The distribution of the boulder is described in detail, as also is the succession of the beds as worked out by Mr. Clement Reid, whose facts and conclusions fit well with the opinions advanced by the author.

A Comparative Study of the Till or Lower Boulder-Clay in several of the Glaciated Countries of Europe—Britain, Scandinavia, Germany, Switzerland, and the Pyrenees, by Hugh Miller.—The sections of foreign till examined by the author occur chiefly in the neighbourhood of the Trondhjem Fjord in Norway, at Berlin and Leipzig in Germany, near the Lake of Geneva in Switzerland, and in the valleys of the Pyrenees

directly south from Pau in Southern France. In these countries and in Britain the till bears an identical character. It is not more variable throughout Europe than the author has found it to be in Scotland and Northern England. On the basement-gneiss at Christiansund, in South-Western Norway, it is the same as on the basement-gneiss of Sutherlandshire; in the great limestone valley of Eaux Chauds, in the Pyrenees, it is scarcely to be distinguished from the till of the limestone valleys of Yorkshire. In all the places mentioned (more doubtfully at Berlin and Leipzig) it bears the unmistakable character of a ground-moraine accreted under the direct weight of glacier-ice; its essential character is that of a rude pavement of glaciated debris ground from the rocks over which the glaciers have passed, with its larger boulders firmly glaciated *in situ* on their upper sides in the direction of ice-movement, and with a tendency to the production of fluxion structure here and there in the matrix, due to the onward drag of the superincumbent ice. In mere indiscriminateness of composition (which is a character much emphasized by glacialists) the till is not to be distinguished from boulder-clays formed under berg- or raft-ice, such as the highest marine clays of the Norwegian coasts, which are stuck promiscuously through with boulders derived from the glaciers of the interior. The glaciation of boulders *in situ* the author finds to be a crucial distinction; he readily detected this "*striated-pavement*" character in the tills of all the districts above mentioned except Leipzig and Berlin, where the boulder clays more resemble the upper boulder-clay (Hessle clay) of the eastern seaboard of England and Scotland, and in the sections examined by him contained no large blocks.

Note on a few of the many Remarkable Boulder-stones to be found along the Eastern Margin of the Wicklow Mountains, by Prof. Edward Hull, F.R.S.—Amongst the evidences of the former existence of an extensive sheet of ice descending from the Wicklow Mountains towards the shores of the Irish Sea is the occurrence of boulder-stones, chiefly formed of granite or granitoid gneiss, derived from the mountainous range to the westward, of a size seldom equalled—probably not surpassed—amongst the British Isles. (1) The Mottha Stone is perched on the summit of Cronhane Hill, above Castle Howard, and is a conspicuous object for all directions. It consists of grey granite, and rests upon Lower Silurian slate; its weight would be about seventy tons. It lies at a level of 816 feet above the sea, and is about 10 or 12 miles from the flanks of Lugnaquilla, whence, as we may suppose, the granite block started on its journey. (2) In the valley between Castle Kevin and Moneystown, where large boulders are numerous, there lies a block of granite about 100 tons in weight. The birth-place of this boulder was probably the mountainous district about Mullaghcleevann, 2783 feet in height, lying at the head of the valley in which is situated the deep waters of Lough Dan, and it probably travelled a distance of 8 or 9 miles in an east-south-east direction. (3) The last boulder-stone is perhaps the largest in the British Islands. It stands behind a cottage by the roadside, near Roundwood Church, and is quite as large as the cottage itself, to which it forms a good protection from the storms descending from the mountains behind. This boulder consists of granitoid gneiss, resting on Lower Silurian slate and grit, and is about 240 tons in weight. The source of this block, which lies at an elevation of about 800 feet above the sea, was probably in the same locality with that of the Castle Kevin boulder, and the distance travelled was about 6 or 7 miles. The blocks above noticed, with many others of smaller size, do not belong to any of the local glaciers which once filled the valleys towards the close of the Glacial epoch, and which have left numerous well-formed moraines in nearly all the principal valleys descending from the Wicklow range. They are to be referred, in all probability, to the earlier stage of intense glaciation, in which the whole district was covered with perennial snows and ice, moving eastward into the hollow now occupied by the waters of the Irish Sea.

On New Facts relating to Eozoon Canadense, by Sir J. William Dawson, F.R.S.—For several years no new facts respecting the Canadian Eozoon have been published, though there has been some discussion on the subject abroad. In so far as the author is concerned, this has arisen from the circumstance that the late Dr. Carpenter had in preparation an exhaustive memoir, for which Canadian material was being supplied, but which was unfortunately left unfinished at his death. The material collected for this has now been placed at the disposal of Prof. T. Rupert Jones, F.R.S., and in the meantime the present note is intended merely to direct attention to some new facts recently obtained. The form of Eozoon has been definitely ascertained to be normally inverted conical or broadly turbinate, except where several specimens have become confluent, or when rounded masses have been based on some foreign body. The larger specimens are traversed by cylindrical or long conical vertical openings (pores or oscula), around which the laminæ, becoming confluent, form an imperfect wall. Some other points of detail were mentioned, and facts were referred to indicating the continuity and definitely stratified character of the beds in the Middle and Upper Laurentian of Canada. A variety of laminated rocks and minerals which had been mistaken for Eozoon were referred to. Their description in more detail will be found in forthcoming memoirs of the Peter Redpath Museum.

Elements of Primary Geology, by T. Sterry Hunt, F.R.S.—The author, after recalling his classification of original or non-clastic rocks into Indigenous, Endogenous, and Exotic masses, based on their geognostic relations, gives in a concise form his theory of the genesis of these various groups of rocks, as taught more at length in his recent volume entitled "Mineral Physiology and Physiography." The superficial portion of a cooling globe, consolidating from the centre from a condition of igneous fusion, he conceives to have been the protoplasmic mineral matter, which, as transformed by the agencies of air, water, and internal heat, presents a history of mineralogical evolution as regular, as constant, and as definite in its results as that seen in the organic kingdoms. This great transformation involves a series of processes, which include: (1) the removal from the protoplasmic mass, through permeating waters, heated from beneath, of the chief elements of the great groups of indigenous crystalline and colloidal rocks, by what he has called the crenitic process; (2) the modification of the residual portion by this lixiviation, which removes silica, alumina, and potash, and, by the intervention of saline waters, brings in additional portions of lime, magnesia, and soda; (3) the partial differentiation, by crystallization and eliquation, of portions of this more or less modified residue, giving rise to the various types of plutonic rocks. The direct and indirect results of subaërial decay through atmospheric agencies, and those of the products of organic life, are also considered. From the operation of all these processes result progresssive changes in the composition alike of plutonic and of indigenous rocks. The endogenous rocks or veinstones are, like the last, of crenitic origin, and may be granitic, quartzose, or calcareous in their characters.

The author next considers the conditions of softening and displacement of indigenous rocks which permit them to assume in many cases the relations of exotic rocks, and to become extruded after the manner of lavas, as seen in the case of trachytes and many granite-like rocks. Such masses he designates pseudo-plutonic. With these are often confounded the endogenous granitic veinstones, which were formed under similar chemical conditions to the indigenous granites. Masses alike of indigenous, endogenous, and exotic rocks may also become displaced, not through softening, but by being forced while in a rigid state, through movements in the earth's crust, among masses softer and more yielding than they.

The author also considers the fluxional structure seen in plutonic and pseudoplutonic eruptive masses, which has led some theorists to regard these as of aqueous origin, and others to maintain that the crenitic stratiform masses themselves are of plutonic origin—two opposite errors which vitiate much of our geological literature. The crenitic process, by removing (from beneath what was the original surface of deposition) the vast amount of material which forms alike the indigenous, the endogenous, and the pseudoplutonic rocks, has effected a great diminution in volume in the protoplasmic mass, besides that due in later times to extrusion of plutonic matter itself. This decrease in bulk of the underlying stratum was a potent agent in producing the universal corrugation of the earlier crenitic rocks, and the frequent discordances observed among them.

The author considers further the gradual diminution of the crenitic process seen in the later periods of Archæan time, and its feebler manifestations in Palæozoic and more recent ages down to the present. He notes moreover, that, as the result of geographical changes, erosion and partial deposition alike disturbed the succession of the later groups of crenitic rocks, none of which can claim that universality and uniformity which belong to the oldest known group, the fundamental granitoid gneiss.

The author concludes with a brief sketch of the great divisions of the indigenous crenitic rocks recognized by him, from the most ancient granitic substratum to the Taconian series, which appears to be the last of the characteristically crystalline indigenous groups, it being, so far as known, succeeded directly by the uncrystalline Cambrian, or the equally uncrystalline Keweenian, which may not, improbably, be itself included in the lower part of the Cambrian series.

Gastaldi on Italian Geology and the Crystalline Rocks, by T. Sterry Hunt, F.R.S.—The author recalled the fact that, in discussing in 1883 the geological relations of serpentines, he had maintained that, although not confined to Archæan rocks (in which they are found at various horizons), those of Italy, believed by some geologists to be in part of Tertiary age, are, so far as his studies go, wholly Archæan, in accordance with the views set forth by Gastaldi. The serpentines and other rocks of the ophiolitic group existed in their present condition in the seas in which were deposited the Eocene strata, which latter, by subsequent terrestrial movements, had been disturbed, broken, and even inverted, so as to seem to pass beneath the ophiolites. The indigenous and neptunian character of serpentines is maintained, while the plutonic hypothesis of their origin has been so far modified by its modern Italian advocates that they now suppose the serpentines to be due to submarine eruptions of a hydrous magnesian mud, which subsequently consolidated into serpentine and even into chrysolite. It is difficult to admit any other than a neptunian origin for the stratiform ophicalcites into which the massive serpentines often graduate. In a letter written in July 1878 to the author, but until recently mislaid, Gastaldi showed that the ophiolites of the Ligurian Apennines and of those of Prato were, like those elsewhere examined by him, protruding portions of the ancient *pietre verdi* zone, identical with that of the Alps, from which the Apennines cannot be distinguished either geologically or geographically. The vast series designated by him as the *pietre verdi* zone, according to Gastaldi, overlies the ancient central or primary gneiss (generally granitoid, but including quartzites and crystalline limestones with graphite, &c.), and has a thickness of many thousand metres, embracing three subdivisions. The lowest of these, sometimes called by him the *pietre verdi proper*, includes serpentines, diorites, euphotides, chloritic schists, &c.; the second is designated by him recent gneiss and granite with mica-schists and hornblendic rocks; while the third consists in great part of soft argillaceous or lustrous schists, with included quartzites, marbles, statuary, and banded dolomites and occasionally also serpentines, the presence of which led Gastaldi to include it with the two preceding subdivisions in his great *pietri verdi* zone; a name which the present writer, with Neri and others would restrict to the lower subdivision, regarded by him as the equivalent of the Huronian of North America; the underlying or central gneiss being the Laurentian; the recent gneiss and mica-schist, the Montalban or White Mountain series, and the upper subdivision, the Taconian or Lower Taconic of North America; the wholly distinct Upper Taconic being an uncrystalline series of fossiliferous Cambrian strata. The writer in this connexion recalled the work of Neri, Gerlach, and others in the western Alps, and that of Von Hauer and his associates in the Lombardo-Venetian Alps, where the same distinction of the true *pietre verdi* zone between the ancient gneiss below and the recent gneiss above had, unknown to him, been pointed out by the Austrian Geological Survey two years before the present writer in 1870 defined and named the younger gneissic series in North America. The absence of the true *pietre verdi* series in some localities, alike in the Alps and in North America, between the older and younger gneisses was noted, as well as the existence of apparent discordances between each one of the four great divisions of Archæan or pre-Cambrian crystalline rocks above named.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—Mr. A. E. Shipley, of Christ's College, has been appointed Demonstrator of Comparative Anatomy; Mr. H. F. Newall, of Trinity College, Demonstrator of Experimental Physics; and Messrs. L. R. Wilberforce and H. L. Callendar, both of Trinity College, Assistant-Demonstrators of Experimental Physics.

Dr. Peile has been elected Master of Christ's College, an election which all classes of University men, and reformers in particular, rejoice in.

A novelty is to be introduced in the education lectures next term, Dr. Francis Warner, of the London Hospital, having consented to give a course of six lectures on the physical and physiological study of children.

The next examination for Entrance Scholarships and Exhibitions at Gonville and Caius College will begin on December 10. Candidates must be under nineteen on that day.

Two scholarships of £80, and four others of £60 to £40, will be offered, with exhibitions. Candidates for Natural Science Scholarships may be examined in any of the following subjects: physics, chemistry, biology, and animal physiology, and will be expected to show proficiency in two of them, of which chemistry must be one. The tutors will forward full particulars on application.

We cannot find space for the long list of lectures in natural science now being given at Cambridge. Prof. Liveing gives a general course of principles of chemistry, and Prof. Dewar lectures on dissociation and thermal chemistry. Prof. Thomson lectures on electricity and magnetism, and on the applications of dynamical principles to physical phenomena. One of the courses of demonstrations at the Cavendish Laboratory is an advanced one on the properties of matter, and on sound. Prof. Lewis lectures on mineralogy, Prof. Stuart on mechanism. Mr. Marr lectures on principles of geology, Mr. Roberts on the palæontology of Echinoidea.

Dr. Vines lectures on the physiology of plants (advanced), Mr. Potter on the geographical distribution of plants.

Prof. Newton lectures on evolution in the animal kingdom; Mr. Gadow on the morphology of Ichthyopsida (recent and extinct); the Curator in Zoology, Mr. Cooke, on the geographical distribution of recent Mollusca.

Prof. Foster's, Dr. Lea's, and Mr. Langley's physiological courses are as usual. Prof. Macalister gives demonstrations on topographical human anatomy, as well as an elementary course.

Prof. Cayley's lectures this term are on quaternions and other non-commutative algebras; Prof. Darwin's on the orbits and perturbations of planets. Mr. Glazebrook lectures on hydrodynamics (waves and sound); Mr. Hobson on spherical and cylindrical harmonics; Mr. Larmor on electrostatics; Mr. Forsyth on modern algebra (binary forms); Dr. Besant on the ory of equations and differential and integral calculus; Mr. H. M. Taylor on higher plane curves; Mr. Pendlebury on theory of numbers; Dr. Glaisher on elliptic functions; and Mr. Stearn on elastic solids.

SCIENTIFIC SERIALS.

American Journal of Science, September.—Notes on the geology of Florida, by William H. Dall. In this paper the results are given of two excursions to Southern and Central Florida undertaken in 1885 and 1887 by instruction of the Director of the United States Geological Survey. Special attention is devoted to the process of contemporaneous rock-formation now going on along the Gulf shores of West Florida, and to the Tertiary rocks which prevail so largely throughout the Peninsula. No coral or coral-reef formation was anywhere observed, and it is evident from these further researches that the hypothesis of Agassiz regarding the geological origin of this region can no longer be maintained.—Notes on the deposition of scorodite from arsenical waters in the Yellowstone National Park, by Arnold Hague. The occurrence of this comparatively rare mineral as a deposition from thermal mineral springs is here noticed for the first time. It is found in several localities in the Yellowstone Park as an incrustation deposited from the waters of several hot springs and geysers, and is especially abundant at the Joseph's Coat Springs on Broad Creek, east of the Grand Cañon. The analysis—yielding Fe_2O_3 34·94, As_2O_5 48·79, and H_2O 16·27—shows this mineral to be true scorodite, a hydrous arsenate of iron, the layers varying from a mere coating to an eighth of an inch in thickness. Wherever observed it occurs as an amorphous deposit, and when pure, leek green in colour.—The effects of magnetization on the viscosity and the rigidity of iron and steel, by C. Barus. An attempt is made in this memoir to verify by a static method the results recently communicated by Mr. Herbert Tomlinson on the changes of viscosity and of elasticity produced by magnetizing iron. It is shown that the increment of rigidity due to magnetization increases at an accelerated rate as the soft, temporarily twisted wire becomes more nearly filamentary. A series of results is also given on the rigidity of magnetized steel temporarily strained and varying in temper from extreme hard to extreme soft. A main object of the paper is to show how the principles here established may

be utilized for the construction of electric dynamometers.—Fauna of the "Upper Taconic" of Emmons, in Washington County, New York, by Charles D. Walcott. This paper deals specially with the fauna represented by *Atops trilineatus* and *Elliptocephala asaphoides* from the black Taconic slate near Bald Mountain, Washington County, as described by Dr. Emmons in his second memoir on the "Taconic System." The paper is accompanied by a plate illustrating nineteen specimens of this fauna.—On the amount of moisture remaining in a gas after drying by phosphorus pentoxide, by Edward W. Morley. This quantity is here determined by the method applied in the case of sulphuric acid, the process consisting in drying the gas with phosphorus pentoxide and then passing it through a weighed apparatus in which the gas is first slightly moistened, then much expanded, and lastly again dried by phosphorus pentoxide.—Is there a Huron group? by R. D. Irving. In this paper the author inquires whether there can be carved off from the upper part of the great complex of rocks ordinarily known as Archæan, a *Huronian* series, entitled to rank with such groups as the Cambrian, Siberian, &c. In this first part of the memoir it is shown that the series on the north shore of Lake Huron mapped by Logan on Plate iii. of the atlas to the geology of Canada (1863) is entitled to rank as a separate group by its intrinsic characters and its structural distinction from the older Archæan and younger Cambrian and pre-Cambrian rocks of that region.

SOCIETIES AND ACADEMIES.

PARIS.

Academy of Sciences, October 3.—M. Hervé Mangon in the chair.—On some properties relative to the action of crystalline plates on light, by M. Mascart. It is shown that a system of waves on the same plane traversing a crystalline plate with parallel faces is decomposed into two systems of polarized waves with unequal retardation which at the exit are reconstituted in a system of waves in a state of vibration different from the first. From this is deduced the theorem that the action on light of any group of crystalline plates, endowed or not with rotatory power, is equivalent to that of a single plate with axis parallel to the incident axis and perpendicular to the incident rays.—On an experiment with M. D. Colladon's artificial waterspout, by M. Mascart. The action of this ingenious apparatus, as well as that of M. Weyher, seems to show that there is undoubtedly an ascending movement in the central part of all cyclonic phenomena. With regard to the recent waterspout in Lake Geneva, it is pointed out that the ascending motion stated to have been witnessed by M. Dufour and other observers, could scarcely be an optical illusion, as maintained by M. Faye. Some of the water seen to ascend was afterwards precipitated as rain, drenching some men engaged on the railway.—Remarks on M. Colladon's recent experiment, by M. Faye. In reply to the foregoing, it is pointed out that in a series of remarkable experiments conducted under like conditions, M. von Bezold, Director of the Berlin Central Meteorological Observatory, has, on the contrary, produced a descending movement in the direction of the long axis. But M. Faye rejects both classes of experiments, holding that his theory is neither refuted by the first, nor confirmed by the second, as none of the apparatus in question really succeeds in reproducing a natural waterspout.—Experimental study of human locomotion, by MM. Marey and Demeny. In continuation of their previous communications on this subject, the authors here analyze, by means of the photochronographic process, the movements of the trunk in walking and running. The accompanying diagrams show the successive figures of a runner photographed from above at intervals of one-tenth of a second.—On the non-existence of spontaneous tetanus, by M. Verneuil. The existence is denied of spontaneous or medical as opposed to traumatical or surgical tetanus. It is shown, however, that besides the latter there also exists a variety of the disorder, for which the term tetanus by absorption is proposed.—Researches on the apparently spontaneous movements of contraction and relaxation which after death are continued in the muscles so long as the *rigor* lasts, by M. Brown-Séquard. The results are described of numerous experiments carried out on rabbits, dogs, and monkeys by means of the graphic process, showing that complex muscular action continues after death throughout the whole period of *rigor mortis*; that is, until putrefaction sets in, which may at times be deferred for several weeks. The action is mostly irregular, but occasionally almost rhythmical, and the more decided movements occur not in the early stages, but towards the end, sometimes in the second, third, and even fourth week. It is made clear that they cannot be attributed to changes of temperature, variations of humidity or ozone, barometric pressure, or other atmospheric influences, nor yet to magnetism or electricity, at least to any great extent. It will be shown in a future communication that they are due to the persistence of muscular irritability; that is, to the fundamental property of the living muscular tissue surviving till arrested by putrefaction.—General results of fresh studies on several series of fatty and aromatic monamines, by M. Malbot. These studies deal with the ethylamines, the propylamines, butylamines, amylamines, caprylamines, and aromatic amines. Their whole history is cleared up, and a general interpretation is arrived at of their formation. Whether occurring in the free state or in combination, they result from a conflict of energies between the rival affinities of ammonia and the amines for the ether and its acid. With regard to their formation, the author's experiments seem to favour the ethylene theory of Berthelot rather than that of ethyl advocated by Hofmann.—A memoir on the syphon barometer was presented by M. Govi, who credits Torricelli with the first idea, and Pascal with the practical execution and first employment of this instrument, the invention of which has been successively attributed to Robert Hooke, Robert Boyle, and Borelli. He shows that the principle was known to Torricelli in 1644, when he used it to explain to Ricci the theory of the cistern barometer; also that Pascal was acquainted with it in 1653, while Hooke mentions it for the first time in 1665, Boyle in 1666, and Borelli in 1667.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

The Student's Hand-book to the Microscope: A Queken Club Man (Roper and Drowley).—Weather: Hon. Ralph Abercromby (Kegan Paul).—Our New Zealand Cousins: Jas. Inglis (Low).—Our Fancy Pigeons: George Ure (Mathew, Dundee).—The Solomon Islands and their Natives: H. B. Guppy (Sonnenschein).—The Solomon Islands, their Geology, &c.: H. B. Guppy (Sonnenschein).—A Sketch of Geological History: Prof. E. Hull (Deacon).—Factors in Life: Prof. H. G. Seeley (S.P.C.K.).—Pictorial Geography of the British Isles: M. E. Palgrave (S.P.C.K.).—Sixth Annual Report of the U.S. Geological Survey: J. W. Powell (Washington).—Journal of the Royal Statistical Society, September (Stanford).—Mind, October (Williams and Norgate).

CONTENTS.

	PAGE
The Scenery of Scotland. By Prof. A. H. Green, F.R.S.	553
Our Book Shelf:—	
"Longmans' Shilling Geography"	555
Bonnier: "Les Plantes des Champs et des Bois"	555
Sinclair and Fyfe: "The Hand-book of Jamaica for 1887–88"	555
Letters to the Editor:—	
The Natural History of the Roman Numerals.—J. Lymburn. (*Illustrated*)	555
The "Sky-Coloured Clouds.—T. W. Backhouse	556
A Light Fog.—W. G. Brown	556
Destruction of Young Fish.—Capt. David Wilson-Barker	557
On Hamilton's Numbers. By Prof. J. J. Sylvester, F.R.S.	557
Modern Views of Electricity. Part I.—II. By Dr. Oliver J. Lodge, F.R.S. (*Illustrated*)	559
New Forms of Construction of Object-Glasses intended for Stellar Photography. By Prof. Edward C. Pickering	562
William S. Symonds	562
Notes	562
Our Astronomical Column:—	
Proper Motion of Ll 26481	564
The Washburn Observatory	564
Astronomical Phenomena for the Week 1887 October 16–22	565
Meteorological Notes	565
The Problem of the Hop Plant Louse (*Phorodon humuli*, Schrank) **in Europe and America.** By Prof. C. V. Riley	566
Education in Germany. By Samuel Smith, M.P.	567
The British Association:—	
Section B.—Chemical Science	568
Section C.—Geology	571
University and Educational Intelligence	575
Scientific Serials	575
Societies and Academies	576
Books, Pamphlets, and Serials Received	576

THURSDAY, OCTOBER 27, 1887.

THE STUDY OF EMBRYOLOGY.

An Introduction to the Study of Embryology. By Prof. A. C. Haddon, M.A. (London: C. Griffin and Co., 1887.)

THE publication of this volume supplies a long-felt want. Many will remember the pleasure with which the first appearance of an English volume solely devoted to embryology was hailed. But there was a good deal of the "lively anticipation of future favours" in the gratitude with which the public received Part I. of the "Embryology" by Foster and Balfour. The first edition of this text-book contained, it will be remembered, a clearly written, well illustrated account of what was then known of the embryology of the chick. Although the details were far in advance of anything previously published on the subject in our language, yet the explanations were so clear and the style so lucid that the book was in every way suitable for the beginner. The public was informed, in the introduction, that the work was to be extended so that it would become a text-book of general embryology. Although this promise was more than fulfilled on the appearance of Balfour's classical work, the young morphologist was to a certain extent the loser by the immense benefit thus conferred upon the more advanced student. The second edition of the former book brought the account of the development of the chick up to date, and a comparatively short description of the essential features of embryological development in Mammalia was added, but the work still remains an enlarged Part I. But in the meantime the immense and ever-increasing development of invertebrate embryology, and the magnitude of the theoretical considerations raised by its advance, rendered it more necessary than ever that the beginner should be in possession of some introduction to this part of the subject; and that the student whom time and opportunity prevented from mastering Balfour's great work should, nevertheless, be permitted to gain some insight into the subject as a whole.

Both these conditions are fulfilled, and corresponding benefits will be conferred upon morphological teaching, by the publication of Prof. Haddon's text-book. In one respect, indeed, it appears to be probable that the "Embryology" of Foster and Balfour will always remain pre-eminent as a preliminary text-book for the young embryologist. The limited scope of that work renders possible a comparatively detailed description of the growth and arrangement of the embryonic membranes and appendages, so that the relative positions of these to each other, to the embryo, and to the yolk-sac, are brought before the student with a clearness and force which could not be attained in a more condensed statement. And the great difficulty with which this part of the subject is grasped by the beginner is apt to be undervalued by the advanced student and by the teacher. Again, the selection of a single type for detailed description renders possible and indeed suggests that the succeeding stages of development should be described after the manner of a diary; and while this method strongly impresses the imagination of the reader, it gives continuity as well as reality to the shifting scenes of embryonic development. It is also probable that such a method, with its constantly repeated recapitulations, and references to events which have been previously described, must afford to the memory an assistance which cannot be given by any other system. Of course the full development of this method is totally impracticable in a volume which deals with general embryology, and the subject is not at present in a condition such that it could be adequately represented by the selection of a few types for detailed description. A book which treats of the development of those mature organisms which are usually selected as types of the morphological series is much to be desired and would be extremely useful, but in the present state of embryological knowledge it is doubtful whether such a volume would represent general embryology as completely as general morphology is represented by the types themselves.

For the reasons given above, an ideal course of embryology will begin wtth Foster and Balfour, continue with Haddon, and end, as far as text-books are concerned, with Balfour. The complete mastery in the first-named work of a single easily accessible and readily investigated type, of considerable complexity and difficulty, will enable the student to grasp the shorter descriptions in Prof. Haddon's volume of all important embryological discoveries hitherto made. Finally, a rapid survey of general embryology being thus gained, the student will pass to the longer descriptions and further details of Balfour's classical work. A course of embryological teaching so complete as this, or so well suited to the needs of the student, does not exist in any other language.

It may be objected that in any such succession of text-books there must be a great deal of waste, from the description of the same developmental histories in rather different language. For the advanced student or the investigator the objection is valid, but I believe that every experienced teacher will agree in the opinion that the young student—for whom alone such a course is necessary—gains immensely by this very fact, and learns far more by reading a second text-book which puts the same facts in a slightly different way, than by reading the first text-book a second time.

It is very high praise of Prof. Haddon's volume to place it in the middle position of such a series. It is well and clearly written, while the adoption of smaller type for theoretical questions or less important details, is a great benefit to the beginner. The figures are drawn so that the primary layers and the organs derived from them can be respectively tinted in uniform colours throughout the book. While great additional clearness will be thus conferred, the student will gain much during the process of colouring. The illustrations are very numerous, and although many of them are roughly executed, and open to exception from an artistic point of view, their meaning is sufficiently clear. In the vast preponderance of the representations of sections among the illustrations, the work only follows the example of all books on the subject in all languages, but it is to be doubted whether the course of the young student is not somewhat impeded by this universal custom. There is no doubt that, as Prof. Weismann remarked to me the other day, the art of section-cutting is a weapon for morphological research

equal in importance to the discovery of the microscope itself. And embryology, far more than any other department of morphology, depends upon this art; indeed, the study may be almost said to date from the introduction of this method of inquiry. Hence there is a strong bias in favour of representing structures in section; and in original papers and advanced treatises this custom is not to be deplored, for the reader knows exactly what is meant by the figures. But even in such works I think that the reader, and the author also, would benefit by the introduction of a few additional illustrations representing the organism, organ, or structure, as a solid object. But there can hardly be two opinions on this subject in the case of an introductory text-book. The beginner cannot readily or correctly reconstruct in imagination the solid structure from a representation of a section, and he must infallibly lose considerably in time by the prevalent custom of representing in only two dimensions objects which really exist in three. Long descriptions might be curtailed, and great additional clearness conferred by the frequent illustration of solid objects, out of which a small portion is represented as cut, on one side only, so as to show the internal structure. But this necessity is not fully recognized in any embryological text-book, although some attempt is made to deal with it in this and in other works. It is to be hoped that in future editions considerable attention may be paid to this mode of illustration, which will be more than repaid by the advantage conferred upon the young student.

In conclusion, the author seems to have included everything of importance in his subject up to the date at which the book was written; so that many important discoveries or theories are described which are necessarily absent from Balfour's work. When from the necessities of space these are only briefly touched upon, the reference to the original papers is to be found in Appendix B., containing a bibliography of recently published works on embryology Hence there is reason to hope that the volume may be found useful to the student who is familiar with Balfour's work. E. B. P.

SOME MATHEMATICAL BOOKS.

The Conic Sections, with Solutions of Questions in London University and other Examination Papers. By G. Heppel, M.A. (London: Baillière, Tindall, and Cox, 1887.)

A New Mode of Geometrical Demonstration, with Examples showing its Application to Lines and Angles, Surfaces, and the Products of Three or more Straight Lines, &c. By D. Maver. (Aberdeen: A. Brown, 1887.)

Easy Lessons in the Differential Calculus: indicating from the Outset the Utility of the Processes called Differentiation and Integration. By R. A. Proctor. (London: Longmans, 1887.)

First Steps in Geometry: a Series of Hints for the Solution of Geometrical Problems, with Notes on Euclid, useful working Propositions, and many Examples. By R. A. Proctor. (London: Longmans, 1887.)

MR. HEPPEL'S little hand-book is not a complete treatise on elementary analytical geometry as usually presented to junior students, but it is a sequel to a previous small work in the same series ("Students' Aid Series"), "On the Geometry of the Straight Line and Circle." The object aimed at in the two works is to fully equip readers for the B.A. and B.Sc. examinations of the London University and similar examinations. Hence a limited portion only is discussed, viz. the equations to the conics; tangents, polars, normals, and curvature; sections of a cone, harmonic pencils, and miscellaneous theorems. Though Mr. Heppel has treated his subject concisely, he has not done his work in a perfunctory manner, for there is much originality exhibited in his mode of treatment, and he has discussed the general equation, not only for rectangular axes, but generally, in a very clear manner. If we mistake not, this clear exposition of a somewhat difficult part of the subject—difficult, that is, to junior students—is the outcome of some years' experience in tuition. In an appendix are given "hints to students" founded on this experience, and solutions to questions, illustrative of the text, which have been taken from the London University papers. There are a few errors in the printing, but they are not of a character to seriously inconvenience the student. We could have wished for a larger page, for then more justice would have been done to the author in the presentment of some of the formulæ. A student who carefully reads the text and transfers the formulæ for the separate conics to larger pages, ought to require no other text-book than this small one for the examinations named above.

Mr. Maver claims for his method the recommendation that it is new. One can hardly expect such to be the case, but we certainly do not remember to have come across it as here applied. The nearest approximation we can lay hands upon is the method of parallel transformation, given in Petersen's "Methods and Theories for the Solution of Problems of Geometrical Constructions" (pp. 46-47); but Mr. Maver has worked out the idea at considerable length and in an elegant manner. An illustration from the principles and from the body of the work will sufficiently explain the scope of the method. "Let AB and CD be two parallel straight lines, and AC, GK, and GM any lines whatever drawn from the line AB to the line CD. If these lines, AC, GK, GM, move in the direction of the parallels AB, CD, so that CF = KL = MD, then we have the space CE = KB = GD (Euc. i. 36). Let the space CE be represented by sAC, which may be read space generated by AC, and so on; then sAC = sGK = sGM." Assuming results which readily flow from the above and which are given in the "Principles," let us now take Example II. The sides AB, BC of a triangle are bisected by CE, AF, cutting in G, to prove CG = 2EG, AG = 2FG; and if BG produced cuts AC in D, then AD = CD. "Let AF be the direction of motion, then sCG = sCF = sBF = sBA = 2sEA = 2sEG; ∴ since sCG = 2sEG, CG = 2EG. In the same way AG = 2FG. Again, if DB be the direction of the motion we have sAD = sAB = 2sEB = 2sEG = sCG = sCD; ∴ since sAD = sCD, AD = CD." There are five chapters, viz. one, containing the principles; two, applications to lines and angles; three, to squares and rectangles; four, the products of three or more straight lines; five, to lines that are in the same straight line. In an appendix the author "proves Euc. i. 36

by the doctrine of motion without any reference to the propositions of Euclid which precede it." In all, the author applies his method to forty-one examples, the treatment of which will present no difficulty to the student. If the method is not thoroughly new, it is true, and offers an interesting field for investigation.

Many of our readers have no doubt made an acquaintance with Mr. Proctor's pages already, as they originally appeared in *Knowledge*. In his preface the author states as his experience:—"I could find no interest in the differential calculus, till, after wading through 200 pages of matter having no apparent use (and for the most part really useless), I found the calculus available for the ready solution of problems in maxima and minima. This little work has been planned with direct reference to my own experience at school and college." In 114 small pages Mr. Proctor very luminously, we think, unfolds the *raison d'être* of the calculus, and by easy yet sure stages carries his reader over a good deal of ground, sufficient for a large class of students. The proofs are clear, and apparently quite level to the comprehension of a student who has a solid, but not necessarily extended, knowledge of elementary mathematics. It is a good introduction to a great subject, and is calculated to entice readers to go on further. Several well-chosen problems are fully worked out, and there are a few others for the student to tackle himself. There are not many errata, but there are slips on pp. 52, 73, 95, 109, and quite a crop on p. 110.

"First Steps in Geometry" is a reprint; it certainly well deserves an extended circulation, especially amongst intelligent mechanics and others who cannot command the assistance of teachers, for they will appreciate the way in which the writer goes about his task. His "method of showing why such and such paths should be tried, even though some may have to be given up, in searching for the solution of problems," is likely to be very suggestive to the thoughtful student. There is a vast quantity of good work in the little book, and the way in which the second book of Euclid is treated ought to find a place in our school text-books.

OUR BOOK SHELF.

The Photographer's Indispensable Hand-book. Compiled by W. D. Welford. Edited by H. Sturmey. (London: Iliffe and Son, 1887.)

THIS book is practically a complete cyclopædia on the subject of photographic apparatus, materials, and processes, &c. Those who intend purchasing articles pertaining to photography cannot do better than look through these pages, where they will find a great amount of useful knowledge and information massed together in one volume.

It would be impossible to describe the various kinds of cameras and processes, &c., which are dealt with here, but we need only add that they are profusely illustrated and well classified.

One of the latest novelties in the way of secret cameras is shown in "the watch," which, when closed, is exactly like an ordinary watch. It is opened by a spring, when a series of about half-a-dozen tubes shoot into position.

A great assortment of the different make and kinds of drop-shutters which at the present time are so largely used for instantaneous work is added.

Ueber Gemüthsbewegungen. Von Dr. G. Lange. Autorisirte Uebersetzung von Dr. H. Kurella. (Leipzig: Theodor Thomas, 1887.)

THE original essay of which this is a German translation is in Danish, and was published in 1885. The author is a Professor of Pathological Anatomy in Copenhagen, and is well known both as a practical physician and as a man of science. He does not pretend to deal fully with the complicated and difficult questions connected with the expression of the emotions. He examines, however, with much care the physical accompaniments of sorrow, joy, terror, and anger; and he offers important suggestions as to the point of view from which the entire subject can be most successfully studied. That emotions are not, in any sense which can be recognized by science, the causes of the physical phenomena associated with them is a proposition on which he lays great stress; and in support of his opinion he presents a number of arguments which deserve the attention of all who are interested in psycho-physiological studies. The German translation is very clear, and will no doubt find readers in England as well as in Germany.

Three Lectures on the Forms of Nasal Obstruction in relation to Throat and Ear Disease. By Greville Macdonald, M.D. (London: A. P. Watt, 1887.)

THESE lectures were originally delivered at the Throat Hospital, Golden Square. They do not constitute a text-book, but the author has embodied in them the results of much inquiry as to various forms of nasal obstruction. The diseases of which he treats are all of common occurrence, yet some of them have hitherto been but inadequately described, and Dr. Macdonald holds that their pathology is often totally misunderstood. His exposition, therefore, should be of service both to the student and to the general practitioner.

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 Scenery of Scotland."

THE review by Mr. A. H. Green, of Geikie's "Scenery of Scotland," published in your paper of October 13 (p. 553), does not, I think, show any accurate appreciation of the disputable points in that work. The fundamental proposition ascribed to Hutton is "that the surface features of the land are, in the main, due to the carving and sculpturing action of denudation." Mr. Green does not seem to be aware that the truth of this doctrine may entirely depend on the definition of the words "surface features," and of the subsequent words "in the main."

As there is probably no actual "surface" existing in the world which has not been weathered more or less (except the very freshest lavas), the doctrine of Hutton, when so stated, is not only true, but it is a truism. Indeed the words "in the main" might be omitted; because it would be substantially true that all "surface features" in this literal sense are due entirely to denudation.

But if the words "surface features" be understood not literally, as confined to any mere visible surface, but as applying to all forms and shapes underlying mere surfaces, then the doctrine is open to great debate, and the truth of it turns entirely on the breadth of interpretation given to the words "in the main."

Living as I do in the Highlands, I maintain that the forms of

our mountains have been largely determined by their geological structure, and by faults, contortions, and subsidences in the strata of which they are composed.

I cannot argue this question here. Suffice it to say that the "Great Gutter Theory," as I venture to call it, does not, in my opinion, explain our hills or our glens. There has been, no doubt, enormous denudation. But "in the main" the forms express structure, and the effects of subterranean force.

Mr. Green refers to the "graphic illustrations" of Mr. Geikie's book. But unfortunately those illustrations are sometimes very incorrect. For example, the general view given of the south-western termination of the Highland ranges, as seen from above Gourock on the Clyde, is a view as defective and incorrect as it is possible for a geological landscape to be. I know that range of hills well, and have seen it since my childhood in every variety of light and shadow. I have also drawn it frequently, and know almost every line of it by heart. It presents a section across a great anticlinal, as was first pointed out to me by Murchison; and it is full of surface markings which reveal its structure. Not one line of these is given in Mr. Geikie's drawing. If he had been sketching a set of mole-hills he could not have made them more featureless—more utterly devoid of their distinctive forms.

Let us have facts before theories. Let us have our hills so drawn as to express the lines of structure as they are seen in Nature, and in their relation to outline. But very often the eye sees nothing except what the brain behind it has preconceived; and a geologist who draws a mountain with a theory of guttering in his head, is pretty sure to make a mess of it.

There is really nothing in the argument about an average level along the tops, as any sure indication of an original "table-land," with all its hollows due to guttering. All sedimentary materials having an average composition, when subjected to strains, pressures, or fractures, would, and must, exhibit average resulting forms. This general fact is equally consistent with more than one explanation.

I believe Mr. Geikie has modified his former views as to the action of ice. A closer inspection of the Highlands will, I am convinced, modify greatly in other ways his teaching as to the small share which structure, and subterranean force, have had in determining the physical geography of the country.

October 15. ARGYLL.

IN your last issue Prof. A. H. Green, reviewing Dr. A. Geikie's "The Scenery of Scotland viewed in Connexion with its Physical Geology," described the alleged resemblance between the Durness fossils and certain North American types as "an announcement of the greatest interest." The fact is certainly of the "greatest interest," but the "announcement" was made nearly thirty years ago by the late J. W. Salter in the *Quarterly Journal* of the Geological Society, 1858, p. 381. Mr. Salter refers to the fauna as "this truly North American assemblage," and compares the species one by one with Prof. Hall's types.

CH. CALLAWAY.

Wellington, Shropshire, October 16.

[WE have referred these letters to Mr. Green, who has sent us the following reply.—ED.]

IT is well known that the Duke of Argyll has long been a strenuous and consistent opponent of the views as to the origin of the surface features of the earth which are accepted by the majority of geologists. Indeed, if I had been disposed to be personal, I do not think that I could have quoted a more pertinent illustration than his Grace of a fact in the history of opinion to which I drew attention in the opening part of my review of the "Scenery of Scotland." He hears not Moses and the prophets, and I fear he will not be persuaded by the pleadings of one of their humbler followers; but if he will let me have my small say, I will first point out that his objection to the expression "surface features" seems to me to savour a little of quibbling. It is a general rule of criticism to interpret any ambiguous words by the context. The whole tenor of my article shows that I did not use the words in the first of the two meanings which the Duke says they may bear. Again, I am quite prepared to admit that geological structure has had a large share in determining the form of the ground; and I cannot find that either Dr. Geikie, or any other upholder of the Gutter Theory (I thank thee, Duke, for teaching me that word: no happier designation could be found), denies that subterranean force has played an important part in determining the physical geology of a country. Rather the contrary, for hear Dr. Geikie himself. He avows himself wishful that his reader should "recognize that a belief in the paramount efficacy of superficial denudation in the origin of the features of the land is compatible with the fullest admission of the existence and potency of subterranean disturbance. Inability to make this recognition," he says, "has led to absurd misconceptions and misrepresentations of the views of those who hold that the topography of the land is essentially the result of a process of sculpture" ("Scenery of Scotland," pp. 95, 96).

I will leave Dr. Geikie to take care of himself and defend the drawing the accuracy of which is impugned by his critic. I do not know the special landscape of Fig. 19, but I have enjoyed a few panoramic views of Highland scenery, and I can honestly say thus much: I have everywhere recognized those surface *markings* (may I again congratulate his Grace on the happiness of this phrase?) which indicate the geological structure of the ground beneath, but I have in every case been still more struck by that general flat-toppedness on which special stress is laid by Dr. Geikie. The comparatively slight prominence given to these surface markings in Fig. (1) will be easily understood if we bear in mind the one point which that cut was intended to illustrate.

I may add that I am extremely sorry if any words of mine seem to imply that I grudge my old friend Salter the credit due to him with regard to the Durness fossils. The expression I have used could be made to bear this meaning, and I am much obliged to Dr. Callaway for giving me an opportunity of disavowing any such intention. A. H. GREEN.

Leeds, October 20.

A Hydroid Parasitic on a Fish.

DURING my studies the past summer at the Newport Marine Laboratory I captured a single specimen of an osseous fish, *Seriola zonata*, Cuv., which exhibits a most interesting example of parasitism or possibly commensalism. Upon the outer wall of its body an extraordinary hydroid was found to have attached itself. As this mode of life is unique for a hydroid, it is thought that a mention of it, and a statement of the peculiar modifications which the hydroid has suffered, may be not without interest to others besides special students of the jelly-fishes. The hydroid is new to science, and on that account the name Hydrichthys is suggested to designate it. The hydroid will later be described and figured under the name *Hydrichthys mirus*, gen. et sp. nov.

The colony of Hydrichthys is found on the side of the body and near to the anal fin of the fish, Seriola. It forms a reddish cluster or patch of bodies, and was at first mistaken for a fungoid growth. When it was examined by means of a microscope its animal nature was easily seen and its hydroid affinities clearly made out. The fish was kept alive in an aquarium and medusæ raised from the attached hydroid. The hydroid colony is composed of two sets of individuals. These two kinds of individuals arise from a flat plate formed of branching tubes, by which the colony is attached to the body of the fish. The two kinds of individuals noticed in the cluster are the sexual bodies (gonosomes), and the "filiform bodies" (structures of unknown function).

The sexual bodies have the form of grape-like clusters of buds mounted on small contractile peduncles, which branch from a central axis or stalk. The filiform bodies are simple, elongated, flask-shaped structures, destitute of appendages, with a central cavity and terminal orifice. Neither of these two kinds of individuals have tentacles around or near a *mouth opening*, nor any structures which can be compared with these bodies, which are almost universal among fixed hydroids.

The first kind of individuals are the gonosomes or sexual bodies. They arise from the flat basal plate of branching tubes, by which the union of the colony with the outer wall of the fish is effected. Each hydroid gonosome consists of a main stem with lateral branches. At the end of each lateral branch there is a crowded cluster of small buds, which are immature jelly-fishes in all stages of growth. Each gonosome resembles a bunch of reddish and orange-coloured grapes.

The filiform bodies are simpler in structure than the sexual clusters or gonosomes. They are destitute of tentacles and are flask-like, with a cavity and terminal orifice. They are very sensitive, and move about with freedom, never, however, being

detached. The fish, Seriola, was kept alive until the larger buds of the grape-like gonosomes separated from the hydroids. These buds are medusæ, different from any which I have ever seen, but with close affinities to common and well-known genera. A large glass aquarium containing several gallons of water was found to be swarming with these medusæ two days after the capture of the Seriola.

Each fully-grown medusa closely resembles the genus Sarsia. It has an oval bell, four broad unbranched radial tubes, and four long simple tentacles. There are no octocysts on the margin of the bell.

If the strange form of the hydroid was not known to me, it would have been very easy to call this medusa a near relative of Sarsia. The medusa belongs to a group, called by Agassiz the Tubularians, but its hydroid is different from that of any other member of the group.
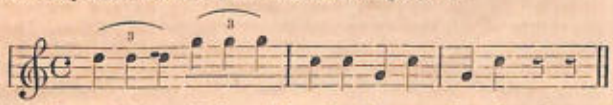
One other parasitic hydroid may be thought to be related to Hydrichthys. I refer to the Polypodium, described from the ova of the sturgeon. A description of Hydrichthys with figures of the fish (Seriola) to which it is attached, and of the hydroid with its medusa, will soon be published by me. As a discussion of its relation to other hydroids has little interest except to a specialist in the study of medusæ, a comparison of Hydrichthys with Polypodium and other genera is reserved until my complete diagnosis of the genus and species. J. WALTER FEWKES.

Cambridge, Mass., U.S.A.
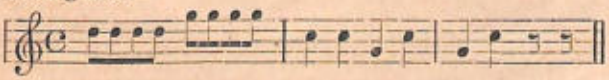
Music in Nature.

IN NATURE for August 11 (p. 343) there is an interesting article on music in Nature; the writer evidently being inclined to deny that true musical notes, and especially several notes in succession having a musical relation to one another, can be found in bird songs. However this may be in the Old World, we have in the New at least one example of a bird which not only sings, or rather whistles, pure and well-sustained musical notes, but has a succession of notes with such intervals as to form a simple melody. I refer to the scarlet tanager.

While we were at The Thousand Islands early in the summer of 1886, one of these brilliant fellows carried on a courtship among the trees close to our cottage, repeating incessantly during the first two days that we heard him the following strain,



in a clear, bright whistle. After the first two days he changed his song thus:—



and during the three weeks that we heard him he made no other variation, except that he occasionally repeated the last two notes a third time, thus filling out the bar. The notes were taken down by a trained musician, and if whistled give the tanager's song exactly.

It may be mentioned that, though perhaps the most brilliant in plumage of our Canadian birds, the male tanager referred to made no attempt at concealment, but swept like a living flame from tree to tree close to the cottage, and when singing preferred to sit on the topmost bough of a pine near by.

A. P. COLEMAN.

Faraday Hall, Victoria University, Coburg, Ontario, October 8.

Swifts.

THE following facts relating to the habits of the swifts were observed by paying close attention to these remarkable birds during the past summer. For more than a month, *i.e.* from June 1 to July 12, we watched them here. On the fine evenings about forty of them (the males I believe), ascended high into the air at about 9 o'clock, and after wheeling about for a minute or two, screaming loudly, fled straight away, sometimes in one direction, sometimes in another. White, in the "Natural History of Selborne," notices that: "Just before they retire whole groups of them assemble high in the air, and squeak and shoot about with wonderful rapidity." But the most wonderful part of the proceeding is that they do not come down again that night. At all events I can show that they do not come down again before 10.30, at which time I do not think they would be able to find their nests under the eaves of the church. Between the dates above-mentioned there were only six days during which I did not see or hear the swifts ascend and fly off. Three of these days were rainy, and the swifts stayed at home, and on three other days I was not able to watch them. The churchyard adjoins the garden of this house, and numbers of swifts build in the church, which is but a few feet from where we sit out and walk about in the summer evenings.

After seeing the high-flying swifts safely off to the south-west at 9.10 one night, I sat on a tombstone under the north eaves where most of them build, until 10.30. Two swifts hawking low for flies entered their nests after 9.10, but one of them was flying low while the high-flyers were in sight, and the other came out of its nest after they had gone, and both had retired before 9.20. On the other side of the church my father (the vicar) and my brother, who both took a keen interest in the doings of the swifts, were keeping watch alternately, and only two low-flyers were out there after the others had gone. The high-flyers did not return. On several other nights we watched until 11 o'clock, though not quite continuously, but quite closely enough to make certain that none returned. I think it most probable that owing to the darkness they do not return until the break of day, and further, that they *remain on the wing all night*. This last feat, though sufficiently startling, will, I am convinced, not be deemed impossible by those who have had good opportunities (and made use of them) for studying the ways of swifts and their wonderful powers of flight. As far as my observation goes, the swift settles nowhere except at its own nesting-place.

I shall be very glad of any information tending to throw light upon the question, and I shall be very pleased to give any of your correspondents any further information within my knowledge concerning this curious habit of the swifts, and the proofs thereof, to set out which in this letter would take up too much of your valuable space.

White also says (p. 180, original edition) he has never seen the swift carrying materials to its nest, and suggests that it usurps that of the sparrow. This does not accord with my own observation here. I have repeatedly seen swifts taking bents of grass in their beaks to their nests, and I have again and again scattered feathers on the wind from the sound-holes in the steeple, and from the steps of the cross in the churchyard, and seen them eagerly seized within a few feet of my head by numerous swifts. Their nests are neat, small, and shallow, and very firm, the materials being glued together by the viscous saliva of the builders. AUBREY EDWARDS.

The Vicarage, Orleton R.S.O., Herefordshire, October 13.

Hughes's Induction Balance.

HAVING just made a Hughes's induction balance, I have, in the course of some experiments with it, observed what was new to me, for I have not seen it mentioned in any account of the balance. I take the liberty, therefore, of asking through your columns whether the explanation resolves itself into the difference between paramagnetic and diamagnetic substances. The apertures of my bobbins are 1½ inch in diameter; my primary current is from three Daniell's, and the break is a bent steel spring whose free point just grazes the surface of a mercury cup, so that the merest touch with a finger causes a series of regular breaks. Now, if I place an iron or steel disk, or ring, such as a key-ring, inside the aperture, the telephone sounds loudly if the plane of the disk or ring is at *right angles* to the plane of the coils; but very very faintly if it is *parallel* to the plane of the coils. On the other hand, if a disk, or ring, or coil of wire, of any of the diamagnetic metals—copper, brass, zinc, silver, gold, aluminium, lead—be used, the telephone sounds loudly if the plane of the disk or ring be *parallel* to the plane of the coils; but very faintly, if at all, when it is perpendicular to the plane of the coils. Further, if a short *bar* of soft iron, or of nickel, be inserted so that the length of the bar is parallel to the plane of the coils, almost no sound is heard; but if it be turned through a right angle so as to be perpendicular to the plane of the coils, the sound is a maximum. Have we in this simple instrument the ready means of distinguishing paramagnetic from diamagnetic substances? J. COOK.

Central College, Bangalore, S. India, September 26.

PROF. KIRCHHOFF.

GEHEIMRATH GUSTAV ROBERT KIRCHHOFF was born at Königsberg on the 12th of March, 1824. He commenced his professorial career at Berlin University as Privat Docent; became Extra-ordinary Professor in Breslau from 1850 to 1854, thereafter till 1874 Professor of Physics in Heidelberg, whence he was finally transferred (in a somewhat similar capacity) to Berlin. His health was seriously and permanently affected by an accident which befell him in Heidelberg many years ago, and he had been unable to lecture for some time before his death.

It is not easy, in a brief notice, to give an adequate idea of Kirchhoff's numerous and important contributions to physical science. Fortunately all his writings are easily accessible. Five years ago his collected papers (*Gesammelte Abhandlungen* von G. Kirchhoff, Leipzig, 1882) were published in a single volume. His lectures on Dynamics (*Vorlesungen über Mathematische Physik*, Leipzig, 1876) have reached at least a third edition; and his greatest work (*Untersuchungen über das Sonnenspectrum*, Berlin, 1862) was, almost immediately after its appearance, republished in an English translation (London, Macmillan). To these he has added, so far as we can discover, only three or four more recent papers; among which are, however, the following, published in the *Berlin Abhandlungen*:—

Über die Formänderung die ein fester elastischer Körper erfährt, wenn er magnetisch oder diëlectrisch polarisirt wird. (1884.)

A subsequent paper gives applications of the results (1884).

Additions to his paper (presently to be mentioned) on the Distribution of Electricity on two Influencing Spheres. (1885.)

While there are nowadays hundreds of men thoroughly qualified to work out, to its details, a problem already couched in symbols, there are but few who have the gift of putting an entirely new physical question into such a form. The names of Stokes, Thomson, and Clerk-Maxwell will at once occur to British readers as instances of men possessing such power in a marked degree. Kirchhoff had in this respect no superior in Germany, except his life-long friend and colleague v. Helmholtz.

His first published paper, *On electric conduction in a thin plate, and specially in a circular one* (Pogg. Ann. 1845), gives an instance. The extremely elegant results he obtained are now well known, and have of course (once the start was given, or the key-note struck) been widely extended from the point of view of the pure mathematicians. The simpler results of this investigation, it must be mentioned, were fully verified by the author's experimental tracing of the equipotential lines, and by his measurements of their differences of potential. A remark appended to this paper contains two simple but important theorems which enable us to solve, by a perfectly definite process, any problem concerning the distribution of currents in a network of wires. This application forms the subject of a paper of date 1847.

Kirchhoff published subsequently several very valuable papers on electrical questions, among which may be noted those on conduction in curved sheets, on Ohm's Law, on the distribution of electricity on two influencing spheres, on the discharge of the Leyden Jar, on the motion of electricity in submarine cables, &c. Among these is a short, but important, paper on the *Determination of the constant on which depends the Intensity of induced currents* (Pogg. 1849). This involves the absolute measurement of electric resistance in a definite wire. Kirchhoff was also the inventor of a valuable addition to the Wheatstone Bridge. To the above class of papers may be added two elaborate memoirs on Induced Magnetism (*Crelle*, 1853; *Pogg. Ergänzungsband*, 1870).

Another series of valuable investigations deals with the equilibrium and motion of elastic solids, especially in the form of plates, and of rods. The British reader will find part of the substance of these papers reproduced in Thomson and Tait's *Natural Philosophy*. There are among them careful experimental determinations of the value of Poisson's Ratio (that of the lateral contraction to the axial extension of a rod under traction) for different substances. These results fully bear out the conclusions of Stokes, who was the first to point out the fallacy involved in the statement that the ratio in question is *necessarily* 1/4.

Kirchhoff's *Lectures on Dynamics* are pretty well known in this country, so that we need not describe them in detail. Like the majority of his separate papers they are somewhat tough reading, but the labour of following them is certainly recompensed. They form rather a collection of short treatises on special branches of the subject, than a systematic digest of it. One of the most noteworthy features of the earlier chapters is the mode in which dynamical principles (*e.g.* the *Laws of Motion*) are introduced. While recognizing the great simplification in processes and in verbal expression which is made possible by the use of the term Force, Kirchhoff altogether objects to the introduction of the notion of *Cause*, as a step leading only to confusion and obscurity in many fundamental questions. In fact he roundly asserts that the introduction of systems of Forces renders it impossible to give a complete definition of Force. And this, he says, depends on the result of experience that in natural motions the separate forces are always more easily specified than is their resultant. He prefers to speak of the motions which are observed to take place, and by the help of these (with the fundamental conceptions of Time, Space, and Matter) to form the general dynamical equations. Once these are obtained, their application may be much facilitated by the introduction of the *Name* Force; and we may thus express in simple terms what it would otherwise be difficult to formulate in words. So long as the motion of a single particle of matter only is concerned we can, from proper data, investigate its velocity and its acceleration, as directed quantities of definite magnitude. Thus we proceed from Kepler's Laws to find the acceleration of a planet's motion. This is discovered to be directed towards the sun, and to be in magnitude inversely as the square of the distance. We may call it by the name Force if we please, but we are not to imagine it as an active agent. Something quite analogous appears in the equations of motion when we introduce the idea of Constraint. The mode in which the idea of Mass is introduced by Kirchhoff is peculiar. It is really equivalent to a proof (ultimately based on experiments) of Newton's *Third Law*. Once, however, it is introduced, the same species of reasoning (which differs but slightly from what we should call Kinematical) leads to the establishment of D'Alembert's and Hamilton's *Principles*, with the definition of the Potential Function, the establishment of Lagrange's Generalized Equations, and the proof of Conservation of Energy, &c. The observational and experimental warrant for this mode of treatment is, according to Kirchhoff, the fact that the components of acceleration are in general found to be functions of *position*. [Kirchhoff's view of Force has some resemblance to, but is not identical with either of, the views previously published by Peirce and by the writer.] This is the chief *peculiarity* of the book, and very different opinions may naturally be held as to its value, especially as regards the strange admixture of Kinematics and Dynamics.

Of the rest, however, all who have read it must speak in the highest terms. A great deal of very valuable and original matter, sometimes dealing with extremely recondite subjects, is to be found in almost every chapter. Among these we may specially mention the investigation

of surface conditions in the distortion of an elastic solid, with the treatment of capillarity, of vortex-motion, and of discontinuous fluid motion (*Flüssigkeitsstrahlen*).

Besides these definite classes of papers, there is a number of noteworthy memoirs of a more miscellaneous character :—on important propositions in the Thermodynamics of solution and vaporization, on crystalline reflection and refraction, on the influence of heat conduction in a special case of propagation of sound, on the optical constants of Aragonite, and on the Thermal Conductivity of Iron.

Finally we have the series of papers on Radiation, partly mathematical partly experimental, which, in 1859 and 1860, produced such a profound impression in the world of science, and which culminated in the great work on the solar spectrum whose title is given above. The history of Spectrum Analysis has, from that date, been one of unbroken progress. Light from the most distant of visible bodies has been ascertained to convey a species of telegraphic message which, when we have learned to interpret it, gives us information alike of a chemical and of a purely physical character. We can analyze the atmosphere of a star, comet, or nebula, and tell (approximately at least) the temperature and pressure of the glowing gas. But, at the present time, the fact that such information is attainable is matter of common knowledge.

This is not an occasion on which we can speak of questions of priority, even though we might be specially attracted to them by finding v. Helmholtz and Sir W. Thomson publicly taking (in full knowledge of *all* the facts) almost absolutely antagonistic views. However these points may ultimately be settled, it is certain that Kirchhoff was (in 1859) entirely unaware of what Stokes and Balfour Stewart had previously done, and that he, with the powerful assistance of Bunsen, MADE what is now called Spectrum Analysis : Kirchhoff, by his elaborate comparison of the solar spectrum with the spectra of various elements, and by his artificial production of a new line whose *relative* darkness or brightness he could vary at pleasure ; Bunsen by his success in discovering by the aid of the prism two new metallic elements.

P. G. TAIT.

ON THE SIGNIFICATION OF THE POLAR GLOBULES.[1]

IT has long been known that the egg of some animals, after becoming mature and before undergoing its embryonic development, throws out certain bodies of globular form, which take no part in the embryonic development, but perish sooner or later. These polar globules have been found on the eggs of nearly all classes of animals, and it has been proved that they are real cells, composed of nucleus and cell-body.

Several theoretical opinions have been expressed in regard to their signification. Some naturalists believe them to be only a kind of excretion of the egg ; others even think them to be of no functional importance, and perceive in them only a remnant of some ancestral process, a recapitulation of some ancient part of the phylogenetic development.

Now it is true that, in many animals, structures occur without any physiological value, but it is also known that such structures—as, for instance, the hind-legs of whales—disappear more and more in the lapse of phylogenetic development. Furthermore, such rudimentary organs never disappear in all species and genera of a large group simultaneously, but in one genus or species they persist longer than in another. Thus, some whales possess certain of the bones of their hind-legs lying between the muscles of the trunk, whilst others have preserved only one bone of the pelvis. Now the polar globules might have been regarded as insignificant and rudimentary as long as they were only known in a few groups of the animal kingdom. But as their existence is now proved in nearly all classes of animals, and as they appear in all of them in the same manner, we are compelled to assume that they possess at least some physiological significance.

Mr. Sedgwick Minot and your illustrious Balfour made a great step forward in attempting—each independently of the other—to attribute a high importance to the expulsion of the polar globules. As you know, they suggested that the egg-cell was originally hermaphrodite, and that the polar globules were the male portion, which had to be thrown off. They based their idea upon the generally accepted view, according to which fecundation is the union of a specific male with a specific female substance. If this is true, then the fecundated ovum contains both these substances in equal quantities ; and the observations upon the segmentation of the egg lead further to the conclusion of E. Van Beneden, that all cells of the body contain these two substances, and that they are all hermaphrodite. The throwing out of polar globules was, according to these views, the means of preventing parthenogenesis, which must have occurred if the male substance had remained in the egg. This was Balfour's opinion, and he formulated the same with all precaution, putting it forward as a supposition, which might prove true or not. He himself even pointed out the way by which a decision could be obtained, in his statement, that, if his theory was true, polar globules would not be found in parthenogenetic eggs. Certainly, if polar globules represent the male substance, they cannot be thrown out in an egg that is not destined to be fertilized, and which therefore would not receive the male substance from another cell

Now, I have tried to decide this question by observing whether parthenogenetic eggs throw out polar globules or not, and I discovered several years ago that polar globules certainly exist in parthenogenetic eggs. I have found them in the summer eggs of Daphnidæ, and later, assisted by my pupil Mr. Ishikava, of Tokio, I have also found them in the parthenogenetic eggs of Cypridæ and of Rotatoria.

Now it is impossible that these polar globules can contain the male part of the egg, and the question arises, What other significance can be attributed to them ?

When I ascertained the facts which I have just described, I was not at the time aware of another fact that I am about to lay before you, and which seems to me to possess an important bearing upon the meaning of polar globules, and of sexual propagation in general. This fact is a very simple one : Parthenogenetic eggs throw out only *one* polar body, whilst sexual eggs throw out two of them.

The importance of this fact lies in the significance of the substance that is thrown out in the polar globules or polar cells. You know well that it is a true cell-division which leads to the formation of polar globules, and that the first polar cell takes away from the egg-cell one-half of the nuclear substance. You are also aware that the second polar cell again takes away half of the nuclear substance remaining in the egg. Hence in sexual eggs three-quarters of the nuclear substance originally contained in the egg-cell are taken away by the two polar cells. In parthenogenetic eggs only one polar cell is formed, and consequently only one-half of the original mass of nuclear substance is removed from the egg-cell.

Now you know that nuclear substance is a very important thing. The experiences and reflexions of the last ten years have led to the general conviction that nuclear substance is the part that controls the whole cell, and that the entire structure as well as the functions of the cell depend upon its minute structure. The nuclear substance is the idioplasma of the botanist Nägeli. Upon the molecular structure of it the form and function of every

[1] Paper read by Prof. August Weismann before the British Association at Manchester.

cell in the body depend, and consequently the form and function of the whole body are determined by the nuclear matter or idioplasma of the first cell, the egg-cell—parthenogenetic or fertilized.

If this theoretical view is correct, then we must be astonished that so much of this important nuclear substance is lost to the egg-cell—namely, one-half by the parthenogenetic ovum, and half as much again by the sexual one. What can be the cause that renders it necessary for this to happen before the egg-cell is able to develop into an embryo?

I will give a short account of my ideas upon the subject.

(1) The nuclear substance or idioplasma of the first polar body must be detrimental to the further development of the egg, for it is thrown out in all kinds of eggs, parthenogenetic as well as sexual, and the embryonic development never begins before the first polar cell has been expelled. Now, if the nuclear substance truly controls the cell and compels it to take a certain shape and a certain histological structure, there must be such a substance, such an idioplasma, also in the youngest egg-cells. This idioplasma causes the egg to develop a yolk possessing a certain colour and structure, it causes the egg to form a shell of a certain thickness and structure; briefly, it compels the young egg-cell to attain a degree of histological differentiation which it did not previously possess. For the youngest egg-cells are essentially similar in most animals, whilst mature egg-cells are very different and can often be very well distinguished in different species. The specific idioplasma of the growing egg-cell—I call it ovogenetic idioplasma—cannot be the same as that contained in the nucleus of the mature egg, and which controls the development of the embryo. It cannot be that idioplasma which determines the development of a certain egg-cell into a duck and not into a swan; it cannot be that kind of idioplasma which I have called *germ-idioplasma*, or simply *germ-plasma*.

Of course there must also be germ-plasma in the *young* egg-cell; I believe that in the youngest germ-cells there is no other idioplasma than germ-plasma, and that this germ-plasma changes into ovogenetic plasma, only a very small part of germ-plasma remaining unaltered.

This remaining part grows with the growth of the egg, and finally attains the same volume as the ovogenetic idioplasma. Then the division of the nuclear substance takes place, and the superfluous ovogenetic substance is removed in the first polar globule, whereupon the egg-cell contains only germ-plasma.

This is my explanation of the removal of the *first* polar cell.

(2) In regard to the second it is clear that an egg that contains only germ-plasma should be capable of undergoing embryonic development, unless the quantity of germ-plasma should prove to be too small. But this is not the case. Parthenogenetic eggs enter upon embryonic development immediately after the expulsion of the first polar globule. Sexual eggs do not thus develop, and we have to inquire into the reason for this. I believe it is because they throw out a second polar cell, which takes away one-half of the germ-plasma left within the egg-cell. After this the quantity of germ-plasma is too small for entering upon embryonic development, and therefore the egg-cell remains undeveloped, unless the lost quantity of germ-plasma be replaced in the process of fertilization. Embryonic development takes place immediately after the union of the germ-plasma of a spermatozoon with the remaining germ-plasma of the ovum. Consequent upon this the quantity of germ-plasma in a fertilized egg again becomes equal to that which was present after the separation of the first polar globule, and also equal to that which enters upon embryonic development in the parthenogenetic egg.

This is perfectly simple, but a great difficulty remains. Why is it necessary that the sexual egg should throw out half of its germ-plasma; why does it not retain the whole quantity of this important substance?

You would perhaps answer, Because the quantities of male and of female germ-plasma, that are united by fecundation, must be equal. Indeed, the facts of heredity lead to the opinion that these two kinds of germ-plasma must be equal in quantity, and we have microscopical observations recorded by Van Beneden, Carnoy, and others, which further support this conclusion. But if the quantity of germ-plasma must be equal in both, why should the germ-plasma of the egg increase so largely as to attain twice the volume of the germ-plasma of a spermatic cell? Nature is not so wasteful as to throw away so important a substance for nothing. There must be an adequate cause why in sexual eggs the germ-plasma must be halved before fecundation can take place.

I believe I can point out the reason why this is necessary, but before I do so I must beg you to first enter with me upon a few theoretical considerations on the subject of heredity.

Heredity depends upon the germ-plasma, as I have said before; the minute molecular structure of the germ-plasma causes the egg-cell to develop into a duck or a swan, it also causes the egg to develop into a Negro or into a European, into a Mr. Smith or into a Mr. Jones; in short, all qualities of the developed individual depend upon the constitution of this germ-plasma. In my opinion sexual propagation implies the union of two different germ-plasmas to form the single nucleus of the egg-cell; and the two substances that are united in the process of fertilization I believe to be equal in size and quantity.

Now let us suppose that we lived at a time when sexual propagation had not yet existed, and that we were present at its origin. We should then observe the union of two different germ-plasmas, both of the same size and quantity, but of a slightly different molecular constitution, one coming from one parent and the other coming from another. Both substances must be thoroughly homogeneous—that is to say, they must be composed of particles that are equal in their chemical, molecular, and morphological constitution. Let us illustrate this by a diagram, in which we represent each germ-plasma as a thread or a loop, which we know to be the microscopical form of germ-plasma and of nuclear plasma in general. For simplicity's sake we will represent only one loop for the germ-plasma of each parent. We have then two loops, the first representing the peculiarities of the germ-plasma of one parent, and the second representing the peculiarities of the other parent, and we will discriminate between them by making the first green and the second red.

These two individual kinds of germ-plasma unite and form together the nucleus of the fertilized egg, which develops into a new individual of the second generation. This individual will form again germ-cells, and each of these germ-cells will contain a germ-plasma, which is not homogeneous, as before, but composed of two halves, derived respectively from the two parents. In each succeeding generation the germ-plasma must attain to a more complicated constitution, it must contain twice as many different kinds of germ-plasma as were contained in the germ-plasma of the preceding generation. If we follow this development of the germ-plasma for a few generations, we shall find that union will take place by sexual propagation between the germ-plasmas of two individuals of the second generation, each containing two different kinds of germ-plasma. In this way the individuals of the third generation will be formed possessing germ-cells which contain four different kinds of germ-plasma. I have called these different kinds of germ-plasma *Ahnenplasma*, a word that can be rendered in English by the term *ancestral plasma*. By sexual propagation the individuals of the third generation would give

rise to individuals of the fourth generation, and the germ-cells of these last individuals must contain eight different ancestral plasmas; similarly the germ-cells of the fifth generation must contain sixteen ancestral plasmas, and so on. It is thus clear that in a very small number of generations the composition of the germ-plasma must become extremely complicated: by the tenth generation it would already contain 1024 different ancestral plasmas.

We do not know how far this may go, because we do not know how small are the primary elements of germ-plasma, nor do we know how many of these elements may be indispensable for the youngest and smallest germ-cells. But if we imagine these elements to be excessively small, this process of doubling the number of ancestral plasmas in each generation must have come to an end after a certain number of generations, whether they were 10, 20, 100, or 1000!

From the time at which the germ-plasma first attained its utmost complexity further sexual propagation was only possible by halving the number of ancestral plasms contained in the germ-plasma. Clearly, this process of halving ought to take place in male germ-cells as well as in female ones, but at this moment we are only sure of its existence in the latter. We have seen that one-half of the germ-plasma contained in the nucleus of the egg-cell is expelled in the second polar cell. That the nuclear substance thus expelled is true germ-plasma, is not a mere supposition, but a certainty. We know of developing eggs which are either fertilized or unfertilized, and in the latter case they develop by parthenogenesis. Such are the eggs of the honey-bee. We may assume that if these eggs remain unfertilized they will expel only one polar globule, but that if, on the other hand, they are penetrated by a spermatozoon they will also expel the second globule. Thus the same idioplasma that is expelled from the fertilized egg remains, and forms half of the first segmentation-nucleus in the parthenogenetic egg. It must therefore be true germ-plasma.

I do not doubt that this is the true significance of the formation of a second polar globule. We can see the necessity on theoretical grounds for the removal of half the number of ancestral germ-plasmas; and we *actually* find that half of the germ-plasma *is* removed in every sexual egg.

If this reasoning be correct, our views on sexual propagation must undergo a total change. Fertilization is no longer an unknown impulse given to the egg-cell by the entrance of a spermatozoon, but it is simply the union of the germ-plasmas of two individuals. The spermatozoon is no longer the spark which kindles the powder, or the relatively small force which converts potential into actual energy, but it is merely the carrier of germ-plasma of a certain individual, possessing the necessary qualities for reaching, penetrating, and fusing with the bearer of germ-plasma from another individual. There are no *essential*, but merely individual, differences between the nuclear substance of the spermatozoon and that of the ovum. There are no such things as male or female nuclear substances, but merely male and female cells, carriers of the immortal germ-plasma. The differences are wholly individual and of merely secondary importance, and nothing corresponding to the ordinary notions implied by the terms male and female exists in germ-plasma.

If this be so, then it is clear that the fact of sexual propagation demands a new explanation. We must attempt to explain the reason why Nature has insisted upon the rise and progress of sexual propagation. If we bear in mind that in sexual propagation twice as many individuals are required in order to produce any number of descendants, and if we further remember the important morphological differentiations which must take place in order to render sexual propagation possible, we are led to the conviction that sexual propagation must confer immense benefits upon organic life. I believe that such beneficial results will be found in the fact that sexual propagation may be regarded as a source of individual variability, furnishing material for the operation of natural selection. I believe that sexual propagation has become prevalent among the higher organisms for the purpose of conserving and multiplying that individual variability which owes its first origin to the Protozoon condition of such higher organisms. But it is not now my purpose to speak further upon this subject: I have already discussed it elsewhere ("Die Bedeutung der sexuellen Fortpflanzung für die Selections-Theorie," Jena, 1886).

Whatever is to be said for the above hypotheses, the facts I have the honour of bringing before you to-day seem at least to prove that sexual propagation depends on the removal of half of the germ-plasma of the egg and the replacement of it by the same quantity of germ-plasma of another individual. This is now a fact which may be regarded as indisputable; and, further, the existence of true parthenogenesis is now proved beyond doubt. For we know now that an egg which expels only one polar globule enters without delay into embryonic development, inasmuch as it has retained the whole of its germ-plasma.

THE TOTAL ECLIPSE OF LAST AUGUST IN JAPAN.

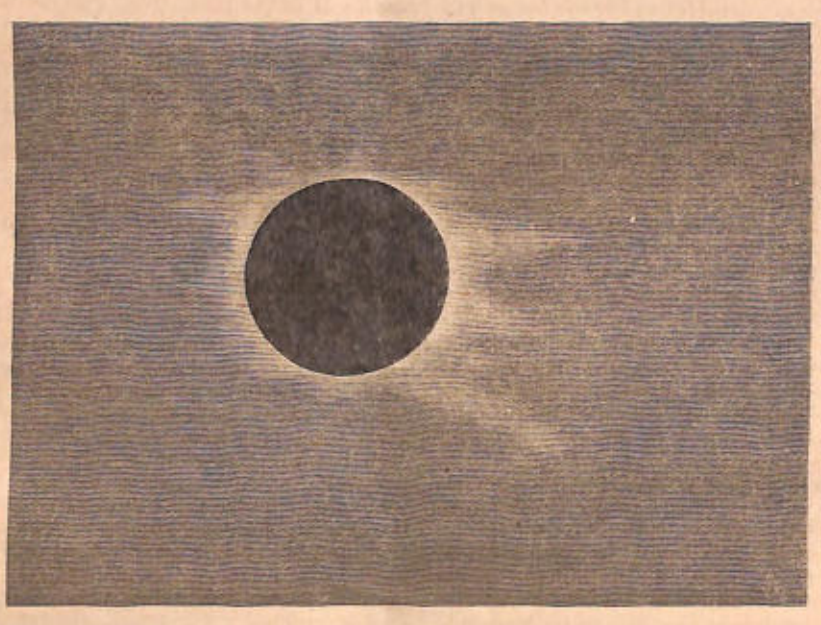
THE eclipse has come and gone, and our little party is on its way home with a few papers and a small box of glass plates—a rather meagre showing for the hard work of our summer months. Although we were so unfortunate as to have uninterrupted cloud throughout the entire duration of the eclipse, our expedition to Japan has not been so dismal a failure all told. Apart from sundry observations of minor importance contributed by volunteer observers at scattering stations for whom I had prepared instructions, Dr. W. J. Holland, who joined the Expedition at my invitation as naturalist, has been actively engaged in botanical and entomological research in fruitful fields, and has a good harvest to report. He has also valuable notes upon his ascent of Nantaisan, Asamayama, and Nasutake (which latter he appears to have been the first foreigner to ascend); while the separate expedition to the summit of Fuji-san (12,400 feet), which I had the pleasure to carry out under the auspices of the Boyden Fund of the Harvard College Observatory, and on which I had the highly-valued co-operation of Dr. E. Knipping, Meteorologist of the Japanese Weather Service, resulted, among other things, in the determination of its rare fitness as a site for astronomical observation—of which more elsewhere.

With reference to the preliminaries of the Eclipse Expedition it is necessary to state that early in the present year the trustees of the Bache Fund of the National Academy of Sciences, Washington, made a grant to Prof. Newcomb, the Superintendent of the *Nautical Almanac*, for observing the total solar eclipse of August 19, 1887. Prof. Newcomb determined the general lines of research to be undertaken, decided upon locating the observing-station in Japan, and placed me in charge of the Expedition. After some weeks of preparation in Washington and elsewhere, I set out for Japan on June 9, and arrived in Yokohama a month later. There was no small difficulty attending the definitive location of the instruments for observing the eclipse, owing to the deficiency of precise meteorological information regarding the region of the shadow-path. All existing data were kindly placed at my disposal by the officers of the Japanese Government, and several of the departments contributed in other ways to the assistance of an expedition which, had the skies been favourable, could not have failed of entire success.

Going northward from Tokio along the line of the Japanese Railway Company, to the courtesy of whose

president, Mr. Narabara, our Expedition is deeply indebted, we pass through Utsunomiya at the distance of 65 miles, and at 113 miles reach Shirakawa, a town of about 10,000 inhabitants, and situate 10 or 12 miles north of the centre of the shadow-path. Here I found a spot forming, in many respects, an ideal location for an eclipse-station. Within a quarter of a mile of the telegraph-office and railway-station, and in an unfrequented part of the town, was the ruin of the celebrated old castle erected some 300 years ago, and occupied by the Abe family until the revolution of 1868. Permission to establish my station on the castle walls was given by Count Oyama, the Minister of State for the Army, under whose control this and similar castles elsewhere in Japan, formerly possessed by the Daimios, now are. The massive walls rise to a height of about 80 feet above the surrounding plain, and afford a capital foundation for the instrumental equipment—not to say the seclusion so desirable in the mounting and adjustment of delicate apparatus. Of the mountain-range 25 or 30 miles to the west and north-west, and its cloud-creating propensity, we had apparently little need for fear—in fact, a month's residence in the castle gave us a large proportion of afternoons on which an entirely satisfactory record of the eclipse in all its stages could have been secured.

Our main instrument was a horizontal photoheliograph of nearly 40 feet focal length, with which we hoped to expose 100 plates during the partial phases of the eclipse; but I had determined also to attempt coronal photography with the same instrument, hoping to obtain eight or ten negatives of the corona of such size that subsequent enlargement would be undesirable. At the focus of this telescope the sun's image has a diameter of 4½ inches, and dry plates 17 × 20 inches had been provided for this work. Also an extra mirror, finely silvered by Brashear, was taken along for the heliostat, to replace the unsilvered mirror ordinarily employed, shortly before totality came on. After the special modifications of the exposing-shutters and plate-holders had been made, and a light-proof tube or camera the whole length of the telescope had been constructed, periodic drill for the work of eclipse-day was at once begun. For some minutes immediately before the beginning and after the end of totality, the partial phase exposures were to be made every fifteen



seconds, while the large plates for the corona, with exposures varying from one second to sixty-four seconds, were to be handled as rapidly as possible: we found that there was a loss of about five seconds between the plates, or something like one-sixth the entire duration of totality. With a very efficient photographic corps, and the drill which we all underwent, I had the best of reasons for anticipating complete success.

As was foreseen, too, we found in photographing artificial crescents—very slender ones—that no image of the plumb-line appeared on the plate; there was thus no initial line of reference for the measurement of position-angles. Mr. Hitchcock, whom I appointed photographer of the Expedition, undertook a variety of experiments to overcome this difficulty, and with entire success; the form of apparatus finally adopted will be detailed in the report of the Expedition. To assist in the operations of the photographic house, we were fortunate in securing the services of Mr. Ogawa, of Tokio, a Japanese photographer of wide experience, and Dr. Y. May King, of Amoy, also a highly-skilled manipulator.

As wet plates appeared to me preferable in many ways to dry ones for the partial phases, Mr. Hitchcock and Mr. Ogawa instituted a thorough series of experiments in the preservation of sensitized films, at first with glycerine, and subsequently more successfully with sugar. The results of this work made the wet plate, with its fine-grained film, as available for rapid manipulation in the photography of celestial phenomena as the dry plate has hitherto been found to be. It was shown that the plates might with entire safety be removed from the sensitizing bath from two to four hours before exposure and development, if treated with the sugar preservatives, and proper precaution was taken to keep the films from drying. The details of this process will be embodied in the report of the Expedition. As an extreme test we exposed, on the day after the eclipse, a box of the plates which had been sensitized and preserved for eclipse-work some twenty-six hours previously, and found that they gave sun-pictures photographically perfect.

In compliance with orders issued by the Secretary of the Navy, two officers from the Asiatic squadron, Lieut. Southerland and Chief-Engineer Pemberton, of the U.S.S. *Monocacy*, reported to me for eclipse-duty on

my arrival in Yokohama, and their services were zealously and most effectively rendered. In addition to his work as executive officer, I placed Lieut. Southerland in charge of the 9-foot coronagraph, sent out by the Pickerings from the Harvard College Observatory. The objective of this instrument was the 7¼-inch Clark glass of the equatorial of my own Observatory at Amherst, while the dry plates, with the instructions for their manipulation, were identical with those furnished by the Pickerings to Prof. Young, who carried to his Russian station a 6⅝-inch Merz glass, also lent from the larger transit-instrument of Amherst College Observatory. I have not yet been able to learn whether Prof. Young was favoured with a clear sky during totality; but, if he was, it is the more regrettable that clouds covered the sun at Shirakawa, as the first serious attempt to obtain trustworthy evidence of rapid changes in the corona has thereby come to nought. It will be many a year before another eclipse occurs with two stations geographically so well placed for this special research as were Russia and Japan. Prof. Pickering desired me, if practicable, to place all or a portion of the corona-apparatus provided by this Observatory on the summit of one of the mountain-peaks of which there are several adjacent to the centre of the shadow-path, notably Nantaisan, 8500 feet high. Dr. Holland made the ascent of this mountain about the middle of July; but his report of its difficulties, together with the highly probable cloudy condition of the summit during the eclipse, led me to abandon farther consideration of this mountain; while the other peaks were too far removed from Shirakawa to permit of occupation with the time and assistance at my disposal. The remainder of Prof. Pickering's apparatus was therefore mounted alongside the photoheliograph at the central station; the double coronagraph, two 5-inch lenses of about 3 feet focus, being operated by Dr. Ames, U.S. Navy, while Dr. D. B. McCartee attended to the exposures with the 4-inch short-focus camera, and Mr. C. R. Greathouse to the exposures of plate-holders for determining the actinic effect of the coronal light.

The valued service of Mr. Pemberton is worthy of special mention here in rendering the photoheliograph less unwieldy for rapid work than I had found it formerly. By means of an ingeniously-devised system of cords and pulleys, led from the heliostat into the photographic house, the reflecting mirror was placed under the immediate and constant control of the chief astronomer making the exposures: it was thus possible to dispense with the customary assistant at the heliostat pier for adjusting the mirror in right ascension and declination. A very simple device made it possible to see the bright reflected image of the sun while at my post in the dark room, and adjust it accurately on the plate without opening the exposing-slide.

The importance of Newcomb's and Langley's observations of the outer corona in 1878, and attempted by Lockyer in 1886, had not escaped me, and I had an occulting-disk mounted on a rod attached firmly to the gable of the photographic house, so that its shadow as cast by the eclipsed sun would fall about 50 feet away, in the area inclosed by the upper castle wall. Here I stationed Mrs. Todd, provided with all the paraphernalia for seeing and sketching in their correct relations the faint outlying streamers of the corona.

Of two 3½-inch telescopes lent by Admiral Yanagi, Hydrographer of the Imperial Japanese Navy, one was reserved for the optical observation of first and fourth contacts, and the search for intra-Mercurial planets; while the other was committed to Dr. Holland, a skilled artist, with instructions to sketch as far as possible all the details of the corona adjacent to the solar poles.

Mr. Nakagawa, the Director of the Naval Observatory, with his assistant, made a thorough series of meteorological observations throughout the eclipse period, following the system elaborated by Von Bezold and recommended by the German Meteorological Conference for the observers in Russia. On the north-west corner of the castle wall I stationed Mr. K. Açino, a student of astronomy in the University, to make detailed and precise observations of the diffraction bands, and to observe if possible the sweep of the lunar shadow across the extensive rice-fields below.

The purely eclipse results of the work at Shirakawa were disheartening in the extreme. The forenoon gave us a perfect sky, with no indication whatever of approaching cloud: all were confident of entire success. But about an hour before the time of first contact, a slender finger of cloud began to rise from the west, coming at first directly above the summit of Nasutake, a volcano about 25 miles away, and which had sprung into unwonted activity during the past night, belching forth for hours enormous volumes of smoke and steam. The sun was entirely invisible during the first half-hour of the eclipse, when a brief interval of partly clear sky gave time for adjusting the heliostat and making ten or twelve exposures. The sun being very faint, only five of these photographs are available for measurement; and these were the only pictures that could be taken with the photoheliograph. The dense clouds, leaving a large clear area most of the time about the zenith, lay over the sun until the eclipse was past, save only a moment shortly after totality, when there was a partial clearing, but too brief, and the sun too faint, to allow of the necessary adjustment of the reflecting mirror.

As totality drew near, it suddenly occurred to me that a good observation of second contact might be possible by watching for the approach of the moon's shadow among the clouds; but my attempt to do this failed, the light appearing to me too much diffused to permit of anything better than a rough approximation to the time of contact. I found subsequently among Mrs. Todd's notes of the eclipse that totality appeared to her to come on, not evenly, but as if by jerks—a phenomenon which may, I think, have been due to the extinction of the sun's light from one cloud after another, as the lunar shadow advanced over the north-western sky. The weather-map for August 19, which came to our station from Tokio the day after the eclipse, gave us some idea of the odds we had been labouring against: the sheet for 2 p.m. showed clouds at all stations of the Meteorological Service except one, and that far removed from the belt of totality. In general, the whole of the main island was obscured on the eventful afternoon, and a view of the eclipse was permitted only to those so fortunate as to be located in the line of the small apertures, here and there, through the general cloud area. These were numerous enough to enable voluntary observers, scattered over the central portion of the belt of totality, and for whom I had prepared instructions, to obtain a goodly number of drawings of the corona. These instructions had been translated into Japanese, and printed and distributed through the co-operation of the Department of Education and the Bureau of Geography of the Department of the Interior. Altogether there are something like a hundred such drawings; but their value is uncertain until they are properly collated. Much the best drawing which I saw was made by Mr. Shuji Isawa, Chief of the Bureau of Compilation of the Department of Education, who was fortunate enough to be located in a spot in Western Japan, where totality was seen in a nearly cloudless sky. He has kindly furnished me with photographs of his drawing, one of which is inclosed.

Other Expeditions in Japan fared ill also—some of them worse than my own. That sent out from the University in charge of Prof. Terao, and located a few miles south of Shirakawa, at Kuroiso, experienced not only heavy clouds, but much rain during the eclipse, and no observations could be made. At Sanjo, on the central line and south-east of Niigata, Prof. Arai, Director of the Meteorological

Observatory, was able to make successful exposures for the corona with a small telescope. It was reported clear during the whole eclipse at Choshi, a point on the eastern coast near the southern limit of total obscuration, but there were no observers or instruments there for scientific work. It was reported cloudy throughout the whole eclipse at Niigata; while a party of observers who had ambitiously climbed to the top of Nantaisan brought down a record of nothing but clouds and fog. On the whole, Japan appears to have been an uncanny spot to lead an eclipse-track across. DAVID P. TODD.

s.s. *Port Victor*, September 20.

THE MÄRJALEN SEE.

LAKELETS, in which the ice-crags of a glacier are mirrored, in which miniature bergs may be seen to float, are of occasional, though of rare, occurrence in the Alps—as for example the Lac de Ste. Marguerite, at the foot of the Ruitor glacier; but the Märjalen See, so far as I know, is unique of its kind. It is not formed at the foot of a glacier, either by partial occupation of a shallow basin worn by the ice-stream in its days of greater strength, or by the pounding back of the glacier torrent by an old terminal moraine; but it is on one side of a glacier, which makes a dam across an upland glen. This barrier at times yields to the pressure of the accumulated water sufficiently to allow of its escape beneath the great ice-stream, and it is a recent incident of the kind, noticed in the *Times* of September 30, which has suggested the present article.

The Great Aletsch glacier, as is well known, is the largest ice-stream not only in the Oberland group, but also in the Alpine chain. Its upper basin is fed by the snows of an almost complete ring of grand peaks, the most conspicuous of which, enumerated from west to east, are the Aletschhorn, the Jungfrau, the Mönch, and the Viescherhörner. All these are considerably above 13,000 feet, and there are several others, less familiar to the ordinary tourist, which either rise slightly above that elevation, or are only a very few hundred feet below it. The great *eismeer* thus formed passes out as a single stream through a "gate in the hills," between the crags of the Faulberg on the east and the base of the Dreieckhorn on the west. This gap is rather more than a mile across, and the glacier for several miles is not less, and is generally rather more, than its breadth at this place. It flows at first slightly to the east of south, then runs almost due south, and finally sweeps gradually round to the south-west. This deflexion is caused by the Eggischhorn, which rises like a great pyramid full in face of the upper course of the glacier to a height of 9649 feet above the sea, or nearly 2000 feet above the surface of the ice. At this spot the Märjalen See is situated, at a height of 7710 feet above the sea, rather less than five miles below the "gate," and rather more than that distance above the end of the Aletsch glacier. This sweeps on along the west flank of the Eggischhorn, until it terminates in the grand gorge of the Massa, at no great distance from the Bel Alp Hotel, a worthy rival in beauty of situation to that on the Eggischhorn.

The Märjalen See is thus formed: the range of the Eggischhorn is continuous with that which makes the left bank of the Aletsch glacier, and divides its comparatively unbroken surface from the narrower and more shattered mass of the Viesch glacier. But this range to the north of the Eggischhorn is deeply notched, so that it is possible to quit the Aletsch glacier and without ascending to reach a depression, barely so high as the surface of the ice, from which one looks down a steep slope on to the surface of the Viesch glacier. From this depression a shallow valley descends towards the west, and is barred, as mentioned above, by the great glacier of the Aletsch. Thus a lake is formed, fed by various streamlets from the slopes on either side and by the melting of the glacier ice. Though now the shrunken stream of the Aletsch glacier does not diverge towards the lake, it was no doubt formerly divided by the opposing mass of the Eggischhorn; then one of its arms occupied the bed of the lake, and after passing over the depression joined itself to the Viesch glacier. Some geologists regard the basin of the Märjalen See as wholly due to the excavatory action of this offshoot of the Aletsch. To myself it appears to be the upper part of a valley, produced in the ordinary way, but subsequently modified in its outlines by the rasping action of the glacier.

At somewhat irregular intervals, but according to popular belief once in seven years, the ice-dam yields sufficiently to allow the pent-up waters to escape beneath the glacier, when the contents of the lake are discharged rapidly down the gorge of the Massa, and, after devastating the fields below, are poured into the Rhone, near Brieg.

In the summer of 1858 I had the good fortune to see the Märjalen See both full and empty. On my first visit I found a lake full 300 yards across at the lower end, and



about three times as long. From the rocky margin on either side the ice arched up in a low flattened curve until its edge at the highest point was about 60 feet[1] above the level of the water. From this it rose in a vertical cliff of almost unbroken ice, of purest white, which was mirrored in the still blue water below. Here and there miniature icebergs were floating, in colour if possible yet purer than the parent glacier, and above the water at the foot of the cliff was a band of turquoise blue. This was produced by the fresh surface of the ice, for the cliff is under-cut by the action of the water of the lake, and to this undercutting the bergs are no doubt partly due.

Next evening I revisited the spot. To my surprise all was changed: the lake had almost wholly disappeared; the glacier cliff which the day before had been doubled by reflexion, was now doubled in reality. Below the upper zone of white ice was now a zone of more than

[1] Probably the height at the present time is not so great. Since 1858 there has been a considerable shrinking in the glaciers of the Alps. When I visited the Märjalen See in 1881, the greatest height of the cliff did not appear to me to exceed 30 feet. In 1858 Prof. Ramsay found by measurement that the greatest height of the cliff above water was 60 feet, and the greatest depth of the lake at its foot 97 feet ("Peaks, Passes, and Glaciers," 1st series, p. 461).

equal thickness of the most exquisite blue; on the dry bed of the lake were stranded the bergs which the day before had floated in its waters, and we could now appreciate their true size. One whose shape we had greatly admired now appeared even more beautiful with its fantastic pinnacles and blue recesses. It was, I estimated, from 30 to 40 feet high, nearly as wide, and considerably longer. Two cubical masses on the opposite shore were in colour the most lovely turquoise blue that I have ever seen. These, no doubt, on the previous day had appeared as mere slabs on the surface. The bed of the lake was covered with a fine mud, on which were numerous tracks or castings, which I attributed to a worm. I did not see any shells, so that probably no mollusks live in the chilly waters of the Märjalen See.[1]

The ice in this part of the Aletsch glacier is comparatively little crevassed. This permits the glacier to act as a dam; the drainage of the lake is no doubt due to some accidental rupture which opens a communication, quickly enlarged by the running water, with the sub-glacial drainage of the glacier.

A traveller in August 1872 was so fortunate as to see the actual escape of the water at the lower part of the glacier. He describes it as follows[2]:—"It was 4.50 p.m. when we arrived (at the Bel Alp Hotel). The domestics drew our attention to a sound like the roar of a cataract, which seemed to descend the Aletsch. For a time the sound was sub-glacial, but a yellow torrent at length appeared on the opposite side of the glacier, smoking and roaring as it tumbled down the declivities of ice. The front of the torrent soon appeared opposite to the Bel Alp, carrying every movable thing along with it. Wishing to get near the torrent, I descended rapidly to the glacier, crossed it, and succeeded in getting quite close to the rushing water. Everywhere impetuous, it was divided into spaces of tolerably uniform slope, separated from each other by steep and broken declivities, down which the water plunged with tremendous fury. At the base of one of these falls it was met by a kind of reflecting surface, by which the rhythmic character of the motion was finely revealed. The water here was tossed upwards in a series of vast parallel fans, carrying with them ice-blocks and stones, and breaking above into a spray as fine as smoke. A bend of the glacier came in for the lateral portion of this spray, and over it the rounded blocks of ice and the stones were showered like projectiles. The sound of the torrent had not abated at bed-time, but this morning all is quiet, and no water is to be seen in the temporary channel."

This sudden discharge of so great a body of water, in addition to damaging the fields immediately below, very considerably raises the level of the Rhone. On the last occasion, September 4, the writer quoted at the beginning of this article states that "the level of the Rhone rose at Brieg 5½ feet, from about 3½ feet to 9 feet, and at Sitten 4 feet, from 6½ feet to 10½ feet. The greatest rise observed since the regulation of the Rhone from the same cause took place on July 19, 1878, and although it was then at Brieg only 5 feet, and at Sitten only 3 feet, it was considered a very fortunate circumstance that the event took place at a very low level of the Rhone for the season."[3] He adds that to avoid such a danger in future it is proposed to enlarge greatly a channel which many years since was cut through the moraine stuff overlying the rock east of the lake, and so provide an outlet towards the Viesch glacier. By this "the volume of its waters will be reduced to about half what it is at present (10,000,000 cubic metres)." So that future travellers will not see the Märjalen See in its full beauty. The lake formed by the advance of the Gétroz glacier, in the upper part of the Dranse valley, the bursting of which in 1818 wrought such fearful devastation, may be regarded as to some extent a parallel case with the Märjalen See, of a more temporary nature, but on a grander scale.

In Sir Charles Lyell's "Principles of Geology" (chapter xvi.), and again in his "Antiquity of Man" (chapter xiv.), are notices of the Märjalen See, and of some beach terraces formed by its waters. He regards it as illustrative of the celebrated parallel roads of Glenroy, but, though this explanation has found very general favour with geologists, I must confess myself unable to accept it. But into this thistle-bed of controversy I must not permit myself to wander.

T. G. BONNEY.

[1] Ramsay found the temperature of the water near the ice-cliff to be 3° C.

[2] J. T. quoted from the *Times* in *Alpine Journal*, vol. vi. p. 100.

[3] An account of this is given by F. V. Salis (*Jahrb. Schweiz. Alpencl.* 1878-79, p. 549. The discharge on this occasion was at first slow. It began at 8 a.m. July 18; by 4 p.m. the lake had sunk 1 metre; during the darkness it sank 4 metres, and by 3 p.m. most of the contents were gone. It was estimated that only 700,000 cubic metres of water out of 10,000,000 remained.

THE BACILLUS OF MALARIA.

A PAPER of unusual interest in relation to the question of the agency of microphytes in the production of disease will shortly appear in Prof. Cohn's botanical *Beiträge* (vol. v. part 2). For many years the efforts of pathologists have been directed in this relation to the subject of malaria. The local conditions which determine the "endemic" prevalence of ague have been studied with considerable exactitude. They are such as to indicate very clearly that the material cause of intermittent fever, although it is generated in the soil, acts through the air. The fact that its influence is restricted within very narrow limits of distance from its source indicates that it is not diffusible like a gas or vapour, but consists of particles which, on various grounds, are surmised to be living organisms of extreme minuteness. Can this be established on evidence which will bear criticism?

All will remember that in 1879 Tommasi Crudeli published (in conjunction with Prof. Klebs) observations which tended to show that in malarious districts a Bacillus inhabits the soil which can be cultivated so as to yield a product capable, when inoculated, of producing in animals a fever of intermittent type, accompanied by the anatomical characteristics of malarious infection. Subsequently it was found by several observers that, during the cold stage of ague, spore-containing Bacilli, conjectured to be identical with those of Crudeli, are to be found in the blood.

These results have been received by pathologists with much misgiving, partly because the experimental proofs appeared inadequate, partly because other observers failed in their endeavours to verify them. Dr. Schiavuzzi, a medical practitioner at Pola, on the Adriatic, appears to have been more fortunate. Following the methods of Dr. Koch, he has sought for organisms in the air of the malarious district near the town in which he resides, and with such success that he is able, in repeated observations, to obtain without fail pure cultivations of a Bacillus which is not only indistinguishable as regards its structure from that of Crudeli, but also produces in animals the characteristic symptoms and pathological changes which belong to ague. The first communication of Dr. Schiavuzzi's results was made to the Accademia dei Lincei more than a year ago (see *Rendiconti*, vol. ii. 1886, April 4), but excited very little attention. It so happened that in the course of the past summer Prof. Cohn visited Pola, and so became acquainted with Dr. Schiavuzzi, who, during the present year, has been pursuing his investigations. In consequence, Prof. Cohn has been able to repeat the Pola experiments in his own laboratory at Breslau, and, so far as possible, to confirm the discovery. The writer had the opportunity, a short while ago, when Prof. Cohn was in England, of reading the proofs of Schiavuzzi's paper, and of seeing the very perfect photographs of the Bacillus which have been made of it at Breslau.

Although it may be admitted that evidence of a more conclusive kind than any which has been offered by Dr. Schiavuzzi is required to establish the truth of his inference, yet there seems to be good reason for thinking that he has approached much nearer to a solution of the question than any of his predecessors. J. B. S.

NOTES.

AT the meeting of the Academy of Sciences, Paris, on the 17th inst., Admiral Mouchez spoke of the preparations which are being made for executing the photographic charts of the heavens. Ten of the photographic telescopes, seven by MM. Henry and Gautier, of Paris, and three by Sir H. Grubb, of Dublin, are expected to be finished by the end of 1888 and forwarded to various observatories in France, Spain, South America, and Australia. With the promised co-operation of England, the United States, and Russia, it is hoped that a good beginning will be made in 1889, and that the vast undertaking will be completed within the time anticipated by the International Congress of last April.

PROFS. CAYLEY, F.R.S., AND M. J. M. HILL retire from the Council of the London Mathematical Society. The new names selected by the Council for submission to the Society at its annual meeting (November 10), are those of Mr. A. Buchheim and Dr. J. Larmor. The De Morgan Medal, which we have already announced as awarded to Prof. Sylvester, F.R.S., will be presented to him at the aforesaid meeting.

ON December 8 next, Herr Friedrich Traugott Kützing will be eighty years of age, and a good many men of science in Germany are anxious to give emphatic expression on the occasion to their respect for his character and their appreciation of his labours. Herr Kützing was one of the first to recognize that the best material for the study of cells and their life is provided by the simplest plants; and the results of his researches are well known to all biologists. It is proposed that a gift of some kind shall be presented to him on his eightieth birthday, and an influential Committee has been appointed to make the necessary arrangements. If any English students of biology would like to associate themselves with their German colleagues in this matter, they should communicate with the secretary of the Committee, Herr Otto Müller, 44 Köthenerstrasse, Berlin, W.

M. NOLAN, student at the Geological Laboratory of the Sorbonne, has been intrusted by the French Ministry of Public Instruction with a mission to study the geology of the Balearic Islands.

A NOTIFICATION in the *Calcutta Gazette* states that the Maharanee of Cossimbazar has given twenty thousand rupees for the promotion of technical education in the Moorshedabad district. The donor of this munificent sum proposes that five thousand rupees shall be spent in purchasing the necessary apparatus and instruments. The interest on the remainder is to be devoted to endowing a class in the Berhampore Collegiate School, and establishing classes in connexion with the college class in some of the neighbouring elementary schools. The Lieutenant-Governor of Bengal accepts the gift, and approves of the scheme proposed by the Maharanee.

AN interesting address on English and foreign technical education was delivered last Saturday by Prof. Silvanus Thompson, at the Aldingham Institute, a school of technical education in Goldington Crescent, St. Pancras. In the course of his address Prof. Thompson drew attention to the fact that in Berlin there is a great State-aided institution in which every known industry is taught. This institution he described as a large building—as big as Buckingham Palace, if not quite so beautiful—standing on a site of about 12 acres. It had something like 500 rooms for technical teaching, and, in fact, was a perfect college, with a good library. He believed that the entire cost of that establishment had been about £960,000. It was a building which had cost about the same amount as one of our ironclads. The entire maintenance of the institution was about £38,000 per annum; but that expenditure and the original outlay were recouped by the well-to-do character of those who had passed through its teaching, and thus had become useful members of society instead of drags upon the country. For such a country as England a site not of 12 acres but of 40 acres would be required. It might cost £4,000,000 to build and £100,000 a year to maintain, and from the results which would follow it would be cheap at the price, for it would enable us to obtain every possible requirement of life from our own handicraftsmen, instead of having to go to foreign countries for what we need. It would do more, for the superiority of English workmanship would cause fresh demands to be made from us, not only throughout our own colonies, but from foreign countries also.

THE Geodynamical Committee of the Italian Meteorological Society held meetings at Aquila from September 6 to 8. A preliminary meeting had been held at Florence in May last to prepare the work of the present one. The object was to formulate practical and uniform directions for the seismological researches undertaken by the Society, and to deduce from our actual knowledge rules to be followed in the construction of houses, so as to diminish the risk of damage in earthquakes and undulatory motions. M. Bertelli, of Florence, explained the theory of his new bifilar instrument for determining the least tremors of the earth, and he was invited to prepare directions for its construction, erection, and use, to be added to the report of the discussion. He also described his apparatus for the protection of telephones from lightning. The choice of a type of seismograph was the subject of a long discussion, and a Committee was nominated for the study of this very important question. The consideration of the best mode of collecting, discussing, and publishing the seismic and micro-seismic observations now being made in Italy was referred to the same Committee. On the consideration of the rules to be followed in the construction of buildings, M. de Rossi, of Rome, indicated the further observations which ought to be made. MM. Denza, De Giorgi, Roberto, Bertelli, and Galli also took part in the discussion. The resolutions adopted will be printed, and distributed to the various municipalities for the instruction of the persons concerned. This meeting of students of seismology is the first that has been held in Italy. The Secretary of this enterprising Society is Dr. O. L. Bianco.

THE other day the Committee of the Chester Society of Natural Science passed a resolution, which was entered in their minutes, expressing their deep sense of the loss the Society had sustained through the death of Mr. John Price, which took place on Friday, the 14th inst. He had reached the ripe age of eighty-four. Upwards of forty years ago, when residing at Birkenhead, Mr. Price occupied himself with the fauna of the Birkenhead shore, and the value of his researches was recognized by various scientific investigators of acknowledged eminence. The last of his observations on the shore were embodied in a paper on the pluteus of the starfish, read before Section D of the British Association meeting at Liverpool in 1852. To the Proceedings of Section D of the British Association he contributed papers from time to time at subsequent meetings. Having settled at Chester in 1858, Mr. Price was soon the means of establishing in the city a Society for the study of natural history. Ten years later, after the appointment of the late Charles Kingsley to a canonry in the Chester Cathedral, this Association was merged in the present Society, founded by Mr. Kingsley, who was one of Mr. Price's intimate friends. Mr. Price accepted,

and retained until his death, the position of Chairman of the Botanical Section of the Society. The resolution to which we have referred concludes as follows: "We shall miss from our meetings and excursions his venerable form, his familiar voice, and his wise counsels, but the name of 'Old Price' is one which will ever live in the Society as that of one of our revered fathers, and one of Nature's truest disciples and humblest and most loyal children."

THE last number of the Journal of the Royal Asiatic Society (N.S. vol. xix.) contains a short paper by Prof. de Lacouperie on the Miryeks, or stone men of Corea. These are huge half-length human figures, carved in stone, and looked upon as relics of a religion of former times. Those described by Mr. Carles in his paper on Corea, read before the Royal Geogaphical Society last year, are about 25 feet high, cut out of some large boulders in the middle of a fir wood in a hill-side. The largest hitherto known is at Unjin, and is shown in a plate prefixed to the paper. It stands about 62 feet high; and the body and head resemble those of the idols in Buddhist temples. A column about 10 feet high runs up from the head, giving support to an oblong slab about the same length; on this stands a smaller column supporting another slab, and from the corners of the two bells are pendent by chains. Prof. de Lacouperie points out some peculiarities about the word Miryek, and suggests that perhaps it is not Corean at all; it may have existed in Corea in its special adaptation to the huge stone statues, without having preserved its original meaning previous to the adoption of Chinese characters. If this be correct, it implies that the religion which produced the erection of the statues was then forgotten or in the shade. They might, he suggests, be due to an early spread of Buddhism in Corea. But it is evident that we must know more of that country before the origin of these curious survivals is clearly explained.

ANOTHER contribution to our knowledge of the group of beautiful dye colouring matters known as safranines has just been published in the current number of the *Comptes rendus* by MM. Barbier and Vignon. It has been known for some time that the nitroso-derivatives of the tertiary aromatic monamines in acting upon the primary monamines give rise to colouring matters, but their nature has hitherto remained undetermined. MM. Barbier and Vignon, however, in the light of their previous work, set out to explore this interesting side group of substituted safranines, with the following successful result. Starting with para-nitroso-dimethyl aniline, $C_6H_4NON(CH_3)_2$, they studied the action of one equivalent of its hydrochloride upon one equivalent of aniline in alcoholic solution, and found that a reaction occurred sufficiently violent to boil the alcohol. Eventually the product dissolved, forming a solution at first yellow, afterwards gradually changing to brown, and finally to bright violet-red, which on cooling deposited a solid. After filtration, washing, and repeated recrystallization this solid was obtained pure in brilliant brown crystalline spangles. Analysis showed that its composition was $C_{16}H_{20}N_4$, and from its reactions there can be little doubt that it is tetra-methyl diamido-azo-benzene. It is not very soluble in water, but, as is characteristic of all the safranines, is soluble in concentrated acids, forming deep-red or violet solutions. This, however, is not the only substance formed during the above reaction, for sodium chloride precipitated from the violet mother-liquor a second crystalline colouring matter, which turned out to be identical with the well-known dimethyl phenylene safranine, $C_{20}H_{18}N_4$. The formation of this latter body helps to explain the course of the reaction, which probably runs as follows: $3[C_6H_4NON(CH_3)_2HCl] + 2C_6H_5NH_2 = C_{16}H_{20}N_4 + C_{20}H_{18}N_4HCl + 3H_2O + 2HCl$. The work of MM. Barbier and Vignon, and of all other workers in this direction, is the more interesting inasmuch as it combines industrial utility with the advancement of pure chemistry, on the one side handing over new materials to the manufacturer, and on the other new facts to the already immense number which stand to the credit of the last few years.

THE *Times* reprints, from a document issued by the Berlin Bureau of Statistics, some interesting information about what is called "the motive force of the world." It appears that four-fifths of the engines now working in the world have been constructed during the last twenty-five years. France owns 49,590 stationary or locomotive boilers, 7000 locomotives, and 1850 boats' boilers; Germany has 59,000 boilers, 10,000 locomotives, and 1700 ships' boilers; Austria 12,000 boilers, and 2800 locomotives. The force equivalent to the working steam-engines represents—in the United States, 7,500,000 horse-power; in England, 7,000,000; in Germany, 4,500,000; in France, 3,000,000; and in Austria, 1,500,000. In these figures the motive power of the locomotives is not included, whose number in all the world amounts to 105,000, representing a total of 3,000,000 horse-power. Adding this amount to the other powers we obtain the total of 46,000,000 horse-power. A steam horse-power is equal to three actual horses' power; and a living horse is equal to seven men. The steam-engines of the world represent, therefore, approximately the work of 1,000,000,000 men, or more than double the working population of the earth, whose total population is supposed to amount to about 1,455,923,000 inhabitants. Steam has accordingly trebled man's working power.

IN the report presented at the eighteenth meeting of the Sunday Lecture Society it is stated that the attendance at the lectures during the last session was less than in the previous year. The Committee also announce that the accounts show an increased balance against the Society. On the other hand, they note with pleasure that good progress is being made by kindred associations in various parts of the country.

A SERIES of elaborate "geological studies," relating to the Dutch West Indies, by Prof. K. Martin, of the University of Leyden, is being issued by E. J. Brill, of Leyden. These "studies" embody the results of researches which Prof. Martin himself has carried on. In the first instalment, which has just been issued, he deals with the geology of Curaçao, Aruba, and Bonaire. This instalment consists of 140 handsome, well-printed pages, and is illustrated by three coloured geological maps, two plates, and thirty-six woodcuts.

A VALUABLE "Statistical Atlas of India," prepared for the Colonial and Indian Exhibition, 1886, and printed at Calcutta, may now be obtained at Mr. Edward Stanford's, Charing Cross. The object of the work is to give a general idea of the character of the country, its inhabitants and agriculture, with the addition of such statistics as may serve to illustrate commercial and educational progress. The maps have been prepared and printed in the office of the Surveyor-General of India at Calcutta, under the special supervision of Colonel Waterhouse and Major Strahan; and the chapters are by writers specially conversant with the subjects with which they deal.

MESSRS. CASSELL AND CO. have ready a new and enlarged edition of "Colour," a scientific and technical manual treating of the optical principles, artistic laws, and technical details governing the use of colours in various arts, by Prof. A. H. Church; and cheap editions of "The Fresh-water Fishes of Europe," by Prof. H. G. Seeley, F.R.S., and "Short Studies from Nature," a series of familiar papers, by eminent authors, on interesting natural phenomena, with full-page illustrations and diagrams.

NEW catalogues of scientific books have just been issued by Messrs. Macmillan and Bowes, Cambridge, Messrs. Dulau and Co., London, and Mr. W. P. Collins, London.

THE Cambridge Scientific Instrument Company have published a descriptive list of "anthropometric apparatus," consisting of instruments for measuring and testing the chief physical characteristics of the human body. This list cancels those previously issued. The instruments have been designed under the direction of Mr. Francis Galton.

A BIOLOGICAL and Microscopical Section has been formed in connection with the Cardiff Naturalists' Society, with Dr. C. T. Vachell, M.D., as President, and Prof. W. N. Parker as Hon. Sec. The inaugural meeting was held on Thursday evening, October 20, in the Biological Department of the University College, when papers were read on the work of the Section by the President and Hon. Sec. The objects of the Section are stated to be the promotion of the study of biology generally, but more especially of the local flora and fauna, including marine as well as land forms.

IT is announced that the following lectures will be delivered at the Royal Victoria Hall, Waterloo Bridge Road. November 1, Mr. A. H. Gilkes, "The First Napoleon;" 8th, Mr. W. L. Carpenter, "Heat" (experiments by means of the projection lantern); 15th, Dr. H. W. Crosskey, "Early Changes in the Earth's Surface and how we get our Knowledge of them;" 22nd, Sir John Lubbock, M.P., "The Habits and Ideas of Savages;" 29th, Mr. W. F. Donkin, "Mountain Climbing in Switzerland and the Caucasus;" December 6, Prof. Boyd Dawkins, F.R.S., "A Bit of Coal;" 13th, Dr. W. D. Halliburton, "The Eye, and how we See."

THE additions to the Zoological Society's Gardens during the past week include two Nisnas Monkeys (*Cercopithecus pyrrhonotus*) from West Africa, presented by Mrs. Benett Stanford; a Brown Capuchin (*Cebus fatuellus*) from Guiana, presented by Mr. Edward A. B. Pitman; a Dusky Ichneumon (*Herpestes pulverulentus*) from South Africa, presented by Mr. L. G. Morrell; a Three-striped Paradoxure (*Paradoxurus trivirgatus*) from India, presented by Mr. J. Millar; a Buzzard (*Buteo vulgaris*), British, presented by Mr. F. Austen; a Pennant's Broadtail (*Platycercus pennanti*) from New South Wales, presented by Mrs. Brooks; a Common Chameleon (*Chamæleon vulgaris*) from North Africa, presented by Mr. Absell; two Burrowing Owls (*Speotyto cunicularia*) from South America, presented by Mr. John Clarke Hawkshawe, F.Z.S.

OUR ASTRONOMICAL COLUMN.

THE PARALLAX OF Σ 2398.—In the *Astronomische Nachrichten*, No. 2676, Dr. E. Lamp, of Kiel, has published a determination of the parallax of the brighter component of this pair (to which his attention was attracted by their large proper motion) referred to two neighbouring stars derived from a series of differences of declination observed with the refractor and filar micrometer of the Kiel Observatory between February 1883 and April 1885; the value of the parallax deduced from this discussion being 0″·34. Wishing to verify this result, Dr. Lamp has made with the same instrument, between May 1885 and March 1887, a further series of measures of differences of declination of each component of Σ 2398 (= D.M. + 59°, No. 1915, mags. 8·2 and 9·0 respectively) referred to three comparison-stars: viz. D.M. + 59°, No. 1911, mag. 7·0; D.M. + 59°, No. 1913, mag. 9·4; and D.M. + 59°, No. 1918, mag. 7·8; and has published the results of his discussion of these measures in Nos. 2807-8 of the above-mentioned periodical. The following tabular statement gives for each series of observations the resulting parallax deduced from the differences of declination between each component of the double star and each of the above-mentioned comparison-stars:—

Period.	Component.	π_1	π_2	π_3	No. of Measures.
		″	″		
1883-84 ...	Σ_1 ...	—	... +0·2958 ...	+0·3801	46
1884-85 ...	Σ_1 ...	—	... +0·2517 ...	+0·4628	44
1885-87 ...	Σ_1 ...	+0·3601 ...	+0·2656 ...	+0·4303	73
1885-87 ...	Σ_2 ...	+0·3808 ...	+0·2636 ...	+0·4199	73

It would appear from this that Comparison-star No. 2 has a sensible parallax relative to Comparison-stars Nos. 1 and 3, and we hope that Dr. Lamp will proceed to investigate it independently. Meanwhile, combining the results obtained from the three stars, the mean parallax is—

$$\text{for } \Sigma_1 = 0''\cdot3520 \pm 0''\cdot0140$$
$$\text{and for } \Sigma_2 = 0''\cdot3548 \pm 0''\cdot0131,$$

or the mean parallax of the system Σ 2398 = 0″·353 ± 0″·014.

NEW MINOR PLANET.—A new minor planet, No. 271, was discovered on October 13 by Dr. Knorre, of Berlin.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 OCTOBER 30—NOVEMBER 5.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on October 30

Sun rises, 6h. 52m.; souths, 11h. 43m. 46·2s.; sets, 16h. 36m.: right asc. on meridian, 14h. 17·7m.; decl. 13° 47′ S. Sidereal Time at Sunset, 19h. 11m.

Moon (Full on October 31, 22h.) rises, 16h. 38m.; souths, 23h. 12m.; sets, 5h. 58m.*; right asc. on meridian, 1h. 47·3m.; decl. 6° 0′ N.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. and declination on meridian. h. m.	° ′
Mercury.	9 21 ...	13 16 ...	17 11 ...	15 49·9 ...	23 5 S.
Venus ...	3 2 ...	9 8 ...	15 14 ...	11 41·4 ...	0 23 N.
Mars ...	1 21 ...	8 13 ...	15 5 ...	10 46·2 ...	4 20 N.
Jupiter ...	7 31 ...	12 14 ...	16 57 ...	14 48·3 ...	5 17 S.
Saturn ...	22 15*...	6 2 ...	13 49 ...	8 34·7 ...	19 2 N.
Uranus...	4 42 ...	10 20 ...	15 58 ...	12 53·9 ...	5 5 S.
Neptune.	17 36*...	1 18 ...	9 0 ...	3 49·8 ...	18 18 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Occultations of Stars by the Moon (visible at Greenwich).

Oct.	Star.	Mag.	Disap. h. m.	Reap. h. m.	Corresponding angles from vertex to right for inverted image. ° °
30 ...	33 Ceti	6 ...	0 58 ...	1 44 ...	193 275
30 ...	35 Ceti	6½ ...	1 54 ...	3 ...	149 325
Nov.					
1 ...	μ Ceti	4 ...	2 34 ...	3 38 ...	109 355
3 ...	B.A.C. 1351 ...	6½ ...	4 19 ...	4 30 ...	217 235
3 ...	75 Tauri	6 ...	6 41 ...	7 21 ...	89 5

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·3 ...	81 16 N. ..	Nov. 2,	3 10	*m*
o Ceti	2 13·6 ...	3 29 S. ...	,, 5,		*M*
T Arietis	2 42·0 ...	17 2 N. ...	,, 4,		*m*
η Geminorum ...	6 8·1 ...	22 32 N. ...	Oct. 30,		*M*
ζ Geminorum ...	6 57·4 ...	20 44 N. ...	,, 30,	4 0	*m*
			Nov. 3,	4 0	*M*
U Hydræ	10 32·0 ...	12 48 S. ...	,, 3,		*m*
S Coronæ	15 16·8 ...	31 47 N. ...	,, 3,		*m*
V Ophiuchi	16 20·5 ...	12 10 S. ...	Oct. 31,		*M*
W Herculis	16 31·2 ...	37 34 N. ...	Nov. 4,		*m*
U Ophiuchi	17 10·8 ...	1 20 N. ...	,, 2,	2 23	*m*
			and at intervals of 20	8	
β Lyræ...	18 45·9 ...	33 14 N. ...	Nov. 2,	22 0	m_2
η Aquilæ	19 46·7 ...	0 43 N. ...	,, 1,	0 0	*m*
W Cygni	21 31·8 ...	44 52 N. ...	,, 1,		*m*
δ Cephei	22 25·0 ...	57 50 N. ...	,, 3,	3 0	*m*

M signifies maximum; *m* minimum; m_2 secondary minimum.

GEOGRAPHICAL NOTES.

LIEUT. WISSMANN, the well-known African explorer, has arrived at Brussels from his journey across Africa. He was accompanied as far as Nyangwé by Lieut. Le Marinel, the route followed being different from that traversed by Wissmann on his first journey. He did not, however, succeed in penetrating the region to the north of the Sankuru, nor in reaching the somewhat mysterious Lake Lundi.

In the *Bulletin* of the Hungarian Geographical Society, vol. xv. fasc. 7, will be found an interesting paper (abstracted in French) on the struggle for existence among plants in the Hungarian Pusztas (steppes).

To the *Bulletin* of the Belgian Geographical Society, No. 4, 1887, M. Louis Navez contributes an instructive paper on the influence of the various geological formations in Belgium, especially upon the people. M. Navez points out that Belgium is specially favourable for such an investigation. In France and Germany the great differences of altitude and latitude, and the diversity of climate, determine phenomena often difficult to distinguish from those which are really due to geology, and are therefore causes of error. In Belgium the differences of altitude, of latitude, and of climate are not of so much importance. The influence of the character of the soil is almost always preponderant, and is easily distinguished. One instance may be given. The great quantity of lime which the Cretaceous soil of the Geer valley contains gives to the straw of the cereals a special suppleness, strength, and whiteness. From this straw are manufactured plaits which have a large sale, and in Paris are ranked next to the straw of Italy for ladies' bonnets. This manufacture is worth from four to five million francs yearly. On the other hand, the absence of calcareous salts in the ground traversed by the feeders of the Lys, render that river eminently suited for the cleansing of flax; hence the fame of the cloths of Flanders. M. Navez believes that the facts he brings together prove that the geological construction of the ground is one of the factors that limit the free will of man and have an active influence on communities.

In Dr. Oscar Baumann's paper on Fernando Po, in No. ix. of *Petermann's Mitteilungen*, the author states that the volcanic group of which the island is a member, forms a line running south-west from the Cameroons, and may be regarded as the result of an eruptive fissure, which on the one side extends from the Cameroons to the island of Annobon, and on the other appears to find in the Rumbi Mountains a continuation into the heart of Africa. The northern half of the island is covered almost entirely by the huge volcanic peak of O-Wassa (Clarence Peak). After careful estimation he gives the summit a height of 9350 feet. The volcano may be regarded as extinct, the fire and clouds of smoke seen at times on the summit being easily explained by the annual burning of the grass. The crater on the top of the mountain is 515 feet in depth, and is inclosed by gray disintegrated walls of basalt. On the thickly wooded and almost impassable slopes are many subsidiary craters. The basaltic rock of the east coast is being gradually crumbled away by the sea, while along the west coast the land is gaining on the sea. The physical geography of the southern half of the island is determined by the mountain range of the "Cordillera of Fernando Po," which in two chains connected by a pass runs practically east and west. These chains culminate in several summits, which have a volcanic character only on account of their basaltic composition. In the south of the island, and apparently quite independent of the Cordillera, there rises a lofty volcanic mass. On the top of one cone-shaped peak, precipitous on all sides, there extends a flat basin surrounded by a circle of hills. This the author supposes to be the remains of a large crater.

M. A. J. Wauters writes a long article in the new number of the *Mouvement Géographique* to prove that Lake Muta-Nzigé, the somewhat problematical lake to the south of Albert Nyanza, belongs to the Congo system, and not to that of the Nile. The latitudes and other data which M. Wauters has on which to base his conjecture are of the scantiest, and extremely doubtful. It seems to us that such conjectural arm-chair geography is a useless waste of time and space. The question of the relation between the Muta-Nzigé and the Albert Nyanza can only be solved by actual exploration. By this time no doubt it has been solved either by Emin Pasha or Stanley, and M. Wauters might therefore put the valuable space of his small journal to a much more profitable use.

METEOROLOGICAL NOTES.

The Hydrographer to the Admiralty has issued a circular stating that the United States Government has given notice that from September 1, 1887, the following storm signals (consisting of day signals of two kinds, also night signals), would be shown on the shores of the Atlantic Ocean, Gulf of Mexico, and Great Lakes, as storm conditions may demand, taking into consideration the fact that westerly winds of high velocity with clearing weather are less dangerous than those from easterly quarters with freezing weather: (1) cautionary signal, a *yellow flag with white centre*, will indicate the winds expected are not so severe but that well-found and seaworthy vessels can meet them without great danger; (2) storm signal (now in use), a *red flag with black centre*, will indicate that the storm is expected to be of more marked violence. In order to afford as exact information as possible regarding the relative positions of the storm and the winds expected, two pendants will be displayed: a *red pendant* will indicate easterly winds, from north-east to south inclusive, and that the storm centre is approaching; a *white pendant* will indicate westerly winds, from north to south-west inclusive, and that the storm centre has passed. Whilst it is intended that the pendant shall indicate positively only whether the winds will be easterly or westerly, yet in order to give still more definite information, the *red* or *easterly pendant* will be hoisted above the cautionary or storm signal, for winds from the north-east quadrant, and below for winds from the south-east quadrant. Also, the *white* or *westerly pendant* will be hoisted above the cautionary or storm signal for winds from the north-west quadrant, and below for winds from the south-west quadrant. Because of the difficulty of varying night signals, they will not distinctly show the force, but indicate the direction of the wind only; a *red light* for easterly winds, and a *red and white light* for westerly winds.

The Pilot Chart of the North Atlantic Ocean, issued by the Hydrographic Office at Washington for the month of September, includes a valuable article on the law of storms, considered with special reference to the approaching season of West Indian hurricanes. The article is accompanied by two diagrams illustrating (1) the circulation of the wind in a tropical cyclone in the northern hemisphere, showing clearly how the wind is drawn in towards the centre of low barometer, and its direction at any point; and (2) a diagram for practical use in finding a ship's position relatively to the centre of the hurricane, by means of the direction of the wind and fall of the barometer. The circles in this diagram are normal isobars, and represent the rate at which the barometer falls as the centre of the storm is approached, so far as our latest knowledge of cyclones can be safely used as a general guide. Full directions are given for the practical use of these diagrams. The Pilot Charts contain frequent extracts from ships' logs, as to the great value of the use of oil in heavy seas, and the Hydrographic Office considers that the testimony is so conclusive that its use is now recognized by every commercial nation. The Charts contain information about winds, currents, fog, and the position of dangerous derelicts, and their speedy dissemination must prove to be of great utility to the maritime community generally.

The *Annalen der Hydrographie und maritimen Meteorologie* for August contains a graphical representation of the distribution of rainfall in the Atlantic and Indian Oceans, compiled by Dr. W. Köppen from materials at the disposal of the Deutsche Seewarte and all other available sources. The latitude is shown in the vertical, and the months in the horizontal direction; each point of the area of the diagram therefore indicates a definite time of the year, and a definite distance from the equator, and the curves show the equal percentages of days of rainfall. The diagrams are explained in the text, and show the relatively large rainfall frequency in the higher latitudes of the North Atlantic in all seasons, but especially during winter; further southwards, towards the region of the trade-winds, all months become drier, but north of the tropics rain falls on an average once in every 5–10 days. In the South Atlantic the conditions are more complicated. From 0°–5° S. autumn rains are prevalent, and from 5°–14° S. the winter rainfall maximum is prominent. In the Indian Ocean the equatorial rain-belt is considerably widened, especially on the southern side. From 2° N. to 12° S. the rainfall frequency does not fall below 50 per cent. in any month, and from June to October it exceeds 70 per cent. Between 10° and 12° S., and also from 25°–30° S. the spring-time is driest; while from 33° S. the rainfall increases rapidly in winter and spring. South of 40° S. it rains eight days out of ten during July and August.

Symons's Monthly Meteorological Magazine for September contains interesting articles on the deficiency of rainfall this year and on the definition of drought. From a comparison

of the rainfall at 27 stations from January 1 to August 31, 1870-79, and for the same period in 1887, and expressing the difference of 1887 from the average as a percentage, it appears that the mean percentage this year has been : for England 59·8, for Wales 60·0, for Scotland 68·5, and for Ireland 60·3. Out of 11 stations in England the drought is unprecedented at 6, in Scotland at 6 stations out of 8, and in Ireland at 5 stations out of 6. Scotland has suffered least, and in parts of England the deficiency has reached the extreme limit that may be expected. There appears to be no good definition of a drought. In 1880 Mr. Symons adopted a classification for his numerous observers, which has generally been used up to the present time, viz. (1) absolute drought, being a period of 14 or more consecutive days without rain ; (2) partial drought, being a period of 28 or more consecutive days in which the total rainfall did not exceed 0·25 inch. But engineers speak of droughts varying from 140 even to 240 days ; these can bear no comparison with the above definitions, and Mr. Symons suggests a third term, viz. long drought, being a period of not less than 60 days with a total rainfall of not less than 2 inches. Opinions are wanted as to how the records of rainfall observers may be best utilized in the form most useful to engineers.

Mr. J. W. Oliver has contributed an article to *Longman's Magazine* for October on the moon and the weather, in which he discusses some of the most important of the popular predictions in which the moon is concerned. He deals (1) with the lunar notions that are utterly absurd, and (2) with those that are explicable by the aid of physical principles. The conclusion at which he arrives is that there is more nonsense than sense in lunar predictions, but that it is unfair to consider the whole subject as unworthy of serious treatment. For instance, atmospheric tides due to the moon's attraction must exist, although generally obliterated by disturbances due to other causes. The author apparently favours Sir John Herschel's statement of the tendency of the full moon to clear the sky, and in support of that theory he quotes the experiments of Melloni and others, showing that moonlight contains a minute proportion of dark heat rays, the effect of which may in a certain measure cause the dispersion of the clouds. The lunar halo is also referred to as an old sign of bad weather. Of 61 halos observed near London 34 were followed by rain within 24 hours and 19 within four days. In the *Mittheilungen aus dem Gebiete des Seewesens* for June, published by the Hydrographic Office of Pola, Capt. C. von Bermann has an article upon the same subject. He deals chiefly with Herr Falb's attempt to reinstate the moon's influence in his work "Das Wetter und der Mond." The result arrived at is that, although the moon has an influence on the weather, it is too infinitesimal, compared with other influences, to be appreciable.

The Annual Report of the Meteorological Observer for Tasmania for the year 1886 gives the results of observations taken at 11 stations and rainfall reports from 37 stations.

THE SIXTH INTERNATIONAL CONGRESS OF HYGIENE AND DEMOGRAPHY IN VIENNA.

(FROM OUR OWN CORRESPONDENT).

THE most important questions dealt with at the Vienna Congress were those relating to preventive medicine, a branch of medical science which originated with Edward Jenner's discovery of immunity from small-pox by means of vaccination. The high value of vaccination and re-vaccination was clearly shown in the Demographic Section of the Congress by statistical tables exhibited by T. Körösi, the Director of the Statistical Office in Buda-Pesth. According to these tables the mortality of the not-vaccinated patients treated in nineteen Hungarian small-pox hospitals was 800 per cent. larger than that of the vaccinated patients, while the receptivity for getting the disease was three and a half times larger in the not-vaccinated as compared with the vaccinated people. In the Fourth Section the question of vaccination was also submitted for discussion by a lecture delivered by a Turkish delegate, Dr. Violi, and a resolution recommending to all Governments the introduction of compulsory vaccination was unanimously adopted.

Thanks to the valuable discoveries of Pasteur, the method of protecting the life and health of men as well as animals by vaccination has been worked out more extensively, and is now applied with the best success against various desolating and destructive diseases. The beneficial effects of the various methods of preventive inoculations, the amount of saving of human and animal life brought about by their use, will easily be perceived by a sketch of the discussions which were held at the last meetings of the Third Section of the Congress.

A special sub-section was formed where the special results obtained by the preventive inoculations in splenic fever, and erysipelas of the pig, were reported and discussed upon. Dr. Lydtin, of Carlsruhe, gave an interesting account of the development of the measures against the different plagues, alluding also to the defects of repressive measures, by which cattle-trade and cattle-breeding are so severely affected. While two centuries ago the first trials made of providing immunity against sheep-pox and other plagues were based on purely empirical views, the recent bacteriological discoveries have led to the scientific method of vaccination with an attenuated virus. Dr. Chamberland. Pasteur's assistant, reported on the results obtained by the preventive inoculations against (charbon) splenic fever in France, Hungary, Italy, and Russia. In the year 1886, 367,208 of sheep and 47,229 of cattle had been subjected to anti-charbon inoculations. Chamberland's statements were considerably supported by the results obtained with Pasteur's methods in Switzerland and at Pakisch, Prussia, which were communicated by Dr. Custer of Zurich and Dr. Lydtin (Carlsruhe). On the other side, the practical value of the preventive inoculations in splenic fever was severely contested by Dr. Löffler, of the Berlin School. An animated discussion on this subject went on for three hours and a half.

By the majority of the speakers it was pointed out that no loss by death—or only a small percentage of such loss—was caused by the inoculation itself in cattle, and that the number of cases of natural splenic fever decreased very much after the preventive inoculations; but in sheep the vaccinations against charbon were not equally favourable. As to the duration of the immunity acquired by the vaccination no positive statements could be made.

Then the usefulness of preventive inoculations against charbon symptomatique (Rauschbrand) was dealt with, and was acknowledged by all the speakers. In the course of the discussion on the preventive inoculations against erysipelas in the pig—a discussion in which Drs. Lydtin, Chamberland, and Prof. Czokor of Vienna, took part—it was stated that by the inoculations the animals get immunity against the disease, but that further improvements of the method are wanted, as there is now a considerable loss of inoculated animals by the vaccination itself, and sometimes the spreading of disease in healthy animals. In conclusion it was resolved on a vote that the experiments on preventive inoculations against splenic fever, charbon symptomatique, "erysipelas of the pig," and the other epizootics ought to be continued under the control of and assisted by Governments.

The battle-ground of the Congress from a scientific point of view was the discussion on the preventive inoculations against rabies. Dr. Chamberland gave a *résumé* of the development of Pasteur's method, and the results obtained by this treatment in France. 2682 persons were treated and the average mortality was only 7·5 per cent.; while the mortality of persons bitten by rabid animals if not inoculated varied between 5 and 30 per cent., according to the statements made by different authors. In answering the objections made by some recent writers who had expressed the opinion that the death of patients from hydrophobia was caused by the inoculations themselves, he maintained that it was proved by experiments made with the spinal cord of those persons that the death was caused by the bite of the rabid animal, not by the inoculations. If animals were inoculated with brain emulsions of those persons they became rabid, but they died on the fourteenth or fifteenth day after inoculation, while they would have succumbed on the seventh day if the death of the persons had been caused by the inoculated virus.

Dr. Bordoni-Uffreduzzi of Turin, in making experiments on dogs and animals, found that various parts of the brain of rabid animals show different degrees of virulence. The pancreatic gland is nearly as virulent as the brain, while the liver and spleen seem to act much less strongly as media of the virus. He had also treated 119 persons bitten by rabid dogs ; three of them died.

Dr. Emmerich Ullmann, assistant of Prof. Albert of Vienna, has performed preventive inoculations in 122 persons. Only persons who could prove by documents the rabid condition of

the biting animal were submitted to the treatment, and only the weak inoculations were used. The rabid condition of the biting animal was in all the cases proved either by experiments with the brain of the animals or by *post-mortem* examinations. Out of the 122 patients treated, whose ages varied between fourteen months and sixty-one years, three died; the rate of mortality being 2·4 per cent. In fourteen cases the wounds were on the head or in the face. To this series belonged two deaths; while in seventy-two cases the upper extremity was the seat of the wound, in which group the third death occurred. As Pasteur had laid great stress on the previous cauterization of the wounds, Dr. Ullmann was carrying out some experiments to examine the influence of cauterization. Animals were infected subcutaneously with the virus of rabies, and then treated with cauterizations by lapis infernalis or fuming nitric acid. All these animals died from rabies, even when the cauterization was applied immediately after the infection, as could be shown by experimental inoculation of their brains. Only two animals cauterized on the spot with the Paqueline (ferrum candens) remained healthy. The eminent value of the preventive inoculations could easily be ascertained by some of the cases treated by Dr. Ullmann. In *Rzeszow*, a Polish village, five persons were bitten by a rabid dog. Three of them were brought to Vienna, where they were treated by Pasteur's inoculations; they remained healthy, while the two others remaining at home not inoculated died from hydrophobia after two weeks. Also in some other cases, persons bitten by rabid dogs but not submitted to Pasteur's treatment died, while the inoculated persons bitten by the same animal were not attacked by the disease. These experiments seem to be crucial experiments, proving clearly the usefulness of Pasteur's method.

It may be also stated that Dr. Ullmann had to defray the expenses for his experiments and for the inoculations from his own pocket, as the Austrian Government refused to grant any subvention for carrying out Pasteur's experiments, "on account of the frequent mishaps"! It is impossible to say from what source the Government obtained knowledge of these mishaps, as it did not try to get any information on the matter either in Paris or in Vienna. On the other side, the Hungarian Government has acknowledged the high merits of M. Pasteur by conferring on him the Order of the Iron Crown.

Dr. von Frisch criticised the statistical data given by Pasteur and Chamberland. He argued that there should be a decrease of cases of rabies if the method were successful; but the number of cases has increased. The "virus fixe" is an inconstant power, the period of incubation after its inoculation being a various one, not restricted to seven or eight days. Inoculation after previous infection does not provide certain immunity. The compulsory inoculation of dogs should be introduced to prevent rabies in man.

Prof. Metschnikoff, the celebrated Russian biologist, reported on the results obtained at the Bacteriological Station of Odessa. 713 persons, bitten by rabid animals, had been treated by Dr. Gamaleia. At first the results obtained were unfavourable, too weak emulsions having been used for the inoculations, as was proved by later experiments. Since July 1886, 532 persons were treated by the intensive method. In 137 out of these 532 cases, emulsions weaker than of two-days' incubations were injected; 9 persons died, the rate of mortality being 6·5 per cent. Of 88 persons inoculated once with a two-days' virus, 2 died (*i.e.* 2·3 per cent.); while among the last 307 cases, treated at least twice with a two-days' emulsion, only 2 deaths (0·6 per cent.) occurred. The average mortality in all these 532 cases was 2·4 per cent. The value of the intensive treatment could easily be understood in the case of persons bitten by rabid wolves. Of 36 persons bitten by rabid wolves, 6 were subjected to the weak treatment, and 2 of them died; the other 30, inoculated with two-days' virus, remaining healthy. In some cases even the one-day's virus was used with the best results; *e.g.* among 5 persons bitten by rabid wolves, inoculated six times with one-day's virus, the outbreak of rabies had been prevented. Experiments on 1500 animals were carried out by Dr. Bardach, who was able to prove the correctness of Pasteur's statements regarding the seven-days' period of incubation of his virus fixe. In some cases, however, the period of incubation was found to be prolonged; but the prolongation was caused by some other parasitic diseases of the animals, which will be described by Dr. Bardach in a forthcoming paper. In concluding his lecture, Prof. Metschnikoff stated that "the results obtained at the Odessa Bacteriological Station are very strongly in favour of Pasteur's discoveries, which decidedly must be regarded as epoch-making."

Dr. De Renzi (Naples), described his experiments on rabbits, in which, by the injecting of brain-emulsion into the blood-vessels, rabies had been produced. But neither by this method nor by subcutaneous injections of the virus fixe was he in every case able to produce rabies in rabbits, which sometimes showed immunity against this infection. He expressed also the opinion that no gradual difference of the attenuation of the virus seems to exist.

In summing up the different views of the speakers Dr. Chamberland was able to state that the high value of Pasteur's discoveries was acknowledged by everybody.

THE MINERAL WEALTH OF THE UNITED STATES.

ON Saturday, October 8, a lecture on this subject was delivered at the American Exhibition by Dr. A. E. Foote, of Philadelphia, who, as Acting Commissioner from Pennsylvania, has exhibited a fine collection illustrating the mineral resources of that great State. The Chairman, Mr. F. W. Rudler, President of the Geologists' Association, congratulated the audience upon being able to listen to one who, from his very extensive and personal observation, was well qualified to treat such a subject.

Dr. Foote pointed out that the geological formation of a country was the basis of its mineral wealth, and illustrated this by the remarkably fine large geological map of the United States compiled by Prof. Hitchcock from the work done by the United States and State Geological Surveys. His statistics were mostly based upon Williams's "Mineral Resources," of 1885.

As a Pennsylvanian he was happy to say that of 95,000,000 of tons of coal mined in that year nearly two-thirds the tonnage, and fully two-thirds the value, was produced in Pennsylvania. Of this amount 34,000,000 of tons was anthracite. The Girard Trust, that noblest of America's educational charities, exhibits a mass of anthracite from their mines in Schuylkill County, Pennsylvania, that measures 22½ cubic feet, and weighs 2256 pounds. Coal is now found in workable quantities in thirty States and Territories. Of iron there was less in value mined in 1884 than the value of the petroleum—£4,000,000 sterling—but with the steady revival of business the production has rapidly increased, until this year it may reach a total of £6,000,000 sterling. The iron area is being developed almost as rapidly as the coal. The greatest activity has been manifested in Alabama, near Sheffield and Birmingham. The latter city is the most wonderful example of rapid, solid business development that America has ever seen. Perhaps nowhere else in the world can such a favourable combination of coal, iron, and limestone be found.

But for the remarkable development of natural gas near Pittsburg, far more of Pennsylvania's capital would have been transferred to this favoured locality. Natural gas was first used for lighting the town of Fredonia, New York, in 1825, and this well, still in operation, was pronounced by Humboldt an eighth wonder of the world. Here, however, it was used simply for lighting, but it is far better suited for heating, owing to the absence of the heavy hydro-carbons. It is principally marsh-gas, ethane, hydrogen, and nitrogen. In October of 1875 it was first used in the smelting of iron at the mills of Spang, Chalfant, and Co., at Etna, near Pittsburg. The gas was brought 17 miles from the "Harvey," the leading well of the region. When turned into the 6-inch iron pipe the pressure was so great that it travelled the entire distance in twenty-two minutes. Mr. Foote visited the works in December 1875, and it was already in full and successful operation, turning out the purest iron, equal to the best Swedish. Its success was so striking that it was almost instantly introduced into all the iron, glass, and other manufactories of this great centre of America's industry. It is also used in all the heating and lighting of the city. The cost is about one-quarter that of coal, and it is estimated that it will this year take the place of coal to the value of £6,000,000 sterling. Not the least of its advantages is its freedom from smoke, so that what was once an unbearably dirty city now has air as pure as that of a country village. The enormous waste that has been going on is illustrated by the history of the "Haymaker" Well, No. 1, which in five years wasted £400,000 worth of gas, or nearly £200 daily. There were hundreds of such wells.

Gold and silver were touched upon very briefly, as others who were

present were expected to speak upon them. The production of gold was £6,500,000. Of this the larger portion was from quartz-mining, the placer production having very much fallen off owing to the unfavourable legislation in California. The speaker referred to the tellurides as true ores of gold, and predicted that while at present they were found in large quantities only in Colorado, they would be found in much greater quantities north and south. The production both of gold and silver would undoubtedly be increased as transportation facilities were increased in localities now nearly inaccessible. The placer mines of North Carolina and Georgia had, he believed, a great future before them. A successful process was required for working the enormous dry deposits of Arizona and New Mexico without water. The silver production is over £10,000,000 sterling. The rich deposits of Leadville (Colorado), the Comstock mines in Nevada, the Silver King and other localities in Arizona, Silver City and Lake Valley (New Mexico), were referred to. At the latter a chamber was found not much larger than a common room from which £100,000 worth of ore was taken. It was chloride, so rich that it would fall in drops of nearly pure silver with the heat of a lighted match.

The American copper mines are the richest in the world; the production being over £4,000,000 sterling, of which the "Calumet and Hecla" claims one-fifth. They have over seventeen years' supply in sight, and practically the supply is inexhaustible. The "Anaconda" of Montana, and neighbouring mines, also produce about one-fifth. These localities are so rich, so easily worked, and above all so accessible, that the extraordinarily rich mines of Arizona, where transportation is much more costly, have suffered severely by the competition. The mines near Clifton (Arizona) produce a charming combination of malachite and azurite, which is one of the most beautiful ornamental stones ever seen. At the Copper Queen mine malachites fully equal to those of Russia, and azurites as finely crystallized as those of Chessy, France, are found.

The lead production is of the value of £2,100,000 sterling, most of which is a by-product of the Rocky Mountain silver mines. This production is so great that it has rendered mining for lead alone unprofitable at the wonderfully rich South-West Missouri region. A very interesting fact is that the vanadates and molybdates of lead replace the phosphates in all the western region. As vanadium salts are rapidly increasing in importance in the manufacture of the aniline dyes, this will be a future source of wealth, for there are mines here that can produce more vanadium than all the rest known in the world. The molybdates from Arizona and New Mexico are the finest ever seen.

The production of zinc was £1,000,000 sterling, three-quarters of which is found in South-West Missouri and Kansas. Four railroads have been constructed within a few years to carry away the rapidly-increasing production. The zinc mines of Franklin, New Jersey, have been known for over a hundred years and are remarkable not only for their richness but for the extraordinary number of rare and beautiful species that are found there and nowhere else in the world. The localities of mercury, nickel, manganese, tin, chromium, platinum, and other metals were also spoken of.

Among non-metallic minerals the phosphates of South Carolina were the principal, over £750,000 sterling being produced annually for fertilizers. Vast deposits of gypsum or plaster-stone are found in Michigan, Ohio, New York, and other localities. Mica is principally mined in New Hampshire and North Carolina. This is the mineral popularly but erroneously known in England as talc. Talc is a very different material, also found in North Carolina, but used for the tips of gas-burners and as a lubricator. The principal use of mica is for the windows of coal-stoves. Many very interesting minerals occur in connexion with the mica, such as beryls of enormous size, emeralds of great value, garnets, and so forth.

The time was so fully taken up with the ores and economic minerals that the gems and ornamental stones were treated of in a lecture delivered on October 22.

THE EXPLORATION OF NEW GUINEA.

ON August 11 (p. 351) we reprinted from the *Sydney Morning Herald* an account of an exploring expedition in New Guinea, conducted by Mr. Theodore Bevan in the steamer *Victory*. Another Australian paper, the *Daily Telegraph* of July 9, gives, with the map which we reproduce, a more detailed narrative, compiled from Mr. Bevan's notes. From this narrative we take the following passages:—

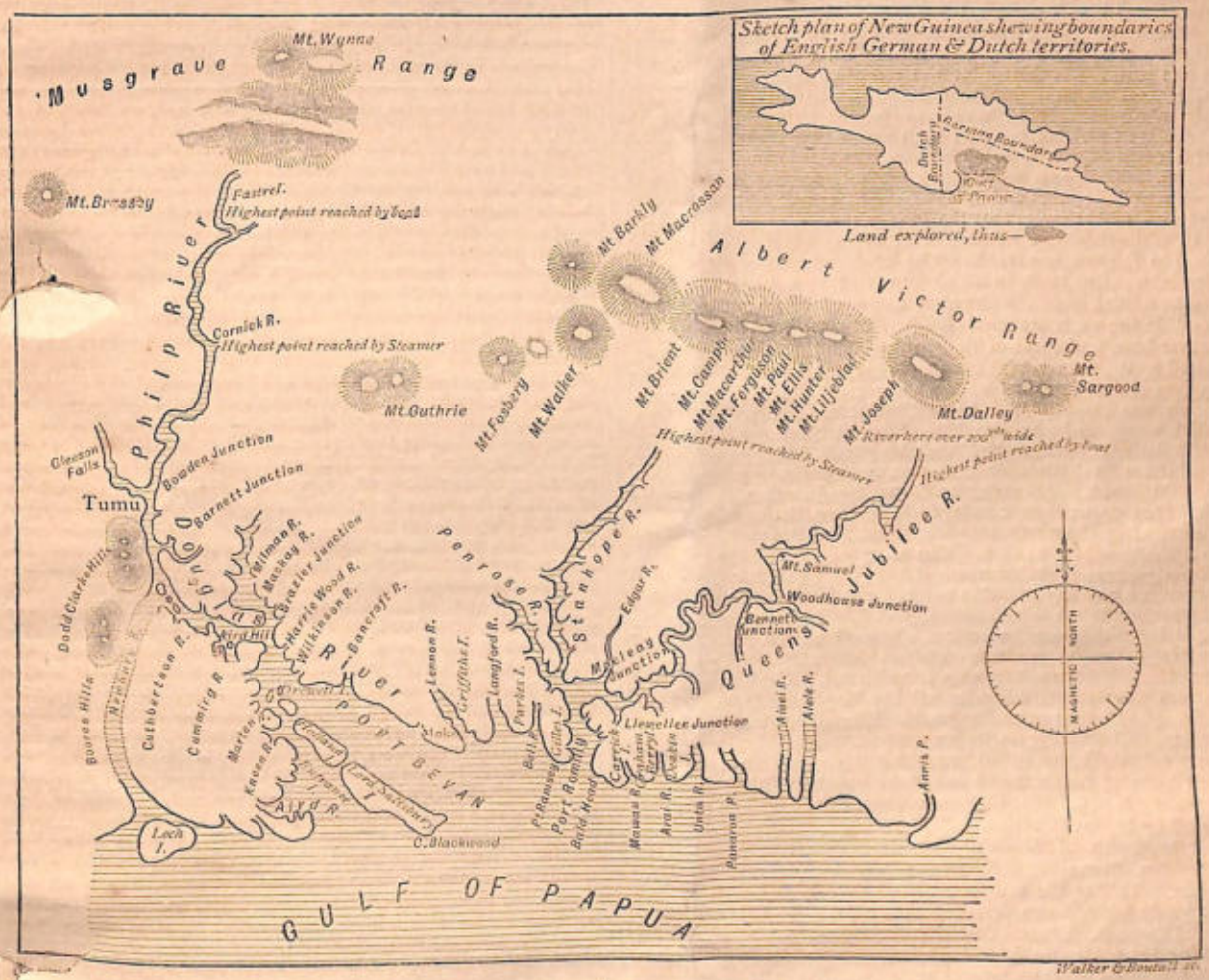
The *Victory* left Thursday Island at 5.30 a.m. of Thursday, March 17, and was headed for Cape Blackwood at the mouth of the Aird River, New Guinea, and distant 220 miles. On the following Saturday, early in the morning, the distant mountains of Papua were sighted, and at 6 o'clock Cape Blackwood was seen. Shaping a course after passing inside the cape to the north of Entrance Island and steering past it, the party found that the River Aird narrowed to about 200 yards, and after steaming from Entrance Island a distance of 5 miles, about 4 p.m. they came to a broad but seemingly shallow stream running into Deception Bay. (Deception Bay is the open space shown on the map between Cape Blackwood and Bald Head.) . . . It was found that the country between Deception Bay and the Aird River was made up of islands instead of being mainland as charted by Blackwood. Further, from a great number of water-ways and river openings on every side, it became evident that this was nothing less than the delta of a large fresh-water river, whose source was in the mountains seen from the coast. Steaming up this river, leaving alluvial scrub-covered islands to the left, and passing broad streams eachover half a mile wide on the right, the party found that deep water was carried to an important junction (named after Mr. John Brazier, of the Australian Museum) right under and to the south-east of Aird Hills. Here the river threw off two branches, one skirting the hills to the south and the other bearing north by east. Brazier Junction was found to be distant, as the crow flies, 30 miles from Cape Blackwood. . . . A portion of the party proceeded in the whale-boat for a distance in a north-westerly direction of about 6 miles, when to their surprise a wide branch of the river was opened up north by west, two important openings towards the south-west being also seen and named after Mr. Cuthbertson and Mr. Cosmo Newbery. It was becoming more and more evident that the main stream of a delta in a large fresh-water river had been reached, the southerly-running channels being divisions, and the Cuthbertson and Newbery Rivers probably discharging their vast volumes of water into Prince George's Inlet several miles westward of Cape Blackwood. This main stream was explored for some 5 miles, the depth being from 2 to 7 fathoms. The country around was of alluvial formation, and scarcely above the level of the river, but thickly covered with virgin forest, the trees reaching a height of 150 feet, and crusted with mosses, fungi, creepers, and orchids in tropical luxuriance. A magnificent view was obtained at one part of tier upon tier of ranges of hills to the northward, and behind these blue mountain peaks of from 7000 to 8000 feet in altitude, and from 40 to 50 miles distant. The river itself in the various branches looked like an immense lake studded with islands. This main channel the steamer proceeded up on the day following, and a second range of low palm-clad hills observed in front on the previous day proved to be the head of the delta. This was named after Mr. J. V. S. Barnett, of Cooktown, Queensland. This spot, as the crow flies, was 45 miles from Cape Blackwood, and only now could it be properly stated that *terra firma* had been reached, for south of them the soil was alluvial, being brought down from the mountains by this great stream, the volcanic cones of Aird Hills, rising to a height of 1620 feet to the south-east, being the solitary exception in these miles of dead-level, scrub-covered, deltaic alluvial flats. North of Barnett's Junction the river flowed between compact banks, through gradually rising country, and with an exclusive fresh-water current.

On March 25, after proceeding up the river a few miles, a third series of palm-topped conical hills were seen, and on the summit were two of their native houses, being about 200 feet in length. It was soon evident that the strange apparition of the steamer gliding into these fastnesses was visible from the shore, as the mellow sound of the conch shell was heard, warning the inhabitants of the scattered village of danger. Slowly the steamer approached, and when abreast of the village and opposite a creek some canoes full of savages were seen scuttling up the place in abject terror. The river now gradually widened out, and two large tributaries, running one from the north-west and the other from the north-east, were seen. This junction was observed to be in latitude 7° 11′, and 144° east longitude. . . . The junction of these rivers was named after Mr. V. R. Bowden, of Thursday Island, the north-west and north-east branches being named after Messrs. Burns and Robert Philp, while the great river running from Bowden Junction into Deception Bay, a distance of over 60 miles, was called after the Hon. John Douglas.

The steamer was taken cautiously up Burns River, the north-west branch, and a splendid view of near mountain ranges was obtained, and apparently a splendid spot to explore. . . . Seven miles up the river an anchorage was come to, the river being over 300 yards wide, and the soundings being from 2 to 6 fathoms. The scenery was picturesque in the extreme. Hills of from 600 to 1000 feet, clothed with verdure, came down to the water's edge. There were cedars, oaks, eucalypti, fig-trees, acacias, pines, palms, and tree-ferns. Feathery bamboos, ferns and varied flora adorned the river banks. Butterflies of gaudy hue, and birds of the brightest plumage, flutter in and out amongst the trees and shrubs. The water was placid and in the deepest recesses of the gorge-like ranges was sombre and cold. . . .

A few miles further up the river a small rapid was passed, and it was then found that there was a break, one arm apparently running in a northerly direction towards ranges about 4 miles distant, and from 2000 to 3000 feet high, and the other arm stretching easterly towards high distant ranges, which closed in the horizon in that direction also, but they appeared to be about 25 miles off. This junction was named after the *Victory*, but the steamer could not pass it to any distance. The highest point reached by the steamer was ascertained to be in south latitude 6° 51′, and east longitude 144° 8′, that is to say 65 miles in a straight course from Cape Blackwood, or nearly 90 miles by river courses. This was on March 31.

A boat party was formed of eleven, and a week's provisions put in, but progress was slow, owing to the strong currents, the rapids, and the heavy rain that now came on in the evening. . . . The highest position inland reached by the whale-boat was within 25 miles of the German boundary, or 80 miles as the crow flies south-by-west of Cape Blackwood, and over 100



miles by river courses. This was in south latitude 6° 39′, and east longitude 144° 11′. It appeared to Mr. Bevan that the natural boundary or water-parting between the river systems of the two territories might be found to exist a few miles to the north of the present German boundary. . . . The nature of the coast for several miles to the westward was now known, and it remained [after return to Deception Bay] to continue explorations to the eastward, and also to settle the point as to whether from so far in the heart of Deception Bay there existed a deep water channel leading out into the Gulf of Papua. Mr. Bevan decided to test this question. Although there were the river openings between them and Bald Head, it was unlikely that there would be room for any considerable river between the newly-discovered Douglas River and the large river of which it had been for years reported that the five openings east of Bald Head were separate mouths. Mr. Bevan first proceeded to Motu Motu, situated at the mouth of the Williams River on the eastern-most boundary of the gulf, and 100 miles distant, in order to send off despatches. A start was made from Motu Motu on Monday, April 11, and after calling at Karama, Silo, Namai, and Vailalla, they anchored off Orokolo, in rather dirty weather. On April 14 the steamer was within a few miles of the Alele River, or first of the five openings east of Bald Head, reported by the natives as leading into one large river. Upon superficially examining the entrances to these five rivers, namely, Alele, Aivei, Panaroa, Unta, and Arai, no safe channel for the steamer could be found, owing to the heavy break on each bar. Off the next opening, or Marwan River, the same conditions were experienced, and there now remained but the wide entrance marked on the Admiralty chart. Towards this opening the steamer was steered, and carefully proceeded in with a 2½ fathoms channel at low water, leading half a mile to the west of Bald

Head, an anchorage being found 2 miles within in 15 feet of sheltered water. Point Ramsay (named by Mr. Bevan) was 3 miles to westward, over an unbroken stretch of water, which also ran far inland to the north. This was an important discovery, as no ship had before been within this opening, and the *Victory* had again passed the confines of the known. In a little bight under Bald Head a village was discovered partly hidden and sheltered by a grove of cocoa-nut trees. Canoes with natives came off, and though shy at first, they afterwards came near. . . . The anchorage was left at 7.45 a.m., and 2½ fathoms deep was taken into a channel 5 fathoms in depth midway between Bald Head and Point Ramsay. For over 8 miles, with a depth of from 5 to 9 fathoms, the vessel proceeded until an important junction was reached. Here land traversed the horizon, and broad arms coming in from north-west to north-east joined the river. This junction was named after the Hon. W. Macleay, of Sydney, and the sheet of water so far traversed from Bald Head was named Port Romilly.

Round the point, and at a distance of 4 miles, a second junction was met with, and named after J. Beveridge, one of the party. At this point the river was nearly half a mile wide, and an extensive mud-flat was met with. Some very fair agricultural land was now passed through, with light chocolate soil, and covered with scrub that could be cleared with ease. Freshwater springs were noticed flowing over the banks. Numerous small deserted huts were passed, and a number of alligators and flying foxes. The rule seemed to apply in these deltaic rivers that the land was making on the convex side, while the deepest channel and strongest current were found close to the concave bank. The country now passed through alluvial swampy land, in which nipa and sago palms flourished amidst a thick scrub. The river narrowed to 60 yards, and at low tide the water was quite fresh. It was found necessary to anchor here, and some of the party, getting into the whale-boat, rowed up the river, which continued to get narrow, until it broke up into several deep but very narrow creeks, and further navigation was closed. The highest point reached up this river, which was named after the Hon. Edward Stanhope, was 7° 14′ south latitude, and 144° 28′ east longitude, being 34 miles due north from Arai River on the coast, or 40 miles by river courses to Bald Head. Returning to Beveridge Junction, the *Victory* was taken up an arm coming in here from the west. Several openings into the arm were passed, but after proceeding 7 miles up it shallowed to 11 feet. The river was named the Penrose, after a gentleman of that name of Yulgilbar, in this colony. On this river a native plantation and some natives were seen. The steamer was taken to an anchorage at Macleay Junction. After exploring with boats the eastern channels of this junction, and meeting with natives who were of an extremely friendly disposition, the *Victory* was taken up the channel. The houses of the natives were raised on piles of the hog-backed shape, open in front and with protruding peaks. The village was called Piri Evorra. Continuing on its way, about midday the steamer, to the great satisfaction of all, ran at right angles into a fine new river running north, north-east, and south. This seemed to form certain proof that they were now in the one large river reported by the coast natives. This junction, which was 11 miles west by north of Macleay Junction, was named after Dr. Llewellyn Bevan, of Melbourne, a family connexion of the leader. Taking the north-east branch of the river, which is 300 yards wide, some fine-looking, well-timbered country was passed through. Several deserted dwellings were noticed. After passing several miles up this splendid river another junction was met with, where a broad stream over half a mile in length came in from the easterly direction, and bifurcated into the channel, and a wide stream flowing south-south-westerly, with a steady fresh-water current flowing seaward. This junction was named the Bennett Junction after a friend. Three miles further up, and still another junction was met with, named after Mr. William Woodhouse, of Sydney. This proved to be the head of the delta of the great fresh-water river up which they had come, and which was named the Queen's Jubilee River. At this point the river again bifurcated, throwing off one main branch half a mile wide running down to Bennett Junction, and the other flowing easterly and southerly. Past Woodhouse Junction the river maintained its width of fully half a mile, and a range of hills 2000 feet high, a few miles distant, was named after Sir Saul Samuel. Mountain peaks of great altitude were visible some 40 miles to the north. Still proceeding up the river, a rapid some miles further on was passed, and soon afterwards it was found necessary to stop, but for one day over 30 miles had been travelled. The highest position reached was south latitude 7° 18′, and east longitude 144° 59½′, and distant 45 miles from Bald Head, and over 100 miles by the remarkably tortuous courses. As the river had now become unsafe, and only two days' coal were left for river work, it was found necessary to return to Bennett's Junction, from which it was hoped that the broad channel leading to the sea would be taken; but the master of the *Victory* demurred to this step, owing to the strong current running. The same objection had to be taken to the opposite, the southerly stream, at Llewellyn's Junction. The course was therefore taken by the one whereby the *Victory* had been brought in, and on Thursday, April 28, Bald Head was passed through again, and after putting in at Orokolo, a course was steered for Motu Motu, which was reached shortly before noon on the following day. York Island was reached on May 1, and Thursday Island on the following day, and on May 3 the *Victory* once more steamed to an anchorage at Thursday Island after an eventful and most successful journey into the interior of New Guinea.

THE WHEAT CROP OF 1887.

SIR J. B. LAWES forwards to us the following information:—

"The very low prices during the last few years have, it is supposed, induced farmers to use a not inconsiderable quantity of their wheat as food for stock. The amount so withdrawn from human consumption is quite unknown. It has been estimated by some to be considerably less than one million, and by others to be even as much as two million quarters within the harvest year. Whatever the amount may be, it is evident that a new element of uncertainty is thus introduced into our estimates of the quantity of imported wheat required to supply the deficiency of the home-grown crop.

"The 'Agricultural Produce Statistics' published at the beginning of the year give, as the result of inquiries in fourteen thousand parishes in Great Britain, and many in Ireland, an average yield of 26·89 bushels per acre for the wheat crop of the United Kingdom in 1886. If we deduct from this amount 2¼ bushels per acre for seed, as we did in the case of our own estimate, it leaves only 7¼ million quarters available for consumption by the population and for stock feeding. The imports less exports for the harvest year ending August 31, 1887, amounted to 17¼ million quarters, making altogether a total of little over 24½ million quarters. But assuming the consumption per head of the population to be 5·65 bushels, which is the figure we have adopted for the last ten years, the amount so required would, independently of the quantity consumed by stock, be 26½ million quarters, or two million quarters more than the estimated available home produce and imports taken together. By the kindness of Messrs. Beerbohm I have been furnished with a statement of the amount of wheat, and of flour reckoned as wheat, in warehouse on July 1, 1886, and July 1, 1887, from which it appears that the stocks were slightly the higher in 1887, whilst it is estimated that subsequently to that date they somewhat increased.

"Our own estimate of the yield of the wheat crop of 1886 was 29¼ bushels. This is considerably higher than that of the Government above quoted; and it is also higher than the estimates of others. According to our figure, the available supply of home produce was nearly 8 million quarters. Even with our higher estimate of the home crop, there is still a deficiency in the imports for the estimated requirements for human consumption, to say nothing of the amount consumed by stock. The evidence so far would thus seem to suggest the question whether there has not been some decline in the consumption per head of the population. At the same time it should be stated that if we take our own estimates of the available home produce and the recorded imports for the whole period of the eleven harvest years 1876–77 to 1886–87 inclusive, for which we have adopted a consumption of 5·65 bushels per head, the result shows precisely that amount available, if no allowance be made for consumption by stock. It is obviously desirable, however, that those who are engaged in forming the estimates of the yield of the wheat crop should also endeavour to ascertain the facts as to the quantity of wheat consumed by stock."

Sir John Lawes next exhibits tables proving his estimate as to the average yield of wheat at Rothamsted, and explains the peculiarities of the late season with regard to the growth of wheat.

He continues as follows:—"The Rothamsted result of 28¾ bushels, which more probably under- than overrates the crop of the country, if calculated upon the slightly increased area this year, namely, 2,383,584 acres, gives an aggregate produce for the United Kingdom of 8,454,275 quarters. Hitherto we have always deducted 2¼ bushels per acre for seed, but this is supposed to be too high an average at the present time, and if we deduct only 2 bushels, there remain 7,858,379, or rather less than 8 million quarters available for consumption. Still estimating the consumption per head of the population at 5·65 bushels, the requirement for the harvest year would be 26,419,940, or nearly 26½ million quarters, of which about 18½ million quarters would have to be supplied by stocks and imports.

"For some reason the imports of wheat have been below the estimated requirements for the last two years. Whether, or to what extent, this is due to previous accumulations, to the home crops having been underrated, or to a reduction in the consumption of bread and flour, there is not sufficient evidence to decide conclusively. If there has been a reduced consumption, the question arises whether there has been an increased consumption of other foods. During the last few years there has been some increase in the number of both cows and other cattle kept, but there has upon the whole been a reduction in the number of both sheep and pigs. In fact, the records, neither of the home production, nor of the imports, of animal foods, afford evidence of any material increase in the consumption per head of such foods.

"Further, a careful examination of the amounts of the imports of other articles used as human food shows in the aggregate a reduction rather than an increase in proportion to the population. In such articles as rice and potatoes, for example, which would to some extent substitute wheat, the decline in the imports is very marked. Thus, whilst during the five years 1877–81, the average annual imports of potatoes amounted to 395,277 tons, during the five years 1882–86 they amounted to only 156,017 tons, or to considerably less than one-half. Nor is it probable that the amount of maize flour used has at all materially affected the consumption of wheat. The indication would thus seem to be, therefore, that if the consumption of wheat has really declined, either the total consumption of food per head of the population has also declined, or that the deficiency in the wheat imports has been compensated by increased supplies of home-grown foods. So far as potatoes are concerned, however, the 'Agricultural Produce Statistics' show a decline in area, in produce per acre, and in aggregate produce, both in 1885 and in 1886 compared with 1884. On the other hand, there has, notwithstanding an increase in the imports of other vegetables, been a considerable increase in the area devoted to market gardening during the last few years, and also an increase in the area of allotment gardens. It would obviously be a ground of satisfaction should further information and consideration show that, notwithstanding the very low prices of grain, there has been a larger consumption of some other home-produced foods.

"Whilst it is obviously of importance to the grower that his wheat crop should yield well, it has ceased to be a question of any interest to the consumer whether the yield of the home crop is a few bushels per acre more or less. Nor does such a difference, on our much reduced area, at all materially affect the supply from foreign sources. During the eight harvest years 1852-53 to 1859-60, which were the first of our estimates of the home wheat crop, nearly three-fourths of the aggregate amount consumed was of home growth, and little more than one-fourth was derived from foreign sources; but during the eight years 1878–79 to 1885–86 little more than one-third has been provided by the home crop, and nearly two-thirds by imports; and were it not for the value of the straw for bedding purposes it is probable that the reduction in the area under the crop would have been even greater than has actually been the case.

"Although greater facilities for acquiring land have been afforded by the Acts of Parliament recently passed, there is not much probability that the result will be an increase in the area under wheat, or other grain crops; or in fact that tillage on a small scale will successfully compete with arable farming as at present practised. Nor is it likely that there will be any permanent extension of peasant holdings of pasture land, excepting in localities where the soil and climate are specially favourable for permanent grass. But garden allotments, as distinguished from peasant holdings or from farm allotments, are of very great advantage to the masses of the population, and will no doubt continue to extend as they have done largely during the last quarter of a century."

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

KING'S COLLEGE, LONDON.—A new laboratory has been fitted up in the College. It is provided with a collection of pathological material, biological, histological, and chemical apparatus, and is intended to afford every facility for obtaining a practical knowledge of bacteriology, and for prosecuting original research in all matters relating to human and comparative micro-pathology.

The laboratory is open to all gentlemen, whether students of other departments of the College or not. The practical courses and lectures are specially intended for medical officers of health, veterinary surgeons, and analysts.

A certificate of attendance will be granted to each member of these courses.

The winter course of lectures with practical work will commence on November 1. There will be about fifteen lectures, and the practical course will last for thirty days. The lectures will be delivered on Mondays, Tuesdays, Thursdays, and Fridays, at 10 a.m.

They will be illustrated with diagrams and typical preparations and followed by practical work in the laboratory for the rest of the day.

Admission will be permitted to the lectures without the practical work.

In the case of medical men in practice, medical officers, and veterinary surgeons of the army, and others whose duties may prevent their attending the laboratory daily, special arrangements will be made for extending the days of attendance over a longer period.

For further particulars apply to Prof. Crookshank, King's College, London, or to the Secretary, J. W. Cunningham.

SOCIETIES AND ACADEMIES.

LIVERPOOL.

Astronomical Society, October 10.—Mr. W. F. Denning, of Bristol, President, in the chair.—This was the first meeting of the seventh session, and sixty-one candidates were proposed as members.—In his opening address the President referred to the last volume of their Journal as exhibiting the varied and attractive character of the work in which the members had been engaged. The angular measurements of fifty binary stars had been completed, and a valuable series of illustrated articles on lunar objects had been published. The remarkable dark patches in the "crape" ring of Saturn were observed and described by members at Bedford, and Louvain in Belgium. There had been a wide-felt regret that the objects of the Society's Eclipse Expedition of August 19 had been defeated by cloudy weather. Observations of Jupiter had been reported, and the increase in the rotation-period of his red spot fully verified. The meteoric section had made considerable progress. To the several members who had so practically aided the Society in its efforts to promote a knowledge of astronomy their warmest thanks were due. The ensuing session gave promise of increased activity, particularly in the stellar, planetary, and meteoric sections. The action of the American members in having so disinterestedly set aside national prejudices to enter into a bond of fellowship with English observers, had afforded great satisfaction, and must lead to a considerable extension in the Society's connexions and sphere of usefulness. The Society owed a debt of gratitude to Mr. W. H. Davies, F.R.A.S., the Hon. Secretary, for the untiring zeal which he had displayed in a very laborious office during several years. Undoubtedly a great future lay before the Liverpool Astronomical Society if its members continued their hitherto united policy. Individual interests and ambitions must be made subordinate to greater aims.

PARIS.

Academy of Sciences, October 17.—M. Janssen in the chair.—Catalogue of the Paris Observatory, by M. Mouchez. The revision of Lalande's Catalogue, made in 1791–1800, and containing the positions of 47,390 stars, was begun in 1854 by Leverrier, continued in 1878 by M. Mouchez, and now completed far enough to begin the publication of the results. The first two volumes, which have just been issued, contain the 7245

stars comprised between oh. and 6h. of right ascension, for which 80,000 observations are recorded. A comparison of the results shows the surprising accuracy of Lalande's observations made with instruments which would now be regarded as very defective.—On the formulas of dimensions in electricity, and on their physical significance, by M. G. Lippmann. Some of these formulas give the idea of a corresponding physical interpretation. But it is shown that no electric magnitude appears susceptible of such interpretation, except where the dimensions may be reduced to those of time, certain electric phenomena having a duration capable of being calculated.—Researches on drainage, by M. Berthelot. Numerous experiments made at Meudon in connection with the study of nitrogen in vegetable soil lead to the general conclusion that the drainage of rain-water carries off a much larger quantity of nitrogen than that supplied to the soil by the atmosphere, and especially by the rain-water itself. This result is destined profoundly to modify the views hitherto accepted regarding the conditions of natural vegetation and of husbandry.—Duality of the brain and of the spinal marrow, by M. Brown-Séquard. It is shown that anæsthesia, hyperæsthesia, paralysis, and various phases of hypothermia and hyperthermia, due to organic lesions of the cerebro-spinal centre, may be transferred from one side of the body to the other. In a word, the author undertakes to establish as the result of a prolonged series of crucial experiments that, contrary to the generally received opinion, each half of the encephalon and of the spinal marrow may equally and independently serve for all the functions of the two halves of these nervous centres. The anæsthesia and analogous affections caused by an organic lesion of the nervous centres are transferred to the opposite side under the influence of a second lesion of those centres; hence it follows that such manifestations are not necessarily effects of the destruction of certain nervous elements endowed with certain functions, but may be the results of purely dynamic actions exercised at a distance by the irritation caused by the lesion. In the same way one half of the encephalon may serve as the seat of the voluntary motions and vaso-motor actions for either half of the body; and so with the spinal marrow, at least so far as concerns sensibility and the vaso-motor actions.—Remarks accompanying the presentation of the second volume of the "Compendium Floræ Atlanticæ, &c.," (Flora of Algeria, Tunis, Morocco), by M. E. Cosson. This volume contains a supplement to the already published notice on the botanical explorations in Mauritania, together with a detailed description of the families, genera, and species from the Ranunculaceæ to the Cruciferæ inclusive.—Observations of Peters's new planet, 270, made at the Paris Observatory (equatorial of the West Tower), by M. G. Bigourdan. The observations extend over the period from October 14 to 16. On the 15th the planet was of the 10·5 magnitude.—A mechanical and automatic registering apparatus of signals transmitted by telegraph and by optical projectors, by M. E. Ducretet. The apparatus here described and illustrated has the advantage over others in general use of automatically recording all messages for the purposes of reference in case of doubt or error occurring in the transmission of signals. It is equally available for ordinary telegraphic service, and for optical, military, and other systems.—Isoclinous magnetic curves, second memoir, by M. C. Decharme. This paper serves as a supplement to the author's previous communication on the isogonous magnetic curves relative to the declination. It deals specially with the isoclinous curves obtained with the dipping needle.—On a new mode of formation of the substituted safranines, by MM. Ph. Barbier and Léo Vignon. It was shown some years ago that the nitrous derivatives of the aromatic tertiary monamines, by acting on the primary monamines, give rise to certain colouring substances. Here are given the results of studies undertaken to determine the true character of these substances.—Researches on the bovine origin of scarlatina, by M. Picheney. The experiments here described tend to confirm the conclusion arrived at in England that scarlatina has its origin in the milk of diseased cows consumed especially by children.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Mattie's Secret: E. Desbeaux (Routledge).—Unfinished Worlds: S. H. Parkes (Hodder and Stoughton).—Bees and Bee-keeping, vol. ii. part 13: F. R. Cheshire (Gill).—British Dogs, No. 12: H. Dalziel (Gill).—Meteorologische Beobachtungen in Deutschland, 1885 (Hamburg).—Report on the Meteorology of India in 1885: H. F. Blanford (Calcutta).—Charts of the Bay of Bengal and Adjacent Sea North of the Equator, showing the Specific Gravity, Temperature, and Currents of the Sea Surface.—Charts of the Bay of Bengal and Adjacent Sea North of the Equator, showing the Mean Pressure, Winds, and Currents in each Month of the Year.—The Vegetable Lamb of Tartary: H. Lee (S. Low).—Austral Africa, 2 vols.: J. Mackenzie (S. Low.)—Report of the Meteorological Service of the Dominion of Canada for the Year ending December 31, 1884 (Ottawa).—The Encyclopædic Dictionary, vol. vi. part 2 (Cassell).—Handbuch der Oceanographie, Band ii.: Dr. Kummell (Engelhorn, Stuttgart).—Cow-Pox and Vaccinal Syphilis: C. Creighton (Cassell).—A Manual for Steam Users: M. P. Bale (Longmans).—Universal Phonography: W. Benson (Chapman and Hall).—Studies in some New Micro-Organisms obtained from Air: Grace C. Frankland and Percy F. Frankland (Trübner).—The Preservation of Fish: J. C. Ewart (Griffin).—Report of the Entomologist, C. V. Riley, for the Year 1886 (Washington).—Modern Lessons in Dynamical Geography and Topography.—Journal of the Chemical Society, October (Gurney and Jackson).—Journal of the Royal Microscopical Society, October (Williams and Norgate).—Indian Meteorological Memoirs, vol. iv. parts 2 and 3.—Sitzungsbericht der K. Academie der Wissenschaften; Math.-Naturw. Classe, Mineralogie, Botanik, Zoologie, Geologie und Palæontologie. Jahrgang 1886, April to December (Wien).—Physiologie, Anatomie und Theoretischen Medicin, 1886, January to December.—Mathematik, Physik, Chemie, Mechanik, Meteorologie und Astronomie, 1886, January to December.—Journal of Physiology, vol. viii. No. 5 (Cambridge).—Annalen der Physik und Chemie, 1887, No. 10 (Barth, Leipzig).—Proceedings of the Bristol Naturalist's Society, vol. v. part 2 (Bristol).

CONTENTS.

	PAGE
The Study of Embryology	601
Some Mathematical Books	602
Our Book Shelf:—	
Sturmey: "The Photographer's Indispensable Handbook"	603
Lange: "Ueber Gemüthsbewegungen"	603
Macdonald: "Three Lectures on the Forms of Nasal Obstruction in Relation to Throat and Ear Disease"	603
Letters to the Editor:—	
"The Scenery of Scotland."—The Duke of Argyll, F.R.S.; Dr. Ch. Callaway; Prof. A. H. Green, F.R.S.	603
A Hydroid Parasitic on a Fish.—J. Walter Fewkes	604
Music in Nature.—A. P. Coleman	605
Swifts.—Aubrey Edwards	605
Hughes's Induction Balance.—J. Cook	605
Prof. Kirchhoff. By Prof. P. G. Tait	606
On the Signification of the Polar Globules. By Prof. August Weismann	607
The Total Eclipse of Last August in Japan. By Prof. David P. Todd. (*Illustrated*)	609
The Märjalen See. By Prof. T. G. Bonney, F.R.S. (*With a Map*)	612
The Bacillus of Malaria	613
Notes	614
Our Astronomical Column:—	
The Parallax of Σ 2398	616
New Minor Planet	616
Astronomical Phenomena for the Week 1887 October 30—November 5	616
Geographical Notes	616
Meteorological Notes	616
The Sixth International Congress of Hygiene and Demography in Vienna	618
The Mineral Wealth of the United States. By A. E. Foote	619
The Exploration of New Guinea. (*With a Map*)	620
The Wheat Crop of 1887. By Sir J. B. Lawes	622
University and Educational Intelligence	623
Societies and Academies	623
Books, Pamphlets, and Serials Received	624

NATURE

A WEEKLY ILLUSTRATED JOURNAL OF SCIENCE.

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

THURSDAY, NOVEMBER 3, 1887.

THE ZOOLOGICAL RESULTS OF THE "CHALLENGER" EXPEDITION.

Report on the Scientific Results of the Voyage of H.M.S. "Challenger" during the Years 1873-76 *under the Command of Capt. G. S. Nares, R.N., F.R.S., and of the late Capt. F. T. Thomson, R.N.* Prepared under the Superintendence of the late Sir C. Wyville Thomson, Kt., F.R.S., &c., and now of John Murray, one of the Naturalists of the Expedition. Zoology—Vols. XX. and XXI. (Published by Order of Her Majesty's Government, 1887.)

THE twentieth volume of the "Zoological Reports of the Voyage of H.M.S. *Challenger*" contains three memoirs, of which the first is "On the Monaxonida," by Stuart O. Ridley, M.A., and Arthur Dendy, B.Sc. The collection of this group of the Sponges was, in the first instance, intrusted to Mr. Ridley, who, to hasten the completion of the work, was afterwards joined by Mr. Dendy.

When about ten years ago Prof. Zittel gave the name Monactinellidæ to an order of Sponges, the position of this group became for the first time clearly defined. Prof. Sollas, some five years later, pointed out that Zittel's name implied a wrong idea, for that the characteristic spicules of the group were just as often "diactinal" as "monactinal," and suggested that, as both these forms were, however, "monaxonid,"—that is, having only one axis, which, in the case of the diactinal forms, passed through both the rays—the group should be called Monaxonidæ. As this group represents a division higher than that of a family, for which the termination "idæ" stands, all subsequent writers have adopted the name "Monaxonida." This group Sollas now regards as a tribe of the sub-class Demospongiæ, but the authors of this Report consider it with Zittel as one of the orders of the class.

The classifications of Gray, Bowerbank, Schmidt, or Carter, have now little but historic interest, while as for the more recent writers it would even seem as if each new series of novelties described necessitated a fresh shuffling of the orders, sub-orders, and families.

In the chapter on the anatomy and histology of the group the subject of the spicules is fully treated. It appears to us that no apology was needed for passing over the writings of Bowerbank on these forms; and when the authors too modestly refer us for further details as to the nature, &c., of siliceous spicules to, among other volumes, those of this author, we prefer, without meaning the slightest reflection on his great labours, to turn instead to the pages of the present Report.

The very difficult subject of a nomenclature for the spicules is treated at some length. Those of this order are divided into the two classes of "mega-" and "micro-" "sclera." In each of these there is a very numerous series of forms, all of which get separate names, founded on some prominent distinguishing character of the spicule. Let us hope that the majority of these names may find acceptance with writers on this group of Sponges, so that one difficulty in its study may be removed.

Passing over the descriptions of the spongin, the arrangement of the skeleton, and those of the ectosome and choanosome, we must briefly notice a very remarkable structure, which would appear to be quite unique, and which is found in a Sponge (*Cladorhiza tridentata*, sp. n.) from a depth of 1600 fathoms. The little Sponge in which this occurs is in shape somewhat like a miniature watch-stand. Embedded in the soft tissues, all around the upper margin of the concavity, a large number of small yellow globular bodies are found. Each globular body consists of a central, more deeply staining and granular portion, surrounded by and embedded in a matrix of faintly staining, perfectly hyaline ground substance. The granular appearance of the central mass is owing to very numerous embedded cells; these are irregular in shape and nucleated. Other peculiar cup-shaped bodies occur towards the periphery of the Sponge, embedded in the matrix. The authors think it probable that the cup-shaped bodies are aggregations of glandular cells similar to those met with in the ectosome of some other Sponges, and hint that the whole structure may be phosphorescent, and serve to attract minute organisms upon which the Sponge feeds. In regard to the canal-

system the authors' general conclusions are quite in accord with those of Vosmaer and Polejaeff. In the classification adopted the order is divided into two sub-orders, Halichondrina and Clavulina. The first of these is divided into four families: I. Homorrhaphidæ, II. Heterorrhaphidæ, III. Desmacidonidæ, and IV. Axinellidæ; the second into I. Suberitidæ, and II. Spirastrellidæ.

Over 200 species or well-marked varieties are described from the *Challenger's* dredgings, but all the new species were first diagnosed in the *Annals and Magazine of Natural History* during the course of last year (1886).

In reference to the geographical distribution, the authors remark that one cannot fail to notice the small number of stations at which these Sponges were found. Out of a total of 277 distinct stations, only 50 are represented as stations for these Monaxonids, and these are supplemented by 20 localities to which no station-number is attached; and these latter were not, it is to be assumed, deep-sea stations. We cannot agree with the suggestion that these forms were overlooked amongst the "rubbish" in sorting out the contents of the trawls and dredges; or with the idea that owing to their fragility they may have been destroyed. Doubtless the true explanation is that "the Monaxonida are not, on the whole, a predominant group in deep water."

While not a predominant group in deep water, still no less than 24 species were found at depths between 1000 and 2000 fathoms, while 46 occurred between depths of 200-1000 fathoms, and 140 species, or exactly double the previous number, were found at depths of from 0-200 fathoms.

The scarcity of Monaxonid Sponges at very great depths is somewhat compensated for by the unusual interest attaching to the species which do occur. Among other facts we find that, while the shallow-water forms are characteristically more or less shapeless in their external form or at the very most digitate or ramose, those from below the 1000-fathom depth have almost without exception beautifully symmetrical and definite shapes.

One of the most beautiful and extraordinary of the species described and figured is *Esperiopsis challengeri*, Ridley: it was taken in some quantity off the east of Celebes Island from a depth of 825 fathoms. From a slightly expanded attached base a slightly curved stem arises, which is composed of densely packed and firmly united stylote spicules: this stem is compressed laterally; numerous short simple branches arise from the concave edge at gradually increasing intervals, the longest of the internodes being at the top; the main stem and each of these branches terminate in fleshy sponge lamellæ, of which there may be six or seven in an apparently full-grown specimen. Each lamella presents the form of a deeply concave, transversely elongated cup: the oscula are confined to the convex surfaces of the lamellæ; the pores are found on the concave surfaces. This species is figured on Plate XVIII. Fifty-one plates accompany this Report.

The second Report in this volume is a supplement to Dr. L. von Graff's Report on the Myzostomida. It includes the description of seven new forms besides fourteen previously described species, all received from Dr. P. Herbert Carpenter; these were found by him while investigating the *Challenger* Crinoids. The author refers to the so-called cysts of *Antedon rosacea*, but declares that in no one case did he find therein any trace of a Myzostoma or any other encysted organism. On the contrary, both in the various pinnule deformities and in the arm swellings, he found a roundish brown foreign body, which was apparently the cause of the deformity. As to the nature or origin of this body nothing has been determined. Three plates of the new species and one of the cysts of *Antedon rosacea* accompany this Report.

The third Report is on *Cephalodiscus dodecalophus*, McInt., by Prof. W. C. McIntosh. This very remarkable new type of Polyzoon was dredged in the Strait of Magellan, and was, when first found, placed among the Compound Ascidians. The late Mr. Busk, Prof. Allman, and Prof. McIntosh, referred it to the Polyzoa. At first sight the flexible cœnœcium might easily be taken for a sea-weed, but it would seem to spread over the surface of the ground and not to grow erect. Of the numerous branches many anastomose; the general surface is spiny or fimbriated; the interior of the stem and branches contains an irregular series of wide canals. The Polypides are described as being perfectly free and at liberty to wander anywhere along the chambers, or even externally through the apertures. Each adult Polypide measures, from the extremity of the cephalic plumes to the tips of the pedicel, about 60-2 millimetres. Large buds in various stages of development arise from the Polypides. The twelve branchial plumes are very conspicuous. The author thinks that Cephalodiscus approaches Rhabdopleura. In an important appendix, by Mr. Sidney F. Harmer, the affinities of this form to Balanoglossus are ably pointed out, and he thinks that this genus (and perhaps Rhabdopleura also), must be removed from the Polyzoa and placed in Bateson's group of the Hemichordata. Seven plates and numerous woodcuts illustrate this Report.

Volume XXI. contains but one Report, that on the Hexactinellida, by Prof. F. E. Schulze, of the University of Berlin. This volume is issued in two parts, the first that of the text, comprising over 500 pages, and the second consisting of an atlas of 104 plates.

This is one of the most important of the fifty-three Reports hitherto published. This group of Sponges early attracted the special attention of the late Sir Wyville Thomson; and it was his intention to describe the Hexactinellids of the *Challenger* Expedition, but the work had scarcely been seriously commenced at the time of his death. It was fortunate that the services of Prof. Schulze were secured for the writing of this monograph, which is a most acceptable and welcome addition to our literature of this group.

In this Report, besides the collection made by the *Challenger*, the results of the previous cruises of the *Lightning*, *Porcupine*, *Knight Errant*, and *Triton* are also detailed, and the material has been further increased by a collection made at Japan by Dr. Döderlein.

The Hexactinellids are those forms of the Sponges in which the siliceous spicules belong to the triaxial type. Omitting the eighteenth century reference to a Sponge belonging to the genus Dactylocalyx, Dr. Gray was the first in the present century to describe some peculiar "glass rope-like" structures in the British Museum under the name of Hyalonema; though without recognizing until long afterwards their real affinities. This was in

1832, and in the following year Quoy and Gaimard figured and described their *Alcyonellium speciosum*. During the next twenty years only five more species were added to the list, the beautiful *Euplectella aspergillum*, Owen, being the most remarkable of these. The last twenty-five years have, however, witnessed an ever-advancing progress in our knowledge of these Sponges, thanks to the labours of Gray, Bowerbank, Wyville Thomson, Schmidt, Kent, Carter, Marshall, Sollas, and, above all, of Zittel, which labours have now culminated in the present Report.

It is scarcely to be wondered at that the beautiful glassy frame-work and the charmingly diversified spicules which form their "skeletons" have always attracted attention to these Sponges—an attraction that will be greatly intensified by the publication of this volume.

The Report opens with a general historical introduction, and then passes on to details of the forms and structures to be met with in the group: herein we find the nomenclature adopted for the spicules. This is followed by the description of the genera and species. It is pleasant to find in the synonymy and specific details that great pains have been taken to mention the work of all previous labourers in the field, and the author shows a due and kindly appreciation of what has been done by those who often had but little light to guide them on their way.

It is not easy to give any analysis of so elaborate a memoir, in which twenty new genera and sixty-five new species are described; while with scarcely an exception the numerous species already described are not only alluded to, but many fresh details are given about them. When it is recollected that but fourteen years ago only thirty species of this group of Sponges were known, the progress of our knowledge of them, it will be recognized, has been great.

These Sponges seem to be widely distributed in all the oceans; the largest number of forms—fifty-seven—being found in the Pacific Ocean; next comes the Atlantic Ocean, in which twenty-four species were found; while only sixteen were found in the South Indian Ocean; but it must not be forgotten that the South Indian Ocean has been very slightly investigated.

As to the bathymetrical distribution of the Hexactinellida, they would appear to be met with in depths of between 95 and 3000 fathoms, being more numerous between the depths of 100 and 900 fathoms, decreasing somewhat between those of 900 and 2500 fathoms, and again markedly diminishing between the depths of 2500 and 3000 fathoms, while below this depth no Hexactinellida Sponges have been found. Euplectellids seem to have a wide range, being met with at the moderate depth of 95 fathoms, and then being pretty evenly distributed down to a depth of 2750 fathoms. The four species of the new genus Holascus frequent great depths, varying from 1375 to 2650 fathoms. The maximum depth as yet known for any of these Sponges is that of 2900 fathoms, at which depth *Bathydorus fimbriatus* was found in the middle of the North Pacific Ocean.

It would be obviously impossible to give even a brief summary of the very remarkable new forms described in this splendid memoir of Prof. Schulze, and it is difficult to convey a correct notion of the beauty of the illustrations forming the large atlas of plates which accompanies the text. The diagnoses of the genera and the descriptions of the species are what one would expect from the well-known skill of the author.

We do not altogether agree with him when he writes that, "after a detailed investigation of a group of animals, it is incumbent on every naturalist who accepts the evolution theory to attempt the appreciation of his results in their relation to the phylogeny of the group." Looking at his array of facts, is it not possible for the thoughtful worker to bear in mind the incompleteness of the phylogenic record, and reverently to wait for more light? There may be nothing to object to in the stately genealogical tree of the Hexactinellida represented on p. 495, but is it not built up on but an incomplete and scanty framework?

One departure in this Report from the ordinary custom in the description of species we notice with regret—viz. that there is no synonymic list affixed to the species, neither are we referred, in connection with each form, to the place or places where it has been previously described. It seems scarcely necessary to point out the inconveniences attending such a style, or the great uncertainty it may on occasions give rise to. The volume opens at the description of *Rosella antarctica*, Carter. To find where it was first described by Mr. Carter we are obliged to refer to the synonymy affixed to the diagnosis of the genus; but here we get no certain information as to how many of the quotations given refer to this species; and this is of course much more confusing when we come to investigate a genus abounding with species, like, for example, Hyalonema. Indeed, by this method an author is very apt to overlook the fact that several writers may refer to the same species under quite different names, and a curious case of this nature, we suspect, occurs under Hyalonema. Dr. Gray, wrongly trusting to a Museum label, replaced the name *Hyalonema sieboldii*, which he had given to the first known species of this genus in 1835, by that of *H. mirabile*, under the impression that he had so named it in the "Synopsis of the Contents of the British Museum," 1832 (misprinted 1830), the year he had got the analysis of its glassy fibres from Mr. Pearsall. Depending on the accuracy of Dr. Gray, many authors referred to the species under this latter name; and further, for some time after the discovery of the Setubal species by Prof. Barboza du Bocage, this species, now known as *H. lusitanicum*, passed as the same species as *H. mirabile* = *H. sieboldii*. Even from the details given by Prof. Schulze, this seems clear; but in the description of Bocage's species (p. 225) no synonymic list is given, and not only does the before-mentioned fact not appear, but we find *H. lusitanicum* placed among those species "the upper end of which was not sufficiently preserved for deciding the question whether there is a sieve-plate or not." It is added that "neither on this specimen (the one figured in the Proc. Zool. Soc. Lond.) nor on others which Bocage afterwards obtained from the same locality could any portion of the sponge body be detected." But on p. 186 we find it stated that *H. lusitanicum* had been dredged from a depth of 480 fathoms south-west of Setubal, "bearing a sponge body with several oscular openings"; and again on the same page that among the Hyalonema found off the coast of Portugal by Barboza du Bocage and others, and named *H. mirabile*, there was one specimen

with an oval cup-shaped body about 8 inches in length and 4 inches in breadth, with a sieve-net on the upper truncated surface of the sponge body, extending evenly over the oscular opening and over the layer of the "spiculate cruciform spicules" in the net beams. We may further add that there were to be found in the Museum at Lisbon nearly a dozen specimens of Hyalonema which were taken at Setubal. "Most of them were preserved in spirits of wine; they were certainly the very finest collection of this remarkable Sponge in Europe. The largest had a stem about 18 inches in height; there were no parasites of any kind on it, and it was furnished with a sponge mass some 8 inches in diameter, and nearly as much in height. A second specimen was very curious, for here two apparently distinct individuals had become matted together: the two glass ropes were interlaced, and the two sponge masses had grown together" (Proc. Dublin Nat. Hist. Soc., vol. v., 1869). It would have been most important to have had the opinion of such an authority as Prof. Schulze as to whether all these specimens from Setubal are referable to Bocage's species; and whether, as we venture to think, Marshall's *H. thomsoni* may not be only a well-marked variety thereof. It is possible that by thus calling attention to the subject we may yet learn more of the treasures of the Museum of Lisbon, and nothing in these remarks can in the very slightest degree detract from the merits and importance of this splendid contribution to our knowledge of the vitreous Sponges. E. P. W.

THE FERN-ALLIES.

Hand-book of the Fern-Allies: A Synopsis of the Genera and Species of the Natural Orders Equisetaceæ, Lycopodiaceæ, Selaginellaceæ, Rhizocarpeæ. By J. G. Baker, F.R.S., F.L.S., First-Assistant in the Herbarium of the Royal Gardens, Kew. (London: George Bell and Sons, York Street, Covent Garden, 1887.)

AS the author states in the preface, "The present Hand-book is planned upon the same lines as Hooker and Baker's 'Synopsis Filicum,' and the two, taken in connection, cover the whole series of the Vascular Cryptogamia." The total number of species described in the "Hand-book" is 566, and as we may now place the number of known ferns at about 3000, the fern-allies may be taken to represent about one-seventh of the recent Vascular Cryptogams. The fern-allies include only eleven genera, and about four-fifths of the species belong to the two genera *Selaginella* (335 species) and *Lycopodium* (94 species). The eleven genera are placed by Mr. Baker in four "natural orders," while the Filices form a fifth: three of these, Filices, Equisetaceæ, and Lycopodiaceæ, being isosporous; and two, Selaginellaceæ and Rhizocarpeæ, being heterosporous. In this way the relationship of the Rhizocarpeæ to the ferns is quite lost sight of; the Selaginellas and Lycopods are separated more widely than is desirable, and no place is left for the fossil heterosporous Equisetinæ. The arrangement adopted by Mr. Baker is very good for herbarium work; but for classificatory purposes it ignores certain palæontological facts which we cannot at the present day afford to overlook. Mr. Baker, however, does not deal with the fossil types, and now that we have such a complete account of the recent forms, let us hope that before long we may have as complete a synopsis of the fossil forms; a work which would be of the greatest interest and importance.

In regard to the geographical distribution of the fern-allies it is interesting to notice that *Equisetum*, *Isoëtes*, and *Pilularia* predominate in the North Temperate Zone. *Lycopodium*, *Psilotum*, *Selaginella*, *Salvinia*, and *Marsilea* are eminently tropical; and *Phylloglossum* is peculiar to the South Temperate Zone. Like the ferns, the fern-allies are best developed in the Tropics; and in the Tropics we also find the greatest number of peculiar species. Thus, out of the 566 species, 484 are met with in the Tropics of the Old and New World; and no less than 402, or 83 per cent., of these are peculiar to the Tropics. As with the ferns so also with the fern-allies, tropical America is richest in species, including 237 species, of which 212 are peculiar. The Southern Temperate Zone yields only 83 species, of which 42, or 51 per cent., are peculiar, the fern-allies being thus much less numerous than the ferns in the southern flora. In the North Temperate Zone 150 species are met with, and of these 48, or 32 per cent., are peculiar. The North Temperate Zone is thus, like the South, deficient in fern-allies as compared with ferns, and this is apparently due to the small number of fern-allies as yet reported from temperate Asia. Only 6 species occur in the Frigid Zone, and, like the ferns, represent about 1 per cent. of the whole, none of the species being peculiar.

It is difficult to realize the amount of labour and research that must have been spent upon the production of this book; but anyone who has attempted to study the genus *Selaginella* will appreciate the masterly manner in which Mr. Baker has dealt with the 335 species of the genus, more than one-fourth of which he has himself described for the first time. Most of the species of Selaginellaceæ and Rhizocarpeæ have been described by Mr. Baker in his papers on the subject which have appeared from time to time, since 1883, in the *Journal of Botany*, but several new species are described in "Fern-Allies" for the first time, recent additions to the rich treasures of Kew. It is to be regretted that Mr. Baker does not more particularly refer to his papers in the *Journal of Botany*, and it is hard to understand why, in the descriptions of *Marsilea concinna* and *M. condensata*, he has omitted the references to the *Journal of Botany*, 1886, pp. 179 and 281 respectively. Then in transferring the matter from the *Journal of Botany* he has altogether dropped out the habitat of *Azolla nilotica*. There are also in the book not a few misprints, and a want of care is shown in numbering and lettering the sections of *Selaginella*. The index is also not quite up to the mark, as in *Marsilea*, with numerous synonyms omitted, and the misprints in *Pilularia* and *Psilotum*. As the index of the "Synopsis Filicum" was published separately as a catalogue of ferns, we may perhaps be permitted to express a hope that this index will not be so published until it is carefully revised. All that is wanting, however, is only a little more careful editing, and the few faults in no way detract from the sterling value of the work.

As the only modern synopsis of the group, it is a work that must be in the hands of every botanist who deals with the Vascular Cryptogams, and it will be a lasting monument to Mr. Baker's critical accuracy and great power of dealing with a difficult set of plants. W. R. McNab.

OUR BOOK SHELF.

The Sailor's Sky Interpreter. By S. R. Elson. (Calcutta: Thacker, Spink, and Co., 1879)

THIS little book, which is written in verse, is a practical storm guide, dealing especially with the October cyclones in the Bay of Bengal. Many years of experience as pilot in the dangerous waters of the bay have made the author familiar with the phenomena of the weather in this part of the world. The details convey many a hint to students of Nature, and above all to navigators interested in the very violent storms which occur periodically at the change of the monsoons, and more especially about October at the close of the summer monsoon. The concluding stanza deals with the rules for avoiding the centre of a cyclone, and on this head the advice is both good and sound, and is at the same time put in a very concise form Sailors are very familiar with rhymes for the "Rules of the Road," but we can scarcely hope that the author's verse will be similarly mastered and remembered. Probably the author himself never contemplated such a use of his work; but yet there are couplets and triplets of Admiral FitzRoy's which have lived for a quarter of a century, and are still valuable aids. In the last volume of the "Indian Meteorological Memoirs" full credit is given to Mr. Elson for his valuable observations on the False Point cyclone, and especial mention is made of the high value of his observations bearing on the movement of the clouds. The author possesses just that local knowledge which in a recent issue of the "Fishery Barometer Manual" the Meteorological Office lamented the want of among its observers around our coasts for the further perfecting of our "Weather Forecasts;" and in the twenty stanzas which he has written he has pithily handed down his experiences for the benefit of his fellow-sailors.

Austral Africa. By John Mackenzie. Two Vols. (London: Sampson Low, 1887.)

THIS work, written by one who understands his subject thoroughly, ought to be cordially welcomed by all who have given any attention to the questions which have caused, during the last few years, so much trouble in South Africa. Mr. Mackenzie is convinced that these questions are not nearly so complicated and difficult as they are generally believed to be, and he has taken great pains to expound clearly and forcibly the policy which, in his opinion, would open up new markets for our commerce in South Africa, and secure the highest and best interests of the natives. The book is addressed rather to politicians than to persons interested in science, but students of the early forms of social institutions will find some statements worthy of their attention in Mr. Mackenzie's account of those native tribes with which he himself has come into contact. Archæologists will be interested, too, in what he has to say about the remarkable stone structures which are found over an extensive district to the east and north-east of Shoshong. These buildings, in the neighbourhood of which are the remains of ancient gold-mines, he compares with Persian towers of refuge and with the ancient round towers of Ireland and Britain.

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.*

Medical Education at Oxford.

THE problem, how far the older Universities should undertake special training for the professions, is fast finding its own solution. A degree is no longer any evidence that its possessor has been through any course of wide general culture preparatory to his technical education. Recent legislation, both at Oxford and Cambridge, has all tended in the direction of enabling the undergraduate to specialize at the earliest possible point in his career. Whether advisable or not, some such movement seemed inevitable if, in the midst of the daily increasing pressure of competition, the Universities were to retain any hold on the educational development of the country. Even Prof. Freeman's articles in the *Contemporary Review* are marked by a tone of querulous despair, rather than by any hope that the tide of innovation may be checked. For knowledge as a luxury or an ornament there is neither leisure nor inclination. Cambridge was the first to yield; but the multitudinous statutes which are every day promulgated at Oxford prove that the latter University is eagerly hurrying along the same path. New schools, new Boards of Faculties have been established; old restrictions have been removed Large sums of money have been expended on new buildings, in which new professors may give instruction in arts and sciences unheard of by the last generation. All this has been done in order that the student may proceed as speedily as possible to those special researches which are to arm him for the battle of life.

The ordinary curriculum at Oxford is now so modified and subdivided that a Bachelor of Arts may have no more extensive general education than that smattering of school-boy knowledge required for the examination called Responsions. It is hardly realized by those who are chiefly responsible for this movement how much the whole life of the University must be altered by so radical a change in its methods and its aims. The statute-book, indeed, is in such a state of chaos that there are few, even among the officials, who can unravel the intricacies of any one Faculty. In the department of medicine an attempt has recently been made, in a pamphlet issued by the Clarendon Press, to afford concise and accurate information to the hitherto bewildered undergraduate. By means of this publication it is possible to trace out the curriculum of an Oxford medical student and to contrast the present with the older system of education. Responsions, or some equivalent test, can be passed while the candidate is still at school, and at the same time he can take an extra subject which will exempt him from the First Public Examination. After reaching the University, only an elementary pass examination in divinity will stand between him and his scientific work He may then give himself up to preparation for one of the Honour Schools of natural science. For this he will have to pass various "preliminaries," for which there are schedules of alternating proportions. Physiology and chemistry are suggested as the most suitable schools, as by their means exemption is gained from portions of the First M.B. Whichever he may select, two years of the most severe application are necessary in order to gain a satisfactory position in the Class List. He will then, in his third or fourth year, be enabled to take his B.A. degree. The study of human anatomy will next absorb his energies. The amplest opportunities are now afforded to those who desire to take up this subject while residing at Oxford. The ideal candidate depicted in this pamphlet is supposed to spend but one academical year in this department. Extraordinary, indeed, must be the powers of the teacher who could impart, and of the pupil who could receive, a sufficiently deep impression of so important a science in so brief a time. After the first examination for the M.B., residence in Oxford would come to an end, and the student would migrate probably to London. With everything in his favour he might be able to obtain his degree in six or seven years from the date of his matriculation. There are other ways in which the course of study might be arranged, but the details are of small consequence. It matters little to the public whether the degree can be obtained in five years or seven. In either case the professional acquirements will be above the average. The eminence of the examiners and the reputation of the University will be a sufficient guarantee that tests are applied of sufficient stringency to exclude the ignorant and incompetent. It is well, however, that the real state of affairs should be fairly recognized and understood by those who have been accustomed to attribute some special virtues to a University degree. It is important also to consider whether in leaving the older methods and yielding, however reluctantly, to the pressure of the hour, a retrograde step has not been taken in the history of medical education. It is always a loss when something even distantly approaching to an ideal is degraded to the level of every-day life.

The older Oxford system, if antiquated and imperfect, had

at least aims of a high and noble character—aims which could not fail to have an elevating effect on those by whom they were entertained. In former times to have taken a degree in arts, as a necessary preliminary to the beginning of a student's purely professional career, may not have meant, and, as a matter of fact, did not in the majority of cases mean, any very high standard of learning or culture. It did, however, carry with it some inestimable advantages which can never be attained under the existing system of specialization. It meant that a young man, while his mind was still plastic to all surrounding influences, was brought into contact with and joined in the same pursuits as fellow-students whose tastes would lead them to different pleasures, and whose circumstances would lead them to a variety of destinations. The physician or surgeon of the future became the companion of those who were afterwards to become clergymen, barristers, or schoolmasters. He read the same books, played the same games, belonged to the same clubs. In this way, however little actual knowledge he may have acquired, he gained an invaluable acquaintance with men's lives and habits. He formed friendships with men destined to follow very different careers. These associations could not fail to be of the greatest value to him in the pursuit of his special profession. I do not mean mere monetary advantage, but that derived from intercourse with men in other walks of life—that interchange of ideas so necessary to a healthy mind. Such a training must have been beneficial to all, but to the student of medicine it was an incalculable boon. Much of his success and much of the good he can hope to do depend on an intimate knowledge of mankind. Without that, no matter how highly trained he may be as a man of science, his acquirements will be of little avail, and his skill can never be used to the highest purpose.

How is the existing system likely to work in this direction? The student is advised and encouraged to enter at once on his special pursuits. He is to apply himself without delay to scientific study, associated with men like himself, plodding along the same track. If he aims at taking honours in natural science, he must curtail his exercise to the limits of a short "constitutional" and cut himself off from the common pleasures of the cricket-field and the river. His very social gatherings tend to consist more and more exclusively of men working in his own department. The Union and other such Clubs are given up for scientific Societies, where he thinks he can combine business with amusement. Such a life can hardly fail to narrow the most sympathetic mind, to hamper and confine the most commanding intellect; it is most unlikely to turn out a practitioner of the highest and most useful type. To live in a clique where priggishness is fostered by the worst kind of mutual admiration is hardly the ideal of University education. Fortunately the curriculum indicated in the pamphlet to which I have referred is not compulsory, and an intending medical student might not be altogether unwise if he decided to pass the first three years of his career in the ordinary pursuits of the University before turning his attention to more technical studies. Even the delay of a year or two would be more than counterbalanced to some by the benefits which such a course would undoubtedly confer.

Oxford. GEORGE I. WILSON.

Migration of Swallows along the Southern Coast.

THE following notes were made by me during a short stay at Lulworth, twelve miles east of Weymouth, from September 16 to 26. They may be of interest to some of your readers, as I have not been able to find the facts I observed recorded in any work on British birds.

When I arrived at Lulworth on the 16th, swallows and house-martins were about, but in no great numbers. On the 19th, in the course of a walk, I observed a few swallows apparently moving eastwards; and this caused me to spend the next morning on the top of a high and narrow ridge of down (Bindon Hill), running parallel with the sea—an admirable position for observation, as the movements of all birds were discernible from it at a long distance. The wind was east-north-east, and the air cold and very clear.

In half-an-hour it became clear to me that a general migration of swallows and martins was taking place along the coast in an easterly direction. The air would be thick with birds over my head for two or three minutes; then for a considerable interval hardly a bird would be visible. An ordinary glance at these dense parties was not enough to prove that they were travelling, or to show in which direction they were going; but by keeping the eye steadily upon them for some little time, and bringing the field-glass to bear on them when the eye failed, it became obvious that they were going east at a steady rate of speed, and apparently following the long spine of chalk down on which I stood, which extends from near Weymouth as far as Poole Harbour. The migration on this large scale lasted during the whole of that morning; in the afternoon the parties did not seem so large.

The next day (the 21st) a strong east wind was blowing, and the birds were not travelling high in air, but creeping steadily along the flanks of the down, and on the lower ground north and south of it. They were continually tacking, but every individual that I followed with my glass was moving swiftly towards the east. Those that were on the southern or seaward side of the down would come upon the sea at one point where the coast turns sharply northwards for a short distance; they did not attempt, however, to leave the land, but turned northwards with the coast, and pursued their way along the heights. On the 22nd and 23rd the same thing went on, but the numbers of the birds seemed to diminish, and they no longer went in parties that were plainly discernible. All this time there were a very few stationary swallows in one or two warm corners by the seaside.

From Dorset I went to Devonshire on the 26th. At Crediton and at Bideford (both warm and sheltered towns), I did not see a dozen swallows or martins in the course of a week; but I learnt that they had gathered for departure a few days before. I have since been informed that the gatherings had been noticed in Cornwall in the first week of the month. I infer that the migration I saw at Lulworth was that of the extreme West of England birds, who were proceeding along the coast to the point at which they cross the Channel. I should be glad to know where that point is.

I had reason to believe that one or two other species were moving up regularly in the same direction. The well-known migration of the pied wagtail was apparently over; but the large number of gray wagtails in a district almost destitute of water was very striking, and, as far as I could see, these also were passing eastwards. But I hope to make further observations next year.

I may add that, on returning to my home in Oxfordshire in the first week of October, I found swallows and martins passing over my village in parties during the earlier hours of each day; but, owing to the want of a convenient elevated position for watching, it was much more difficult to follow their movements than it had been at Lulworth. W. WARDE FOWLER.

Swifts.

THOUGH I cannot add anything to the interesting and valuable evidence given by your correspondent, in your last issue, with regard to swifts remaining on the wing during the dark hours of a summer night, it reminds me of a most beautiful exhibition of their flight which I witnessed at Moscow this last summer. It was on August 2, as the last rays of the setting sun were lighting up the domes and cupolas of that wonderful city, which we gazed upon from the heights of the Kremlin for the first time, that we noticed hundreds of these birds wheeling round their summits or darting hither and thither in every direction. At the same time the matchless Russian bells were pealing forth from every bell-tower in honour of the Empress's birthday, which was to be celebrated on the morrow, and it was surely difficult to believe that the swifts were not revelling in the music like ourselves, especially as I cannot remember ever seeing them again in such numbers, though our visit to Moscow was prolonged for ten days, and we frequently visited the Kremlin at the same hour. E. BROWN.

Further Barton, Cirencester, October 29.

Hughes's Induction Balance.

THE points noted by Mr. Cook on page 605 (vol. xxxvi.), are merely the well-known facts that a magnetic body has most effect when presented to the coils end-ways, *i.e.* with its greatest dimension along their axis, whereas a substance which acts mainly by conduction has most effect when presented flat-ways, or parallel to their face. Any possible effect due to diamagnetism is far too small to be thus easily noticed. OLIVER J. LODGE.

The Ffynnon Beuno and Cae Gwyn Caves.

I THINK it would be well for geologists and anthropologists to allow the age of the deposits and stone instruments found at these caves to remain an open question for the present. At present I have had no opportunity of seeing any papers on the subject, and I know of no opinions other than the one expressed by Dr. Hicks, NATURE, vol. xxxvi. p. 599. I am however fairly well acquainted with the glacial deposits of North Wales and with Palæolithic implements, and I have seen the caves and the tools found at and in them. My quite unbiased opinion is and will so remain,—unless I get very convincing proof to the contrary,—that the drift at the caves has been without doubt relaid; and is no more a true glacial deposit than the valley gravels of the Thames. As for the tools—one in the British Museum (South Kensington), and one in Denbighshire—they belong to the *very latest* of Palæolithic times, and might be passed for Neolithic; the Denbighshire example seen by me is a knife-flake with fine secondary chipping up one edge.

Dunstable. WORTHINGTON G. SMITH.

SYNTHESIS OF GLUCOSE.

ANOTHER important acquisition to our store of knowledge has recently been made. Glucose, commonly called grape-sugar, has been artificially prepared by Drs. Emil Fischer and Julius Tafel in the chemical laboratory of the University of Würzburg. This happy achievement, which is announced in the number of the *Berichte* just received, is one which has long been looked forward to, and which cannot fail to give deep satisfaction in chemical circles all over the world. As is generally the case in syntheses of this description, not only has the sugar itself been actually prepared, but, what is at least quite as important, considerable light has been thrown upon that much-discussed question—the constitution of sugars. A most remarkable, and yet only to be expected, attribute of this artificial sugar is that it is found to be entirely incapable of rotating a beam of polarized light. As is well known, there are several naturally-occurring varieties of glucose, all of which may be expressed by the same empirical constitution $C_6H_{12}O_6$, and all possessing the power of rotating the plane of polarization: dextrose, or grape-sugar, the best-known of these varieties, as its name implies deviates the plane of polarization to the right, as do several other less important varieties; while lævulose, or fruit-sugar, rotates the plane to the left. But in artificially preparing a glucose of the composition $C_6H_{12}O_6$ there is just as much tendency for one kind to be formed as another, and the probability is that both dextro and lævo are simultaneously formed, and thus neutralize each other, producing a totally inactive mixture. It may be that, as in the case of racemic acid, the two kinds are formed side by side and neutralize each other in the solution; or it may even be that, as is the case with truly inactive tartaric acid, there is a true neutralization within the molecule itself; which of these hypotheses is correct is a question for further work to decide.

The substance employed as the base of operations was acrolein, $CH_2{=}CH{-}CHO$, the aldehyde derived by oxidation of allyl alcohol. The acrolein was first converted to its dibromide, $CH_2Br\,.\,CHBr\,.\,CHO$, which was then treated with cold baryta water, whereupon the bromine was removed by the barium leaving the artificial sugar in solution. The real difficulty was found to be in the isolation of the sugar, but this was eventually overcome by the use of phenyl hydrazine, $C_6H_5\,.\,NH\,.\,NH_2$, which forms a hydrazine compound of the formula $C_{18}H_{22}N_4O_4$ with the new sugar, very similar to the compounds formed by phenyl hydrazine with ordinary dextrose and lævulose. This phenyl hydrazine compound was then found to yield by reduction a base $C_6H_{13}NO_5$, which, on treatment with nitrous acid, parted with its nitrogen and left a syrupy substance, possessing all the properties of sugars, and distinguished only from ordinary grape-sugar by its optical inactivity.

The actual operations were performed briefly as follows:—

Seventy-five grammes of pure crystallized barium hydrate were dissolved in a little over a litre of water, and 50 grammes of previously redistilled acrolein dibromide added drop by drop, the flask being continuously agitated, surrounded by ice-cold water, for about an hour. In a similar manner eight successive quantities were treated until in all about 400 grammes of acrolein dibromide had been converted into sugar. These eight separate portions were then mixed, slightly acidified with sulphuric acid, and the barium precipitated with a solution of sodium sulphate. After removal of all the barium by filtration the solution was neutralized with soda and evaporated down to 1½ litres. On cooling, a solution of 50 grammes of the hydrochloride of phenyl hydrazine and 50 grammes of crystallized sodium acetate in 100 cubic centimetres of water were added; after standing twelve hours a reddish-brown resin separated out and was removed by filtration. 150 grammes more of phenyl hydrazine hydrochloride and the same quantity of sodium acetate were then added, and the solution warmed upon a water-bath; after again standing some time the solution became turbid, and in course of four hours a dark-coloured precipitate, partly crystalline and partly resinous, separated out. After washing and drying, and subsequent agitation with ether and trituration with alcohol to remove organic impurities, and extraction of the inorganic salts by hot water, the phenyl hydrazine compound was finally isolated.

Analysis of the recrystallized compound indicates that its composition is $C_{18}H_{22}N_4O_4$, and its properties are very similar to those of the phenyl hydrazine compound of ordinary grape-sugar, the melting-points of the two bodies being identical, 205° C. It is almost insoluble in water, ether, and benzene, and only with difficulty soluble in hot alcohol; it is more soluble in glacial acetic acid, but the solution soon becomes dark red. It crystallizes from hot alcohol in pretty little prism aggregates, while the ordinary grape-sugar compound crystallizes in spherical aggregates of fine needles. It is further distinguished from the latter compound inasmuch as a layer 20 cubic centimetres thick, is without action upon a beam of polarized light.

When reduced by means of zinc dust and acetic acid, a base was produced analogous to the one formed by the reduction of the corresponding phenyl hydrazine compound of grape-sugar. This base was difficult to isolate, owing to the non-crystallizable nature of its acetate; the fact was fortunately discovered, however, that its oxalate was crystalline, and readily obtained pure. Hence its analysis has been effected, and the numbers found point to the composition $(C_6H_{13}NO_5)_2\,.\,C_2H_2O_4$. This base reduces Fehling's solution strongly on warming, and with phenyl hydrazine regenerates the parent compound; but, once again, is optically inactive.

Finally, by the action of nitrous acid, nitrogen at once began to be evolved, and when the evolution ceased the liquid was neutralized with soda, evaporated *in vacuo*, and the residue extracted with alcohol. On evaporation of the alcohol the sugar was left as a bright brown syrup, free from nitrogen and ash, of sweet taste, and capable of instantly reducing Fehling's solution.

Up to the present time two hypotheses as to the constitution of sugars have pretty evenly balanced each other. According to one, sugars are considered, in virtue of their power of reducing ammoniacal silver solutions, as aldehydes containing also alcohol groups; on these lines grape-sugar would be formulated, $CH_2OH{-}(CHOH)_4{-}CHO$. But it has since been shown that the property of reducing ammoniacal silver solutions is not confined to aldehydes, for the series of bodies known as ketone alcohols also possess it; hence grape-sugar may also be written $CH_2OH{-}(CHOH)_3{-}CO{-}$

CH_2OH. Both theories account for most of the hitherto known reactions of the glucoses, hence the matter has remained an open question. Drs. Fischer and Tafel, however, consider that their synthesis from acroleïn, which is itself an aldehyde, points to the probability of the former hypothesis being the correct one. The action of baryta water upon the dibromide evidently causes a simple exchange of bromine for hydroxyl, and the first product of the reaction is almost as certainly glycerine aldehyde, $CH_2OH—CHOH—CHO$. This latter substance, however, appears to polymerize at once under the influence of the baryta water into sugar, two molecules of glycerine aldehyde uniting to form a molecule of glucose.

In consideration of the fact of its derivation from acroleïn, the name acrose has been applied to the sugar which has been, with so much skill and steady determination, synthetically formed and isolated; and there can be no doubt that this name will stand as a memento of the progress made in organic chemistry during the year 1887. A. E. TUTTON.

MODERN VIEWS OF ELECTRICITY.[1]

PART II.—CURRENT ELECTRICITY (*continued*).

IV.

Electrical Inertia.

RETURNING now to the general case of conduction, without regard to the special manner of it, we must notice that, if a current of electricity is anything of the nature of a material flow, there would probably be a certain amount of inertia connected with it, so that to start a current with a finite force would take a little time; and the stoppage of a current would also have either to be gradual or else violent. It is well known that if water is stagnant in a pipe it cannot be quite suddenly set in motion; and again, if it be in motion, it can only be suddenly stopped by the exercise of very considerable force, which jars and sometimes bursts the pipe. This impetus of running water is utilized in the water-ram. It must naturally occur, therefore, to ask whether any analogous phenomena are experienced with electricity; and the answer is, they certainly are. A current does not start instantaneously: it takes a certain time—often very short—to rise to its full strength; and when started it tends to persist, so that if its circuit be suddenly broken, it refuses to stop quite suddenly, and bursts through the introduced insulating partition with violence and heat. It is this ram or impetus of the electric current which causes the spark seen on breaking a circuit; and the more sudden the breakage the more violent is the spark apt to be.

The two effects—the delay at making circuit, and the momentum at breaking circuit—used to be called "extra-current" effects, but they are now more commonly spoken of as manifestations of "self-induction."

We shall understand them better directly; meanwhile they appear to be direct consequences of the inertia of electricity; and certainly if electricity *were* a fluid possessing inertia it would behave to a superficial observer just in this way.

But if an electric current really possessed inertia, as a stream of water does, it would exhibit itself not only by these effects but also mechanically. A conducting coil delicately suspended might experience a rotary kick every time a current was started or stopped in it; and if a steady current were maintained in such a coil it should behave like a top or gyrostat, and resist any force tending to deflect its plane.

Clerk Maxwell has carefully looked for this latter form of momentum effect, and found none. One may say, in fact, that nothing like momentum has yet been observed in an electric current by any *mechanical* mode of examination. A coil or whirl of electricity does not behave in the least like a top.

Does this prove that a current has no momentum? By no means necessarily so. It might be taken as suggesting that an electric current consists really of two equal flows in contrary directions, so that mechanically they neutralize one another completely, while electrically—*i.e.* in the phenomena of self-induction or extra-current—they add their effects. Or it may mean merely that the momentum is too minute to be so observed. Or, again, the whole thing—the appearance of inertia in some experiments and the absence of it in others—may have to be explained in some altogether less simple manner, to which we will proceed to lead up.

Condition of the Medium near a Circuit.

So far we have considered the flow of electricity as a phenomenon occurring solely inside conductors; just as the flow of water is a phenomenon occurring solely inside pipes. But a number of remarkable facts are known which completely negative this view of the matter. Something is no doubt passing along conductors when a current flows, but the disturbance is not *confined* to the conductor; on the contrary, it spreads more or less through all surrounding space.

The facts which prove this have necessarily no hydraulic analogue but must be treated *suorum generum*, and they are as follows:—

(1) A compass needle anywhere near an electric current is permanently deflected so long as the current lasts.

(2) Two electric currents attract or repel one another, according as they are in the same or opposite directions.

(3) A circuit in which a current is flowing tends to enlarge itself so as to inclose the greatest possible area.

(4) A circuit conveying a current in a magnetic field tends either to enlarge or to shrink or to turn half round according to the aspect it presents to the field.

(5) Conductors in the neighbourhood of an electric circuit experience momentary electric disturbances every time the current is started or stopped or varied in strength.

(6) The same thing happens even with a steady current if the distance between it and a conductor is made to vary.

(7) The effects of self-induction, or extra-currents, can be almost abolished by twisting the covered wire conveying the current closely on itself, or even by laying the direct and return wire side by side; whereas they may be intensified by making the circuit inclose a large area, more by coiling it up tightly into close coil, and still more by putting a piece of iron inside the coil so formed.

Nothing like any of these effects is observable with currents of water; and they prove that the phenomena of the current, so far from being confined to the wire, spread out into space and affect bodies at a considerable distance.

Nearly all this class of phenomena were discovered by Ampère and by Faraday, and were called by the latter "current-induction." According to his view the dielectric medium round a conducting circuit is strained, and subject to stresses, just as is the same medium round an electrically charged body. The one is called an electrostatic strain, the other an electro-magnetic or electrokinetic strain.

But whereas electrostatic phenomena occur *solely* in the medium—conductors being mere breaks in it, interrupters of its continuity, at whose surface charge-effects occur but whose substance is completely screened from disturbance—that is not the case with electro-kinetic phenomena. It would be just as erroneous to conceive electro-kinetic phenomena as occurring solely in the insulating medium as it would be to think of them as occurring solely in the conducting wires. The fact is, they occur in

[1] Continued from vol. xxxvi. p. 585.

both—not only at the surface of the wires like electrostatic effects, but all through their substance. This is proved by the fact that conductivity increases in simple proportion with sectional area; it is also proved by every part of a conductor getting hot; and it is further proved in the case of liquids by their decomposition.

But the equally manifest facts of current attraction and current induction prove that the effect of the current is felt throughout the surrounding medium as well, and that its intensity depends on the nature of that medium; we are thus wholly prevented from ascribing the phenomenon of self-induction or extra-current to simple and straightforward inertia of electricity in a wire like that of water in a pipe.

We are thus brought face to face with another suggestion to account for these effects, viz. this: Since the molecules of a dielectric are inseparably connected with electricity, and move with it, it is possible that electricity itself has no inertia at all, but that the inertia of the atoms of the displaced dielectric confer upon it the appearance of inertia. Certainly they do sometimes confer upon it this appearance, as we see in the oscillatory discharge of a Leyden jar. For a displaced thing to overshoot its mean position and oscillate till it has expended all its energy, is a proceeding eminently characteristic of inertia; and so, perhaps, the phenomena of self-induction are similarly, though not so simply, explicable.

Further consideration of this difficult part of the subject is however best postponed to Part III.

Energy of the Current.

I have now called attention to the fact that the whole region surrounding a circuit is a field of force in which many of the most important properties of the current (the magnetic, to wit) manifest themselves. But directly we begin thus to attend to the whole space, and not only to the wires and battery, *a very* curious *question* arises. Are we to regard the current in a conductor as propelled by some sort of end-thrust, like water or air driven through a pipe by a piston or a fan, or are we to think of it as propelled by side forces, a sort of lateral drag, like water driven along a trough by a blast of air or by the vanes of paddle-wheels dipping into it? Or, again, referring to the cord models, Figs. 5, 6, and 13, were we right in picturing the driving force of the battery as located and applied where shown in the diagrams, or ought we to have schemed some method for communicating the power of the battery by means of belts or other mechanism to a great number of points of the circuit?

Prof. Poynting has shown that, on the principles developed by Maxwell, the latter of these alternatives, though apparently the more complicated, is the true one; and he has calculated the actual paths by which the energy is transmitted from the battery to the various points of a circuit, for certain cases.

We must learn, then, to distinguish between the flow of *electricity* and the flow of electric *energy*: they do not occur along the same paths. Hydraulic analogies, at least hydraulic analogies of a simple kind, break down here. When hydraulic power or steam power is conveyed along pipes, the fluid and its energy travel together. Work is done at one end of the tube in forcing in more water, and this is propagated along the tube and reappears at the distant end as the work of the piston. But in electricity it is not so. Electric energy is not to be regarded as pumped in at one end of a conducting wire, and as exuding in equal quantities at the other. The *electricity* does indeed travel thus—whatever the travel of electricity may ultimately be found to mean—but the energy does not. The battery emits its energy, not to the wire direct, but to the surrounding medium; this is disturbed and strained, and propagates the strain on from point to point till it reaches the wire and is dissipated. This, Prof. Poynting would say, is the function of the wire: it is to dissipate the energy crowding into it from the medium, which else would take up a static state of strain and cease to transmit any more. It is by the continuous dissipation of the medium's energy into heat that continuous propagation is rendered possible.

The energy of a dynamo does not therefore travel to a distant motor through the wires, but through the air. The energy of an Atlantic cable battery does not travel to America through the wire strands, but through the insulating sheath. This is a singular and apparently paradoxical view, yet it appears to be well founded.

Think of a tram-car drawn by an underground rope, like those in the streets of Chicago or Hampstead Hill. A contact piece of iron protrudes from the bottom of the car and grips the moving rope, which is thus enabled to propel the car. How does the energy of the distant stationary engine reach the car? *Viâ* the rope and the iron connector, undoubtedly. They both have to be strong, and are liable to be broken by the transmitted stress.

Next, think of an electric tram-car driven by means of a current taken up from an underground conductor, like that of Mr. Holroyd Smith at Manchester, or at the late Inventions Exhibition. A contact piece of wire rope protrudes from the bottom of the car and drags a little truck along the conductor, which is thus enabled to supply electricity to the electro-magnetic motor geared to the wheels. How does the energy of the distant dynamo reach the car in this case? *Not viâ* the wire connector; not even *viâ* the underground conductor. It travels from the distant dynamo through the general insulating medium between cable and earth, some little enters the conductor and is dissipated, but the great bulk flows on and converges upon the motor in the car, which is thus propelled. All the energy of the conducting wire is dissipated and lost as heat: it is the energy of the insulating medium which is really transmitted and utilized.

Phenomena peculiar to a Starting, or Stopping, or Varying Current.

There is a remarkable fact concerning electric currents of varying strength, which has been lately brought into prominence by the experimental skill of Prof. Hughes, viz. that a current does not start or stop equally and simultaneously at all points in the section of a conductor, but starts at the outside first. This fact is naturally more noticeable with thick wires than with thin, and it is especially marked in *iron* wires, for reasons which in Part III. will become apparent; but the general cause of it in ordinary copper wires can very easily be perceived in the light of the views of Prof. Poynting just mentioned.

For, remember that a current in a wire is not pushed along by a force applied at its end, so as to be driven over obstacles by its own momentum combined with a *vis a tergo*; but it is urged along at every point of its course by a force just sufficient to make it overcome the resistance there, and no more, the force being applied to it through the medium of the dielectric in which the wire is immersed. A lateral force it is which propels the electricity; and it naturally acts first on the outer layers of the wire or rod, only acting on the interior portions through the medium of the outside.

To illustrate this matter further, rotate a common tumbler of liquid steadily for some time and watch the liquid; dusting powder perhaps over it to make it more visible. You will see first the outer layer begin to participate in the motion, and then the next, and then the next, and so on, until at length the whole is in rotation. Stop the tumbler, and the liquid also begins gradually to stop by a converse process.

If the liquid sticks together pretty well, like treacle, the motion spreads very rapidly: this corresponds to a

poor conductor. If the liquid be very mobile, the propagation of motion inward is slow; this corresponds to a very good conductor. If the liquid were perfectly non-viscous, it would correspond to a perfect conductor, and no motion would ever be communicated to it deeper than its extreme outer skin.

Think now of a long endless tube full of water, say the hollow circumference of a wheel, and spin it: the liquid is soon set in rotation, especially if the tube be narrow or the liquid viscous; but it is set in motion by a lateral not an end force, and its outer layers start first.

Just so is it with a current starting in a metal wire. If the wire be fine, or its substance badly conducting, it all starts nearly together; but if it be made pretty thick, and of well conducting substance, its outer layers may start appreciably sooner than the interior. And if it were infinitely conducting, no more than the outer skin would ever start at all.

In actual practice the time taken for all the electricity in an ordinary wire to get into motion is excessively short—something like the thousandth of a second—so that the only way to notice the effect is to start and reverse the current many times in succession.

If the hollow-rimmed wheel above spoken of were made to oscillate rapidly, it is easy to see that only the outer layers of water in it would be moved to and fro; the innermost water would remain stationary; and accordingly it would appear as if the tube contained much less water than it really does. The virtual bore of the pipe would, in fact, for many purposes be diminished. So is it also with electricity; the sectional area of a wire to a rapidly alternating current is virtually lessened so far as its conducting power is concerned; and accordingly its apparent resistance is slightly higher for alternating than for steady currents. The effect is however too small to notice in practice except with thick wires and very rapid alternations.

By splitting up the conductor into a bundle of insulated wires, thus affording the dielectric access to a considerable surface of conductor, the force is applied much more thoroughly, and so the effect spoken of is greatly lessened. The same thing is achieved by rolling out the conducting-rod into a flat thin bar. Making the conductor hollow instead of solid offers no particular advantage, because no energy travels *viâ* the hollow space, it still arrives only from the outside; *unless*, indeed, the return part of the circuit is taken along the axis of the hollow like a telegraph cable. In this last arrangement all the energy travels *viâ* the dielectric between the two conductors, and none travels outside at all. It will be perceived therefore that, as in static electricity, the term "outside" must be used with circumspection: it really means that side of a conductor which faces the opposite conductor across a certain thickness of dielectric.

We learn from all this that, whereas in the case of steady currents the sectional area and material of a conductor are all that need be attended to, the case is different when one has to deal with rapidly alternating currents, such as occur in a telephone, or, again, such as are apt to occur in a Leyden-jar discharge (see Part I., p. 560), or in lightning.

In all these cases it is well to make the conductor expose considerable surface to the propelling medium—the dielectric—else will great portions of it be useless.

Hence, a lightning-conductor should not be a round rod, but a flat strip, or a strand of wires, with the strands as well separated as convenient: and though I have not yet mentioned the special effect of iron, I may as well say here that iron is about 90,000 times worse than copper for the purpose of a lightning conductor in respect of the phenomenon just described, seven times as bad on account of its inferior conducting power, and about twice as good as copper because of its higher melting-point and specific heat.

The Question of Electrical Momentum again.

We are now able to return to the important question whether an electric current has any momentum or not, as it would have if it were a flow of material liquid. Referring to Part I. (p. 533), a hint will be found that the laws of flow of a current in conductors—the shape of the stream-lines, in fact—are such as indicate no inertia, or else no friction. Now Ohm's law shows that at any rate *friction* is not absent from a current flowing through a metal; hence it would appear at first sight as if *inertia* must be absent.

The stream-lines bear upon the question in the following kind of way. If an obstacle is interposed in the path of a current of water, the motion of the water is unsymmetrical before and behind the obstacle. The



Fig. 14.—Stream-lines of water flowing through a pipe with an obstruction in it.

stream-lines spread out as the water reaches the obstacle, and then curl round it, leaving a space full of eddies in its wake (Fig. 14).

But if one puts an obstacle in the path of an electric current—say by cutting a slit in a conducting strip of tinfoil—the stream-lines on either side of it are quite symmetrical, thus—

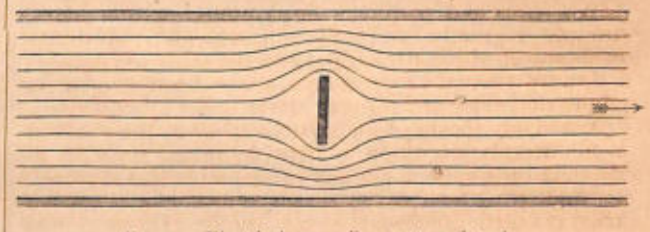


Fig. 15.—Electrical stream-lines past an obstacle.

And this is exactly what would be true for water also, if only it were devoid either of friction or of inertia, or of both.

Is not this fact conclusive, then? Does it not prove the absence of momentum in electricity?

Plainly the answer must depend on whether there is any other possible mode of accounting for this kind of flow. And there is.

For suppose that water, instead of being urged by something not located at or near the obstacle—instead of being left to its own impetus to curl round or shoot past as it pleases—suppose it were propelled by a force acting at every point of its journey, a force just able to drive it at any point against the friction existing at that point and no more; then the flow of water would take place according to the electrical stream-lines shown in Fig. 15.

An illustration of such a case is ready to hand. Take a spade-shaped piece of copper wire or sheet, heat it a little, and fix it in quiescent smoky air; looking along it through a magnifier in a strong light you will see the warmed air streaming past the metal according to the stream-lines of Fig. 15; and this just because the moving force has its location at the metal surface, and not in some region below it. (See Lord Rayleigh, NATURE, vol. xxviii. p. 139). One cannot indeed say that it is propelled at every

point of its course, but it is propelled at the critical points where the special friction occurs, and this comes to sufficiently the same thing.

We learn, therefore, that stream-lines like Fig. 15 prove one of three things, not one of two; and the three things are: (1) that the fluid has no friction; or (2) that it has no inertia; or (3) that it is propelled at every point of its course.

If any one of these is true of electricity, there is no need to assume either of the others in order to explain the actual manner of its flow. Now we have just seen that, according to Prof. Poynting's interpretation of Maxwell's theory, the third of the above is true—electricity is propelled at every point of its course; consequently, as said in Part I. (p. 533), the question of its inertia so far remains completely open.

Voltaic Battery.

Leaving this singular mode of regarding the subject for the present, to return to it perhaps after Part III., let us proceed to ask how it comes about that a common battery or a thermopile is able to produce a current.

If we allow ourselves to assume the existence of an unexplained chemical attraction between the atoms of different substances, an explanation of the action of an ordinary battery cell is easy. You have first the liquid containing, let us say, hydrogen and oxygen atoms, free or potentially free—that is, either actually dissociated or so frequently interchanging at random from molecule to molecule that the direction of their motion may be guided by a feeble directive force. Each of these atoms in the free state possesses a charge of electricity—the hydrogen all a certain amount of positive electricity, the oxygen twice that amount of negative. Into this liquid you then plunge a couple of metals which attract these atoms differently: for instance, zinc and copper, which both attract oxygen, but zinc more than copper; or, better, zinc and platinum, the latter of which hardly attracts it at all; or, better still, zinc and peroxide of lead, one of which attracts oxygen, the other hydrogen.

Immediately, the free oxygen atoms begin moving up to the zinc, the free hydrogen atoms to the other plate.

When one speaks of the plates attracting the atoms, it is not necessary to think of their exerting a force on all those in the liquid, distant and near: all that is necessary is to assume a force acting on those which come within what is called "molecular range" of its surface—a distance extremely minute, and believed to be about the ten-millionth part of a millimetre. If the zinc plate removes and combines with all the oxygen atoms which come within this range, they will be speedily replaced by others from the next more distant layer by diffusion, and these again by others, and so on. And thus there will be a gradual procession of oxygen atoms all through the liquid towards the zinc, the rate of the procession being regulated by the force acting, and by the rate of diffusion possible in the particular liquid used. All the atoms which reach the zinc neutralize a certain portion of its electricity by means of the positive charge they carry, and thus very soon it would become positively electrified enough to neutralize its attractive power on the similarly charged oxygen atoms, and everything would stop. But if a channel for the escape of its electricity be provided by leading a wire from it to the copper plate, the circuit is completed, the electricity streams back by the wire, and the procession goes steadily on. The electricity thus imparted to the copper, or platinum, neutralizes any repulsion it exerted on the negatively charged hydrogen atoms, and makes them in a similar way begin a procession towards it, deliver up their charges to it, combine with each other, and escape as gas.

Without going into all the niceties possible, this mode of thinking of the matter at least calls attention to some of the more salient features of a battery.

If, instead of two different plates, plates of the *same* metal be immersed, they will need to be oppositely electrified by some means before they are able to cause the two opposite processions, and so maintain a current in the liquid. This plainly corresponds to a voltameter.

Taking advantage of the known fact that the atoms are charged, Helmholtz avoids the necessity for postulating any chemical (non-electrical) force between zinc and oxygen, by imagining that all substances have a specific attraction for electricity itself, and that zinc exceeds copper and the other common metals in this respect.

He would thus think of the zinc attracting, not the oxygen itself, but its electric charge; and so would liken a battery cell still more completely to a voltameter. The polarization or opposition force acting at the hydrogen-evolving plate he would account for by the attraction of hydrogen for negative electricity, and the consequent repugnance of the hydrogen atoms to part with their charges.

Thermo-electric Pile.

A thermopile may be thought of in the following way, but in trying to understand the nature of these actions at present one must admit that some speculation and vagueness exist.

We have seen that when electricity is propelled through or among the molecules of a metal it experiences a certain resistance or opposition force which is exactly proportional to the speed of its motion. In other words, there is a connexion between matter and electricity in many respects analogous to fluid friction but varying accurately as the first power of the relative velocity. Hence, if an atom of matter be vibrating about a fixed point, it will tend to drive electricity to and fro with it; but if it be only one of a multitude, all quivering in different phases, they will none of them achieve any propulsion. This may be considered the state of an ordinary warm solid. But if from any cause a set of atoms could be made to move faster in one direction than in the reverse direction—to move forwards quickly and backwards slowly—then such an unsymmetrically-moving set *will* exert a propulsive tendency and tend to drive a current of electricity forwards, simply because the force exerted is proportional to the velocity, and so is greater on the forward journey than on the return.

Wherever conduction of heat is going on along a substance the atoms are in this condition. They are driven forward infinitesimally quicker, by the more rapidly moving atoms at the hot end, than they are driven back by the less rapidly moving atoms in front. And hence such a slope of temperature exerts a propulsive tendency: there is an electromotive force in a substance unequally heated.

This fact was discovered theoretically and verified experimentally by Sir William Thomson.

But not only is there such a force at a junction of a hot and cold substance, there is also a force at the junction of two substances of different kinds, even though the temperature be uniform. It is not quite so easy to explain how it now comes about that the atoms at this kind of junction are moving faster one way than the other; nevertheless, such a thing is not unlikely, considering the state of constraint and accommodation which must necessarily exist at the boundary surface of two different media. However it be caused, there is certainly an E.M.F. at such a junction.

Thus, then, in a simple circuit of two metals, with their junctions at different temperatures, there are altogether four electromotive forces—one in each metal, from hot to cold or *vice versâ*, and one at each junction; and the current which flows round such a circuit is propelled by the resultant of these four.

But the contact force at a junction is by no means confined to metals. It occurs between insulators also, and it is to it that the striking effects produced by all frictional electric machines are due.

By thus noticing that the connexion between matter and electricity, known as resistance and defined by Ohm's law, is competent to produce contact electromotive forces, we may perceive how it comes to pass that in good conductors such forces are so weak, while in insulators they are so strong. Electricity slips through the fingers of a metal as it were, and the driving force it can exert is very feeble; while an insulator gets a good grip and thrusts it along with violence.

The metals differ in their gripping power, and, roughly speaking, the best conductor makes the worst thermo-electric substance. A bad conductor, like antimony, or, still better, galena, or selenium, or tellurium, makes a far more effective thermo-electric element than a well-conducting metal. Not that specific resistance is all that has to be considered in the matter; there is also a specific relation between each metal and the two kinds of electricity. Thus, iron is a metal whose atoms have a better grip of positive than of negative electricity, and so a positive current gets propelled in iron from hot to cold. Copper, on the other hand, acts similarly on negative electricity, and it is a negative current which is driven from hot to cold in copper. And all the metals can be classed with one or other of these two, except perhaps lead, which appears to grip both equally, and so to exert no differential effect upon either.

Passage of Electricity through a Gas.

There remains to be said something about the way in which electricity can be conveyed by *gases*.

The first thing to notice is that there is no true conduction through either gases or vapours; in other words, a substance in this condition seems to behave as a perfect insulator—perhaps the only perfect insulator there is. Not even mercury vapour is found to conduct in the least. This shows that mere bombardment of molecules, such as is known to go on in gases, is not sufficient either to remove or to impart any electric charge.

The commonest way in which electricity makes its way through a gas, setting aside the mere mechanical conveyance by solid carrier, is that of disruptive discharge. Let us try and look into the manner of this a little more closely, if possible.

First of all, since locomotion is possible to the molecules of a gas the same as of any other fluid, it is natural to ask why electrolysis does not go on as in a liquid. Now, for electrolysis in a liquid two conditions seemed necessary: first, that the atoms or radicles in a molecule should be oppositely charged with electricity; second, that they should be in such a condition (whether by dissociation or otherwise) that interchanges of atoms from molecule to molecule, or, in some other way, a procession of atoms, could be directed in a given direction by a very feeble or infinitesimal force.

Since a gas does *not* act as an electrolyte, one of these conditions, or perhaps both, must fail. Either the atoms of a gas-molecule are not charged, which is a plausible hypothesis for elementary gases, or else the atoms belonging to a gas-molecule remain individually belonging to it, and are not readily passed on from one to another.

When one says that a gas does not act as a common electrolyte, the experimental grounds of the statement are that a finite electrostatic stress certainly is possible in its interior—a stress of very considerable amount; and when this stress does overstep the mark and cause the electrode to yield, the yielding is evidently not a quiet and steady glide or procession, but a violent breaking down and collapse, due to insufficient tenacity of something. One may therefore picture the molecules of a gas, between two opposite electrodes or discharge terminals maintained at some great difference of potential, as arranged in a set of parallel chains from one to the other, and strained nearly up to the verge of being torn asunder. In making this picture one need not suppose any fixture of individual molecules: there may be a wind blowing between the plates; but all molecules as they come into the field must experience the stress, and be relieved as they pass out.

If the applied slope of potential overstep a certain limit, fixed by observation at something like 33,000 volts per linear centimetre for common air, the molecules give way, the atoms with their charges rush across to the plates, and discharge has occurred. The number of atoms thus torn free and made able to convey a charge by locomotion is so great that there has never been found any difficulty in conveying any amount of electricity by their means. In other words, *during* discharge the gas becomes a conductor, and, being a conductor by reason of locomotion of atoms, it may be called an electrolytic conductor.

But whether the charge then possessed by each carrier atom intrinsically belonged to it all the time, or whether it was conferred upon the components of the molecules during the strain and the disruption, is a point not yet decided.

What is called "the dielectric strength" of a gas—that is, the strain it can bear without suffering disruption and becoming for the instant a conductor—depends partly on the nature of the gas, and very largely on its pressure. Roughly, one may say that a gas at high pressure is very strong, a gas, at low pressure very weak. An ordinary electrolyte might be called a dielectric of zero strength.

One reason why pressure affects the dielectric tenacity of a gas readily occurs to one: it is certainly not the only one, but it can hardly help being at least partially a *vera causa*; and that is, the fact that in a rare gas there are fewer molecules between the plates to share the strain between them.

Thus if 40,000 volts per centimetre break down ordinary air, 40 volts per centimetre ought to be enough to effect discharge through air at a pressure of about $\frac{1}{4}$ millimetre of mercury; and at a pressure of 50 atmospheres 2,000,000 volts per centimetre should be needed.[1]

A Current regarded as a Moving Charge.

To review the ground we have covered so far. We first tried to get some conception of the nature of electrostatic charge, and the function of a dielectric medium in static electricity. We next proceeded to see how far the phenomena of current electricity could be explained by reference to electrostatics. For a current, being merely electricity in locomotion, need consist of nothing but a charged body borne rapidly along.

Charge a sphere with either positive or negative electricity, and throw it in some direction: this constitutes a positive or a negative current in that direction. There is nothing necessarily more occult than that. And a continuous current between two bodies may be kept up by having a lot of pith balls, or dust particles, oscillating from one to the other, and so carrying positive electricity one way, and negative the other way. But such carriers, as they pass each other with their opposite charges, would be very apt to cling together and combine. They might be torn asunder again electrically, or they might be knocked asunder by collision with others. Unless they were one or other, the current would shortly have to cease, and nothing but a polarized medium would result.

Instead of pith balls, picture charged atoms as so acting, and we have a rough image of what is going on in an electrolyte on the one hand, and a dielectric on the other. The behaviour of metals and solid conductors is more obscure. Locomotive carriage is not to be thought of in them; but, inasmuch as no new phenomenon appears in their case, it is natural to try and

[1] It is true that tension per unit area, or energy per unit volume, is proportional to the *square* of the potential-slope, and I attach no special importance to the simple proportion assumed in the text. There is a great deal more to be said on these subjects, but this is scarcely the proper place to say it.

picture the process as one not wholly dissimilar; and this is what in one place we tried to do; with, however, but poor success.

I have said that an electric current need be nothing more occult than is a charged sphere moving rapidly; and a good deal has been made out concerning currents by minutely discussing all that happens in such a case. But, even so, the problem is far from being a simple one. One has to consider not only the obviously moving charge, but also the opposite induced charge tied to it by lines of force (or tubes of induction, as they are sometimes called), and we have this whole complicated system in motion. And the effect of this motion is to set up a new phenomenon in the medium altogether—a spinning kind of motion that would not naturally have been expected; whereby two similarly charged spheres in motion repel one another less than when stationary, and may even begin to attract, if moving fast enough; whereby also a relation arises between electricity and magnetism, and the moving charged body deflects a compass needle. Of which more in the next Part. OLIVER J. LODGE.

(*To be continued.*)

THE TWEEDDALE COLLECTION.

THE great collection of birds formed by the late Marquess of Tweeddale has now safely arrived in London, and has been deposited in the Natural History Museum at South Kensington. It is sufficient to say that it equals in extent the valuable donation of American birds presented by Mr. Osbert Salvin and Mr. F. Du Cane Godman, numbering about 27,000 specimens; and though inferior in number of individual skins to the great Hume collection, which reached the phenomenal number of 63,000 specimens, it is not inferior in interest to either of these wonderful collections. Mr. Hume thoroughly worked the territory of the British Asian Empire from Scinde to Assam and Manipur, from Khatmandu to Ceylon, and from Tenasserim to Singapore; but to the eastward of these countries the work had been continued by other naturalists, and the results of their labours are largely represented in the Tweeddale collection, which now forms part of the British Museum.

On the death of the late Marquess, his entire collection and library were bequeathed by him to his nephew, Capt. R. G. Wardlaw Ramsay, of Whitehill, a naturalist of high promise and performance; and in the moment of satisfaction at receiving his magnificent donation one cannot help feeling great regret that the many cares and duties incident upon his succession to the family estates at Whitehill have temporarily deprived him of the leisure necessary for the working out of the great collection left to him by his uncle. The facilities for ornithological study, however, at the Natural History Museum, are now rapidly becoming so perfect that one may reasonably hope that he will, in common with all ornithologists, be able to work in that institution with the same comfort as in his own museum in Scotland. If in future years the student of birds finds that at South Kensington the work he loves can be done more expeditiously and with command of a larger series of specimens than in any other Museum in the world, his gratitude will be largely due to the four naturalists we have mentioned—Mr. Allan Hume, Messrs. O. Salvin and F. D. Godman, and Capt. Wardlaw Ramsay—for the unexampled generosity which has led them to present to the British nation the wonderful collections which will make our Ornithological Museum famous for all time.

Many naturalists who read this article will remember how, twelve years ago, the entire collection of bird-skins in the British Museum was contained in a few book-cases in a dingy cellar at Bloomsbury, where all the skins were kept in wooden boxes—a barbarous method, which was not only clumsy, but actually harmful to the specimens themselves. The development of the collection since that era is one which any English naturalist may consider with pride. Not only is the invaluable series of skins in the British Museum now well cared for and properly housed, but the *raison d'être* of the large collections in private hands has been removed. It is admitted on all sides that had the facilities of study in the old days been such as they now are in the Natural History Museum, there would have been no need for ornithologists to devote their private means to the formation of the collections which have, however, now become the foundation of the greatest Ornithological Museum in the whole world.

The three great collections which have enriched the British Museum during the last two years have each been, in their way, of supreme importance for zoological science. The Hume collection was a perfect marvel in the way of complete series of specimens. Not only are the various plumages of the Indian birds exemplified in a manner hitherto unheard of, but even the geographical ranges of most of the species are illustrated in a perfect way by the series of specimens contained in the collection. The Salvin-Godman donation consisted of American birds, and added hundreds of species to the British Museum which were desiderata to that collection. Though not so rich in series of various plumages as the Hume collection, the number of gaps in the quota of American birds which their donation filled was simply enormous, and from being one of the most backward in regard to its neotropical collection of birds, the British Museum is now one of the foremost as regards the value of its American series.

The Tweeddale collection "takes up the running," so to speak, where Mr. Hume left off, and it must not be supposed that the donation now made by Capt. Wardlaw Ramsay is merely the collection of skins left to him by his uncle. To imagine this would be but a poor appreciation of the energy which has led him during the last few years to develop and greatly increase the collection by the addition of a large number of birds obtained during his military career in the East, and by hundreds of other valuable specimens acquired since his uncle's death. Thus the skins from the Kurrum Valley in Afghanistan, and from the Karen Hills in Burmah, obtained by Capt. Ramsay himself, are supplementary additions of the highest value to the Hume collection, inasmuch as Mr. Hume never had correspondents in these parts, and the specimens from the Andamans and Nicobars are also of great importance; but of course the interest of the Tweeddale collection centres round the expedition to the Philippine Archipelago made by Mr. Alfred Everett for the late Marquess. Mr. Everett visited several islands on which no zoologist had previously trod, and as a natural result he discovered some beautiful new species of birds which are still unrepresented in any other collection but that of Capt. Ramsay. Altogether Mr. Everett furnished material for twelve important memoirs by the Marquess of Tweeddale, and the number of Philippine types now presented to the British Museum adds immensely to the wealth of the donation. Lord Tweeddale was also greatly interested in an obscure family of birds—the Drongos, or Crow-shrikes (*Dicruridæ*)—and possessed a wonderful collection of these birds, although it may be stated that there is scarcely a family of Oriental birds which is not strongly and completely represented in the collection.

Ornithologists will understand the nature of this noble gift of Capt. Ramsay when they learn that in addition to the collection of birds he has also presented the whole of the splendid Tweeddale library (nearly 3000 volumes) to the British Museum, to be placed in the Bird-Room, alongside of the collection of skins, for the benefit of students of ornithology. The Tweeddale library is one of the best in the world, containing many rare volumes which

we have not seen elsewhere, and this donation alone is worth several thousands of pounds. With a series of bird-skins now numbering nearly a quarter of a million, and with the best ornithological library in the world, it will be strange if the work done at the British Museum in future be not rendered an easy and an enjoyable task, though it must be remembered that the very magnitude of the collection contributes to the difficulty of its exact study. The writer may be excused an expression of deep gratitude to the ornithologists who have enriched the collection under his charge, so that from a series of (at the most) 40,000 skins, the number of bird-skins has been raised in fifteen years to more than 200,000, and he merely adds a hope that he may see the British Museum become the repository of all the work of English ornithologists, not only from this country, but from all parts of the Empire.

This article has dealt merely with the three great donations which have been received during the last two years, and has not recorded the many other collections, of almost equal importance, which have been acquired by the Trustees of the British Museum since 1872, the results of the life-work of such naturalists as Sclater, Wallace, Gould, and others of whom the country is proud, the acquisition of whose collections also is a source of the greatest encouragement to the writer.

R. BOWDLER SHARPE.

THE STORM OF OCTOBER 30.

THE gale which swept over the southern part of England on the morning of Sunday the 30th was both sudden and severe. On the previous day the weather was exceptionally fine over the country generally, and in many places it was a truly "pet" day. The Meteorological Office, in their morning report referring to the barometric rise which was going on in the south and west, remarked that "some improvement in the weather is therefore likely in the south." In the afternoon of Saturday, however, there were signs of approaching bad weather, and by six o'clock a disturbance was shown to be situated off Scilly, the barometer reading 29·4 inches. The Meteorological Office considered the situation sufficiently menacing for the issue of storm signals, and the south cone was hoisted in the south and south-west districts. During the night the storm passed in an east-north-east direction over the southern counties of England, travelling at the rate of about thirty miles an hour. The centre passed almost directly over London at about five o'clock in the morning, when the wind changed suddenly about 180°, the barometer at the time registering 28·86 inches, and in the next two hours the mercury rose 0·4 of an inch. At Greenwich Observatory the anemometer recorded 17·2 lbs on the square foot at 7·5 a.m., which is equivalent to an hourly velocity of about sixty miles. By 8 a.m. the centre of the disturbance had passed to the eastward of our islands and was situated a short distance off Yarmouth. The storm afterwards travelled in a north-easterly direction, maintaining somewhat its former rate of movement, and on Monday morning the central area was in the neighbourhood of Stockholm. The gale was rather severe on our southern coasts, but its principal violence was felt in the English Channel and on the French and Danish coasts. The Paris *Bulletin* shows that at many of the stations the wind reached the full force of a hurricane, and the sea was terrific. The amount of rain which fell during the storm was unusually heavy, 1·59 inches being registered at Scilly, and upwards of an inch at other stations in the south of England and also in the north of France. As is commonly the case with these quick-travelling and rapidly-developing storms, the disturbance was a "secondary" to a larger disturbance which was passing from off the Atlantic to the northward of our islands.

ROBERT HUNT, F.R.S.

MR. ROBERT HUNT, whose death we have already briefly announced, was born at Devonport, then called Plymouth Dock, on September 6, 1807. His father was a naval officer who perished, with all the crew, in H.M.S. *Moucheron*, in the Grecian Archipelago. Robert Hunt, left to his mother's care, was destined for the medical profession; and, having been placed with a surgeon in London, he attended the anatomical lectures of Joshua Brooks; but his studies were interrupted by failing health, and his medical training was never completed. In 1840, Mr. Hunt became secretary to the Royal Cornwall Polytechnic Society at Falmouth. His earliest contributions to science were in connection with photography—a subject to which he applied himself with assiduity immediately on the announcement of Daguerre's discovery in 1839. Mr. Hunt's investigations led to the discovery of several new processes, which were either described in the *Philosophical Magazine* or announced to the British Association. His experimental researches on the chemical activity of the highly refrangible rays of the solar spectrum, his work with the actinograph, and his study of the influence of light upon the germination of seeds and the growth of plants, formed the subject of numerous papers between 1840 and 1854. Mr. Hunt's "Researches on Light" appeared in 1844. His "Manual of Photography," which was the first general work on the subject published in this country, passed through six editions.

While Mr. Hunt was in Cornwall he undertook some interesting inquiries, conjointly with the late Mr. Robert Were Fox, into the electrical phenomena of mineral veins; and he also entered upon an examination of the air in some of the Cornish mines. In 1845 he came to London, at the invitation of Sir H. T. De la Beche, to succeed Mr. Thomas Jordan, as Keeper of Mining Records at the Museum of Economic Geology, then recently established in Craig's Court. On the establishment of the Government School of Mines in 1851, he was appointed Lecturer on Mechanical Science, and opened his course with an address on the importance of cultivating habits of observation. After holding this position for two sessions he resigned it to the late Prof. Willis, and undertook for a short time the duties of Lecturer on Physics. In 1854 Mr. Hunt was elected a Fellow of the Royal Society.

For the last thirty years Mr. Hunt's energies have been mainly directed to the collection and collation of statistical information relating to British mining and metallurgy. From 1853 until the abolition of the Keepership of Mining Records he published regularly the annual volumes of "Mineral Statistics," containing a vast mass of voluntary returns obtained by his personal influence. As a member of the Royal Coal Commission of 1866, he undertook the statistical part of the inquiry, and published detailed information on the coal resources of the country.

The technical education of the metal-mining population of the West of England was a subject that Mr. Hunt always had at heart. He was an earnest advocate for the establishment of local mining schools, and should be regarded practically as the founder of the Miners' Association of Cornwall and Devon—a body now amalgamated with the Mining Institute. In 1883, Mr. Hunt published a voluminous work on "British Mining." After the death of Dr. Ure he consented to edit the "Dictionary of Arts," and brought out successively the fifth (1860), sixth (1867), and seventh (1875) editions of this work. At the same time Mr. Hunt possessed great literary taste, which found scope in several lighter works, such as his "Poetry of Science," "Panthea, or the Spirit of Nature," and the "Romances of the West of England." Mr. Hunt's long, busy, and useful life was closed on the 17th ult. His remains were interred in Brompton Cemetery.

NOTES.

THE vacancy in the representation of Cambridge University, caused by the death of Mr. Beresford Hope, raises again the question of the desirability that the Universities should be represented in Parliament by men of distinguished culture, whether literary or scientific. Men of science will be glad to hear that a movement is on foot in Cambridge to induce the President of the Royal Society to allow himself to be nominated as a candidate for the membership of the University. A meeting will be held on Saturday for the purpose of considering the question of the representative. It is believed that Prof. Stokes, if he finds the feeling to be strong in favour of his acceptance, will regard it as his duty to place his services at the disposal of the electors. A more distinguished representative never offered himself for the suffrages of any University in this country. His presence in the House of Commons would be another pledge that questions involving the interests of science would be discussed with adequate knowledge in that assembly.

IN presenting the prizes to the successful students of the Bath Lane Science and Art School at Newcastle-on-Tyne, Lord Randolph Churchill expressed the opinion that when "the State has laid the foundation by freely contributing to elementary education, localities ought to come in and ought to build on that foundation whatever edifice may be necessary for the further and higher technical education of the artisan." "This school," he continued, "is essentially the result of pure local effort, pure local energy, and pure local pride. You have, I understand, carried on the whole work of this school without the smallest assistance from Government of any sort or kind. Now, I was saying that technical education is supposed to be a great requirement of the present day, and I was using the illustration of your school to show that localities can if they wish, if they have the energy and the determination, supply that technical education for themselves." In a letter to the *Times* a writer signing himself "Y." has exposed the ignorance displayed by Lord Randolph Churchill in this astonishing statement. The Newcastle school, instead of being carried on simply by means of "local effort," has been largely aided by the Government. As "Y." points out, the last Report of the Science and Art Department shows that in 1886 the payments on results made to the school were for science £1212 10*s*., and for art £238 10*s*., while the students' fees for instruction in science were £1228, and in art £150. It is discreditable that a man in Lord Randolph Churchill's position should be capable of making such a mistake as this. The incident is important, for it indicates the spirit in which too many of those who talk wildly about "economy" approach the consideration of the grave question as to the duty of the State with regard to education. Lord Randolph Churchill has been Chancellor of the Exchequer, and may some day be Chancellor of the Exchequer again. With his crude notions and inaccurate information, the injury he might do in this position to our educational system is simply incalculable.

THE College of State Medicine, lately incorporated, ought to be a remarkably successful institution, if we may judge from the names of its officers. The Chairman of Council is Sir Joseph Fayrer, K.C.S.I., F.R.S. The following are the members of the Council:—Sir John Watt Reid, K.C.B., Sir Thomas Crawford, K.C.B., Sir William Guyer Hunter, K.C.M.G., M.P., Sir Henry Roscoe, F.R.S., Sir Douglas Maclagan, Surgeon-General William Robert Cornish, C.I.E., Richard Quain, F.R.S., Edward Klein, F.R.S., Robert Brudenell Carter, and Arthur Trehern Norton. The following are *ex-officio* members:—The President of the Sanitary Institute, the President of the Society of Medical Officers of Health, the President of the Public Health Medical Society, the Professor of Public Health to the College. Mr. James Cantlie is the honorary secretary.

THE first Congress of the Dutch Society of Naturalists lately met at Amsterdam under the presidency of Dr. Stoknis, who delivered an address on nationality and natural science. Among the other addresses were the following: on Martinus of Marum, who made a large electric machine at the end of the last century, by Prof. Bosscha (Delft); and on the education of future naturalists, by Prof. Spruyt (Amsterdam).

AN Exhibition of Textile Goods and Machinery will be held at Warsaw about the middle of December next. It will be open to all countries.

THE expedition which the Finnish Archæological Society despatched to the Upper Yenisei last summer, to prosecute archæological researches in that locality, has just returned to Helsingfors. It has brought back drawings of about thirty stone figures, and copies of a large number of inscriptions, hitherto not deciphered, on a rock, on nine raised stone slabs, and on many stones along the upper course of the Yenisei. The expedition has also gathered a vast collection of objects belonging to the Siberian Bronze Age.

IN his latest Annual Report, Mr. Putnam, Curator of the Peabody Museum of American Archæology and Ethnology, says that during the past year several large collections of special interest have been added to the Museum. The most important is the Bucklin collection from ancient graves in Peru, principally at Ancon. This collection is particularly rich in textiles and in ornaments and implements made of silver and bronze; and among the objects in pottery there are many new forms and styles of ornamentation. Another collection of over 300 specimens of pottery obtained from the province of Piura, Peru, has also been purchased, and nearly every vessel adds some important feature to the already instructive Peruvian collection in the Museum. A third collection consists of 337 pottery vessels, a number of whistles and other objects made of pottery, 245 stone implements, and several large carved stones, some circular, and others resembling animals, supposed by some archæologists to be seats, and by others to be metates. This collection has been catalogued and placed in the exhibition cases with the other objects from the ancient graves in Chiriqui. It was obtained from the well-known collector of antiquities in Chiriqui, Mr. J. A. McNiel, who has resided in the State of Panama for many years. Mr. Putnam expresses much regret that Mr. McNiel has not been able to keep together the contents of each grave. He is dependent on the sale of the specimens for the means to carry on his work, so that many of the objects he obtained are now widely scattered, and archæologists have no means of tracing the development of the arts of the people, which could have been done had the collection been kept together and the associations of every object carefully noted.

WE have received the Proceedings of the U.S. National Museum during the year 1886. This is the ninth volume of the series. It contains many interesting and valuable papers, some by members of the scientific corps of the National Museum, others by writers who have made portions of the collections of the Museum subjects of special study. The volume opens with a list of fishes collected in Arkansas, the Indian Territory, and Texas, in September 1884, with notes and descriptions, by Mr. D. S. Jordan and Mr. C. H. Gilbert. Among the other papers are: notes on fulgurites, by Mr. G. P. Merrill; a review of Japanese birds, by Mr. L. Stejneger; a catalogue of animals collected by the Geographical and Exploring Commission of the Republic of Mexico, by Mr. F. Ferrari-Perez; a description of six new species of fishes from the Gulf of Mexico, with notes on other species, by Mr. D. S. Jordan and Mr. B. W. Evermann; and Norsk naval architecture, by Mr. G. H. Boehmer. At the end of the volume there are twenty-five plates, each accompanied by its explanation.

SOME of the difficulties with which the curator of a museum in tropical climates has to contend are described in the last report on the Colombo Museum. Mr. Haly states that naphthaline is not so powerful a protection against the effects of climate as was anticipated. It seems to prevent the attacks of mites, but it is powerless against fungus. It is hoped that it will ward off the attacks of the fish insect on the labels. As an instance of the rapidity with which this pest works, it is mentioned that one case was re-painted, and the objects rearranged and labelled. No naphthaline was procurable at the time, and in a fortnight several labels had been defaced and several numbers lost. Carbolic acid and corrosive sublimate have both been mixed with the gum, but their use is objectionable, as they discolour the labels, and do not afford permanent protection. Every object in a tropical climate, Mr. Haly says, ought to be exhibited on its own stand, and that stand labelled by hand in black or white paint. The Museum has also been attacked lately by a fungus. Not only have the specimens themselves been attacked, but the wood of the teak cases, and even the glass, has been covered. In one case the insects were absolutely rolled round and connected together by its fine filaments—filaments so fine as to be invisible through the glass. Naphthaline, benzine, cyanide of potassium, carbolic acid, and other substances have all been tried in vain: the only check to its growth was citronella oil.

THE "Educational List and Directory of the United Kingdom for 1887-88" (Sampson Low), edited by Mr. William Stephen, has just been published. This is the second issue of the work. The editor's aim is to concentrate within reasonable space the names of the chief educational institutions of the Kingdom. Besides being a guide for the use of parents and guardians, and a directory for all who give attention to educational matters, the volume is interesting, as Mr. Stephen claims, on account of the fact that it is the first methodical effort to unite for practical purposes the designations of our educational institutions, from the Universities downwards, in England, Wales, Scotland, and Ireland. No "descriptive matter" has been introduced.

THE Cardiff Naturalists' Society have issued a valuable descriptive list of the indigenous plants found in the neighbourhood of Cardiff, with a list of the other British and exotic species found on Cardiff Ballast Hills. The compiler is Mr. John Storrie, Curator of the Cardiff Museum.

A STALACTITE cave has been discovered near Steinbach in the Upper Palatinate. It can only be approached by a shaft 1 square metre in diameter and 40 metres deep. The cave is divided into several compartments, through one of which a stream of water slowly flows. The numerous stalactites are of great beauty. Another stalactite cavern, equalling the celebrated Dechen cavern, both in extent and peculiarity of form, has been discovered in the so-called Billstein, between Hirschberg and Warstein (Westphalia). The interior consists of several chambers. Numerous animal remains (probably prehistoric) have been found in the cave.

THE death is announced of Herr August Kappler, whose excellent book on Dutch Guiana is well known. He died at Stuttgart, aged seventy-one.

WE regret to announce the death of Dr. E. Luther, Professor of Astronomy at the Königsberg University, also Director of the Observatory. He was born February 24, 1816.

THE weather in Iceland during the summer has been very unusual. The ice did not leave the north and east coast till the middle of September, or quite a month later than usual. Storms and fogs have been very frequent. The last mail brings news that the weather was then (the middle of October) dry and fine. Frost had, however, set in in several parts. This is the last news we shall have from the island until next spring.

AT a recent meeting of the Wellington (New Zealand) Philosophical Society, Sir James Hector exhibited samples of trachyte tuff and breccia, constituting the auriferous deposit lately found in the level ground west of Te Aroha. The material, which appeared to be somewhat of the nature of an infiltrated quartz reef which had been decomposed and then distributed as a surface deposit, was found to contain gold at a rate varying from two ounces to fourteen ounces to the ton. The gold occurs in twisted angular flakes and grains, and is associated in a light feldspar sand with heavier grains of quartz mica and titanic iron. Sir James Hector is of opinion that it will probably prove to be the outcrop of an important reef, from which the sulphides have been removed by decomposition, so that gold is left in its free state. The gold is the usual alloy of the district—consisting of gold 80·47 per cent., silver 16·91, loss 2·62, previous assays having varied from 77 to 84 per cent.

THE last number of the *Excursions et Reconnaissances* of Saigon contains an account by M. Navelle of a journey which he made in Annam from the port of Thi-Nai, commonly called Quin-hon, to Bla. The route lay through the great town of Binh-Dinh, and by the ruins of Quin-hon, at one time the capital of the Chams or Ciampois. This leads the traveller to narrate the vicissitudes of the once powerful kingdom of Ciampa, which was overthrown in the fifteenth century, after seven centuries of contest with Annam. The narrative is mainly interesting from the circumstance that the traveller visited a number of important towns hitherto unseen by Europeans. At the town of Dong-pho, he met an official who at one time performed curious functions. The Kinh-li was an Annamite official appointed to reside beyond the frontiers to organize Annamites who fled from their native country, and to direct their raids against neighbouring States. These vagabonds, thus directed, acted as the van-guards of regular Annamite invasion. M. Landes, in the same number, continues his researches into the folk-lore of the races of French Indo-China. In the present instance he gives the tales and legends of the Tjames, Chams, or Ciampois, above-mentioned. They have long been subjugated, and are now divided into two groups, one inhabiting the Bin-thuan province, the other Cambodia. Until recently they were amongst the most unknown peoples of the peninsula, but M. Aymonier's accounts of his long exploration in Binh-thuan, which were published in recent numbers of *Excursions*, have thrown much light on the subject. The stories published by M. Landes were collected from the mouth of a Cham, and are mostly fairy tales.

DR. KARL PETTERSEN, Director of the Tromsö Arctic Museum, has lately written a pamphlet on the state of the drift-ice in the Arctic seas during the last few years. In this pamphlet he offers some suggestions as to the way in which attempts to reach the North Pole should be made. "It seems to me," he says, "that every year shows more and more clearly that it is a sheer waste of life and money to despatch casual and erratic expeditions to the North Pole. In my opinion the result would be attained most easily, surely, and cheaply by despatching every year, for a period of ten or eleven years, a certain number of well-equipped steamers from certain suitable spots towards the Pole. As the ice-masses in the Polar Basin are, without doubt, in a constant but varying motion, this plan would enable one or another of the expeditions to seize the right moment for a dash northward. We could not, of course, be absolutely certain of success, for experience has proved that the state of the ice in a particular locality at a particular time does not enable us to predict what it will be in the same locality in the following year. Still, the opportunity to reach a high latitude would present itself sooner or later. The expeditions of past years having almost conclusively demonstrated that it will be

impossible to reach the North Pole along the west coast of Greenland, the *point d'appui* for the journeys on the plan advocated would be confined to the European and Asiatic Polar seas. The routes I should recommend are four: viz. one along East Spitzbergen to Franz Josef Land, and northwards, starting from the north of Norway; one east of Franz Josef Land, starting from the Yenisei or Obi; one *viâ* Franz Josef Land, starting from the New Siberian Islands or the Lena; and one from a suitable spot in Behring Strait. I have every reason to believe that if four such expeditions were in readiness in these localities every year during a period of eleven years, we should by the end of that time, by one or another of the routes, have solved the problems which still face us around the Pole. Probably the scheme might be carried out most advantageously by international co-operation, as in the case of the Polar Research Expeditions of 1882-83. In any case, I venture to think that the plan of any expeditions should not be finally formed before July, or, if possible, August. By that time many of the huntsmen have returned from their first voyage to several parts of the Arctic Sea, and the expeditions would be in possession of a fair knowledge of the state of the ice in each. I believe that, should the route chosen be *viâ* Spitzbergen or Novaya Zemlya, a careful study of the weather and wind in North Norway during the spring and early summer would benefit Polar expeditions immensely, showing whether the route to the north or east of Spitzbergen should be followed, or the more eastern one by Novaya Zemlya."

THE additions to the Zoological Society's Gardens during the past week include a Grand Eclectus (*Eclectus roratus*) from Moluccas, presented by Miss P. Lockwood; a Goffin's Cockatoo (*Cacatua goffini*), habitat uncertain, presented by Miss Barton; a Water Rattlesnake (*Crotalus adamanteus*), a Water Viper (*Cenchris piscivorus*), two American Black Snakes (*Coluber constrictor*), a Chicken Snake (*Coluber quadrivittatus*), two Mocassin Snakes (*Coluber fasciatus*) from Florida, presented by the Natural History Society of Toronto; two Green Lizards (*Lacerta viridis*), twelve Spotted Salamanders (*Salamandra maculosa*), two Common Snakes (*Tropidonotus natrix*) from Italy, presented by Messrs. Paul and Sons; an Algerian Tortoise (*Testudo mauritanica*) from Algeria, deposited; an Aye-Aye (*Chiromys madagascariensis*) from Madagascar, purchased; six Painted Terrapins (*Clemmys picta*), two Corn Snakes (*Coluber guttatus*), two Milk Snakes (*Coluber eximius*), two Mocassin Snakes (*Tropidonotus fasciatus*), two Ribbon Snakes (*Tropidonotus saurita*), two Hog-nosed Snakes (*Heterodon platyrhinos*), two Grass Snakes (*Cyclophis vernalis*), six Dekay's Snakes (*Ischnognathus dekayi*), nine American Green Frogs (*Rana halecina*), ten Noisy Frogs (*Rana clamata*), a Wood Frog (*Rana sylvatica*), a Changeable Tree Frog (*Hyla versicolor*), nine Red-backed Salamanders (*Plethodon erythronotus*) from Canada, received in exchange; a Blood-breasted Pigeon (*Phlogenas cruentata*), bred in the Gardens.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 NOVEMBER 6-12.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on November 6

Sun rises, 7h. 4m.; souths, 11h. 43m. 44·2s.; sets, 16h. 24m.: right asc. on meridian, 14h. 45·2m.; decl. 15° 59′ S. Sidereal Time at Sunset, 19h. 26m.

Moon (at Last Quarter November 8, 17h.) rises, 19h. 50m.*, souths, 3h. 54m.; sets, 11h. 58m.: right asc. on meridian, 6h. 54·4m.; decl. 20° 14′ N.

Planet.	Riser.	Souths.	Sets.	Right asc. and declination on meridian.
	h. m.	h. m.	h. m.	h. m. ° ′
Mercury..	9 9 ...	13 3 ...	16 57 ...	16 4·7 ... 23 21 S.
Venus ...	2 57 ...	8 59 ...	15 1 ...	11 59·6 ... 0 22 S.
Mars ...	1 16 ...	8 0 ...	14 44 ...	11 1·4 ... 7 59 N.
Jupiter ...	7 12 ...	11 53 ...	16 34 ...	14 54·4 ... 15 44 S.
Saturn ...	21 48*...	5 35 ...	13 22 ...	8 35·5 ... 19 0 N.
Uranus...	4 17 ...	9 54 ...	15 31 ...	12 55·4 ... 5 14 S.
Neptune..	17 7*...	0 49 ...	8 31 ...	3 49·1 ... 18 16 N.

* Indicates that the rising is that of the preceding evening.

Occultations of Stars by the Moon (visible at Greenwich).

Nov.	Star.	Mag.	Disap.	Reap.	Corresponding angles from vertex to right for inverted image.
			h. m.	h. m.	° °
6 ...	ξ Geminorum ...	5½ ...	22 34 ..	22 51 ...	338 302
8 ...	7 Leonis ...	... 6½ ...	22 30 ...	23 8 ...	90 182
9 ...	ψ Leonis ...	... 6 ...	1 52 ...	2 42 ...	78 181

Nov.	h.	
7 ...	8 ...	Mercury stationary.
8 ...	0 ...	Saturn in conjunction with and 1° 1′ north of the Moon.
9 ...	2 ...	Jupiter in conjunction with the Sun.
12 ...	1 ...	Venus in conjunction with and 3° 42′ south of the Moon.

Saturn, November 6.—Outer major axis of outer ring = 42″·2; outer minor axis of outer ring = 13″·5; southern surface visible.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·3 ...	81 16 N. ...	Nov. 7,	2 49	*m*
			,, 12,	2 29	*m*
U Monocerotis ...	7 25·4 ...	9 33 S. ...	,, 9,		*M*
U Ophiuchi	17 10·8 ...	1 20 N. ...	,, 7,	3 9	*m*
		and at intervals of		20 8	
U Sagittarii... ...	18 25·2 ...	19 12 S. ..	,, 7,	0 0	*m*
R Scuti	8 41·5 ...	5 50 S. ...	,, 7,		*m*
β Lyræ... ...	18 45·9 ...	33 14 N. ...	,, 6,	3 0	*M*
S Vulpeculæ ...	19 43·8 ...	27 0 N. ...	,, 8,		*m*
η Aquilæ	19 46·7 ...	0 43 N. ...	,, 8,	5 0	*m*
S Sagittæ	19 50·9 ...	16 20 N. ...	,, 6,	1 0	*m*
			,, 9,	1 0	*M*
δ Cephei	22 25·0 ...	57 50 N. ...	,, 10,	3 0	*M*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
Near the Pleiades ...	60°	20° N.	... Bright; rather slow.
From Camelopardalis	102	73 N.	... Very swift.
Near η Leonis	149	22 N.	... Swift; streaks.

GEOGRAPHICAL NOTES.

THE Danish Government has decided upon hydrographically measuring and charting the Guldborg Sound, the new harbour at Odense, in the Island of Funen, and the Randers and Mariager fjords in Jutland. Two vessels will also be engaged in preparing a naval chart of the coast around Denmark. It has also been decided to despatch an Expedition—the cost being estimated at £4000—to Iceland next summer, for the purpose of effecting hydrographical measurements around that island. Great fjords and waterways are here still unmeasured—a danger to navigation and a loss to science. It is believed that these researches may lead to important scientific discoveries, principally as regards zoology, meteorology, and geography. Moreover, they would probably be of great benefit to the Iceland fisheries, which are far from being thoroughly developed on account of the ignorance of existing fishing-banks, the temperature of the sea, &c. The researches, which will be similar to those carried out around Norway for some years past, will be effected with the Government schooner *Ingolf*, a vessel particularly adapted for the purpose. It is intended to employ the months of May, June, July, and August in this work, which, it is estimated, will be fully accomplished in five years.

IN the *Bollettino* of the Italian Geographical Society for September Signor E. Modigliani concludes his series of papers on Nias, with a detailed account of the physical features, natural history, and social condition of that island. The hilly surface is

relieved in some places by extensive open plains covered with tall grasses, and the forest vegetation is rapidly disappearing, owing largely to the wasteful habits of the natives. The accompanying seismic tables contain records of the earthquakes that occurred between the years 1843–86, some of which were very violent and attended by marine disturbances destructive to shipping, and driving boats and barges hundreds of yards inland. But there are no active volcanoes, and the prevailing formation appears to be a much-weathered compact limestone resting on gray or bluish Miocene marls and other argillaceous clays. The geological as well as the zoological conditions show that Nias, like the other islands running parallel to the west coast of Sumatra, must have formerly been connected with the mainland; and as Sumatra itself at one time certainly formed part of the Malay Peninsula, this chain of insular groups would appear to mark the original line of the Asiatic seaboard in this direction. Signor Modigliani's collections include 178 birds, representing 62 species, of which 8 are described as new, but allied to corresponding species in Sumatra; consequently the separation must have taken place at a very remote period—a conclusion also confirmed by other considerations. His rich zoological collection, comprising over 7000 specimens, has been presented to the Museum of Genoa.

METEOROLOGICAL NOTES.

THE Report on the Meteorology of India in 1885, being the eleventh year of the series, has just been published, and contains an immense mass of valuable information. The accumulation of Indian statistics during the last ten years may be best shown by a comparison of the following figures:—The number of stations at which the mean temperature is recorded has increased from 51 to 127, and the rainfall stations from 134 to 471. From this huge volume of 516 large quarto pages we can only note here a very few general remarks. Considerable attention is paid to solar radiation, the chief feature of which is found to be that the maximum intensity generally occurs during the winter half of the year (October to March), when the sun is in southern declination, and the thickness of the absorbing atmosphere traversed by the sun's rays is at a maximum. This is said to be apparently due to the fact that over a large part of India the atmosphere is most cloudy in the summer and autumn months. It would appear from the mean result of the sun-thermometer readings in all parts of India that the average intensity of solar heat had reached a minimum in 1884, and in 1885 underwent an appreciable increase. The duration of bright sunshine is now regularly recorded at four stations, and Mr. Blanford considers that the sunshine-recorder promises to be even more important than the sun-thermometer, since the duration of sunshine is a more direct measure of the amount of solar heat reaching the earth's surface than the registry of its mean maximum intensity. Anemometers are in use at nearly all the stations, and fourteen of them are large anemographs of the Kew pattern. The resultant direction of the winds is, however, computed by Lambert's old formula, which is based on the assumption that the force of all winds is equal, an assumption which is obviously often very misleading. The work is accompanied by maps showing the positions of the meteorological observatories and the mean distribution of temperature, pressure, and wind.

M. GARRIGOU-LAGRANGE describes, in the *Annuaire de la Société Météorologique de France*, his apparatus for registering the ascending and descending air-currents. The experimental anemometer is fixed on a mast at Limoges, and consists of four small fans moving round a vertical axis, and recording, by electrical arrangements, on a drum covered with ruled forms. The curves obtained by this method show plainly the upward or downward movement of the air, and the velocity is easily read off. The experiments show that ascending winds are generally stronger and more frequent than the descending, owing no doubt to the eddies caused by obstacles met with by the currents.

IN *Das Wetter* M. Seemann discusses the results of the storm-warnings issued by the *New York Herald* between September 1886 and January 1887, and finds that out of twelve warnings only three were quite successful, and three partially so. The wording of the telegrams is not so absolute as formerly; many of the depressions pass to the northward of our islands, and our weather is *disturbed*, although the gales do not always strike our coasts; judged in this light, the warnings may be more successful than when actual storms are predicted. On pp. 83–87 of the same journal Dr. C. Lang describes his method of predicting night-frost, from the position of the dew-point. From the Munich observations for 1879–86 he finds that 441 predictions of night-frost could be given, of which 89 per cent. were successful. In 8 per cent. of the cases, night-frost did not follow, and in 3 per cent. frost occurred unexpectedly. This method may be of considerable benefit to agriculturists; reference to this subject has been made by Mohn, Buchan, and others.

DR. GROSSMANN has published "Meteorologische Divisionstafeln" (Hammerich and Lesser, Altona), which will be very useful to meteorological observers in the calculation of monthly means, and daily means from hourly observations. For a dividend of four figures the quotient is given by simple inspections, and for more figures by interpolation by means of a table of proportional parts. The principle of construction is due to Dr. Köppen, and some of the tables were printed in *Aus dem Archiv der Deutschen Seewarte*, vol. i. 1878. Dr. Crelle's well known "Tables de Calcul" (Reimer, Berlin, 1869) are equally useful, but being in a large volume are not easily accessible to observers generally. The chief merit of Dr. Grossmann's tables lies in their publication in a form specially suitable to the wants of meteorological observers, and at the low price of about sixpence for single copies. They extend to four pages only, and are intended to be pasted on cardboard.

THE Italian Central Meteorological Office has published its *Annali* for the year 1884, in three thick folio volumes, containing a mass of meteorological, magnetical, and astronomical data. The meteorological services of Italy are very complicated, as in addition to the Central Office there are many large and independent establishments from which we possess long series of observations, some of them dating from the middle of last century. The Government established a Committee for Weather Telegraphy in 1863, under M. Matteuci, and in 1865 the Meteorological Section of the Ministry of Agriculture commenced the issue of the *Meteorologia Italiana*; from this originated the present office in 1877, located at the Collegio Romano. The service is now under the able superintendence of M. Tacchini, and includes 135 stations of the second order, some of which, *e.g.* those of Stelvio, Valdobbia, Cimone, and Etna are important mountain observatories; in addition to which there are 515 rainfall stations, and stations at the military settlements of Assab and Massowah. The work contains valuable discussions on thunderstorms, evaporation, and on the microscopical examination of atmospheric dust.

A SPECIAL meeting of the Meteorological Society of Mauritius, in honour of the Queen's Jubilee, was held on August 6, on which occasion Dr. Meldrum gave an interesting sketch of the origin and labours of the Society, and of the Royal Alfred Observatory. Charts were exhibited showing the tracks of the cyclones in the Southern Indian Ocean in each year from 1856 to 1886, also magnetograms showing simultaneous disturbances at Zi-ka-wei and Mauritius on January 9, 1886; and curves showing the apparent connection between solar, magnetical, and auroral phenomena, and also between solar phenomena and the frequency of cyclones, rainfall, and the depth of water in rivers, together with other articles of interest. Dr. Meldrum stated, with reference to the history of meteorology in the island, that a memorial was presented to the Governor in April 1851, suggesting the desirability of systematic observations, and the Meteorological Society was consequently founded on August 1 of that year. Shortly afterwards the office of Government Meteorological Observer was created, to which office Mr. Meldrum succeeded in 1862, and the two institutions, although distinct, have co-operated with each other. Regular observations were commenced at the new Observatory (*see* NATURE, vol. xxxvi. p. 546) in November 1874. We cannot enumerate here the many useful works which have been carried on, but the Hydrographer to the Admiralty has expressed the wish that the track charts should be published, and Mr. Meldrum has been informed that the Meteorological Council would probably publish them. He also states that tide-gauges will soon be erected at two points on the coast, and expresses a desire for the establishment of a high mountain station.

IN the *American Meteorological Journal* for September, Mr. H. Allen discusses the theory of the outflow of air under falling rain. It has been assumed by some meteorologists that the rain, drops carry with them and compress the air, which, flowing out produces at times a considerable wind velocity (see article on

"Lightning" in Johnson's Encyclopædia). Several hypothetical cases are considered, and the author concludes that, although his computations may need some modification after further study, we can safely say that no appreciable velocity of air is produced by compression from falling rain. The same journal contains a long paper on the theory of the wind-vane by Prof. G. E. Curtis, reprinted from the *American Journal of Science* for July last. He discusses the relative stability of a straight vane, and that with a double or spread tail. The first reference to the latter is apparently in *Voigt's Magazin*, 1797, and this form has been in common use in England since about 1840. The formulæ show that for a frictionless bearing (1) that the oscillations of both vanes are smaller as the vanes are longer and larger; (2) that the spread vane is always more stable than the straight vane; and (3) that this advantage in stability is greater for long vanes than for short vanes, and is independent of the wind velocity. The author finds that, with equal friction, a spread vane is the more sensitive, and that consequently for two vanes of equal sensitiveness the spread vane will have the greater friction and will come to rest more quickly.

THE *Jahresbericht* of the Central Physical Observatory of St. Petersburg for 1885 and 1886, and the *Annalen* for the year 1885, have recently been published. The Russian system is very important, not only as being the most extensive on the globe, but on account of the great climatic contrasts and the completeness of the observations. The present Director, Dr. H. Wild, of Berne, was appointed in 1867, and under his able superintendence the number of stations has greatly increased and the quality of the observations has much improved. The central Observatory is situated about a mile from the sea, on the island of Wassili-Ostrow; the principal observing department has been transferred since 1878 to Pawlowsk, about four miles distant, and is placed under the superintendence of Dr. E. Leyst. The observations for 1885 are contained in two quarto volumes of about 700 pages altogether. In addition to the first class observatories, the number of stations of the second order amounts to 255, and of these the monthly and yearly results of 208 are published on the international scheme; from 38 of the stations the observations are published *in extenso*. Many new stations have been added recently, especially in Siberia, and in newly-acquired territories, *e.g.* Merv, Batoum, &c. One of the Siberian stations, viz. Werchojansk (lat. 67° 34', long. 133° 51') is stated by Dr. Köppen to be the coldest known point of the earth. The mean temperature there for the year was − 2°·9 F. The mean for January and December was − 62°·9, and the minimum in January − 88°·6 (far beyond the range of the usual tables). The mean temperature of July rose to 60°·6, and the minimum for that month was 39°·2. The number of rainfall stations for which the observations are given is 650, against 252 in the previous year; the data published are the monthly values, the maximum fall in 24 hours, and the number of days of rain and snow. A complete catalogue of the meteorological observations in Russia and Finland, by Dr. E. Leyst, giving the life-history of each station, has been published in the *Repertorium für Meteorologie* this year. This work also contains many valuable discussions of the vast amount of materials available for the purpose. The index of the *Annalen*, being mostly in Russian, is difficult to refer to.

THE WORK OF THE INTERNATIONAL CONGRESS OF GEOLOGISTS.[1]

I.

ELEVEN years ago the Association met at Buffalo. It was the year of the Centennial Exhibition, and we were honoured by the presence of a number of European geologists. This naturally opened the subject of the international relations of geology, and the proposition to institute a Congress of Geologists of the world took form in the appointment by the Association of an International Committee. The project thus initiated found favour elsewhere, and there resulted an international organization, which up to the present time has held three meetings. It was convened first at Paris in 1878, then at Bologna in 1881, and at Berlin in 1885. Its next meeting will be held in London next year, and an endeavour will be made to secure for the United States the honour of the fifth meeting. The original Committee of the Association has been continued, with some change of membership, and has sent representatives to each session of the Congress.

The work of the Congress, as originally conceived and as subsequently undertaken, has for its scope geologic nomenclature and classification, and the conventions of geologic maps. The particular classifications attempted are the establishment of the major divisions used in historic and stratigraphic geology and the subdivision of volcanic rocks. In nomenclature three things are undertaken: (1) the determination of the names of historic and stratigraphic divisions, (2) the formulation of rules for nomenclature in palæontology and mineralogy, and (3) the establishment and definition of the taxonomic terms of chronology (period, epoch, &c.) and of stratigraphy (system, series, &c.). The map conventions most discussed are colours, but all signs for the graphic indication of geologic data are considered. The Congress has also undertaken the preparation of a large map of Europe, to be printed in forty-nine sheets.

The work was for the most part planned at the Paris meeting, and Committees were appointed to formulate subjects for action by the Congress at subsequent sessions. Briefly stated, the work accomplished to the present time is as follows. Agreement has been reached as to the rank and equivalence of the taxonomic terms employed in chronology and stratigraphy, a set of rules for palæontologic nomenclature has been adopted, and many sheets of the map of Europe have been prepared for the engraver. A partial classification of stratified rocks has been agreed to, and also a partial scheme of map colours, but the reports of proceedings indicate that action in these matters is tentative rather than final.

It is understood that both of these subjects will have prominent place in the proceedings at the London meeting, and the American Committee is endeavouring to prepare itself for representative action at that meeting by ascertaining the opinions of all American geologists on the various subjects. It has asked this Section to set apart a day for the discussion of some of the more important questions, and it can hardly be doubted that the Section will realize the mutual advantage of thus assigning the time requested. I am personally so impressed with the importance of the possible work of the Congress that I shall devote the present hour also to its consideration.

The first thing the Congress did was to select names for a set of categories to express the taxonomic rank of stratigraphic divisions on the one hand, and of chronologic divisions on the other. In the terminology of zoology and botany the words kingdom, class, order, family, genus, species, and so forth, however difficult of definition they may severally be, nevertheless are used always in the same order of inclusion. No systematist in those sciences would think of grouping orders together and calling them a family, or of styling a group of families a genus. But in geology there is no such uniformity of usage. With some writers a group is larger than a series, with others it is smaller. With some an age includes several periods, with others a period includes several ages. There are even writers who ignore the distinction between stratigraphy and chronology; and among the classifications submitted to the Congress is one in which an age is subdivided into systems. There is a manifest advantage in bringing order out of this chaos, and so great is the utility of uniformity and perspicuity that the decisions of the Congress in this regard will unquestionably be followed by future authors. The terms and the order adopted by the Congress are as follows. Of stratigraphic divisions, that with the highest rank is *group*, then *system*, *series*, and *stage*. The corresponding chronologic divisions are *era*, *period*, *epoch*, and *age*. This order of rank is strange to most English readers and writers, and so is one of the terms—*stage*; but the strangeness is only a temporary disadvantage, and will not seriously retard the adoption of the convention. The fact that we have previously used the words in a different sense, or that their etymology might warrant a different meaning, need not deter us, for we know from frequent experience that the connotations of a word transferred from one use to another quickly disappear from consciousness, leaving it purely denotative. The introduction of the word *stage*, which can hardly be said to have had an English status heretofore, or at least the introduction of some new word for that part of the column, was necessitated by the restriction of the word *formation* to a special meaning—the designation of mineral masses with reference to their origin.

[1] Vice-Presidential Address read to Section E of the American Association for the Advancement of Science, August 10, 1887, by Mr. G. K. Gilbert.

The same restriction vacated another office that had been filled by formation, and to this office no appointment was made. I refer to the use of the word to denote indefinitely an aggregate of strata—as in saying, this formation should be called a series rather than a system. This is an important function, for which some provision must be made. I suggest that we may advantageously enrich our language by the permanent adoption of *terrane*, a word whose English meaning has not been well established.

The fixation of the chronologic terms creates a similar difficulty. We have crystallized out of our magma the terms *era*, *period*, *epoch*, and *age*, and there remain in the ground-mass only *eon*, *cycle*, and *time*. Of these, *eon* has a poetic connotation which seems to unfit it for this particular use; *cycle* implies repetition or recurrence; and *time* has been so generally applied to unlimited duration that it is difficult to apply it also to limited duration, even though the nature of the limitation be indefinite. On the whole, *time* seems open to the least objection, but I cannot help regretting that either *period* or *age*, both of which have heretofore passed current in the indefinite sense, was not reserved by the Congress for that function. With English-speaking peoples the word *eon* could have been better spared for the definite series.

But while the terms selected by the Congress are not beyond criticism, the benefits to be derived from an agreement in an orderly system are so great that I for one shall unhesitatingly adopt them as they stand—provided, of course, that the Congress makes no effort to improve its selection. A small reform of this nature yields its profit to this as well as future generations, and I hold it a duty to favour even those reforms which involve so much effort and pains that their blessings cannot be realized by those who initiate them. Such are the exchange of our English spelling for a rational system, and the exchange of decimal notation in arithmetic for a binary notation. My application of the new nomenclature begins with this address, in the preparation of which I have experienced its utility. That you may have no difficulty in interpreting my reformed language, I have placed the taxonomic legend on the wall, with the addition of the complementary indefinite terms—terrane and time.

Terranes.		Times.
Group.	...	Era.
System.	...	Period.
Series.	...	Epoch.
Stage.	...	Age.

There are propositions before the Congress to distinguish the names of individual groups, systems, series, and stages by means of terminations, those of the same rank having the same termination. Thus it is proposed by a Committee that every name of a group shall end in *ary*—Tertiary, Primary, Archeary; it is proposed that names of systems end in *ic*—Cretacic, Carbonic, Siluric; it is proposed that names of series end in *ian*—Eifelian, Laramian, Trentonian; and it is proposed that stage names terminate with *in*. Another Committee suggests that *ic* be used for stages instead of systems. The adoption of such a plan would enable a writer or speaker to indicate the taxonomic rank of a terrane without adding a word for that purpose. If he regarded a certain terrane taking its name from Cambria as a system, he would call it the Cambric; if he esteemed it only a series, he would say Cambrian; and there would be no need of adding the word system or series in order to express his full meaning. Conversely, the reader or hearer would always learn its taxonomic rank, or supposed rank, whenever a terrane was mentioned. These I conceive to be the advantages derivable from the change, but they would not be the only effects. It would become impossible for a geologist to name or allude to a terrane without declaring its rank, and the consequences of this would be evil in many ways. In the first place, one could not discuss terranes from any point of view without expressing an opinion as to their taxonomy, and the change would thus contravene one of the most important rights of opinion—namely, the right to reserve opinion. Again, geologists who differed as to the rank of a terrane would necessarily terminate its title differently, and a needless synonymy would thus be introduced. In the third place, the created necessity for taxonomic discrimination on all occasions would tend to direct undue attention to taxonomic problems. Taxonomy would be conceived by many geologists as an end instead of a means, just as correlation has been conceived, and energy would be wasted in taxonomic refinement and taxononomic controversy. It is convenient for purposes of description and comparison to classify the strata that constitute a local columnar section in phalanges of various magnitude or rank, but the criteria on which we depend for discrimination are in the nature of things variable, and offer ground for endless difference of opinion; and it would be extremely unfortunate to have such differences perpetually brought to the foreground.

Another subject considered by the Congress is the nomenclature of palæontology. A Committee appointed for the purpose formulated rules for the establishment of the names of genera and species, and their report was adopted by the Congress. I have no opinion to express as to the wisdom of the rules, but it is a matter of surprise that a body of geologists assumed to speak with authority on the subject. From one point of view palæontology is a part of geology; from another point of view it is a part of biology. In so far as it names genera and species it is purely biologic, and it would seem proper that the students of fossils unite with the students of living animals and living plants in the adoption of rules of nomenclature.

A similar remark applies to the nomenclature of mineralogy, in regard to which no action has yet been taken. The most intimate relations of systematic mineralogy are with chemistry.

Yet another projected work of the Congress is the classification of eruptive rocks. Up to the present time action has been deferred, and it may reasonably be hoped that no scheme of classification will be adopted. If there existed a system of classification which gave general satisfaction and had stood the test of time, there would be little harm—and little or no advantage—in giving it the official stamp of approval. If the main features of a classification were well established and the residuary discrepancies were recognized as unessential, it is conceivable that some benefit might be derived from the submission of the matter to an assembly of specialists. But the actual case is far different. Not only is there wide difference as to the classification of volcanic rocks, but there is no agreement as to the fundamental principles on which their classification should be based, for we still lack an accepted theory of volcanism. At the same time observation is being pushed with great vigour, and with the aid of new and important methods. With the rapid growth of knowledge and ideas, classifications are continually remodelled, and the best is in danger of becoming obsolete before it has been printed and circulated. Should the Congress enter the lists, one of two things would occur. Either its classification would be treated like that of an individual, and ignored as soon as a better one was proposed; or it would be regarded as more authoritative, and new facts would for a time be warped into adjustment with it. In either case the reputation of the Congress would eventually suffer, and in one case science would suffer also.

There remain to consider the two most important undertakings of the Congress, the classification of terranes and the unification of map colours. The Congress is attacking these subjects indirectly by means of a third undertaking, the preparation of a geologic map of Europe, and this method of approach has had the effect of making it difficult properly to interpret its action. There can be no doubt that those who originally organized the work contemplated the enactment of a stratigraphic classification to be applied to the entire earth, and the selection of a colour scheme for use either in all geologic maps or in all general geologic maps. But at the Berlin session the Committee in charge of work on the map of Europe pressed the Congress for the determination of questions on which hung the completion of the map, and many hasty decisions were reached, while not a few disputed points were referred to the Map Committee. The debates indicate that much or all of this work was provisional or of merely local application, but the resolutions adopted show little qualification. It should be added that the official minutes of the meeting are still unpublished. In view of the uncertainty thus occasioned I shall not attempt to characterize the attitude of the Congress on the subject of classification, but shall merely develop my individual view.

It is the opinion of many who have discussed the general classification of terranes by convention of geologists that the smallest unit of such classification should be the stratigraphic system. What is a stratigraphic system? The Congress implies a definition in saying that a system includes more than a series and less than a group, and that the Jurassic is a system; but this gives only a meagre conception, and we need a full one. As the problem of classification demands a true conception of a system, and as there is reason to believe that a false conception is

abroad, it is proper that in seeking the true one we begin with the elements.

The surface of the land is constantly degraded by erosion, and the material removed is spread on the floor of the ocean, forming a deposit. This process has gone on from the dawn of geologic history, but the positions and boundaries of land and ocean have not remained the same. Crust movements have caused the submergence of land, and the emergence of ocean bottom, and these movements have been local and irregular, districts here and there going up while other districts went down. The emergence of ocean bottom exposes the deposit previously made on it, and subjects it to erosion. In this way every part of the known surface of the globe has been the scene of successive deposition and erosion, and in many districts the alternations of process have been numerous. It is manifestly impossible that either erosion or deposition should ever have prevailed universally, and it has been established by the study of stratigraphic breaks that a time of erosion has often interrupted deposition in one region while deposition was uninterrupted in another.

In transportation from its region of erosion to its place of deposition detritus is assorted, and it results that the simultaneous deposits on the bottom of an ocean are not everywhere the same. Equal diversity is shown in the ancient deposits constituting geologic formations. It is a general fact that synchronous formations have not everywhere the same constitution.

Many of the variations in deposits are correlated with depth of water and distance from shore, and it results that elevation and subsidence in regions of continuous deposition produce changes in the nature of the local deposit.

The animals and plants of the earth are not universally distributed, but are grouped in provinces. In the geologic past similar provinces existed, but their boundaries were different, shifting in harmony with the varying geography of the surface. From time to time the barriers separating contiguous provinces have been abolished, suffering them to coalesce; and conversely new barriers have arisen, creating new provinces. From the earliest Palæozoic to the present time the species of animals and plants have been progressively modified, the nature of the modification depending on local conditions. The faunas and floras of different provinces thus become different, and the longer the provinces remain distinct the greater is the divergence of life. The removal of a barrier either produces a new fauna by the fusion of the two previously separated, or else obliterates one and extends the area of the other. In either case there is a change toward the unification of life, and in either case there is an abrupt change in a local fauna. Thus the secular evolution of species, combined with the secular and kaleidoscopic revolution of land areas, leads to two antagonistic tendencies, one toward diversity of life on different parts of the globe, the other toward its uniformity. The tendency toward uniformity affords the basis for the correlation of terranes by comparison of fossils; the tendency toward diversity limits the possibilities of correlation.

If now we direct attention to some limited area and study its geology, we find that under the operation of these general processes it has acquired a stratigraphic constitution of a complex nature. Its successive terranes are varied in texture. Breaks in the continuity of deposition are marked by unconformities. The fossils at different horizons are different, and when they are examined in order from the lowest to the highest, the rate of change is found to vary, being in places nearly imperceptible and elsewhere abrupt. It is by means of such features as these—that is, by lithologic changes, by unconformities, and by life changes—that the stratigraphic column is classified into groups, systems, series, and stages. A system is a great terrane separated from terranes above and below by great unconformities or great life breaks or both. Smaller unconformities, smaller life changes, and lithologic changes are used for the demarcation of series and stages; and, on the other hand, exceptionally great unconformities and life breaks are used to delimit groups. As the same criteria determine groups, systems, and series, differing only in degree, the precise definition of the term system is impossible, and in many cases the gradation of a terrane as a group, a system, or a series is largely a matter of convenience. From this point of view a system is somewhat artificial, but there is a more important sense in which it is natural. It is limited by stratigraphic or palæontologic breaks above and below, and these breaks are natural. The taxonomist is not warranted in dividing systems where no such break exists.

Transferring now our attention to some other area, distant from the first, and studying its stratigraphy, we find that the same principles enable us to divide it independently into stages, series, systems, and groups. Its fossils are not the same, but they are to a certain extent similar, and the sequence of life is approximately parallel. We cannot compare stage with stage, nor series with series perhaps, but we can compare system with system, and making the comparison we discover that the breaks are at different places. While one area was upraised and subjected for a time to erosion, the other received continuous deposition. While life in one area, enjoying constant conditions, was almost unchanged for long ages and even epochs, it was revolutionized in the other by the irruption across some obsolescent barrier of strong and aggressive faunas and floras. The systems of one area, therefore, do not coincide with the systems of the other in their beginning and ending. They may differ in number, and they may differ greatly in magnitude, and in the duration they represent. They are, in fact, a different set of systems.

The case I have described is ideal, but not false. It represents the common experience of those who have developed the geologic histories of remote districts, and attempted to correlate them with the geologic history of Europe. There does not exist a world-wide system nor a world-wide group, but every system and every group is local. The classification developed in one place is perfectly applicable only there. At a short distance away some of its beds disappear and others are introduced; further on, its stages cannot be recognized; then its series fail, and finally its systems and its groups.

If I have properly characterized stratigraphic systems—if they are both natural and local—it goes without saying that the classification of the strata of all countries in the dozen or so systems, as proposed by some of the members of the Congress, is impossible.

I hasten to add that from the point of view of these gentlemen what they advocate is not necessarily impossible, for they have a different conception of a system. They regard it not as local but as universal. It is their privilege to define their terms as they please, and we will not dispute about mere words, but I cannot too strongly or too earnestly insist that a system which is universal is artificial. It may be natural in one geologic province, but it is artificial in all others. Take for example the Jurassic. It is a natural system in Europe. In the eastern United States no strata are called Jurassic with confidence, and at the west the rocks called Jurassic merge with those called Triassic. In India, Medlicott tells us, a Jurassic fauna occurs at the summit of a great natural system containing a Permian fauna near its base. In New Zealand, according to Hutton, a continuous rock-system, dissevered by great unconformities from other systems, bears at top fossils resembling those of the lower Jurassic, and lower down fossils of Triassic facies. To establish a Jurassic system in either of these countries it is necessary to divide a natural system, and a Jurassic system thus established would be necessarily artificial.

This is the sort of classification implied by the assumption that systems are world-wide. It is not impossible, but it is highly unadvisable. It is classification for the sake of uniformity, and its uniformity is Procrustean. The natural systems of a region are the logical chapters of its geologic history. If you group its strata artificially according to the natural divisions of another region, you mask and falsify its history. The geologic history of the earth has as great local diversity as its human history. As in human history, there are inter-relations and harmonies and a universal progress, but these are perceptible only in the general view, and the student whose preconceptions lead him to exaggerate the harmonies and ignore the discrepancies perverts the meaning of every page.

I prefer, therefore, my own definition of system, making it natural and consequently local, and I earnestly oppose any attempt to coerce the geology of one country in a rigid matrix formed over and shaped by the geology of another country.

The ideas I oppose have arisen in connection with the work of correlation. Some geologists appear to regard correlation as the determination in distant localities of identities; the more philosophic regard it as the determination of the actual relations, whether they be of identity or difference. With the former the basis of correlation is the universality of geologic systems; with the latter it may be said to be the universality of geologic time.

Now in the comparative study of local geologic histories, just as in the comparative study of local human histories, it is a

matter of convenience to have a common scale of time. It is not essential, but it is highly convenient. In human history we use an astronomic scale of equal parts, designating each unit by a number. In geology no scale of equal parts is available, and we employ the eras and periods, and to some extent the epochs, of the local geologic history first deciphered—that of Europe. These time-divisions bear the same names as the groups, systems, and series of strata whose deposition occurred within them.

So far as the science of geology is concerned the selection of Europe as its first field of study was a matter of chance, and the adoption of the European time scale as a general standard may therefore be said to have been accidental. Though the local rock scheme on which it is based is natural, the time scale, considered as universal, is arbitrary. Another locality would have afforded a different scale, but its authority would neither be greater nor less. The scale being recognized as arbitrary, and a mere matter of convenience, it is legitimate to modify and fix it by formal convention. The Congress can do good service to geologic technology by putting it in the best possible shape and giving it an official status. In my judgment only a small number of divisions should be admitted, not more than the number of periods of the European scheme. In a general way the durations represented by the co-ordinate divisions should be as nearly equal as practicable, but a certain concession might be made to chronologic perspective on account of our superior opportunities for studying the later history. Some of the shorter periods might perhaps be united under new names. Each line of division between periods should be defined by means of a stratigraphic plane of division, and this can be done with precision if a locality is made part of the definition.

Especially should pains be taken to declare the arbitrary nature of the scale. Even with this precaution it will be misconstrued by many, for there is a tendency of the mind to attach undue weight to classification. Wherever we draw lines of separation, we lose to a certain extent the power to recognize continuity. When, for example, the clock strikes twelve on New Year's Eve, time seems to stop and begin again. We speak of the achievements of the nineteenth century—and despite ourselves we think of them too—as though a new industrial epoch began in A.D. 1800. And so it is easy for the beginner in geology to accept as discontinuous the eras and periods of which his text-book treats, and it is hard for him afterward to unlearn the lesson.

There is reason to believe that confusion of ideas in regard to geologic classification has been fostered by the employment of the same set of names for the divisions of the time scale and for the local terranes on which they are founded. It might be well to furnish the time scale with names suggesting times—such names as the brothers Rogers applied to the terranes of Pennsylvania; but so radical a change is hardly feasible, especially as we should thus lose the mnemonic connection of times with corresponding terranes. I propose, as a means of accomplishing the end with the least inconvenience, that a set of time words be derived from the terrane names by modifying the final syllables. The time words should all have the same termination, and that should differ from any terminations occurring in the terrane names. I suggest for the ending of time words the syllable *al*. With such a nomenclature, Jurassic and Devonian would denote only certain European rock systems, while Jural and Devonal would denote periods of the standard time scale; and we could speak of the Chico-Tejon series as partly Eocenal and partly Cretaceal without seeming to imply the existence in California of the Eocene and Cretaceous systems of Europe.

A few minutes ago I opposed the differentiation of words by terminations because it abrogated the power of indefinite expression; I now favour it for the same reason. It is well to be indefinite as to the taxonomic rank of terranes while their characters are imperfectly known, but it is not well to confuse terranes with times.

It is not to be assumed that a time scale adopted now as the best possible will continue indefinitely to be the best possible; the day will inevitably come when it can be improved. In the fuller light of the future we may recognize as very unequal periods that we now deem equivalent, and the possibilities of defining pre-Cambrial periods are unlimited. Even now there are announced beneath the lowest fossil-bearing terrane of the Lake Superior region two systems of clastic rocks limited above and below by great unconformities, and Irving demands their recognition as a group, distinct from the Archæan. If his voice is heard, the time scale will include an era between the Palæozoal and the Archæal, and this era will supply the needs of the systematist until great additions have been made to our present knowledge of the older rocks.

(*To be continued.*)

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

OXFORD.—The resignation of Prof. Prestwich and the continued illness of Prof. Moseley have produced two gaps in the scientific professoriate this term. Though the Professor of Geology has left Oxford, his successor will not be appointed until after Christmas. In Prof. Moseley's absence the work of the morphological department is being carried on by Mr. Hatchett Jackson and Mr. G. C. Bourne, who has just been elected to a Fellowship at New College.

In the department of physics, Prof. Clifton is giving no honour lectures, but has an elementary course on mechanics and acoustics. Mr. Selby is lecturing on elasticity and on electrostatics, both treated mathematically. At Christ Church, Mr. Baynes gives lectures and practical instruction in electricity and magnetism. Sir John Conroy and Rev. F. J. Smith are lecturing on elementary physics at Balliol and Trinity.

The chemical department at the Museum has been strengthened by the addition of Mr. V. H. Veley to the staff; the usual courses of lectures are being given by Mr. Fisher and Dr. Watts, and Prof. Odling is lecturing on glycerine and its derivatives. At Christ Church, Mr. Vernon Harcourt has a class for quantitative analysis, and lectures on elementary chemistry. Sir John Conroy is lecturing on chemical physics at Balliol.

The Physiological Laboratory is continuing its usual work with little alteration. Prof. Burdon Sanderson's lectures are on circulation, respiration, and bodily motion.

Prof. Balfour, besides lecturing on extinct plants for the Professor of Geology, is giving a course on general morphology. Dr. Tylor continues to expound the Pitt-Rivers Collection, a fresh portion which will shortly be rearranged and opened to the public.

Mr. Primrose McConnell is lecturing on the principles of agriculture, mainly for the benefit of India Civil Service candidates.

During this term there will be an examination for Scholarships in Natural Science at Balliol, Exeter, Christ Church, and Trinity jointly, beginning on November 17.

A pamphlet has just been issued from the Clarendon Press giving full information as to the methods of obtaining medical degrees in Oxford, and the instruction in medicine provided by the University.

CAMBRIDGE.—The Museum and Lecture Rooms Syndicate have issued another report pressing for new accommodation for Pathology and Physiology, and suggesting that the department of Pathology, including its Museum, could be located in the old chemistry buildings when the new laboratory is completed. They recommend the building of new rooms for Human Anatomy and Physiology on the Corn Exchange site at the northern end of the Museum site. It is an important feature of this scheme that it would leave free the present Human Anatomy Schools and Museum to be added to the frontage available in Downing Street for a Geological Museum.

Messrs. W. H. Macaulay and W. B. Allcock have been appointed Moderators for the year beginning May 1, 1888. Messrs. J. Larmor and W. Welsh are appointed Examiners in Part I. of the Mathematical Tripos for the same year.

Dr. Smith's prizes for the present year have been adjudged as follows:—(1) Mr. Augustus Edward Hough Love, St. John's College, for an essay on "Small Free Vibrations and Deformation of a thin Elastic Shell," and on "The Force and Forced Vibrations of an Elastic Spherical Shell containing a Given Mass of Liquid." (2) Mr. Arthur Berry, King's College, for an essay on "Joint Reciprocants."

SCIENTIFIC SERIALS.

The Quarterly Journal of Microscopical Science for August 1887, vol. xxviii. part 1, contains the following papers:—On the anatomy of the Madreporia, part 3, by Dr. G. Herbert Fowler, (plates i. and ii.) In this memoir the author deals with the

anatomy of *Turbinaria mesenterina* (?), of *Lophohelia prolifera*, of *Seriatopora subulata*, and of Pocillopora, with a note on the skeleton of Flabellum.—On the anatomy of *Mussa corymbosa*, and *Euphyllia glabrescens*, and on the morphology of the Madreporian skeleton, by G. C. Bourne (plates iii. and iv.)—On the intra-ovarian egg of some osseous fishes, by Dr. Robert Scharff (plate v.)—Observations on the structure and distribution of striped and unstriped muscle in the animal kingdom, and a theory of muscular contraction, by C. F. Marshall (plate vi.) The author concludes that in all muscles which have to perform rapid and frequent movements a certain portion of the muscle is differentiated to perform the function of contraction, and this portion takes on the form of a very regular and highly modified intracellular network. This network, by its regular arrangement, gives rise to certain optical effects, which cause the peculiar appearances of striped muscle; the contraction of the striped muscle-fibre is probably caused by the active contraction of the longitudinal fibrils of the intracellular network; the transverse networks appear to be passively elastic, and by their elastic rebound cause the muscle to rapidly resume its relaxed condition when the longitudinal fibrils have ceased to contract; they are possibly also paths for the nervous impulse.—On the fate of the muscle-plate and the development of the spinal nerves and limb plexuses in birds and mammals, by Dr. A. M. Paterson (plates vii. and viii.)—On the ciliated pit of Ascidians and its relation to the nerve-ganglion and so-called hypophysial gland, and an account of the anatomy of *Cynthia rustica* (?), by Lilian Sheldon, Bathurst Student, Newnham College (plates ix. and x.). Suggests that the original function of the ciliated pit was the aeration of the brain, with which it communicates in the case of Clavellina; where also its posterior part acts as a reservoir to carry off the secretion of the gland.—On the tongue and gustatory organs of *Mephitis mephitica*, by Dr. Frederick Tuckerman (plate xi.) This memoir is preceded by an interesting account of the literature relating to the position and structure of the taste organs of vertebrates.—On the quadrate in the Mammalia, by Dr. G. Baur. He thinks that there is little doubt but that the quadrate of the lower vertebrates is contained in the sygomatic process of the mammals.—On the hæmoglobin crystals of rodents' blood, and on an easy method of obtaining methæmoglobin crystals for microscopic examination, by Dr. W. D. Halliburton.

Bulletin de l'Académie Royale de Belgique, August.—Note on the oscillations of a pendulum produced by the displacement of the axis of suspension, by E. Ronkar. The object of these researches is to ascertain the possibility of recording the slight oscillations in the crust of the earth by means of a freely suspended pendulum. It is shown that the pendulum retains a movement imparted by a certain number of horizontal undulating impulses, whenever the duration of the oscillation of the pendulum is the same as that of the axis, but not otherwise. From this may be deduced an experimental process for determining the periodical irregularities in the movement of diurnal rotation.—On the colloidal sulphuret of cadmium, by Eug. Prost. To the colloidal solutions of arsenious, stannic, and other sulphurets already determined, the author here adds the sulphuret of cadmium, which was hitherto known only in the insoluble state. He obtains a colloidal solution of this compound by passing hydrosulphuric acid through water holding in suspension freshly precipitated cadmic sulphur, and afterwards eliminating by the action of heat the hydrosulphuric acid dissolved in the liquid. A spectroscopic study of this clear yellowish liquid shows that the cadmic sulphur is really in a state of solution, the solution presenting all the characters hitherto ascribed to all dissolved colloidal substances.—Description of some new Cucurbitaceæ, by M. Alfred Cogniaux. This paper contains an account of fourteen new species and of several varieties, forming an important addition to the author's general monograph of this family published in De Candolle's "Monographiæ Phanerogamum."

SOCIETIES AND ACADEMIES.

LONDON.

Entomological Society, October 5.—Dr. Sharp, President, in the chair.—Mr. Jacoby exhibited a specimen of *Aphthonoides beccarii*, Jac., a species of Haltica having a long spine attached to the posterior femora; also a specimen of *Rhagiosoma madagascariensis*.—Mr. Stevens exhibited a very dark specimen of *Crambus perlellus* from the Hebrides, which its captor supposed to be a new species. Mr. Porritt remarked that this brown form of *Crambus perlellus* occurred at Hartlepool with the ordinary typical form of the species, and was there regarded as only a variety of it.—Mr. Slater exhibited a specimen of *Gonepteryx cleopatra*, which was stated to have been taken in the North of Scotland. Mr. Jenner Weir remarked that although the genus *Rhamnus*—to which the food-plant of the species belonged—was not a native of Scotland, some species had been introduced, and were cultivated in gardens.—Mr. South exhibited about 150 specimens of *Boarmia repandata*, bred from larvæ collected on bilberry in the neighbourhood of Lynmouth, North Devon, including strongly marked examples of the typical form, extreme forms of the var. *conversaria*, Hüb., a form intermediate between the type and the variety last named, and examples of the var. *destrigaria*, Haw. Mr. South said that an examination of the entire series would show that the extreme forms were connected with the type by intermediate forms and their aberrations.—Mr. Poulton exhibited young larvæ of *Apatura iris*, from the New Forest; also eight young larvæ of *Sphinx convolvuli* reared from ova laid on the 29th of August last. He said the life-history of the species was of extreme interest, throwing much light upon that of *Sphinx ligustri*, as well as upon difficult points in the ontogeny of the species of the allied genera *Acherontia* and *Smerinthus*. Mr. Stainton said he was not aware that the larvæ of *Sphinx convolvuli* had ever before been seen in this country in their early stages. Mr. McLachlan remarked that females of this species captured on former occasions, when the insect had been unusually abundant, had been found upon dissection to have the ovaries aborted.—Mr. R. W. Lloyd exhibited specimens of *Elater pomonæ*, and of *Mesosa nubila*, recently taken in the New Forest.—Mr. Porritt exhibited a series of melanic varieties of *Diurnea fagella*, from Huddersfield, and stated that the typical pale form of the species had almost disappeared from that neighbourhood.—Mr. Goss exhibited, for Mr. J. Brown, a number of puparia of *Cecidomyia destructor* (Hessian Fly), received by the latter from various places in Cambridgeshire, Norfolk, Suffolk, and Wiltshire. He also exhibited a living larva of *Cephus pygmæus*, Lat. (the Corn Sawfly), which had been sent to Mr. Brown from Swaffham Prior, Cambridgeshire, where, as well as in Burwell Fen, the species was stated to have been doing considerable damage to wheat crops. Mr. Verrall, in reply to a question by Mr. Enock, said he believed that the Hessian Fly was not a recent introduction in Great Britain, but had been here probably for hundreds of years. He admitted that he was unable to refer to any but recent records of its capture. Prof. Riley said he was unable to agree with Mr. Verrall, and believed that the Hessian Fly had been recently introduced into this country. Its presence here had not been recorded by Sir Joseph Banks, by Curtis, by Prof. Westwood, by the late Mr. Kirby, or by any other entomologist in this country who had given especial attention to economic entomology. It seemed highly improbable, if this insect had been here so many years, that its presence should have so long remained undetected both by entomologists and agriculturists. Prof. Riley said it had been stated that the insect was introduced into America by the Hessian troops in 1777, but this was impossible, as its existence at that date was unknown in Hesse. Mr. McLachlan, Capt. Elwes, Mr. Verrall, Mr. Jacoby, and Dr. Sharp continued the discussion.—Mr. J. Edwards communicated the second and concluding part of his "Synopsis of British *Homoptera-Cicadina*."—Prof. Westwood contributed "Notes on the life-history of various species of the Neuropterous genus *Ascalaphus*."—Capt. Elwes read a paper "On the Butterflies of the Pyrenees," and exhibited a large number of species which he had recently collected there. Mr. McLachlan said he spent some weeks in the Pyrenees in 1886, and was able to confirm Capt. Elwes' statements as to the abundance of butterflies in that part of the world. The discussion was continued by Mr. Distant, Mr. White, Dr. Sharp, and others.

Mineralogical Society, October 25.—Anniversary Meeting.—Mr. L. Fletcher, President, in the chair.—After the reading of the Report, the following were elected Officers and Council for the ensuing session:—President: L. Fletcher. Vice-Presidents: Rev. S. Haughton, F.R.S., W. H. Hudleston, F.R.S. Council (in place of Messrs. Burghardt, Danby, Dobbie, and Lewis, the retiring Members): Prof. A. H. Church, Townshend

M. Hall, Colonel C. A. M'Mahon, J. Stuart Thomson. Treasurer: Rev. Prof. T. G. Bonney, F.R.S. General Secretary: R. H. Scott, F.R.S. Foreign Secretary: T. Davies. Auditors: B. Kitto, F. W. Rudler.—The following papers were read:—On a meteoric iron, containing crystallized chromite, found in Greenbrier Co., West Virginia, about the year 1880, by L. Fletcher, President.—On the nature and origin of clays, by J. H. Collins.—Note on the occurrence of what may prove to be a new mineral resin, by J. Stuart Thomson.—On a variety of glaucophane from the Val Chivone (Cottian Alps), by Rev. Prof. T. G. Bonney, F.R.S.—On the discovery of leucite in Australia, by Prof. J. W. Judd, F.R.S.—On proustite containing antimony, by H. A. Miers and G. T. Prior.—Description of a new student's goniometer, by H. A. Miers.—On rutile needles in clays, by J. J. H. Teall.

PARIS.

Academy of Sciences, October 24.—M. Janssen in the chair.—On naphthol as an antiseptic, by M. Ch. Bouchard. From the experiments here described it is shown that naphthol, hitherto limited to the local treatment of certain cutaneous diseases, may with perfect safety be applied inwardly. Its antiseptic and toxic properties have been accurately determined, with the result that, owing to its slight solubility, it is to be preferred in certain cases to all known antiseptic medicines.—Remarks on the physical principle on which is based M. Clausius's new theory of steam-motors, by M. G. A. Hirn. The view here contested is that the cylinder may be regarded as impermeable to heat, and consequently that the exchange of heat between its walls and the steam at each stroke of the piston is a factor which may be neglected by the practical mechanician. M. Hirn claims that most English and American engineers have adopted his views in the "Hirn-Zeuner controversy."—On the congelation of ciders, by M. G. Lechartier. The author's experiments make it evident that the fermentation of ciders is not destroyed but only diminished even after being kept for nine days at a temperature of 18° C. below freezing-point.—Remarks accompanying the presentation of the "Statistique de la Superficie et de la Population des Contrées de la Terre," by M. E. Levasseur. This work, which appeared originally in the *Bulletin de l'Institut international de Statistique* for 1886-87, comprises 103 tables, in three parts—the first devoted to Europe, the second to the other divisions of the globe, the third to general conclusions and comparative details for the whole earth. In this part the area and population of the various divisions of the world are thus tabulated for the year 1886:—

	Area in millions of square kilometres.	Population. In millions.	Density per square kilometre.	Ratio to the whole population of the world.
Europe	10·0 ...	347 ...	34 ...	23·4
Africa	31·4 ...	197 ...	6 ...	13·3
Asia	42·0 ...	789 ...	19 ...	53·2
Oceania	11 ...	38 ...	3·5 ...	2·6
North America	23·4 ...	80 ..	3·4 ...	5·4
South America	18·3 ...	32 ...	1·7 ...	2·1
	136·1	1483	10·9	100·0

It is pointed out in the introduction that nearly two-thirds of mankind are concentrated in a relatively small space, about 11 millions of square kilometres, or one-twelfth of all the dry land, divided into three great groups: West, Central, and South Europe (245 millions of inhabitants, and 3·5 millions of kilometres); the Anglo-Indian Empire (254 and 3·6); and China, with Manchuria and Japan (430 and 4).—On the third scientific voyage of the *Hirondelle*, by Prince Albert of Monaco. Besides many hundreds of floats sent adrift between the Azores and Newfoundland, several captures were made from great depths with the sounding-gear, which worked easily down to 3000 metres from the surface. Amongst the prizes were several undescribed fishes, Gorgons, siliceous Sponges of the Hexactinellid family, a soft Urchin (*Phormosoma*), numerous Amphipod and Isopod Crustaceans, Solasters, Ophiures, and Hyas of great size, besides a moon-fish weighing nearly 300 kilogrammes, and furnished with a true caudal appendage.—On Newton's chromatic circle, by M. G. Govi. It is shown that this law, of which Newton himself offered no demonstration, is often at fault, because it expresses no certain theoretic principle, nor any rigorously observed theoretic fact. Nevertheless it may still yield approximately correct useful results when it is required to express the complex sensations experienced by the organ of sight.—Positions of Brooks's Comet (January 22, 1887) measured with the 8-inch equatorial of the Observatory of Besançon, by M. Gruey. The positions are calculated for the period ranging from February 24 to April 29.—On magnetizing by influence, by M. P. Duhem. The author communicates the chief results of some studies based on the principles of thermodynamics, and undertaken for the purpose of removing some of the difficulties presented by Poisson's theory.—Action of sulphureted hydrogen on the salts of cobalt, by M. H. Baubigny. Some years ago the author showed that all the salts of nickel are transformed to sulphides when their solutions are treated with hydrosulphuric acid at the ordinary temperature. He now shows that a like treatment of the salts of cobalt yields very similar results.—On the quantitative analysis of titanic acid, by M. Lucien Lévy. A new method of analysis is described, which is more rapid and yields more accurate results than that hitherto in use.—On certain processes capable of increasing the resistance of the organism to the action of microbes, by M. Charrin. It is shown, by experiments carried out on rabbits, that under specified conditions the resisting power of the animal may be greatly increased and rendered more or less complete and lasting by inoculating or injecting the soluble products of the cultivated virus of certain microbes.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Philip's Handy Volume Atlas of the British Empire (Philip).—Practical Chemistry: Muir and Carnegie (Clay).—Elementary Chemistry: Muir and Slater (Clay).—Essays relating to Indo-China, 2nd series, 2 vols. (Trübner).—On a Surf-bound Coast: A. P. Crouch (Low).—The Mammoth and the Flood: H. H. Howorth (Low).—The Natural History of Commerce, 3rd ed.; The Technical History of Commerce, 3rd edition; The Growth and Vicissitudes of Commerce, 3rd edition; Recent and Existing Commerce, 3rd edition: Dr. J. Yeats (Philip).—Proceedings of the American Academy of Arts and Sciences, December 1886 to May 1887 (Boston).—Bulletin de la Société Impériale des Naturalistes de Moscou, 1887, No. 3 (Moscou).—Zeitschrift für Wissenschaftliche Zoologie, xlv. Band, 4 Heft (Williams and Norgate).—Morphologisches Jahrbuch, Eine Zeitschrift für Anatomie und Entwickelungsgeschichte, xiii. Band, 1 Heft (Engelmann, Leipzig).—Encyklopædie der Naturwissenschaften. Zweite Abth. 44 und 45 Lief. Handwörterbuch der Chemie; Erste Abth. 52 und 53 Lief. Handwörterbuch der Zoologie, Anthropologie, und Ethnologie (Williams and Norgate).—Journal of the Scottish Meteorological Society, 3rd series, No. iv. (Blackwood).—Animals from the Life, edited by A. B. Buckley (Stanford).

CONTENTS.

PAGE

The Zoological Results of the "Challenger" Expedition 1
The Fern-Allies. By W. R. McNab 4
Our Book Shelf:—
Elson: "The Sailor's Sky Interpreter" 5
Mackenzie: "Austral Africa" 5
Letters to the Editor:—
Medical Education at Oxford.—George I. Wilson . 5
Migration of Swallows along the Southern Coast.—W. Warde Fowler 6
Swifts.—E. Brown 6
Hughes's Induction Balance.—Dr. Oliver J. Lodge, F.R.S. 6
The Ffynnon Beuno and Cae Gwyn Caves.—Worthington G. Smith 7
Synthesis of Glucose. By A. E. Tutton 7
Modern Views of Electricity. Part II.—IV. (*Illustrated*). By Dr. Oliver J. Lodge, F.R.S. 8
The Tweeddale Collection. By R. Bowdler Sharpe 13
The Storm of October 30 14
Robert Hunt, F.R.S. 14
Notes 15
Astronomical Phenomena for the Week 1887 November 6-12 17
Geographical Notes 17
Meteorological Notes 18
The Work of the International Congress of Geologists. By G. K. Gilbert 19
University and Educational Intelligence 22
Scientific Serials 22
Societies and Academies 23
Books, Pamphlets, and Serials Received 24

THURSDAY, NOVEMBER 17, 1887.

POLITICS AND THE PRESIDENCY OF THE ROYAL SOCIETY.

THE combination of vigorous intellect, profound knowledge, and scrupulous integrity, is not so common among our legislators, that a good citizen, whatever his political convictions, can have any feeling but one of satisfaction at the entrance into the House of Commons of the new member designate for the University of Cambridge. Prof. Stokes's foes (if indeed he have any foes), no less than his friends, will concur in attributing these qualifications to him. No man in the scientific world is, or deserves to be, more respected or more popular.

In that world many will doubtless find an additional source of congratulation in this public recognition of the merits of their colleague by the dominant political party in the University of Cambridge. And many will probably entertain the hope that the addition of another man of science to the three or four, who already occupy seats in the House of Commons, may do something towards the enlightenment and guidance of the House and of the Government, when scientific questions come under discussion.

In the minds of thoughtful men, more or less familiar with the realities of political and official life, however, it is probable that reflections of a less satisfactory nature may arise. They may regret that faculties which are so eminently fitted to serve science should inevitably be devoted to the interests of a party. Inevitably, because, with whatever high resolves the nominee of the Conservatives of Cambridge enters Parliament, he will find, before he has been there a week, that he is expected to do what the Whips bid him to do. And again such persons may think, not unreasonably, that Science is every day becoming more and more able to look after her own interests; and that, for her own honour and dignity, it is better that they should be neglected than that they should be promoted by back-stairs agencies. Moreover, experience may suggest that the deliberate judgment of the majority of scientific men, upon any question in which State intervention is called for, may be widely different from the view taken by this or that member of their body who happens to have a seat in Parliament; and that it is extremely undesirable that less legitimate methods of influencing a Minister should be substituted for the present fair and open mode of placing a case before him by responsible and authorized deputations.

But, whatever doubts may be entertained as to the service which has been, or can be, rendered to science by scientific members of Parliament, it is obviously within the right of every man to judge for himself whether he will become one or not. So far as Prof. Stokes is simply a very distinguished mathematician and physicist, it is for him, and for him alone, to decide between the claims of science, on the one hand, and those of political and ecclesiastical conviction on the other.

At the present moment, however, Prof. Stokes is something more than an eminent investigator and teacher: he is President of the Royal Society; and, as such, he enjoys all the prestige which is given by the fact that, in the eye of the public, he has the oldest, the strongest, and the most widely representative body of men of science in the country at his back. The President is the organ and mouth-piece of the Council of the Royal Society—a body which has frequent and important relations with the Government; and, as such, it may often be his business to represent to the Government the conclusions at which the Council arrives. It is therefore highly important that the freedom of the President's intercourse with Ministers should be in no way trammelled by his political relations.

It may be quite safely affirmed that Prof. Stokes's political and ecclesiastical views were not taken into consideration by those who placed him in the chair of the Royal Society. The last half-dozen of his predecessors, to go no further back, have sedulously abstained, during their occupancy of the chair, from holding office in any other Society, no less than taking part in any public, and especially political, action about which the opinions of the Fellows could be divided. Prof. Stokes has not followed this prudent example. Some little time ago he accepted the Presidency of a body of pronounced theological tendencies; and he now accepts the nomination of a no less pronounced political party, and, since our note upon his candidature appeared, he has issued an address in which he promises to devote himself to certain party objects.

It does not appear that Prof. Stokes has obtained, or, indeed, sought, the sanction of the Council or of the Society, at large, for this departure from precedent. For such it is, in spite of the fact that Sir Isaac Newton was a member of Parliament during his Presidency, and that many peers have occupied the chair. But it is obvious that a peer need not be a party politician; and, as regards the precedent of Sir Isaac Newton, it is enough to point out that the House of Commons of the end of the nineteenth century is a very different body from the House of Commons of the beginning of the eighteenth century. The position of an independent member has become impossible; and those who refer to Prof. Stokes's address will see that, whatever his first feelings may have been, he, now at any rate, does not propose to be anything but a staunch Conservative.

No doubt there are many staunch Conservatives in the Royal Society, but no doubt also there are many equally staunch Liberals and Radicals; and if it had entered into the imagination of the latter that Prof. Stokes would carry the prestige of the Presidency into the service of their political opponents, it may be doubted whether they would have voted for him. The same argument would apply with equal force if Prof. Stokes happened to be a Liberal. The question before us is one not of party, but of principle.

We are in the midst of a great political struggle, and it may be safely predicted that the force of party feeling will increase rather than diminish for years to come. If it is permissible that the President of the Royal Society may be a political personage, the minds of the Fellows on St. Andrew's Day will be divided between two sets of considerations. Not only will each ask, "Is A.B. the best man for the Presidency in the interests of science and of the Society?" which is the only question he ought to put; but he will ask, "Is A.B. of my politics, or the opposite?"

It is eminently true of political passion that a "little leaven leaveneth the whole lump"; once inoculate the Royal Society with that virus, and the poison will spread through the whole organism. The Council practically chooses the President: it will therefore be necessary to look to the politics of the Councillors. The Fellows elect the Council: have a care, therefore, to the politics of the new Fellows. We may yet see a politico-scientific caucus. Some years ago a most sagacious and experienced man of affairs in the United States was asked why, in drawing up the constitution of a new University, he had not given such persons as the Governor and Chief Justice of the State an *ex-officio* position on the governing body. "Ah," said he, with a shrewd smile, "if you only knew the trouble my colleagues and I have taken to render it impossible for any political person to have anything whatever to do with the administration of the University! We know to our cost that wherever politics enters corruption follows."

The records of the Royal Society tell us of more than two centuries of scientific life, fertile in good work and unstained by anything worse than an occasional outbreak of prejudice or jealousy. The only occasion on which it ever manifested a political bias was in the case of Priestley; and it has no reason to be proud of that episode.

The Society is now at the parting of the ways. Either it will continue its beneficent work for untold ages to come, untroubled by the transitory political and social storms raging around it; or, headed by politicians pledged to serve their party, it will gradually be dragged down into that miserable slough in which no capacity seems proof against the temptation to sophistical special pleading and no character strong enough to refuse degrading subserviency to party exigencies.

The occasion is grave and demands action. It is for the President, by the course which he may think fit to adopt, to determine what that action shall be.

THE STORAGE OF ELECTRICAL ENERGY.

The Storage of Electrical Energy. By Gaston Planté. (London: Whittaker, 1887.)

TO the author of this book we owe the use of lead plates instead of platinum plates in voltameters. His experiments showed that, after repeated charging and discharging of lead-plate voltameters, accumulators of energy were producible which might be employed in a great variety of useful ways. He showed that his accumulators might be charged in parallel by a few Bunsen or Daniell cells, and discharged in series. As his accumulators had small internal resistances, he was able to give to circuits either of small or great resistance very considerable supplies of electric power for short times, and as an experimenter he availed himself of this novel power in heating wires, melting beads of metal, and generally of observing effects produced by strong currents.

Many of the phenomena observed by him were new, and well worthy of being recorded, as they were recorded, in the proceedings of scientific Societies; and the present book, in addition to a fine portrait of the author, and many other engravings, and a dedication to the Emperor of Brazil, seems to be merely a collection of these papers of M. Planté, published between the years 1859 and 1879. In the first chapter of the book and part of the second we find an interesting account of experiments with various electrodes in voltameters, which led the author to use lead instead of platinum, and of the forms which the author gave to his cells, with directions for their formation, and speculations as to the chemical actions involved. The remaining twelve and a half chapters may be regarded as almost solely devoted to the "effects created by currents combining quantity with high tension"—to use the old-fashioned phraseology which Mr. Elwell, the translator, has thought fit to use upon the title-page—and to the author's speculations upon things in general.

The infancy of the electric accumulator lasted to 1879, its boyhood to 1883, and we may now be said to know it in its manhood. The advance since 1879, not only in our knowledge of the chemical and electrical actions going on in the accumulator, but also in our methods of applying this knowledge, has been quite as wonderful as the advance made in any other part of applied physics. Batteries of accumulators capable of driving boats 80 feet long, of driving numbers of tram-cars, of maintaining large installations of electric lights, are now in actual use. Plates of lead are now used as in 1879, but the salts of lead in contact with the metallic plates are attached mechanically, hundreds of devices having been tried and rejected or adopted in the last eight years for the purpose of obtaining great capacity and longevity. Of these great changes, the results of numerous, most costly, and carefully conducted experiments, made by scientific men, M. Planté tells us nothing. He was in charge of the accumulator in its infancy; it was taken away from him in 1879, and its subsequent history seems to be as unknown to him as the boyhood and early manhood of Harry Bertram were to Dominie Sampson.

The dominie looked upon his pupil, now grown to be a man, as if he were still a boy who was about to resume his childish studies, and in the same way it is probable that M. Planté regards the accumulator of 1887 as in no respect different from the laboratory toy with which he obtained such remarkable effects prior to 1879. M. Planté gives in this book what may be regarded as the history of the infancy of the electric accumulator; and it is obvious that if he had written it as charmingly as Mrs. Molesworth herself could have written it for the nursery, yet, with the misleading title which it possesses, he has given occasion to the ordinary reader to feel greatly disappointed. We are here assuming that M. Planté shares with Mr. Elwell the responsibility of publication, and also of change in the name of the book from that of the first edition—"Recherches sur l'Électricité"—published in 1879, which is the only French edition with which we are acquainted.

The technical terms used by the translator are not now so familiar to students as they used to be in the good old times when *strength*, *intensity*, *quantity*, and *power* of a current were synonymous with each other or with electromotive force.

It was this freedom in "the older electricity" which enabled statements like "The E.M.F. was thus found equal to 1·41, the current from the Bunsen cell being 1" (p. 17) to be enjoyed by readers. Other statements like

this: "We have found that the resistance of secondary cells of the various dimensions which we have used varied from 2 to 5 metres of a copper wire 1 millimetre in diameter" (p. 64), show that M. Planté sought for greater exactness in his measurements than many of his contemporaries during the infancy of the electric accumulator.

It was to be expected that in suggesting yet untried applications of secondary batteries the author should make statements which any student now knows to be erroneous. An example is to be found at p. 105, where it is suggested that, by using a secondary battery, two ordinary Bunsen cells might be enabled to work a continuous voltaic arc. As was also to be expected in such a republication of papers as this, many of which read like the contents of an inedited laboratory note-book, there are repetitions of the same facts and sentiments.

Unfortunately there is another resemblance to laboratory notes in much of the matter of this book which cannot be so readily forgiven. One often confides to one's note-book a speculation which is based on a very far-fetched resemblance between two phenomena. And it is quite possible to find in a note-book such a note as this (p. 198): "The experiment described above (158) in which a cloud of metallic oxide, torn from an electrode by the current, takes a spiral motion in the body of a liquid under the influence of a magnet, seemed of a nature to explain, in particular, the remarkable form of spiral nebulæ." Then follows a description of the nebulæ observed by "Lord Ross," and the further remarkable note: "In view of so striking a similarity, may it not be reasonably supposed that the nucleus of these nebulæ may be formed by a veritable electrical furnace; that their spiral form is probably caused by the presence of celestial bodies powerfully magnetized, and that the direction of the curve of the turns in the spiral must depend upon the nature of the magnetic pole turned towards the nebula."

This sort of thing may be found in the note-book of almost any laboratory worker, but it is astonishing to find that M. Planté has not only published it in the proceedings of a scientific Society, but actually publishes it again after he has had many years of leisure for reflection and for verification. These speculations occupy many chapters of the book. M. Planté describes some natural phenomenon, such as globular lightning, the formation of hail, water-spouts, cyclones, the aurora, atmospheric electricity, spiral nebulæ, or solar spots; he then begins to write on the vague analogy existing between this natural phenomenon and some isolated phenomenon observed by him in the laboratory, and after he has written some pages, the analogy becomes very indistinct; but he continues to write in the hope that if he writes long enough he may obtain clearer ideas. Of the same kind are his "views" of the nature of electricity. He finds that when successive intense currents are sent through fine wires, which are, of course, greatly heated, the wires lose their straightness in curious ways. It is very interesting to read about the observed phenomena, but unfortunately we have the author's speculations as well. He says (p. 247):—"The phenomena we have just described (313-20) are of a nature to throw some light on the mode of propagation of electricity. The molecular vibrations revealed by knots formed in a metallic wire, by the curious noise, and by a notable change in its cohesion under the influence of the passage of the dynamo-static current which we have just studied, must be produced in a lesser degree in conducting substances traversed by electric currents of very low tension. This vibration may be too feeble to be perceptible, but it is not the less real. We are then able to conclude that the electric movement must diffuse itself in substances after the manner of a purely mechanical motion, by a series of very rapid vibrations of the more or less elastic matter through which it passes."

He then goes on in his last chapter, without a thought of the possibility that very rapid heating of a not perfectly homogeneous conductor might explain his phenomena, to build up a theory of electricity from these isolated facts with the help of a few far-fetched analogies, and he publishes his theory without further verification. In spite of our great obligations to M. Planté, we feel that he has set the very worst example possible to the probable readers of his book, in publishing these vague speculations of his.

JOHN PERRY.

FRITSCH'S CRUSTACEAN FAUNA OF THE CHALK OF BOHEMIA.

Die Crustaceen der Böhmischen Kreideformation. Von Prof. Dr. Anton Fritsch und Jos. Kafka. Pp. 55. (Prague: Selbstverlag, in Commission von Fr. Rivnáč, 1887.)

THERE is probably no sedimentary deposit in the whole series of the stratified rocks with which one is more familiar than the Chalk. This is doubtless due to its peculiar whiteness, and to the fact of its occupying so large an area in our eastern and south-eastern counties, and its prominence in the coast-sections of Yorkshire, Kent, and Sussex, and the opposite coast of France; forming at Dover those white cliffs which gave to our shores their ancient name of Albion.

In the Cretaceous formation, however, we include a set of other beds, very dissimilar from the Chalk in appearance and composition, but which, on stratigraphical and palæontological grounds, seem to form a natural rock-system. These are known as the Upper Greensand, the Gault Clay, the Lower Greensand, and the Wealden Beds, comprising marls, sands, clays, and even fresh-water limestones. Without entering into details as regards the minor divisions, we may say that the major proportion of these deposits are marine, as shown by the organic remains contained in them. The Chalk itself, from its general purity, must have been formed in a deep and open sea; indeed, the researches which have been carried on in the North Atlantic Ocean show that the materials for a continuous bed of limestone with flint-nodules are now being deposited at depths of from 400 to 2000 fathoms, while many forms of life met with there are analogous to those of the Chalk.

That this old Cretaceous sea must have been of very wide extent is proved by the enormous area over which its sediments have been traced, as shown on our geological maps; whilst outliers and vast beds of

flint-gravel derived from the Chalk give evidence of a still wider region once covered by its waters, but whose deposits have since been removed by denudation.

The *white* Chalk, whence the name "Cretaceous" was taken, is almost wholly confined to the Anglo-Parisian area, where the system was first studied, but the formation, varying in lithological characters, may be followed from England into France, Belgium, Holland (Maestricht), Denmark (Faxoe), south of Sweden, Hanover, Brunswick, Saxony, Bavaria, Bohemia, Moravia, Switzerland, Austria, and the chain of the Alps, the Mediterranean Basin, including parts of Spain, south of France, Italy, Greece, Asia Minor, Sicily, and North Africa. This latter is the well-known "Hippurite Limestone" of the South of Europe, which stretches away to Persia and the Himalayas, and extends over the greater part of the continent of India. Cretaceous fossils have also been traced as far south in Africa as Natal.

The vastness of the Cretaceous system in North America far exceeds even our largest computation of its aggregate mass in the European area, being from 11,000 to 13,000 feet in thickness; whilst in our own hemisphere it probably does not exceed 7000 feet as a whole. It extends across the breadth of the North American continent, and over wide regions in South America, marked by many of the characteristic fossils of the Cretaceous rocks of Europe. But the evidence of contiguity to land in North America demonstrated by plant and animal remains far surpasses our own very limited records of shore and shallow-water conditions in Cretaceous times in Europe. Nevertheless we do possess at Aix-la-Chapelle, and in Saxony and Bohemia, Upper Cretaceous beds containing plant remains, such as leaves of *Acer*, *Alnus*, *Credneria*, *Cunninghamites*, and *Salix*, with Conifers akin to *Sequoia* and *Pandanus*, South African, and Cape *Proteaceæ*, and many Cryptogams, chiefly ferns, such as *Gleichenia*, *Lygodium*, *Asplenium*, &c. These have been dealt with elsewhere, as have also the Cephalopoda ("Cephalopoden der Böhmischen Kreideformation," von Dr. Anton Fritsch; Prague, 1872).

The present monograph presents us with descriptions and figures of seventy-two species of Crustacea obtained from eight localities and well-marked beds in the Cretaceous formation of Bohemia. These are divisible into Cirripedia (twenty-one species), Bivalved Entomostraca, Ostracoda (twenty-one species), Decapoda-Macroura (eighteen species), Decapoda-Brachyura (twelve species). The Cirripedia, with one exception, all belong to the stalked division (Lepadidæ), or "barnacles," eleven species being common to our own Chalk and Gault. In these are included two varieties of that most aberrant genus *Loricula*, first described by Sowerby from the English Chalk, and afterwards more fully by Charles Darwin. This pedunculated genus, by a retrograde development, no longer stands supported on its stalk, but lies prone, attached by one side to the surface of some shell, or other foreign body, its five rows of peduncular imbricating scales (over 100 in number) serving to form a dermal covering to the soft parts of the animal, which must have been distorted in its mode of growth somewhat as the flat-fishes (Pleuronectidæ) are modified as the result of their recumbent habits.

A *Balanus*, referred to a new genus (*Balanula*?), is supposed to represent a sessile form of Cirripede. Such a form, *Pyrgoma cretacea*, was described from the Upper Chalk of Norfolk by H. Woodward in 1868 (see *Geol. Mag.*, vol. v. p. 258, pl. xiv. figs. 1-3), but the "acorn-shells," Sessile Cirripedes, mostly belong to the Tertiary and Recent periods, in which they attain a large development all over the globe.

The Ostracoda have been determined by Herr Joseph Kafka, Dr. Fritsch's assistant, in the Museum at Prague. Of the twenty species here treated of, five have been previously figured and described as new by Herr Kafka in the *Sitzungsb. K. böhm. Gesell. Wiss.*, Prag, 1885. The figures and woodcuts of the old species have been mostly taken from Prof. von Reuss's memoir on the Microzoa in Geinitz's "Elbithalgebirge," and the new species are here also figured in woodcuts, some of which leave much to be desired as to "finish" of characteristic features. Figs. 24 and 25 appear to belong to *Macrocypris*, and not to *Bairdia*. Fig. 26 has no relation to *Bairdia*, but may be a *Cytherella*. The representation of *Cythere reticulata*, Kf. (Fig. 32, *a*, *b*, *c*), has some peculiarities which better figures perhaps would clear up. Though not mentioned by Herr Kafka, ten of the species are found also in the English Chalk, and the others (excepting Fig. 32) have near allies in that formation in Western Europe. It is stated that in Bohemia the Ostracoda are mostly found in the Senonian stage. Only *Cytheridea perforata*, and four other species, come from the Turonian beds of Weissenberg.

Turning to the higher forms of Crustacea, the Decapoda (crabs and lobsters), only a single species, *Enoploclytia leachii*, Mantell, is recognized as being identical with our Chalk Crustacean fauna; but the genera *Hoploparia*, *Callianassa*, *Palæocorystes*, *Necrocarcinus*, *Etyus*, and *Astacus* are represented by corresponding species in the two areas. *Callianassa* is said to be represented by six species. This is a burrowing form, of which only the great chelate appendages are usually found fossil, or are brought up in the dredge from deep water, and it is extremely doubtful, judging from the author's figures, whether more than about three out of six of Fritsch's species can be maintained. One Greensand species occurs in Ireland, and the well-known *Callianassa faujasii* described eighty-eight years ago from the Uppermost Chalk of Maestricht. We have also a Tertiary form described from the upper marine series, Hempstead, Isle of Wight. All these species are very nearly related to each other.

Perhaps one of the most interesting forms described by Dr. Fritsch is his *Stenocheles esocinus*, the long slender-toothed chelæ of which agree closely with those of *Astacus* (?) *zaleucus*, W. Schm., a Crustacean dredged up in 1000 fathoms during the *Challenger* Expedition near St. Thomas in the West Indies.

The present work is illustrated by ten chromolithographic plates and seventy-two text figures.

This series of fine memoirs, which is being issued by Dr. Fritsch from the Royal Bohemian Museum, Prague, will certainly maintain the merit, and serve to enhance the reputation, of that great institution, which has, quite recently, been so well endowed by the magnificent bequest of the late Dr. Joachim Barrande, the historian and palæontologist of the Silurian system of Bohemia.

OUR BOOK SHELF.

Manual of Mineralogy and Petrography, containing the Elements of the Science of Minerals and Rocks. By James D. Dana. Fourth Edition, Revised and Enlarged. Illustrated by numerous Woodcuts. (New York: Wiley and Sons; London: Trübner and Co., 1887.)

THAT a new edition of this important and admirable manual has been issued will be good news to all interested in mineralogy, and especially to the teacher and student. The book, which now consists of 517 pages, is well arranged throughout, and contains, as all such books should do, a full index. The whole body of mineralogical science is here brought to focus, and the present edition, in that part of it relating to the description of minerals, is brought down to the year 1886, many new species described during the past six years being included. The chapter on rocks has been re-written, re-arranged, and enlarged, and many illustrations are new. We would suggest to the learned author that in the next edition a chapter on meteorites and their mineralogy would form an appropriate and much-valued addition.

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.*]

"A Conspiracy of Silence."

THE article which I contributed to the September number of the *Nineteenth Century*, on the Coral Islands of the Pacific, has done what I intended it to do. It has called wide attention to the influence of mere authority in establishing erroneous theories and in retarding the progress of scientific truth. The vehement assault made upon it in the current number of the same review by Prof. Huxley, and the article by Prof. Bonney in this journal, are to me gratifying evidences of success. But both of these writers are entirely wrong in the interpretation they put on a few expressions in my paper. They interpret these expressions as conveying imputations on the probity and honour of scientific men in the habitual and wilful suppression or discouragement of what they know to be truth. But there is nothing to justify this interpretation. I have made no such accusation, and if any one else were to make it I should join the two indignant Professors in repudiating it. Scientific men are not only as good as other men in this way, but generally a great deal better. Prof. Huxley has been irritated by some "anonymous sermon," which I have not seen and for which I am not responsible. He admits that it is in this anonymous production that the "slanders" against scientific men have taken the peculiarly offensive form; but he maintains that this unknown writer has been "inspired" by my article on Coral Islands. On the strength of this assumption—which may be true for aught I know—he goes on through some seven pages to dissect certain parts of my paper, and to read into it a great deal that is due to his own excitement and to nothing else.

I have no difficulty in expressing clearly and without any circumlocution exactly what I do mean, and what I have intended to say. Prof. Bonney interprets it very fairly, in abstract, when he says that the moral of my paper is, "Beware of idolatry." Some theory, hypothesis, or doctrine, is propounded by a great man. It becomes established, partly perhaps by certain inherent elements of strength, or at all events of attractiveness. But soon it stands unassailable, and unassailed, upon the vast foundations of general acceptance and admitted authority. It becomes what Prof. Huxley on a celebrated occasion, and with at least a momentary insight, called "a creed." The effect of such a position is tremendous. Some men who see cause to doubt are daunted. They keep silence. Others are prevented from even thinking on the subject. A few who do think, and who do doubt, and who do venture to express their doubts, are discouraged and discountenanced. A great many others take refuge in a suspended judgment, even after the production of evidence which, in the absence of a "creed" and of authority, would have been deemed conclusive. In all this there may be, and in general there is, nothing worse than timidity on the part of those who are the laggards, or the opponents, in some great advance. It is more difficult for some men than for others to face a prevalent opinion or an accepted doctrine. It is all very well to say, as Prof. Bonney says, that "to the man of science truth is a pearl of great price, to buy which he is ready to part with everything previously obtained." But scientific men are human. They are, I admit, immensely superior to the politicians, especially just now. But they have their failings, and everyone who knows the history of science must be able to call to mind not one instance only, but many instances, in which the progress of knowledge has been delayed for long periods of time by the powerful and repressive influences of authority, exerted in one or other of many ways.

My contention is that Darwin's theory on the origin of the Coral Islands is a case in point. I believed in it or accepted it, for many years, as others did. Prof. Bonney admits that I have described it not only fairly, but as forcibly as if I were still its advocate. This is exactly what I tried to do. I now hold that it has been disproved, and has been replaced by another theory quite as grand, and more in harmony with other natural laws which are of universal operation, but have been only lately recognized. I affirm, farther, that this new theory or explanation has been received with the timidity, the discouragement, the discountenance, and the obstruction which are characteristic in such cases. That Dr. Geikie has supported it, is most creditable to him. But his voice is not enough to disprove the truth of my contention. That Prof. Huxley and Prof. Bonney should be unable to make up their minds upon such evidence as has been before us now for several years is, in my opinion, a strong confirmation of the law which is operating upon them. There are some discoveries in science—some explanations of curious phenomena—which are self-luminous. They shine with their own light. The moment they are suggested, with a few cardinal and certain facts to illustrate them, they are their own proof. Everything that turns up speaks in support of them. My conviction is that such is the character of Mr. Murray's theory of the coral island formations in the Pacific.

Prof. Huxley challenges me to re-affirm with better proof the fact I allege—that Mr. Murray has met with discouragement. I respond at once to that challenge. I have seen the letter from Sir Wyville Thomson in which that naturalist urged and almost insisted that Mr. Murray should withdraw the reading of his papers on the subject from the Royal Society of Edinburgh. This was in February 1877. No special reason was assigned, but the terms of the letter indicate clearly that Sir Wyville dreaded some injury to the scientific reputation of the body of naturalists of whom he was the chief, and for whom, as connected with the *Challenger* Expedition, he was in some degree responsible. He had not himself at that time, I believe, fully accepted the new doctrine. But that would have been no sufficient reason for discouraging free discussion, if it were indeed as free as it ought to be. In my article I understated the delay which was thus occasioned. Three years, not two, elapsed

before Mr. Murray was at perfect liberty to advocate his views in the proper place, before a scientific body.

But the challenge of Prof. Huxley has brought to my knowledge a new bit of circumstantial evidence to the same effect, which is highly significant. Among the investigators of the Pacific corals no man has done better work than Dr. Guppy, Surgeon of H.M.S. *Lark*. Since my article was written, his volumes on the Solomon Group of islands have been published. The geological volume is an admirable memoir. It is the record of observations as patient, detailed, and conscientious as have ever been made on the great geological problem which is at issue. After his return home he was advised by Mr. Murray to offer a paper on his researches to the Geological Society of London. He did so in the spring of 1885. But his paper was refused—much to Dr. Guppy's disappointment. It was not orthodox. His facts effectually removed some difficulties in the way of Mr. Murray's theory—facts which in more than a corresponding degree were adverse to the theory of Darwin. As a consequence the Royal Society of Edinburgh has had the honour of receiving and publishing Dr. Guppy's most interesting memoir. As a Scotchman I am proud of this contrast. I make no accusation of wilful unfairness against the authorities of the Geological Society of London, of which my critic Prof. Bonney was, I believe, at that time the President. They did not consciously discourage truth. On the contrary, they probably smelt heresy. But if their minds had been free from this prepossession—if they had been alive to the breadth and sweep of the questions at issue, and open to receive with welcome the crucial evidence bearing upon them which is contained in Dr. Guppy's paper—the rejection of it would have been impossible.

As regards Darwin's own state of mind upon the subject, I can only say that my information was as good as that in the possession of Prof. Huxley. I am not struck by the perfect candour of his reference to Darwin's letter to Prof. Semper in October 1879. If he had quoted the very next sentence to that which he does quote, a very different impression would have been left on the reader's mind. But I attach no importance to this point. I prefer to believe that Darwin's mind was open to conviction, and to hope that others will follow his example.

ARGYLL.

The Theories of the Origin of Coral Reefs and Islands.

I WAS pleased to see Prof. Bonney's article on the Duke of Argyll's strictures on scientific men ("A Conspiracy of Silence," NATURE, November 10, p. 25). It is to be hoped that the rhetoric and methods of Parliamentary debate will not become common in scientific controversy. The Duke is, however, not the first who has tried to show "that if Darwin had lived he would with his well-known candour have been the first to admit the truth of Murray's theory," &c., &c. This I submit is a species of rhetoric which is out of place in scientific discussion.

It so happens that shortly after the appearance of Mr. Murray's paper "On the Structure and Origin of Coral Reefs and Islands," in NATURE, August 12, 1880 (p. 351), I had occasion to write to Dr. Darwin, and in my letter the following passage occurs, which I only quote to make Darwin's answer intelligible :—

"*September* 21, 1880.

"I think the theory Mr. Murray sets forth—that the cones or peaks, on which he considers atolls have been formed, have been levelled up by pelagic deposits, and thus brought within the limits of reef-building coral growth—a very far-fetched idea."

To which Darwin with his usual acumen replies :—

"*Beckenham, September* 22, 1880.

"I am not a fair judge, but I agree with you exactly that Murray's view is far-fetched. It is astonishing that there should be rapid dissolution of carbonate of lime at great depths and near the surface, but not at intermediate depths where he places his mountain peaks.

"Dear Sir, yours faithfully,
"CH. DARWIN."

As so far there appears to have been no *written* expression of Darwin's views published, this quotation may be of value.

T. MELLARD READE.

Park Corner, Blundellsands, November 11.

Earthquake at the Bahamas.

I AM instructed by the Meteorological Council to inclose copies of reports from the Resident Justice and Light-keepers of Inagua, Bahamas, relating to an earthquake on September 23 last, which you may think worthy of a place in NATURE.

ROBERT H. SCOTT,
Secretary.

Meteorological Office,
116 Victoria Street, London, S.W.
November 11.

The Resident Justice at Inagua to the Colonial Secretary, Nassau.
In re Earthquake at Inagua.

Resident Justice's Office, Inagua,
September 27, 1887.

I HAVE to report that this island was visited by a severe shock of earthquake at 7 a.m. of the 23rd instant; the effect on the light tower, the keeper reports, was terrific, two nuts on the iron stauncheon of the smoke-stack were broken, and several cylinders. A portion of the stone wall around the Residency, and other private property were thrown down in Matthew Town.

At 8.10 p.m. another shock was felt, no damage at the township; at the light station the cylinder on the lamp was broken, and the keepers were compelled to extinguish the light to prevent conflagration. A new cylinder having been placed in position, the light was again lit in about six or eight minutes after the accident.

At midnight another shock was felt, and the light-keeper reported next morning several cracks in mortar inside of the light tower; the light continued good.

Since the 23rd instant several light shocks have been felt, which keeps the people in a state of alarm.

We have had no arrival from Hayti and neighbouring islands, and it is feared that some of them have greatly suffered.

(Signed) G. R. MCGREGOR,
Resident Justice.

The Hon. Robt. Butler, Acting Colonial Secretary.

Principal and Assistant Light-keepers, Inagua, to the Inspector of Lighthouses, Nassau.

Inagua Light Station,
September 29, 1887.

SIR,—I beg most respectfully to report for your information that this station and island was visited by several severe shocks of earthquakes on the 23rd, 24th, 25th, and 26th instants. The shock on the former date was felt at 7 a.m., which shook the tower and dwelling severely. Two nuts forming a part of fastening of iron rods in the upper part of lantern supporting upper barrel and smoke-pipe were wrenched off and smashed several cylinders.

The second shock, at 8.10 p.m., shook the tower very much, and smashed the cylinder on lamp. The light was then extinguished to prevent fire, which last about eight minutes [*sic*], when the light was again exhibited and kept burning bright and clear until daylight. There was another shock felt during the night, but not so severe. I noticed several cracks on the walls in the tower, which may be the mortar only. The latest shock was on the morning of the 26th at 1.3.

I am glad to say that the lamp and machinery are in good working order, but there will be slight repairs required.

The latest shock felt was at midnight of the 27th.

I also inclose the head of nut, the length of which is seven-eighths of an inch on inside.

I have, &c.,
(Signed) BYRON N. JONES,
Principal;

CORNELIUS S. E. LOTMAN,
Assistant.

The Inspector of Lighthouses.

RESEARCHES ON METEORITES.

I.

ON October 4 I communicated to the Royal Society a preliminary note embodying some results I had obtained in observations on meteorites, undertaken with a view of obtaining additional information on some parts of the spectrum of the sun.

Some years ago I commenced a research on the spectra of carbon in connection with certain lines I had detected in my photographs (1874) of the solar spectrum. I have been going on with this work at intervals ever since; and certain conclusions to which it leads, emphasizing the vast difference between the chemical constitution of the sun and of some stars, recently suggested the desirability of obtaining observations of the spectra of meteorites and of the metallic elements at as low a temperature as possible.

I have latterly, therefore, been engaged on the last-named inquiries. The work already done, read in conjunction with the work on carbon, seems to afford evidence which amounts to demonstration on several important points.

The researches are still very far from complete, and the results must be given with great reserve, as the astronomical observations with which I have had to compare my laboratory work have been frequently made under conditions of very great difficulty.

A full report on the work, so far as it has gone, made to the Solar Physics Committee, which I have also communicated to the Royal Society, was read to-day, and I have received permission to publish part of it in this week's NATURE.

The general conclusions at which I have so far arrived may be stated as follows :—

I. All self-luminous bodies in the celestial spaces are composed of meteorites, or masses of meteoritic vapour produced by heat brought about by condensation of meteor-swarms due to gravity.

II. The spectra of all bodies depend upon the heat of the meteorites, produced by collisions, and the average space between the meteorites in the swarm, or in the case of consolidated swarms upon the time which has elapsed since complete vaporization.

III. The temperature of the vapours produced by collisions in nebulæ, stars without C and F but with other bright lines, and in comets away from perihelion, is about that of the bunsen burner.

IV. The temperature of the vapours produced by collisions in α Orionis and similar stars is about that of the Bessemer flame.

V. The line of increase of temperatures of the swarms of meteorites, and of subsequent cooling of the mass of vapour produced, and the accompanying phenomena, may be provisionally stated as follows :—

SEQUENCES OF SPACING AND TEMPERATURES (PROVISIONAL).

From Cold to Hot = Sparse to Dense Swarms.

	Spectrum of interspace. H.	Spectrum of interspace. C.	Spectrum of vapour of meteorite. *Radiation.*	Spectrum of vapour of meteorite. *Absorption.*	Spectrum of meteorite. *Radiation.*
Nebulæ (without F)	Nil	Nil	Mg (500) ± 495	Nil?	Dimly continuous.
Comets 1866 and 1867	Nil	Nil	Mg (500)		
Nova Cygni after collision ...	Nil	Nil	Mg (500)... ...		
Stars with bright lines (without F)	Nil	Nil	Fe, Mn	Broad band 475	
Nebulæ (with F)	H	Nil	Mg (500) ± 495	Nil?	Continuous.
Stars with bright lines (with F).	H	Nil	Fe, Mn	D and *b* and other lines and bands	
Comets under mean conditions of collision	Nil	C	Mg(*b*)	Nil.	Continuous.
Comets at perihelion...	Nil	C	Meteorite lines.	(?)	Vividly continuous.
Stars, Class III.*a*	Nil	C		Meteorite flutings and lines	
Mixed swarms—					
R Geminorum a...	H	C	Meteorite lines.	Meteorite lines	
Nova Orionis at maximum.	H	C		Meteorite flutings and lines	

Condensation.

Stars, Classes I. and II.			Continuous ...	High-temperature lines of substances present in meteorites	The radiation from individual meteorites now gives place to radiation from the interior vaporous and subsequently consolidated mass of the condensed swarm.

Subsequent Cooling.

Stars	Class II. some stars, including sun ...		Continuous ...	K in excess	
	Class III.*b*			Flutings of carbon ...	

VI. The brilliancy of these aggregations, at each (increasing) temperature, depends on the number of meteorites in the swarm —*i.e.* the difference depends upon the quantity, not the intensity, of the light.

VII. The existing distinction between stars, comets, and nebulæ rests on no physical basis.

VIII. The main factor in the various spectra produced is the ratio of the interspaces between the meteorites to their incandescent surface.

IX. When the interspace is very great, the tenuity of the gases given off by collisions will be so great that no luminous spectrum will be produced ("nebulæ" and "stars" without F bright). When the interspace is less, the tenuity of the gas will be reduced, and the vapours occupying the interspaces will give us bright lines or flutings ("nebulæ" and "stars" with F bright). When the interspace is relatively small, and the temperature of the individual meteorites therefore higher, the preponderance of the bright lines or flutings in the spectrum of the interspaces will diminish, and the incandescent vapour surrounding each meteorite will indicate its presence by absorbing the continuous-spectrum-giving light of the meteorites themselves.

X. The brighter lines in spiral nebulæ, and in those in which a rotation has been set up, are in all probability due to streams of meteorites with irregular motions out of the main streams, in which the collisions would be almost *nil*. It has already been suggested by Prof. G. Darwin[1] —using the gaseous hypothesis—that in such nebulæ "the great mass of the gas is non-luminous, the luminosity being an evidence of condensation along lines of low velocity according to a well-known hydrodynamical law. From this point of view the visible nebula may be regarded as a luminous diagram of its own stream-lines."

XI. New stars, whether seen in connection with nebulæ or not, are produced by the clash of meteor-swarms, the bright lines seen being low-temperature lines of elements the spectra of which are most brilliant at a low stage of heat.

XII. Most of the variable stars which have been observed belong to those classes of bodies which I now suggest are uncondensed meteor-swarms, or stars in which a central more or less solid condensed mass exists. In some of those having regular periods the variation would seem to be partly due to

[1] NATURE, vol. xxxi. p. 25.

swarms of meteorites moving around a bright or dark body, the maximum light occurring at periastron.

XIII. The spectrum of hydrogen seen in the case of the nebulæ seems to be due to low electrical excitation, as happens with the spectrum of carbon in the case of comets. Sudden changes from one spectrum to the other are seen in the glow of meteorites in vacuum tubes when a current is passing, and the change from H to C can always be brought about by increased heating of the meteorite.

XIV. Meteorites are formed by the condensation of vapours thrown off by collisions. The small particles increase by fusion brought about again by collisions, and this increase may go on until the meteorites may be large enough to be smashed by collisions, when the heat of impact is not sufficient to produce volatilization of the whole mass.

XV. Beginning with meteorites of average composition, the extreme forms, iron and stony, would in time be produced as a result of collisions.

XVI. In recorded time there has been no such thing as a "world on fire" or the collision of masses of matter as large as the earth, to say nothing of masses of matter as large as the sun; but the known distribution of meteorites throughout space indicates that such collisions may form an integral part of the economy of Nature. The number of bodies, however, subject to such collision is small, and must, it would appear, form but a small percentage of the celestial bodies, seeing that they must be consolidated.

XVII. *Special solar applications.*

α. The solar spectrum can be very fairly reproduced (in some parts of the spectrum almost line for line) by taking a composite photograph of the arc spectrum of several stony meteorites, chosen at random, between iron meteoric poles.

β. The carbon which originally formed part of the swarm the condensation of which produced the sun has been dissociated by the high temperature brought about by that condensation.

γ. The indications of carbon which I discovered in 1874 (Proc. R.S., vol. xxvii. p. 308) will go on increasing in intensity slowly until a stage is reached when, owing to the reduction of temperature of the most effective absorbing layer, the chief absorption will be that of carbon—a stage in which we now find the stars of Class III. *b* of Vogel's classification.

δ. At the present time it seems probable that among the chief changes going on in the solar spectrum are the widening of K and the thinning of the hydrogen lines.

EXPERIMENTS UPON WHICH THE FOREGOING CONCLUSIONS DEPEND.

A. Experiments upon carbon.

The main conclusions which may be stated here are that there are two systems of flutings which depend upon temperature only.

At low temperatures all compounds of carbon give a set of simple flutings, the brightest of which are at wave-lengths 4510, 4830, 5185, and 5610. At higher temperatures there is a series of compound flutings, the brightest edges of which are at wave-lengths 4380, 4738, 5165, and 5640. In the case of compounds of carbon with hydrogen there is an additional fluting at wave-length 4310, and this is the only criterion for the presence of hydrocarbon among the flutings shown on the map. (See Map 3.)

B. Experiments upon the luminous phenomena of the various metals volatilized in the bunsen burner and the oxy-coal-gas blow-pipe flame as compared with the phenomena seen at higher temperatures.

The main conclusions are that certain lines, bands, and flutings are seen in the bunsen burner, that a larger number is seen in the flame, and that the total number seen in the burner and flame is small.

The order of visibility in the bunsen is, roughly—

Lines ...	Mg, Na, Li, Tl, Sr, Ba, Ca, K, Mn, Bi
Bands	Ca, Sr, Ba
Flutings	Mg, Mn

All the observations both of bunsen and oxyhydrogen flame may be condensed as follows:—

In metals of the alkalies	Na, K, Li
,, ,, alkaline earths ...	Ca, Sr, Ba
In magnesian metals	Mg, Zn, Cd
In iron metals	Fe, Ni, Co, Mn, Cr
In metals which yield acids	Bi, Ti, W
In copper metals	Cu, Tl
In noble metals	Ag, Hg
In earthy metals	Ce

The following table shows the positions of the principal lines, bands, and flutings seen in the spectrum of each of the metals examined, arranged roughly in the order of their intensities.

It should here be stated that as some of the researches have had to deal with feeble illumination small dispersion has been of necessity employed, and to make the observations along the several lines comparable a one-prism spectroscope has been so far used throughout. Hence the wave-lengths given are in all cases only approximate. With this proviso the lines observed have been as follows:—

Lines

In bunsen—

Mg	5183, 5172, 5167, 4586, 5201.
Na	5889, 5895.
Li	6705.
Tl	5349.
Sr	4607.
Ba	5534.
Ca	4226.
Mn	5395.
K	6950.
Bi	4722.

Seen on passing from the temperature of the bunsen to that of the oxy-coal-gas flame—

Fe	5268, 5327, 5371, 4383, 5790, 6024.
Cu	5105, 5781, 5700.
Cr	5202, 5203, 5207, 5410.
Zn	4810, 4911.
Cd	5085.
Ni	5476.
Ti	5128, 5338.
W	5490, 5511.
Ag	5208, 5464.
Hg	5460.
Ce	5273, 5160.

Bands

In bunsen—

Ca	5535, 6250, 6500, 6000.
Sr	6050.
Ba	5150, 5250, 5330, 4860.

Seen on passing from the temperature of the bunsen to that of the oxy-coal-gas flame—

Co	4710, 4920, 5170, 5460.

Flutings

In bunsen—

Mg	5000
Mn	5580, 5860, 6145, 5340

Seen on passing from the temperature of the bunsen to that of the oxy-coal-gas flame—

Ba	6010, 6350, 6480
Cr	5360, 5570, 5800, 6040
Fe	6150
Cu	6050, 6130
Zn	5460, 5680, 4985, 5140, 5340

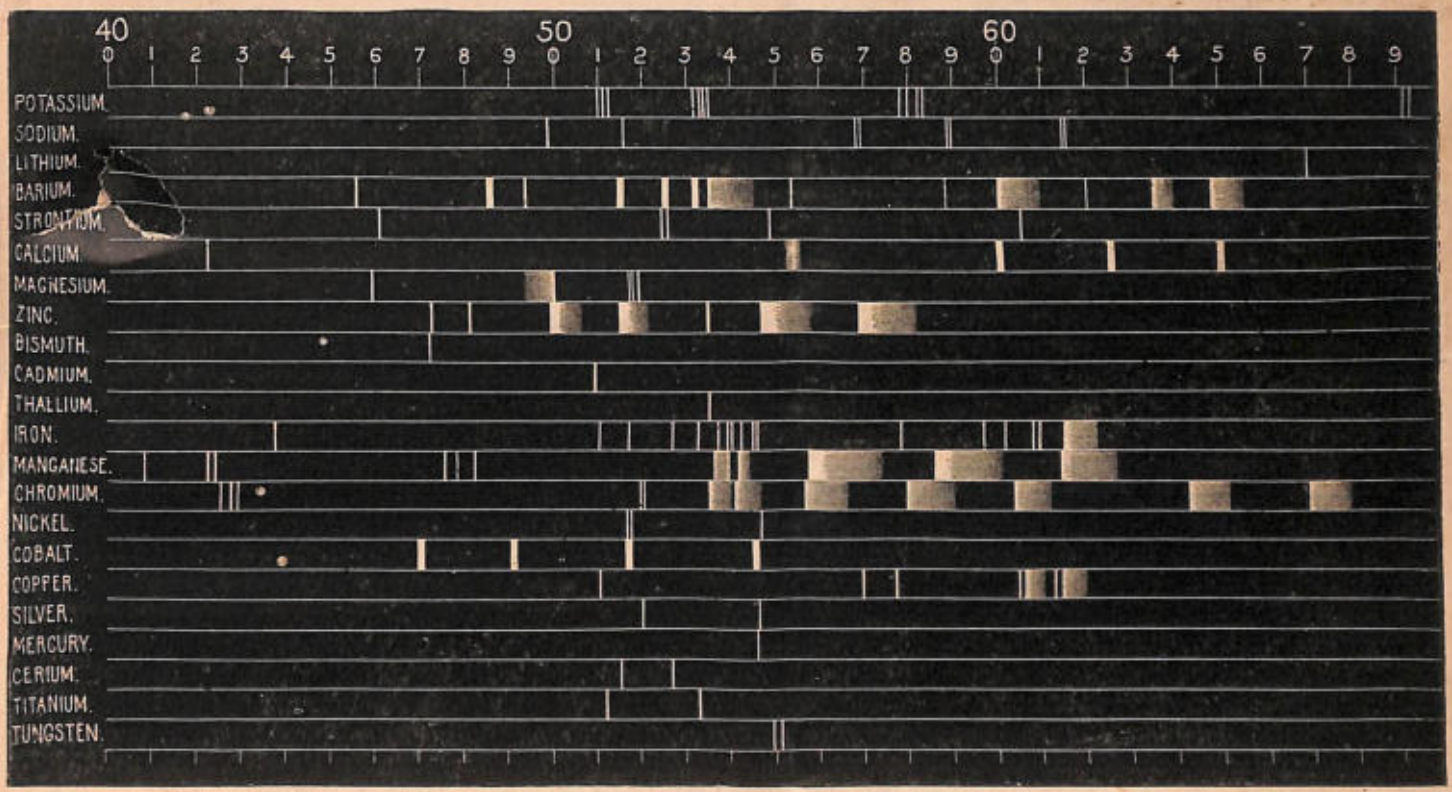
All the flutings, with the exception of magnesium, have their maxima towards the blue, and shade off towards the red end of the spectrum. (See Map 1.)

C. Experiments upon Mg at low temperatures.

I have again gone over the experiments already communicated to the Royal Society (Proceedings, vol. xxx. p. 27), and in addition have observed the spectrum of the metal burning in the centre of a large bunsen burner, in which case we get the line at 5201, and the fluting in the position of *b* without the fluting at 500. In the Bunsen as ordinarily employed the fluting at 500 far eclipses the other parts of the spectrum in brilliancy, and at this temperature, as already observed by Messrs. Liveing and Dewar (Proc. R.S. vol. xxxii. p. 202), the ultra-violet line visible is that at 373. Lecoq de Boisbaudran has observed the lines in the chloride at 4705 and 4483 ("Spectres Lumineux," p. 85).

D. Experiments upon the glow of Na and Mg in vacuum tubes.

A small piece of sodium, free from hydrocarbon, was placed in the lower limb of an end-on spectrum tube, and arrangements made for observing the spectrum of the gas evolved when the sodium was heated. Having first obtained as perfect a vacuum as possible, the sodium was gently heated, and the spectrum of the gas then gave nothing but the C and F lines of hydrogen. The pump being stopped and the sodium heated, a point was reached when C and F became very dim and were replaced by the structural spectrum of hydrogen.



Map 1.—Spectra of metals at the temperature of the oxy-coal-gas blowpipe.

In another experiment the sodium was replaced by a piece of magnesium along the end-on tube. The same process being gone through, similar phenomena were observed, but in the latter case there was a line at 500, in addition to the lines seen in the case of sodium.

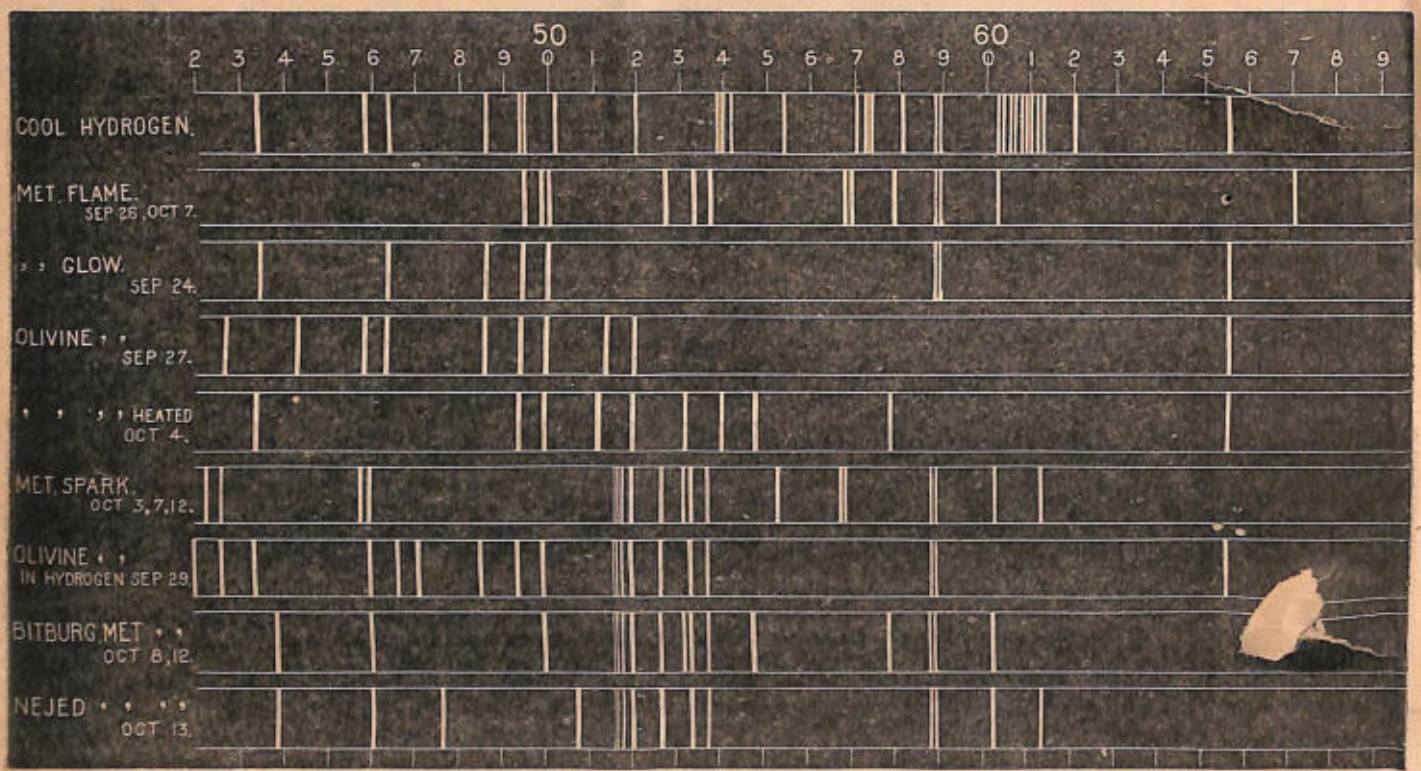
The important point, then, is the existence of a line at 500 in the spectrum when magnesium is heated, and the absence of such a line in the gas evolved from sodium under the conditions stated.

E. Experiments upon the conditions under which the C and F lines of hydrogen disappear from the spectrum.

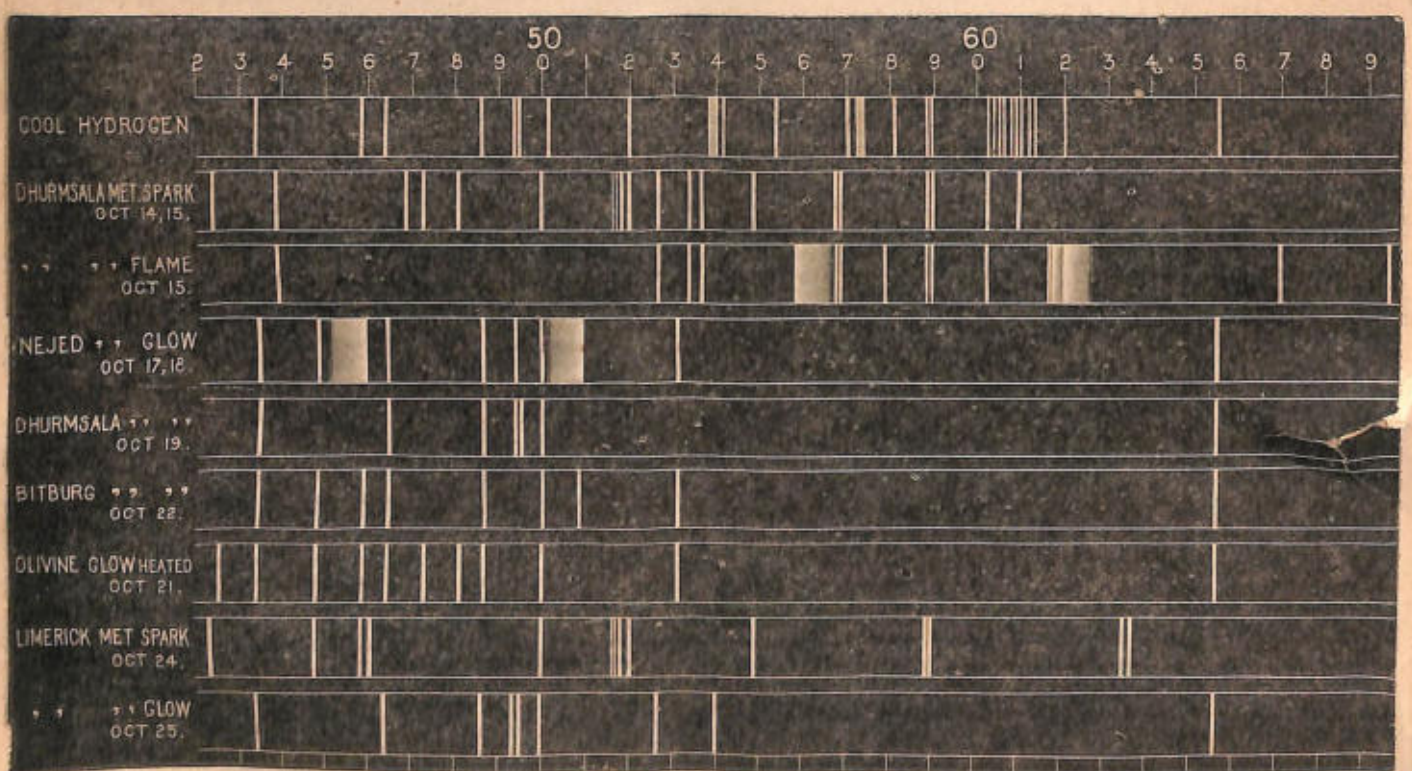
The association of the bright lines of hydrogen with nebulæ and many of the stars with bright lines and the so-called new stars points out at once that it is important to consider the various changes which hydrogen can undergo under various conditions of temperature and pressure. I pointed out many years ago that, when under certain conditions the spectrum of hydrogen is examined at the lowest possible temperature, the F line retains its brilliancy long after C disappears; and the fact that, after all the lines of hydrogen may be made to disappear from the spectral tube, the spectrum which remains visible, and is sometimes very brightly visible, is also due to hydrogen, has always been a matter of thorough belief in my mind, although so many observers, down even to M. Cornu not so very long ago, have been inclined to attribute it to the existence of "impurities."

I began to map the so-called structural spectrum at the College of Chemistry in 1869, but other matters supervened which prevented the accomplishment of this work. This, however, is a matter of small importance, because quite recently Dr. Hasselberg has communicated to the St. Petersburg Academy an admirable memoir on the subject, accompanied by a map (Mémoires de l'Académie Impériale, series vii. vol. xxx. No. 7, Hasselberg). The brightest portions of the structure spectrum are shown in Map 2.

The most convenient way of obtaining a supply of hydrogen for investigations of this kind is to use a little sodium which has never been in contact with hydrocarbon, or a piece of magnesium wire; to place them in the low end of a glass tube, one part of



MAP 2.—Spectra of olivine and meteorites under various conditions.



MAP 2A.—Spectra of olivine and meteorites under various conditions.

which can be used as an end-on tube, and then, after getting a vacuum so perfect that the spark will not pass, to slightly heat the metal. After a time the spectrum of hydrogen, sometimes accompanied by the low-temperature flutings of carbon, begins to be visible alike from the sodium and the magnesium.

If the vacuum has been very perfect to start with, the bright lines C and F will at first be visible without any trace of structure, and the hydrogen will be of a magnificent red colour. If now the action of the pump be stopped and the sodium be still more heated, a point will be reached at which the conductibility of the gas is at its maximum, and then, the jar not being in circuit, the structure-spectrum of the gas will be seen absolutely alone, without any trace of either C or F. The gradual disappearance of the F line is very striking, and when the bright line is out of the field the lines due to the structure seem to be enhanced in brilliancy.

The brightest part of the spectrum is then that near D; in the blue-green we have a line at 464 more refrangible than F, and then a double line at 4930 and 4935; other less refrangible lines are seen. These are phenomena seen associated with sodium, but if we use the hydrogen produced from a piece of magnesium wire or from a crystal of olivine, under the same circumstances we find that so far as the lines of hydrogen go the phenomenon remains the same, but that there is then visible in the spectrum a line at 500, which has been recorded in the spectrum of magnesium under other conditions, not only by myself but by Dr. Copeland.[1]

F. Experiments upon the spectra of meteorites at low temperatures.

The later observations recorded have been made on [illegible]d meteorites, fragments of which have been in the kin[illegible]anner placed at my disposal.

I. In the oxyhydrogen flame.

The observations gave in all only about ten or a dozen lines belonging to the metals magnesium, iron, sodium, lithium, and potassium, and two flutings, one of manganese, and one of iron.

II. With a quantity coil without jar.

The observations gave in all about twenty lines belonging to the metals magnesium, sodium, iron, strontium, barium, calcium, chromium, zinc, bismuth, and nickel, and four lines of unknown origin.

III. When heated in a vacuum tube when a current is passing along it.

A small piece of iron meteorite was inclosed in the middle of a horizontal tube, so that the spark might be made to pass through the tube and over the meteorite. After complete exhaustion has been obtained, the first spectrum observed when the tube, end on, is placed in front of the spectroscope, is a spectrum of hydrogen. The carbon flutings are only visible occasionally. If the meteorite then be very gently warmed by placing a Bunsen burner at some distance below the tube, the glow over the meteorite is seen to change its colour, and the line at 500 is constantly, and another line at 495, apparently exactly in the position of the second line of the spectrum of the nebulæ, is occasionally, seen. This line is more refrangible than the structure line of hydrogen in this region, which occupies the same position as the barium line. This, however, if the heating is continued, especially in the case of stony meteorites, is soon succeeded by a much more brilliant green glow, in which magnesium *b* and many other lines appear, now accompanied by the carbon flutings. The observations made under all the above conditions are shown in Maps 2 and 2A.

In these observations if a line in the meteorite spectrum were coincident with a metallic line, with the dispersion employed, in the absence of the brightest line of that metal, the line was regarded as originating from some other substance. Thus a line was sometimes seen at 5480, apparently coincident, with the dispersion employed, with the green lines of Sr and Ni; sometimes the brightest line of Sr at 4607 was absent, and it was then fair to assume that the presence of 5480 was due to Ni, but in the presence of 4607 it might be due to Sr.

[1] "To this table must be added 500·6 mmm. as the wave length of the first line in the great band of magnesium as determined by M. Lecoq de Boisbaudran from the spark-spectrum of the chloride of that metal, which evidently agrees with the flame-spectrum, in this region at least. It is worthy of note that this line almost absolutely coincides with the brightest line in the spectra of planetary nebulæ" (Dr. Copeland, *Copernicus*, vol. ii. p. 109).

COMPARISONS OF THE FOREGOING OBSERVATIONS AMONG THEMSELVES AND WITH THOSE MADE ON VARIOUS ORDERS OF CELESTIAL BODIES.

The discussions have taken, in the first instance, the form of comparisons of the different phenomena observed, and for this purpose all recorded observations of flutings and bright lines and dark lines in stars, comets, nebulæ, &c., have been carefully mapped in addition, all records having, when necessary, been brought to a common scale. Having these maps, I could then compare the totality of celestial observations with the laboratory work to which reference has already been made.

The following are among the comparisons already dealt with:—

I. The spectra of meteorites observed under the various conditions, chiefly considering magnesium, iron, and manganese, with the bright lines observed at low temperatures.

The main conclusions are:—

(1) That only the lowest temperature lines of Mg, Na, Fe, Cr, Mn, Sr, Ca, Ba, K, Zn, Bi, and Ni are seen in the meteorites under the various conditions. They are not all seen in one meteorite or under one particular condition; the details of individual observations are fully recorded in Maps 2 and 2A.

(2) That in the case of Mg the line most frequently seen is the remnant of the fluting at 500, while in a photograph the main ultra-violet line recorded is the one at 373, previously recorded under these conditions by Messrs. Liveing and Dewar. In the quantity spark other lines are seen, notably b_1, b_2, b_4, and 5201. The line at 500 was considerably brightened when the number of cells was reduced, thus showing it to be due to some molecule which can exist best at a low temperature.

(3) That in the case of Mn the only line visible at the temperature of the bunsen burner, 5395, is the only line seen in the meteorites.

(4) That the lines of iron seen in the meteorites are those which are brightest when wire gauze is burned in the flame. The chief of these are 5268, 4383, 5790, and 6024; it is possible, however, that the two latter are due to some substance, not iron, common to the gauze and the meteorites.

II. The spectra of meteorites generally, with the bright lines and flutings seen in luminous meteors, comets, and some "stars."

a. Luminous meteors.

With regard to the records of luminous meteors, it may be remarked that the observations, so far as they have gone, have given decided indications of magnesium, sodium, lithium, potassium, and of the carbon flutings seen in comets. The following quotations from Konkoly and Prof. Herschel are among the authorities which may be cited for the above statement.

"On August 12, 13, and 14, I observed a number of meteors with the spectroscope; amongst others, on the 12th, a yellow fireball with a fine train, which came directly from the Perseid radiant. In the head of this meteor the lines of lithium were clearly seen by the side of the sodium line. On August 13, at 10h. 46m. 10s., I observed in the north-east a magnificent fireball of emerald-green colour, as bright as Jupiter, with a very slow motion. The nucleus at the first moment only showed a very bright continuous spectrum with the sodium line; but a second after I perceived the magnesium line, and I think I am not mistaken in saying those of copper also. Besides that, the spectrum showed two very faint red lines."[1]

"A few of the green 'Leonid' streaks were noticed in November (1866) to be, to all appearances, monochromatic, or quite undispersed by vision through the refracting prisms; from which we may at least very probably infer (by later discoveries with the meteor-spectroscope) that the prominent green line of magnesium forms the principal constituent element of their greenish light."[2]

Again, later on in the same letter, Prof. Herschel mentions Konkoly's observation of the bright *b* line of magnesium in addition to the yellow sodium line in a meteor on July 26, 1873.

I again quote from Prof. Herschel:—

"On the morning of October 13 in the same year, Herr von Konkoly again observed with Browning's meteor-spectroscope the long-enduring streak of a large fireball, which was visible to

[1] Konkoly, *Observatory*, vol. iii. p. 157.
[2] Herschel, letter to NATURE, vol. xxiv. p. 597.

the north-east of O'Gyalla. It exhibited the yellow sodium line and the green line of magnesium very finely, besides other spectral lines in the red and green. Examining these latter lines closely with a star-spectroscope attached to an equatorial telescope, Herr von Konkoly succeeded in identifying them by direct comparison with the lines in an electric Geissler-tube of marsh-gas. They were visible in the star-spectroscope for eleven minutes, after which the sodium and magnesium lines still continued to be very brightly observable through the meteor-spectroscope."[1]

The green line "*b*" of magnesium occurring as a bright line in luminous meteors indicates that their temperature when passing through our atmosphere is higher than that of the bunsen, and we may add of comets as generally observed, although some exhibit the *b* lines of magnesium and those of iron when at perihelion, as shown later on.

The two lines which Konkoly supposes are probably due to copper will, I expect, be found to be iron lines when other observations are made of the spectra of meteors.

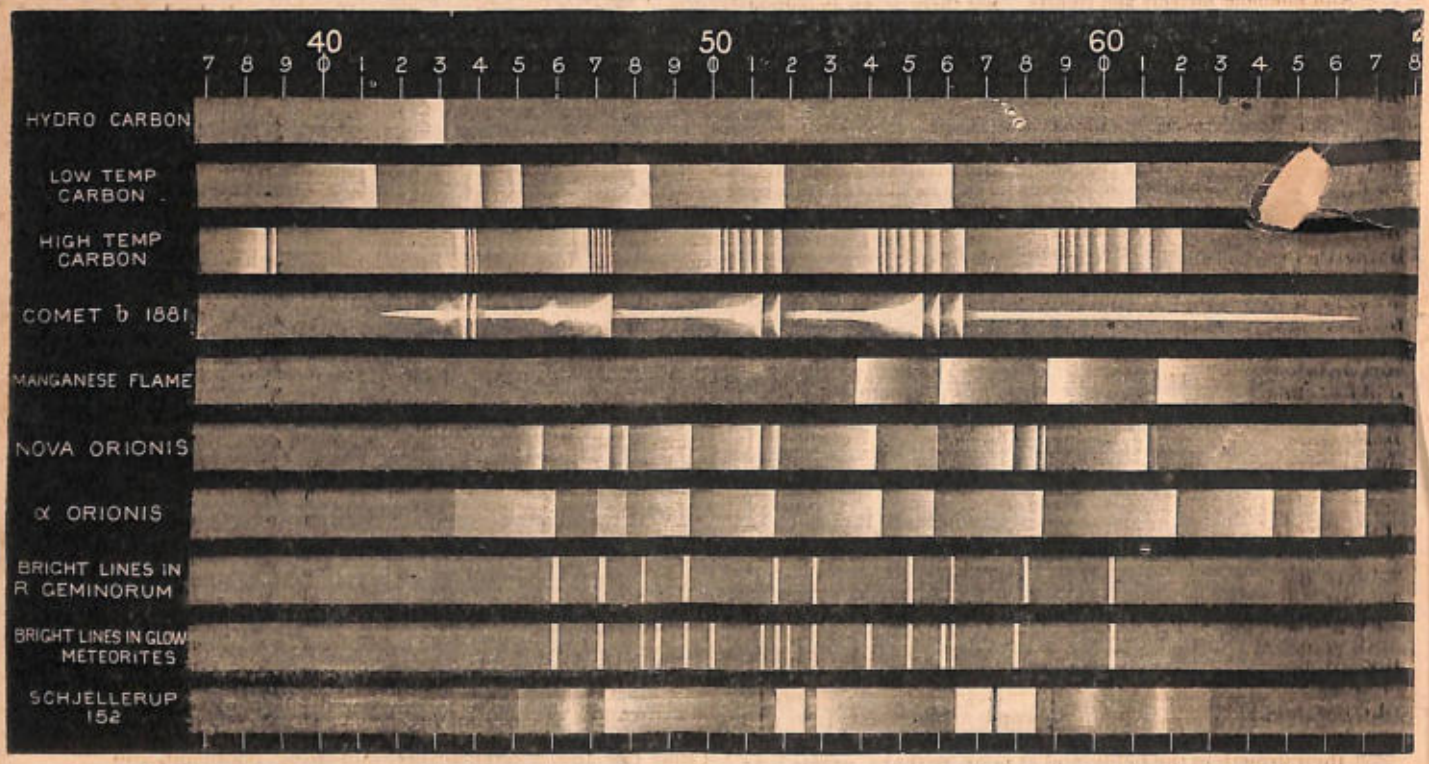
The main conclusions from this comparison are then: (1) that the temperature of luminous meteors is higher than that of the bunsen flame; (2) that the meteorites which produce the phenomena we are now discussing are hotter than those in the experimental glow taken generally; and (3) that in both cases flutings of carbon may be seen.

B. Comets.

When the meteorites are *strongly* heated in a glow-tube, the whole tube when the electric current is passing gives us the spectrum of carbon.

When a meteor-swarm approaches the sun, the whole region of space occupied by the meteorites, estimated by Prof. Newton in the case of Biela's comet to have been thirty miles apart, gives us the same spectrum, and further it is given by at all events part of the tail, which in the comet of 1680 was calculated to be 60,000,000 miles in length. The illumination therefore must be electrical, and possibly connected with the electric repulsion of the vapours away from the sun; hence it is not dependent wholly upon collisions.

Passing now from the flutings seen in cometary spectra, it is found that most of the lines which have been observed at perihelion are coincident with lines seen in experiments with meteorites,



MAP 3.—Comparison of flutings seen in the spectra of "stars" and comets with flutings of carbon, manganese, and zinc; and, in the case of R Geminorum, lines with remnants of flutings and lines seen in a meteorite glow. (The Zn fluting is at λ 544 in α Orionis.)

while the low temperature lines of Mg are absent. In the great comet of 1882, to which particular attention has been given on account of the complete record of its spectrum by Copeland,[2] the lines recorded were the D lines of sodium, the low-temperature iron lines at 5268, 5327, 5371, 5790, and 6024, the lines seen in the manganese spectrum at the temperature of the bunsen burner at 5395 and 5425, and a line near *b* which might be due to magnesium, or to a remnant of the carbon fluting. In addition to these there was a line at 5475, probably due to nickel, the absence of the blue strontium line indicating that it is not likely to be the green line of strontium. There were also four other lines less refrangible than D, the origin of which has not yet been determined. As the comet got further from perihelion the lines gradually died out, those which remained longest being the iron line at 5268 and the line near *b*. The absence of D before the disappearance of all the lines is probably to be accounted for partly by the greater brightness of the continuous spectrum in that region.

In the comets of 1866-67, when seen away from the sun, the only line seen was the one at 500.[1]

It is fair to myself to say that I was not aware of these observations when I began to write this paper. The fact of the line at 500 remaining alone in Nova Cygni made it clear that if my views were correct, the same thing should happen with comets. It now turns out that the crucial observation which I intended to make was made twenty years ago.

[1] Herschel, letter to NATURE, vol. xxiv. p. 507.
[2] *Copernicus*, vol. ii. p. 234.

[1] "In January 1866 I communicated to the Royal Society the result of an examination of a small comet visible in the beginning of that year (Proc. R.S. vol. xv. p. 5). I examined the spectrum of another small and faint comet in May 1867. The spectra of these objects, as far as their feeble light permitted them to be observed, appeared to be very similar. In the case of each of these comets the spectrum of the minute nucleus appeared to consist of a bright line between *b* and F, about the position of the double line of the spectrum of nitrogen, while the nebulosity surrounding the nucleus and forming the coma gave a spectrum which was apparently continuous" (Huggins, Proc. R.S. vol. xvi. p. 387).

In Comets *b*, 1881, and *c*, 1882, the only lines recorded were magnesium *b*; but, as before, the apparent absence of other lines might be due to continuous spectrum.

Of the five bands shown in Huggins's photograph of the spectrum of Comet Wells, taken with a wide slit, no less than three agree fairly in position with three lines seen in the spectra of meteorites. The wave-lengths of these are 4253, 4412, and 4769, and it is interesting to note that, so far, the origin of these lines is undetermined. The two remaining bands are at wave-lengths 4507 and 4634.

It is seen, then, that the spectra of comets—when their internal motions are relatively either slow or fast, and when therefore the number of collisions, and with them the heat of the stones in collision, will vary extremely—resemble the spectra of meteorites seen in glow tubes.

(7) "Stars" with flutings which have been observed in the laboratory and in luminous meteors and comets.

The most prominent bright flutings of carbon are not only observed in luminous meteors and comets, but in stars of Class III.*a*, and in some "Novas," notably Nova Orionis. So far, then, these bodies may in a certain measure be classed with luminous meteors and comets. But there is an important difference in the phenomena, for we have absorption as well as radiation. The discussion shows that the dark (or absorbing) flutings in these bodies are partly due to the absorption of light by the most prominent flutings of Mn and Zn, seen at low temperatures.[3] This inquiry is being continued.

We have, then, in these bodies a spectrum integrating the *radiation* of carbon and the *absorption* of Mn and Zn vapour.

[illegible] law of parsimony compels us to ascribe the bright fluting [illegible]ces in these stars to the same cause as that at work in comet [illegible]ere we know it is produced by the vapours between the individual meteorites or repelled from them.

Hence we are led to conclude that the absorption phenomena are produced by the incandescent vapour surrounding the individual meteorites which have been rendered intensely hot by collisions.

These stars, therefore, are not masses of vapour like our sun, but clouds of incandescent stones.

We have here probably the first stage of meteoritic condensation.

J. NORMAN LOCKYER.

(*To be continued.*)

FAIRY-RINGS.

THE rains have come, and we have heard from all sides of the prolific crops of mushrooms and toadstools—paddock-stools, as they are termed in some northern districts—which have been springing up in the meadows and woods of England, Wales, and Scotland. Not only is surprise evinced at the marvellously rapid up-growth of these fungi, for the popular mind may well be amazed at that until a knowledge of the biology of these plants is more universal, but country people and dwellers in towns alike exclaim at certain other phenomena associated with their growth in the fields, and at none, perhaps, so much as what have been known from of old as "Fairy-rings" in these and, *Hexenringe* and *Cercles de sorcières* on the Con- points whose fairy-rings, like very many other poetical There have of late years undergone the process of how the explained away to an extent which, although it in no way removes the beauty from them, demands from us an admiration of a more stimulating and healthful character than the old awe which they inspired was capable of producing.

Disbelief prevails regarding Prospero and the beings that

> "By moonshine do the green sour ringlets make,
> Whereof the ewe not bites;"

and

> "whose pastime
> Is to make midnight mushrooms."

Fairy-rings are more or less regular and complete rings of grass, sharply distinguished from the ordinary grass surrounding them by means of their darker hue, more luxuriant growth, and other characters; in spring or autumn they are to be found with vigorous growths of mushrooms or toadstools springing from their outer margins, and the centre of the ring is often marked by a very poor crop of withered-looking herbage.

Before proceeding to give an account of the modern explanation of these remarkable objects, a few statements may be made as to their sizes, structure, and occurrence.

They are not always complete or regular rings, but may be parts of circles or ovals, or mere wavy strips. Nor are they always provided with the outer belt of fungi, though the rule is that a good season sees them so accompanied; if not, they do not remain long. In the typical cases, where the ring is annually provided with its fringe of fungi, it may go on increasing in size for years: records exist of rings which have been known to go on flourishing for forty or sixty years, and large rings on a hill-side could be seen from a considerable distance.

As to their sizes, they are known to commence as very small patches, but specimens have been measured as much as 60 feet and more in diameter. Indeed one observer refers to a fairy-ring which was nearly 100 feet across. While regarding these cases as rare extremes, it is well known that rings 12–20 feet in diameter have often been recorded, and, as we shall see, these must be several or many years old.

Although fairy-rings are usually noticed in meadows and on pasture lands, they are found on hills as well as in valleys, on dry soil as well as on wet, in woods and on heaths, and even in rocky places and situations near the sea. Perhaps the only generalization possible in this connexion is that they do not occur on highly-cultivated rich land.

On regarding carefully a typical fairy-ring, it may be found to present the following characters:—The central area, encompassed by the dark-green ring, consists of poor or even withered herbage—it may be of inferior grasses alone, or of these mixed with other plants. Then comes the band of luxuriant grass forming the ring proper: the grass composing this may be of more than one kind—*e.g. Lolium perenne* (the perennial rye-grass), *Dactylis glomerata* (the cock's-foot grass), and *Bromus mollis* (the soft brome) are common.

These grasses are rank, tall, and of a distinctly darker, bluer green hue than the rest; it is their coarseness, height, and especially the deeper colour, which render them so prominent. Fringing this ring, at the proper season, are found the spore-bearing heads of the Agarics *i.e.* the mushrooms or toadstools as the case may be; and if the observer digs carefully below the soil, he will find that these Agarics spring from a felted mass of root-like threads, the mycelium of the fungus. Then, outside all, comes the general herbage of the pasture, or whatever it may be: this is often scanty, indicating poor soil, and in any case is less luxuriant and lighter in colour than the rank herbage of the ring itself.

As with the herbage composing the rings, so the Agarics fringing them may be of different kinds. In the autumn the fairy-rings of this country and on the Continent commonly contain *Marasmius oreades*, Fr., a small pale mushroom with cream-coloured gills, and much esteemed as an esculent. It has a somewhat strong aromatic odour, and its mycelium is attached to the roots of the grasses among which it grows. It must not be confounded with certain acrid species allied to it.

The common mushroom (*Agaricus campestris*, L.) is also frequently found in large circles, fringing more or less complete fairy-rings. Among other forms may be mentioned the gray *Agaricus terreus*, Schoeff, not uncommon in beech- and fir-woods; the "parasol mushroom" (*Agaricus procerus*, Scop.), also not uncommon in fir-woods and pastures, and spoken of as one of the best of the esculent forms; also *Agaricus personatus*, Fr., with a lilac or purple stem. This is a late form,

good to eat, and called the "Blewit." *Agaricus subpulverulentus*, Pers., is also not uncommon, and several others are known.

In the spring, fairy-rings have been found containing *Agaricus gambosus*, Fr., an edible mushroom known in England as the "St. George's mushroom," and much esteemed on the Continent.

There are also other forms, several of them poisonous, or at least inedible or dangerous; and even puff-balls are known to be associated with fairy-rings.

And now we come to the question, How do these fairy-rings arise and increase? It cannot be wondered at that the people of earlier days, wishing to explain a phenomenon which none could overlook, sought for satisfaction in their myths and folk-lore, and believed them to be "caused" by fairies and elves and other mystic beings of the woods and fields, dancing in circles beneath the moonlight, and enchanting the ground into a richness which it did not previously possess.

Then came the era of science, and people were dissatisfied with beliefs, and in course of time the followers of De Candolle at least tried to solve the problem according to what was known of Nature. It was at least necessary to explain (1) why the centre of the ring is so poor, (2) why the fungi are confined to the margin, and (3) why the ring goes on enlarging, as continued observation showed that it did.

The first theory of any merit was, that the "ring" takes its origin from a single mushroom, which sheds its spores from the gills down on to the ground around the thick stem: this necessarily produces a ring of spores, as the stem dies down in the centre. Now the physiologists of those days believed that a plant excretes into the soil at its base substances which are harmful to its further development, and so, they argued, the soil on the inside of the ring of spores is poisoned, as it were, and only the outer spores produce new plants. The new mushrooms come up in a ring, and in their turn shed spores in a ring of rings; but since the soil on the inside of all these rings is poisoned by the excreta, only the outer series can germinate and grow, and thus a new ring arises next season, and so on. But, it was thought, though the excreta are injurious to the growth of the same plant (the fungus in this case) in that particular soil, other plants can grow there (in the present instance, grasses), and so a ring of rank grass follows on, which in its turn spoils the soil for its own kind as it increases.

Now it has to be admitted that there was much ingenuity in this hypothesis, and it was maintained for some time; until, in fact, physiologists had to give up the excretion theory as not in accordance with observed facts.

Then followed the beginnings of the celebrated doctrine of the rotation of crops, and the facts accumulated about fairy-rings had to be looked at again. They had become too much for the excretion theory; how did they look when regarded from the new point of view? First, however, we may bear in mind the fact noticed by several observers. When two fairy-rings gradually extend so as to interfere, the green circles coalesce and form a single ring: evidently the conditions of the soil in the wake of the advancing ring are such that the grass of another advancing ring cannot go on luxuriating there. It is true this fact was as easily made use of by those who maintained the excretion theory as by those who advanced the theory we are now going to examine.

It gradually came to be recognized that the reason one species of plant cannot be continually grown on the same soil was not because the first crop poisons the soil by leaving injurious excreta behind it, but because it *takes away* certain mineral substances in such proportions that too little is left for the well-being of a second crop of the same species; in other words, it *exhausts* the soil of certain necessary ingredients. A crop of some other species may be raised on the partially exhausted soil, however, provided it is a plant which does not need the materials now deficient, in such large quantities as its predecessor. This is, roughly sketched, the *rationale* of the doctrine of the rotation of crops, and it was subsequently suggested that the "fairy-rings" we are considering are a natural illustration of this. The vegetable physiologists then came to the conclusion that the fungus causes the fairy-ring by exhausting the soil of certain substances which are necessary to its existence, and is only able to produce continued crops by extending centrifugally into soil which still yields these substances: the grass, however, does not need these substances in such large proportions, and so follows the fungus. But, as we have seen, the grass which immediately follows the fungus is particularly rank and luxuriant, and it was necessary to find an explanation for this fact. It was then suggested that the dying mycelium of the fungus acts as a manure for the grass to feed upon, and until this is exhausted the growth is peculiarly rich and rampant.

Before leaving this part of our subject, it should be pointed out that Dr. Wollaston, in an essay on fairy-rings published in the Philosophical Transactions of the Royal Society so long ago as 1809, ventured on the explanation that the fungi spread in rings, because the soil was, by their mycelium, progressively "exhausted of some peculiar pabulum necessary for their production. . . . An appearance of luxuriance of the grass would follow as a natural consequence, as the soil in the interior of a circle would always be enriched by the decayed roots of fungi of the preceding year's growth."

Meanwhile, the physiology of plants was passing into a more scientific phase of existence, and the beginnings of modern agricultural chemistry were made; and in 1846 an important contribution to our knowledge of fairy-rings was afforded by Way, who chemically analyzed the soil, the herbage, and the fungi of some of these curious formations. This chemist found that the fungi of his fairy-rings were remarkably rich in phosphoric acid and in potash; and that they also contained relatively large quantities of nitrogen. We know now that this is true of fungi generally, but these facts were by no means so well understood at that time. Way also analyzed the grasses composing the ring, and found that they also contained a larger proportion of phosphoric acid and potash than the herbage in the neighbourhood, but by no means so much as the fungi: the grass also contained considerable quantities of nitrogen.

The net result of these investigations was to explain fairy-rings as an illustration of the rotation of crops, but of course putting the explanation on much firmer grounds. Way also pointed out that as the rank green grass was cut or otherwise removed, valuable ingredients (phosphorus, potassium, alkalies, &c.), were removed with it, and so the crops of grass further inwards become [illegible] and poorer, accounting for the bare patches of [illegible] inside the dark ring.

Messrs. Lawes and Gilbert, whose magnificent [illegible]ntinuous [illegible]ments on the vegetation of agriculture will never be forgotten, supported the above view of the matter, and showed that the dark green colour of the rank grass is due to the relatively large quantities of nitrogen. It was at this time (about 1850) customary to suppose that plants obtained their nitrogen from the atmosphere, a view now known to be erroneous from the brilliant researches of Boussingault, and of Lawes and Gilbert themselves. On this supposition the extraordinary accumulation of nitrogen (in the fungus and rank grass) was thought probably due to a power on the part of the fungus of taking nitrogen from the air. Subsequently the whole matter was again taken in hand by Messrs. Lawes and Gilbert, and the results published in the Journal of the Chemical Society, 1883.

The chief additional facts may be summarized as follows:—The fungi remove large quantities of carbon, nitrogen, and especially phosphoric acid and potash, from the soil. The soil inside the ring contains less nitrogen than that under the ring, and this again less than the soil outside the ring; a gradual exhaustion of nitrogen, then, is taking place as the fungus and rank grass extend the ring centrifugally, and this is promoted by the removal of the grass.

These observers also demonstrated the spread of the mycelium: it is in greatest abundance just below the outer edge of the ring. They conclude that the fungus has powers of obtaining nitrogen from compounds in the soil which are not available to the roots of the green herbage, but after the decay of the fungus mycelium the grasses can avail themselves of part of the nitrogen. The grasses—being plants containing chlorophyll—of course obtain their carbon from the carbon dioxide of the atmosphere; but the fungus—equally of course, in the light of physiology—obtains its carbon from some organic substances in the soil. The accumulation of phosphoric acid and potash has already been accounted for.

We may now sum up, then, the rational explanation of these curious fairy-rings as follows.

A mushroom spore may be supposed to start its growth in or beneath the dung of cattle, or a bird, on poor soil; the first crop of mushrooms, produced from the mycelium to which the spore gave rise, exhausts the soil of available carbon, nitrogen, phosphorus, potash, and other substances, storing all it can get in its own substance. The mycelium extends centrifugally "into fresh fields and pastures new," and the next crop of mushrooms arises at a distance from the centre; and so the growth proceeds. The grasses, among the roots of which this extension is going on, now avail themselves of the rich manure afforded by the decomposition of the older mycelium, and a struggle for existence is set up which results in the victory of the coarsest and rankest-growing species. These in their turn exhaust the available supply, and if cut it is removed in their substance: no wonder, then, that the inner parts of the area are poor, and support little or no herbage.

Messrs. Lawes and Gilbert's researches also showed that if the growth of the herbage is promoted by means of manures containing much available nitrogen the fungi are found to suffer, and the "fairy-ring" may be brought to an end. Again, unfavourable seasons of drought may cause the death of the mycelium, and rings which have flourished for years be thus destroyed.

We have attempted in this article to give a complete explanation of the rise and progress of "fairy-rings," as afforded by modern science. That much is clear which was previously obscure will have to be conceded; but are all the facts covered by the explanation? There are some inquiring spirits who are never satisfied with an explanation, and we run the risk of being classed among these malcontents, but there are one or two curious little points which still obtrude themselves upon our attention.

There is, in the first place, some difficulty in realizing how the fungi manage to obtain their large supplies of carbon and nitrogen and other elements from poor shallow soil, in the absence of larger quantities of organic matter than may occur: there is, in fact, considerable difficulty about the whole question of the nutrition of the fungus. A second point is that we find the ultimate filaments into which the mycelium of the fungus breaks up becoming lost among the roots of the grasses; and if the latter are carefully washed and examined with the microscope, their fibrils and root-hairs can be seen to be infolded by delicate hyphæ, and in some cases the root-hairs are pierced by them. We do not know that this has been demonstrated before, but we find it the rule with *Marasmius*, and have already succeeded in detecting something of the kind in other forms.

Now this looks very like parasitism; and we are tempted to pause before accepting the last explanation of fairy-rings as conclusive, or covering all the facts. It may be, in fact, that the hyphæ of the fungus stimulate the roots of the grasses to increased activity: this would account for the rampant growth and the result of the struggle for existence. Subsequently the hyphæ kill the grass-roots—or at any rate those of some species—which accounts for the bare patches in some rings. It also easily explains the sources of the carbon and nitrogen, if the hyphæ absorb nutritive materials from the hard-working grass-roots. This being the case, fairy-rings become still more interesting, since they afford an illustration of symbiosis of a peculiar kind, at any rate during part of the time that the grass and the fungus are in contact; and it seems not improbable that the theory of the formation of fairy-rings will have to be modified somewhat as follows.

A fungus-spore starts its mycelium among the roots of the grasses, and the hyphæ obtain a hold on some root-hairs and fibrils; the mycelium thus parasitic on the roots reacts in a stimulating manner on the latter, and we have a symbiotic relationship established between the fungus and the host. The consequence is that both flourish, and become rampant. It may be that only some grasses are thus stimulated, or even attacked, and this will affect their struggle for existence, and result in the selection of a few coarse forms. In time the hyphæ or the roots get the upper hand, and this is expressed in the survival of the grass, or its decay; in some cases it is clear that hyphæ are living at the expense of dead and dying roots.

However, until the results of investigations at present going on are set forth more at length, it is impossible to say which of the above explanations is the true one; in any case, the attachment of fungus hyphæ to the living grass-roots needs explanation, and it must also be allowed that at present we have no satisfactory theory to account for the nutrition of these rampant mycelia. But this is not the place to do more than point out how interesting the subject is, and how promising a field for further research it offers.

NOTES.

Mr. W. Bateson, Fellow of St. John's College, Cambridge, who has just returned from a zoological expedition to Central Asia, and is well known for his researches on *Balanoglossus*, has been awarded the Balfour Memorial Studentship in Animal Morphology.

The second meeting of the newly-formed Anatomical Society of Great Britain and Ireland will be held on Tuesday, November 22, at University College, Gower Street, at 5 p.m. The following papers will be read:—Prof. Sir William Turner, F.R.S., (1) "Variations in the Hippocampus Major and Eminentia Collateralis," by Robert Howden, and (2) "A Metallic Body in the Spinal Canal," by David Hepburn; (3) "Minute Anatomy of Clarke's Column in Spinal Cord of Man, the Monkey, and the Dog," by Dr. Mott; (4) "The Arteries at the Base of the Brain," by Prof. Bertram C. A. Windle; (5) "Note on the Functions of the Sinuses of Valsalva and Auricular Appendices, with some Remarks on the Mechanism of the Heart and Pulse," by Mayo Collier. A number of interesting exhibits are also announced.

On Tuesday evening the second part of an important paper upon the causes of accidents in mines and the development of measures and applications for combating or avoiding them was read by Sir Frederick Abel at the Institution of Civil Engineers. The first part of the paper was read in May last, at the close of the session. Sir Frederick's ideas will be discussed at the meeting of the Institution next Tuesday.

WE are glad to hear that the Scottish University Extension Scheme is likely to prove successful. A brilliant start has been made in Perth, where Dr. H. R. Mill is giving a course of lectures on physiography to a class of over 240 students.

SOME time ago it was arranged that three lectures on "Heredity and Nurture" should be delivered at the South Kensington Museum, on behalf of the Anthropological Institute, by Mr. Francis Galton, President of the Institute. We are requested to state that these lectures have been po-tponed in consequence of Mr. Galton's indisposition.

PROF. J. MCK. CATTELL'S paper on "The Psychological Laboratory at Leipzig," to be read before the Aristotelian Society on the 21st, will contain an account of the aim of experimental psychology, of the Leipzig Laboratory, and of the researches which have been carried on and are being carried on in it. The paper will be published in the January number of *Mind*.

THE borings in the Delta of the Nile carried on by the Royal Society have been brought to a standstill by the breaking of the pipe. The depth reached is over 324 feet, still without the solid rock being found. It is possible that the work may be recommenced upon a larger scale.

SEVERAL years ago three Russian "lady doctors" started at Tashkend a consulting hospital for Mussulman women. From the beginning the experiment proved a success, and the popularity of the hospital has been increasing ever since. During the last twelve months no fewer than 15,000 consultations have been given.

THE Russian Consul at Kashgar writes to the Russian Geographical Society that his endeavours to obtain from the Chinese authorities permission to erect a memorial to Adolf Schlagintweit on the very spot where Schlagintweit was killed have not been successful. The memorial will be erected in the Russian cemetery, where it will be at least protected from injuries.

THE money necessary for sending out M. Wilkitski to make pendulum observations in Novaya Zemlya has been granted by the Russian Geographical Society. He will be accompanied by a naturalist, M. A. Grigorieff.

THE Moscow Society of Naturalists invites those scientific bodies which would like to receive, in return for their own publications, the series of the Moscow *Bulletin*, to communicate with the Secretary of the Society.

REPORTS from Bergen, in Norway, seem to indicate that another great rush of herring under the west coast may be expected this winter, similar to those which have taken place periodically during the last two centuries through some unknown cause. The two greatest rushes on record were those of 1740 and 1807.

DURING a hailstorm at Mors, in Denmark, a few days ago, a flash of forked lightning—the only one occurring—struck a farm, and, having demolished the chimney-stack and made a wreck of the loft, descended into the living-rooms on the ground-floor below. Here its career appears to have been most extraordinary; all the plaster around doors and windows having been torn down, and the bed-curtains in the bed-rooms rent to pieces. An old Dutch clock was smashed into atoms, but a canary and cage hanging a few inches from it were quite uninjured. The lightning also broke sixty windows and all the mirrors in the house. On leaving the rooms it passed clean through the door into the yard, where it killed a cat, two fowls, and a pig, and then buried itself in the earth. In one of the rooms were two women, both of whom were struck to the ground, but neither was injured.

THE last two numbers of the *Folk-Lore Journal* (vol. v. Parts 3 and 4) exhibit very varied fare, and show how this interesting Society is gradually embracing the whole world. Side by side with Miss Courtney's Cornish folk-lore, we have Mr. Mitchell-Linne's birth, marriage, and death rites of the Chinese, followed by the indefatigable Mrs. Murray-Aynsley's account of secular and religious dances in Asia and Africa, which extends over both numbers, and in Part 3 is succeeded by Mr. Clouston's two Pacific folk-tales. Folk-lore amongst the Somali tribes follows that of British Guiana, and is succeeded by Cornish, Irish, Malay, and North Friesland tales. Dr. Gaster's paper, in the same part, on the modern origin of fairy-tales, is a very suggestive one. Its conclusion, after an examination of certain examples, which "can be infinitely multiplied," is "that the literature of romance and novel, be it a religious romance or one of chivalry, has passed nowadays to a great extent into the literature of fairy-tales, and that, far from being the basis, the fairy-tales are the top of the pyramid formed by the lore of the people. They are the outcome of a long *literary* influence, as well as an oral one, which was exercised upon the mind and soul of the people during centuries." What may be called the editorial matter—the notes, news, &c.—is of the usual varied and interesting character.

SIR D. SALOMON'S little work on accumulators, issued by Messrs. Whittaker and Co., has passed rapidly through two editions. A third and much improved edition, with many illustrations in the text, will be ready shortly.

WE have received the first instalment of what promises to be an important book, "Die Elektricität des Himmels und der Erde," by Dr. Alfred Ritter von Urbanitzky. The complete work will contain about 400 illustrations, including several coloured plates. The publisher is A. Hartleben, Vienna.

WE have received the first number of the *American Journal of Psychology*, edited by Prof. G. S. Hall. The object of this periodical, as the editor explains, is to record psychological work of a scientific, as distinct from a speculative, character. The present number contains, besides reviews and notes, articles on the following subjects: the variations of the normal knee-jerk and their relation to the activity of the central nervous system, by Dr. W. P. Lombard; dermal sensitiveness to gradual pressure-changes, by Prof. G. S. Hall and Mr. Y. Motoro; a method for the experimental determination of the horopter, by Christine Ladd-Franklin; and the psycho-physic law and star magnitudes, by Dr. J. Jastrow.

SIX Bulletins of the United States Geological Survey, Nos. 34–39, have been sent to us. The subjects are: on the relation of the Laramie Molluscan fauna to that of the succeeding fresh-water Eocene and other groups, by Dr. C. A. White; physical properties of the iron-carburets, by Mr. C. Barus and Mr. V. Strouhal; the subsidence of fine solid particles in liquids, by Mr. C. Barus; types of the Laramie flora, by Mr. L. F. Ward; peridotite of Elliott County, Kentucky, by Mr. J. S. Diller; and the upper beaches and deltas of the glacial Lake Agassiz, by Mr. W. Upham.

IN a paper which has just been reprinted from the Transactions of the New York Academy of Sciences, Mr. J. S. Newberry maintains that the decorative ideas expressed in the monuments of the ancient inhabitants of Central America have a close resemblance to the carvings executed by the Indians of the north-western coast of America, and by the people of the Pacific Islands. "Hence," says Mr. Newberry, "I am inclined to believe, as has been suggested by Baldwin, that the seeds of this ancient civilization were brought from the East Indian Archipelago from island to island across the Pacific, and that finally reaching our continent, and prevented by the great and

continuous chain of the Cordilleras from further eastward migration, it slowly spread southward to Chili, and northward to our western territories."

FIVE years ago the increase of wolves in France had become so serious that the Government found it necessary to raise the awards for killing them. In 1882, 423 wolves were killed; in 1883, 1316; in 1884, 1035; in 1885, 900; and in 1886, 760. The awards are now 200 francs for the killing of a wolf which has attacked human beings; 150 francs for one in young; 100 francs for a male wolf, and 40 francs for a cub.

IN the current number (vol. i. No. 4) of the Journal of the Pekin Oriental Society, the well-known scholar Dr. Edkins writes on local value in Chinese arithmetical notation. The principle of local value is used in Chinese commerce, strokes being used instead of special symbols for 1, 2, 3, &c., the relation of the strokes to each other showing the value of the symbol. The abacus, with its upright strings and balls, is only a help to calculation, and does not contain any new principle. Dr. Edkins describes calculating slips which have been in use in China from the most ancient times. It is curious to notice that the principle of local value adopted by the Chinese was from left to right as with ourselves. The slips here mentioned, in which local value played an important part, had been in use fourteen centuries and probably more, when in the fourteenth century the abacus was introduced. Dr. Edkins assigns the origin of the principle of local value to the Babylonians, for several reasons. The first Chinese example known to us is dated B.C. 542, while in the sixteenth century B.C. the Babylonians could extract the cube and square roots of numbers: the Hindoos do not seem to have been proficients in mathematics at so early a date as B.C. 542; so that the probability is the principle of local value in arithmetical notation found its way to China through the Phœnician traders. The Chinese, in fact, acquired it where they acquired a knowledge of the clepsydra, the dial, astronomy, and astrology.

THE creation of provincial museums in Eastern Siberia is progressing very favourably. The example given by the Minusinsk Museum has been followed at Yeniseisk, and will be followed at several other towns. The Minusinsk Museum has now 4000 specimens of plants, 2000 of animals, and 1500 of minerals. The anthropological department has numerous models of huts and houses of the Russian and native population. The archæological collection is especially interesting; it contains 218 implements of the Stone Age, 1260 of the Bronze Age, and 1850 of the Iron Age. There is, moreover, a collection of implements used in, and produced by, local domestic trades. The whole is described in a good catalogue. Last year the Museum was visited by 8000 persons.

TWO bones which were found some time ago at Pitchery Weather Central Queensland, attracted the attention of several terested in science. They were lately exhibited at a the Royal Society of New South Wales, and Mr. explained that they were portions of the vertebral column of an extinct reptile, *Plesiosaurus*. From the transverse elongation of the portions preserved, the bones partook more of the facies of the Plesiosauri of the Cretaceous group than of those found in the Lower Mesozoic deposits.

DR. SCHWERIN, who was despatched last year by the Swedish Government to the Congo, in order to ascertain whether that place was suitable for the establishment of a Swedish colony, and to make scientific researches, has returned to Sweden with good results. He also reports having made an interesting discovery at the mouth of the river, viz. the marble pillar or *padro* erected here by Diego Cam in 1484, the first Portuguese traveller who reached the Congo. The Portuguese were in the habit of raising such *padroes*, bearing the arms of Portugal, in prominent places on the West Coast of Africa, when taking possession of territory, and it was known that one had been erected by Cam at the mouth of the Congo, but it was believed that it had been destroyed. However, Dr. Schwerin, having worked out a theory of his own, searched for this ancient monument some 6 miles further inland than the position indicated on English charts, viz. Point Padro, and here he found it. Dr. Schwerin is preparing an exhaustive account of his work on the Congo, at the expense of the Swedish Government.

AN electric railway for the dinner-table is one of the recent achievements of French ingenuity (*La Nature*, October 29). It makes the presence of servants unnecessary. The train, which runs on a line along either side of the table before the diners, consists of a platform pivoted on two bogies, one of which carries the motor, while the other is merely a supporting truck. The expenditure of electric energy is but slight, and the train is said to be thoroughly under control of the host.

THERE has been much speculation as to how the ancient Egyptians managed to erect their enormous monoliths, sometimes 100 feet in height and weighing hundreds of tons. An interesting recent article in the *Revue Scientifique*, by M. Arnaudeau, offers the explanation that water was employed. Round the obelisk, lying horizontally, with the base towards the pedestal, was raised a circular inclosure, of height equal to that of the monolith. This latter had pieces of wood, or other floats, fitted to it, especially at the upper part; so that when water was brought into the inclosure, the obelisk rose gradually to the vertical. The process may be simply imitated by introducing the end of a screw nail into a piece of cork, putting it in a basin, and then introducing water.

THE pulverizing of minerals for analysis often consumes much time, requiring, as it does, great care. A mill for the purpose, constructed on the model of the wet mill in porcelain work, has been recently brought before the Berlin Chemical Society by Herr Zulkowsky (*Berichte*, October 24). The grinding-surfaces are both agate, and the circular runner, on a vertical axis, has a sector cut out of it, and one edge of this rounded. The mill is driven by water-power, a pressure of two to three atmospheres being sufficient.

IN a paper on colour-blindness, contributed to vol. v. Part 2, of the Proceedings of the Bristol Naturalists' Society, Prof. W. Ramsay suggests that the particular defect which causes colour-blindness may lie in the brain, not in the eye. Certain persons, he points out, are incapable of judging which of two musical tones is the higher, even when they are more than an octave apart. Yet such persons hear either tone perfectly; the defect is not one of deafness. "It must be concluded," says Prof. Ramsay, "that in such a case the brain is the defaulter. And it may equally well be the case that the inability to perceive certain colours is not due to a defect in the instrument of sight—the eye, but to the power of interpreting the impressions conveyed to the brain by the optic nerve. If this is the case, the problem is no longer a physical one: it falls among those with which the mental physiologist has to deal."

A SUPPLEMENTARY mail has just arrived from Iceland, from which we learn that in spite of the ice which has blockaded the eastern and northern shores of the island there has been a good summer and autumn inland, and the harvest has been above the average. However, on the east coast the ice did not disappear till the middle of September, and on the north coast it has not remained so long as during this summer since 1846, and even then the ice-masses were far smaller than this year. In spite of this the weather has been unusually warm inland. Dr. Th. Thoroddsen, the well-known Iceland explorer, has been travelling in the north-western peninsula this year. The fisheries have entirely

failed this autumn on the north and east coast, on account of the drift-ice, but they have been good on the south coast.

THE additions to the Zoological Society's Gardens during the past week include a Mongoz Lemur (*Lemur mongoz* ♂), an Olive-gray Lemur (*Hapalemur olivaceus*) from Madagascar, presented by Capt. J. Bonnerville; an Anubis Baboon (*Cynocephalus anubis*); an Angolan Vulture (*Gypohierax angolensis*) from West Africa, presented by Capt. Augustus Kent; a Peregrine Falcon (*Falco peregrinus*), European, presented by Mr. J. G. Keulemans; a —— Scops Owl (*Scops* ——) from Baltcitan, Himalayas, presented by Mr. John H. Leech, F.Z.S.; two Rough-scaled Zonures (*Zonurus cordylus*) from Robben Island, South Africa, presented by Mr. W. K. Sibley.

OUR ASTRONOMICAL COLUMN.

THE ASTRONOMICAL SOCIETY OF FRANCE.—The science of astronomy has become so increasingly popular in France within the last few years, and Frenchmen have done so much to aid its progress that there is ground for wonder that hitherto there has been no Society in France explicitly devoted to its interests. Such a Society, on lines very similar to those of our own Royal Astronomical Society, has at length been founded, and its first meeting was held on October 12, M. Camille Flammarion, the President, being in the chair. MM. Paul and Prosper Henry, General Parmentier, and M. E. L. Trouvelot are the Vice-Presidents; and MM. Gérigny and Gunziger the Secretaries; whilst Dr. Lescarbault, M. G. Secretan, and M. Ch. Trépied are amongst the members of Council. At the first meeting, M. Trouvelot read a paper on a remarkable double shadow of the first satellite of Jupiter, observed by him in 1877 when at Cambridge, U.S.; and M. Ch. Mousette exhibited a fine photograph of a sunspot, and some large-scale photographs of portions of the solar spectrum.

THE LICK OBSERVATORY.—The *Sidereal Messenger* for the current month states that Mr. E. E. Barnard, of Nashville, Tenn., and Mr. J. M. Schaeberle, of the Ann Arbor Observatory, both well known for their cometary discoveries, have been appointed as astronomers at this Observatory.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1887 NOVEMBER 20–26.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on November 20

Sun rises, 7h. 29m.; souths, 11h. 45m. 44·8s.; sets, 16h. 3m.: right asc. on meridian, 15h. 42·4m.; decl. 19° 42′ S. Sidereal Time at Sunset, 20h. 0m.

Moon (at First Quarter November 22, 11h.) rises, 12h. 15m.; souths, 16h. 43m.; sets, 21h. 18m.: right asc. on meridian, 20h. 41·0m.; decl. 17° 52′ S.

Planet.	Rises.	Souths.	Sets.	Right asc. and declination on meridian.
	h. m.	h. m.	h. m.	h. m. ° ′
Mercury..	6 48 ...	11 22 ..	15 56 ...	15 18·5 ... 16 56 S.
Venus ...	3 1 ...	8 48 ...	14 35 ...	12 44·5 ... 3 22 S.
Mars ...	1 6 ...	7 35 ...	14 4 ...	11 30·9 ... 5 1 N.
Jupiter ...	6 34 ...	11 10 ...	15 46 ...	15 6·7 ... 16 36 S.
Saturn ...	20 53* ...	4 40 ...	12 27 ...	8 36·1 ... 19 0 N.
Uranus ...	3 27 ...	9 2 ...	14 37 ...	12 58·2 ... 5 31 S.
Neptune..	16 8 ...	23 49 ...	7 30* ...	3 47·4 ... 18 10 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Occultations of Stars by the Moon (visible at Greenwich).

Nov.	Star.	Mag.	Disap.	Reap.	Corresponding angles from vertex to right for inverted image.
			h. m.	h. m.	° °
20 ...	B.A.C. 7202 ...	6 ...	16 46 ...	18 2 ...	124 279
20 ...	B.A.C. 7209 ...	6½ ...	17 35 ...	18 32 ...	159 259
20 ...	19 Capricorni ...	6 ...	20 30 ..	21 30 ...	114 347

Nov.	h.	
21 ...	1 ...	Mercury at least distance from the Sun.
21 ...	6 ...	Neptune in opposition to the Sun.
24 ...	10 ...	Venus in conjunction with and 1° 6′ north of Uranus.
26 ...	23 ...	Mercury stationary.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·3 ...	81 16 N. ...	Nov. 22,	1 48	*m*
R Arietis	2 9·7 ...	24 32 N. ...	,, 25,		*m*
λ Tauri...	3 54·4 ...	12 10 N. ...	,, 25,	5 40	*m*
U Ophiuchi	17 10·8 ...	1 20 N. ...	,, 22,	5 27	*m*
			and at intervals of	20 8	
β Lyræ...	18 45·9 ...	33 14 N. ...	Nov. 22,	6 0	*m*
η Aquilæ	19 46·7 ...	0 43 N. ...	,, 24,	22 0	*M*
S Sagittæ	19 50·9 ...	16 20 N. ...	,, 22,	19 0	*m*
			,, 25,	19 0	*M*
δ Cephei	22 25·0 ...	57 50 N. ...	,, 20,	21 0	*M*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
The Andromedes ...	24 ...	44 N. ...	Very slow; with trains.
Near μ Ursæ Majoris.	155 ...	40 N. ...	Swift; streaks.

GEOGRAPHICAL NOTES.

THE Owen Stanley Range of New Guinea, which has been so long known at a distance, has at last been ascended Dec. Mr. E. H. Martin, of Queensland, in August last, reached the summit of the range, which he found to be 13,205 feet high. He reports the north side of the range to be a paradise with great tree-ferns, palms, and other magnificent tropical vegetation. Mr. W. R. Cuthbertson, the leader of the Australian Geographical Society's Expedition, started for Port Moresby on July 20 last, with Mr. G. Hunter as interpreter. Mr. Cuthbertson has not yet succeeded in ascending to the highest point of the Owen Stanley Range, as he intended, but ascended Mount O'Bree, 10,240 feet.

IN No. xi. of *Petermann's Mitteilungen*, Dr. Paulitschke describes Captain Stuart King's journey into the country of the Ejssa and Gadabursi Somali, some 70 miles to the south of Zeyla, in 1886. The paper is accompanied by a map. Dr. von Jhering and P. Langhans conclude their long and elaborate memoir on the southern colonial region of Rio Grande do Sul. Dr. Hans Schinz, who has been so long in the Lake Ngami region, criticizes severely Mr. Farini's narrative of his journey to the Kalahari Desert, the conclusion being very adverse to the trustworthiness of Mr. Farini's narrative. Perhaps the most important contribution to this number is a beautiful map of the Russo Afghan frontier region, based upon the work of Colonel Holdich's Commission. It is remarkable that while Colonel Holdich's work is carefully locked up in the India Office as "confidential," so far as English geographers are concerned, it should be accessible to the geographers of other countries.

HERR KRAUSE has returned to the German settlement Mr. C. Togo, on the Gold Coast, from his journey from Salaga Dahomey. He has collected from 600 to 800 specimens of solid plants and seeds, a large number of insects, and a Laramie specimens of prehistoric articles found between M... K... Timbuktu.

THE principal paper in the third part of this year's *Bulletin* of the Paris Geographical Society is an account of a journey made in 1881 by Count de Chavagnac, from Fez to Morocco, north-east to Mekenessa, and eastwards across the numerous wadis that run south into Wed Mellouja, and as far as Ajda. There is also a paper containing a good deal of useful information, and accompanied by an excellent map, on the ports of Tonquin, by M. J. Renaud. M. Datreuil de Rhins concludes his useful summary of our knowledge of Eastern Tibet.

THE session of the Royal Geographical Society began on Monday, with a paper on Siam, by Mr. J. McCarthy, Superintendent of Surveys in Siam. Mr. McCarthy has been at work for seven years on the survey of Siam, and some of the results he described in his paper, and embodied in the map by which

it was illustrated. After indicating the position of Siam in the Malay Peninsula, the author went on to say that on the west is a chain of mountains which runs in an unbroken range to Singapore, the southernmost limit of the Malay Peninsula; some of its peaks between Burmah and Siam rise to a height of 7000 feet, while one peak in the Malay Peninsula reaches 8000 feet. On the east there is another range of mountains which forms the grand watershed of all the rivers that flow into the Gulf of Tonquin and Chinese Sea on the one hand, and the Meinam Kong on the other. There are peaks in this range that reach even 9000 feet above mean sea-level. Besides these ranges there is another which breaks away from the western range from a point north-east of Chingmai, and forms the watershed between the Meinam and Meinam Kong valleys. In this range, at the source of the eastern branch of the Meinam, are famous salt-wells. The salt is procured at depths varying from 35-45 feet—in these land-locked countries as valuable as money. The greater part of the valley of the Meinam Kong and the Meinam is flat, diversified by isolated hills, and broken and jagged ridges of limestone mountains. The most important river, though not the largest, is the Meinam Chau Phraya. It is the Nile of Siam, a good rice harvest very much depending on whether the river overflows its banks or not. The eastern branch of the river is specially known for the numerous crocodiles which yearly carry off some victims. Two other rivers converge towards the Meinam, the Mei Klong and Bang Pla-Kong. All these rivers are connected by canals, rendering communication easier in a country where roads are conspicuous by their absence. The Meinam Kong is the largest river, and flows through the northern and eastern parts of the kingdom, receiving the waters of many large affluents; but the channel of this mighty river is so blocked with large rocks and cataracts, that its navigation is very difficult, and in some parts impossible even for native craft. Mr. McCarthy then went on to describe some of his journeys in detail, especially the one to the north-east frontier, which led him through scenes of surpassing beauty, and during which he opened up much new ground.

THE Arctic land seen by Sannikof eighty years ago has been seen again by the Expedition of MM. Bunge and Toll from the northern extremity of the Kotelnyi Island. The Great and Small Liakhov Islands, the Thadeus Island, and New Siberia have also been visited by the Expedition, which has returned with rich zoological, botanical, and geological collections. Throughout the summer of 1886 the ice on the Siberian coast did not move from the shores, and the hunters said that the sea had not been clear from ice since the *Vega* Expedition.

METEOROLOGICAL NOTES.

WE have lately been subjected to a series of storms which fortunately in the British Islands is not of very common occurrence. The storm of October 30, which was noticed in NATURE a few days after its occurrence, had scarcely left our shores before a fresh disturbance was approaching us from off the Atlantic, and by the evening of Monday, the 31st, another gale was blowing in Ireland, and during the night this storm extended to all parts of the British Islands. The central area of low barometer readings, which primarily occasioned the renewal of disturbed weather, kept to the westward of our coasts, but the Daily Weather Chart of November 1 shows that two secondary disturbances had been formed, one having its centre in the St. George's Channel, and the other over the Bay of Biscay. The very severe gale experienced in the south-west and west of England on November 1 was due to the former of these, the storm area passing during the day slowly up the Irish Sea. The fall of the barometer for this gale amounted to 1·02 in. at Pembroke in fourteen hours, from 6 p.m. 31st to 8 a.m. 1st; and at Lyme Regis the wind attained the velocity of 83 miles an hour between 7 and 8 o'clock in the morning. Another disturbance skirted to the westward of Ireland on the evening of the 2nd, and during the following day, causing southerly gales in many parts of the country, the barometer standing below 29 inches over the whole of the United Kingdom. On the evening of the 3rd another subsidiary was formed in the Irish Sea, and subsequently passed over the north of England, causing gales and disturbed weather in parts adjacent to its path. Before the expiration of the week a fresh disturbance was shown in the west, and on Saturday, the 5th, the barometer was again falling: the force of the wind, however, was not severe, although it blew a fresh gale in places. It will be seen from this notice that no fewer than five distinct storms were experienced in seven days, and in each case the wind was accompanied by heavy rain.

THE Meteorological Council have published Part I. of the "Hourly Readings" for 1885 (January to March) made at their self-recording observatories, together with the daily means, daily maxima and minima, and the daily range for pressure and temperature. Hourly values have now been issued in either lithographed or printed form since 1874, and afford valuable data for discussion in various ways, although the hourly means are not calculated. Corrections are given for reducing the barometric observations to mean sea-level. In connection with these observations it may be mentioned that the Meteorological Institute of the Netherlands lately published an interesting paper by M. Schokker on atmospheric disturbances studied by means of the hourly readings issued by the Meteorological Office and elsewhere; he traced on charts the positions of depressions for various hours, and showed that many phenomena which are clearly traceable from hourly observations are entirely lost sight of on charts giving only one or two hours a day. He also quoted instances where timely warning of storms could have been given, which were not possible from the usual observations received by telegraph.

THE Hydrographic Office of the United States calls special attention to a new form for reports of storms, fog, ice, and derelicts, issued for the use of trans-Atlantic steamers. This form replaces those hitherto issued by that Office and the Signal Service, and the information thus collected is immediately utilized in preparing the telegrams sent daily to France by the United States Signal Service for the benefit of westward-bound vessels. Captains of trans-Atlantic steamships are requested, in the interest of navigation, to send in prompt and complete reports. No doubt British ship-owners will instruct their officers to co-operate in this enterprising experiment, as this country has at least equal interest with others in the safety of Atlantic navigation.

A DISCUSSION on the distribution of cloud over the eastern part of the North Atlantic, by Dr. W. Köppen, will be found in the *Annalen der Hydrographie und Maritimen Meteorologie* for October. The author points out that the cloud-conditions over the Atlantic are now fairly well known from the publications of the Meteorological Office (Captain Toynbee's great work for nine 10° squares), and the six 10° squares discussed by the Deutsche Seewarte. Dr. Köppen gives a table showing the mean monthly cloud from 20°-50° N. and from 10°-40° W., and the number of observations used, for every 5°, showing that, with regard to longitude, in the months January to April the cloud decreases north of 10° N. as we approach the shores of Africa and Europe, while in the other months this does not hold good. South of the equatorial calm-belt, May has the least cloud towards the east of the district, and in the months September to February the least cloud is towards the west. The differences of the amount of cloud with regard to latitude are much more decided, and these changes are shown on a map of equal lines of mean cloud, on the same plan as was adopted by the author in his discussion of the rainfall (NATURE, vol. xxxvi. p. 617). He also compares the cloud and rainfall curves for the yearly period, and draws attention to their marked difference in the zone of 15°-27° N. lat. While the tropical summer rains cease between 15° and 20° N., the summer maximum of cloud extends as far as 25° N. In the same way the winter maximum of cloud only extends southwards to 25° N., while the rain extends to 17° N. In these latitudes the minimum of cloud falls in the autumn, and the minimum of rain in spring. Only from 15°-17° to the southwards is the amount of cloud in spring less than in autumn, while northwards of 27° N. both minima coincide in the late summer season. Between 15° and 20° N. the end of the long dry season, lasting from February to June, is very cloudy. The author also compares his cloud-results with those obtained by M. Teisserenc de Bort from independent data (NATURE, vol. xxxvi. p. 15), and on the whole expresses himself satisfied at the agreement between the two investigations.

WE have the pleasure of recording the commencement of the publication of meteorological observations in the *Boletin de Estadistica* of Puebla (Mexico). Observations taken three times a day are published for several stations, and monthly means for several others. The stations are generally at great altitudes above the sea.

PART 2, vol. iv. of the Indian Meteorological Memoirs contains a very lucid discussion of the disastrous storm which visited Orissa in September 1885, and whose centre was at False Point on the 22nd, drawn up by Prof. A. Pedler. This storm is of considerable meteorological interest from several points of view: viz. the rapidity of its formation; its smallness, the diameter at the part of greatest wind-force being only from 100 to 200 miles; its enormous fierceness; particularly as it approached the land; and the decided indraught towards the centre as opposed to the circular theory; the extraordinary low reading of the barometer, 27·135 inches, being recorded at False Point at 6h. 30m. a.m. of the 22nd. The reading at 8h. p.m. of the 21st was 29·622 inches, thus giving a fall of 2·487 inches in 10½ hours. This is the lowest pressure ever recorded in a storm in the Bay of Bengal, and in fact is the lowest on record for any part of the world.

THE Journal of the Scottish Meteorological Society for the year 1886 contains a large amount of useful information, and testifies to increased activity, both observational and experimental. Among the various papers, all of which are of the highest importance, may be specially mentioned, (1) an address by the Hon. R. Abercromby on the modern developments of cloud knowledge (see NATURE, vol. xxxv. p. 575); (2) discussions on the winds and rainfall of Ben Nevis, and on a peculiarity of the cyclonic winds of the mountain, which has an important bearing upon weather forecasting, viz. the outflow of the wind from the cyclone when the centre is north or east of Ben Nevis towards an anticyclone or area of high pressure somewhere in an opposite direction. The prevalent wind on the Ben is north, while south-east and west-south-west are secondary points of maxima. Compared with the winds of other stations in the north of Scotland and Ireland, the wind curve is quite different. The year divides about equally into cyclonic and non-cyclonic periods. The most frequent cyclonic wind is south-west; next to this comes north, apparently due to the cyclones passing to the north of Ben Nevis. The relative frequency of the winds in non-cyclonic periods is quite different: while north still retains its place as a maximum point, the most frequent wind is south-east. In the curve for the whole year tha west-south-west winds are chiefly due to cyclonic winds, south-east to non-cyclonic, and north to both systems. In both systems the north-west wind is wettest while it blows, and the east is driest. The south-east winds, which are generally west at low levels, are the driest on Ben Nevis, with the exception of the east winds. The total amount of precipitation for the year was nearly 108 inches; the wettest month was November, 14·6 inches; and the driest February, 2·8 inches. The journal also contains an interesting account of the biological work of the Scottish Marine Station, and the results of observations at the Northern Lighthouse Station, at the stations connected with the Medical Department, including observations in Iceland, Faroe, and Uruguay, and at fifty-five stations established by the Scottish Meteorological Society, and well distributed over the country.

GEMS AND ORNAMENTAL STONES OF THE UNITED STATES.

ON Saturday, October 22, an evening lecture on this subject was delivered by Dr. A. E. Foote, of Philadelphia, in the Trophy Hall of the American Exhibition. The speaker was introduced by Mr. F. W. Rudler, the President of the Geologists' Association.

Dr. Foote remarked that hitherto mining for gems in the United States had been of a very desultory character, being principally carried on in connection with mica and other mines. The emerald and Hiddenite mines of North Carolina and the tourmaline mines of Maine are the only ones which have been worked systematically. The gems peculiar to America are *chlorastrolite*, *zonochlorite*, and *Hiddenite*. Chlorastrolite, or green star-stone, was discovered by Prof. J. D. Whitney, of the United States Geological Survey, about forty years ago. The only place where it is found is Isle Royale, Lake Superior. The island, belonging to the State of Michigan, forty miles long and five miles wide, and about twenty miles from the mainland, is composed of amygdaloidal trap, in the almond-shaped cavities of which the gem principally occurs. This green stone has a radiating structure, and shows a beautiful *chatoyance* similar to cat's-eye and other fibrous minerals.

Zonochlorite is a green-banded stone, similar to chlorastrolite in composition, discovered by Dr. Foote at Neepigon Bay on the north shore of Lake Superior. The full description was published in the Transactions of the American Association for the Advancement of Science in 1872. Its hardness is about 7; it takes a very high polish, and if it could be found in sufficient quantities would undoubtedly be extensively used.

Hiddenite is a green variety of the well-known species spodumene. A yellow variety from Brazil has been cut as a gem for many years. The green variety has been known for about seven years, and is fully as beautiful, and valued as highly, as the diamond. It occurs in connection with emeralds in North Carolina. Of *gold quartz* about £28,000 worth is sold annually. Most of this comes from California, where it is not only used as a gem, but in the manufacture of various ornaments.

Although the flexible sandstone, the reputed gangue of the diamond in Brazil, is found in mountain masses in North Carolina and other States, no very large diamonds have as yet been discovered. Many small ones are recorded from California, North Carolina, Virginia, and elsewhere. The largest was found at Manchester, near Richmond, Virginia, and weighed 23¾ carats in the rough and 11⅛ carats cut. Prof. Whitney states that the largest found in California was 7½ carats. Rubies and sapphires have been found in the rock in the corundum mines of North Carolina, and Mr. C. S. Bement has an uncut green one in his collection that would give 80 to 100 carats' worth of good stones, one of which would probably weigh 20 carats. The largest red and blue crystal weighs 312 pounds, and belongs to Amherst College. The best sapphires are found in the placer mines of Montana. Asteriated corundums are found in Pennsylvania and elsewhere.

About £2200 worth of quartz or rock crystal is mined annually. The best localities are Hot Springs (Arkansas), North Carolina, New York, and Virginia. A portion of a mass that must have weighed over 40 pounds was recently received from Alaska, that cut a hand-glass 3 inches by 5. Rock crystal is frequently dug up in the prehistoric mounds, and was used by the medicine-men and others for telling future events. Amethysts are found in very fine specimens in Pennsylvania, Georgia, Texas, and the Lake Superior region. From the latter region they are very remarkably lined, some specimens showing "phantom crystals" equal to the Hungarian. Near the Yellowstone National Park and in the chalcedony forests of Arizona are tree-trunks, some of which are 100 feet long, mineralized by the action of silicated waters. Some of these trees are still standing upright, others, having fallen, bridge deep chasms. The once hollow cavities of some are lined with amethyst, others with agate. The Arizona agatized or jasperized wood shows the most beautiful variety of colours of any petrified wood in the world. Probably the most remarkable locality anywhere for smoky quartz, or cairngorm stone, is Pike's Peak, Colorado. Here it is found in a graphic granite associated with Amazon-stone, which also makes a very beautiful green ornamental stone. The rutilated quartz, or Cupid's arrows, is found in remarkably fine specimens in North Carolina. Perhaps the most remarkable mass is one 7 inches by 3½, now in the collection of the Academy of Natural Sciences of Philadelphia. The crystals of rutile are about the size of knitting-needles. Some of the North Carolina rutile has been cut, furnishing brilliant gems, closely resembling carbonado. The rutile, geniculated till it forms a perfect circle or rosette, from Magnet Cove, Arkansas, is often mounted and worn as a gem. While opals are found at many places in the United States, they do not rival those of Queretaro in Mexico. Here are found not only the "milky opals that gleam like sullen fires in a pallid mist," but fire opals and almost every other variety known. Rhodonite, in specimens suitable for polishing, is found in Massachusetts and New Jersey. At the latter locality were obtained the finest crystals ever seen. The garnets from New Mexico and Arizona are superior to the "Cape rubies" from South Africa; and from Alaska the most beautiful crystals ever seen, in a setting of gray mica schist, have recently been obtained.

The New Mexican turquoise is mined to the value of about £700 annually. It has recently been described very fully by Prof. Clarke, Curator of the Mineralogical Department of the National Museum, and is especially interesting as being the material from which the "chalchihuitls," or most sacred images of the Aztecs, were made. The Indians still regard it as a lucky stone.

Labradorite, lately so popular for gems and ornamental stones, is found in many localities. The tourmalines of Maine are

probably the finest in the world. Here are found the Oriental sapphire, ruby, and emerald, in perfection.

Topaz has recently been found at Pike's Peak, Colorado, in large quantity. Some masses weighed 2 pounds each; and very fine clear white stones have been cut, weighing from 125 to 193 carats.

Among ornamental stones should be mentioned a very beautiful variety of serpentine from Maryland, called verd antique, which is being largely used in the interior decorations of the Philadelphia Court House. Another variety, resembling jade, is the green williamsite from Pennsylvania. Alabaster of various colours abounds in many localities; and marbles, some as beautiful as the Mexican onyx, are found in nearly every State. The malachite and azurite, jet, and many other gems of minor importance were briefly described.

THE OCTOBER METEOR-SHOWER OF 1887.

THE display of Orionids has been recently observed at this station with greater success than has attended my efforts in any previous year. This shower has not, perhaps, exhibited such richness as it did in 1877, but the present occasion has been more favourable as regards the conditions; the moon being absent from the morning sky, and a period of tolerably clear weather occurring just at the important time.

In all, I numbered ninety Orionids between October 11 and 24, and the radiant-point during this period exhibited a stationary position amongst the stars. The shower has this year met with rather a formidable rival in a bright display of forty-five meteors from a radiant at 40° + 20° close to ε Arietis. I have witnessed the latter stream in several preceding years, though not in such conspicuous strength, and have particularly referred to it in the *Monthly Notices*, vol. xliv., pp. 24–26, as furnishing many bright fireballs at this season.

It will be convenient to arrange my new observations in a tabular form:—

Date 1887 Oct.	Period of Observation. h.	h.	Real Duration.	Meteors seen.	Orionids.	Arietids.	Radiant of Orionids.	
11	7½ to	12½	4½	30	1	2		
12	8½ ,,	12½	4	31	2	1	91°	+ 17°
13	10 ,,	12	2	16	1	1		
14	9½ ,,	16¾	7	75	1	10		
15	7 ,,	8½	7½	86	17	7	91°	+ 16°
	10½ ,,	17						
17	8 ,,	12½	4½	29	3	3	90°	+ 15°
19	13 ,,	15	1½	19	10	—	90½°	+ 15½°
20	10 ,,	15½	5½	61	22	9	90°	+ 14½°
21	9 ,,	16	6½	76	23	7	92°	+ 14°
23	12½ ,,	14	1½	13	1	3	—	—
24	12 ,,	14½	2½	23	9	2	91°	+ 16°
11 nights			46¼	459	90	45	91°	+ 15°

The 16th and 22nd were overcast, and on the 19th and 23rd the observations were much obstructed by clouds. It is noteworthy that I only recorded one Orionid on October 14 during a watch of seven hours, though on the following night this shower supplied seventeen meteors.

The radiant-point of the October meteors has long been accurately known. Prof. A. S. Herschel observed it with great precision on October 18, 1864, and October 20, 1865, and found the centre at 90° + 16°, and 90° + 15° respectively, in those years. All the best of later determinations have agreed closely with these results, and it will be noticed that my value for the present year, as given above, is nearly identical with them. In further confirmation I may mention that Mr. David Booth, of Leeds, observed more than sixty shooting-stars during a watch of five hours, from 10½h. to 15½h. on the night of October 20 last, and saw twenty-four Orionids which gave a sharply-defined radiant at 90° + 16°.

One of the principal objects of my late observations was to ascertain whether the radiant centre of this stream showed any displacement of position on successive nights, and similar to that affecting the Perseids of August—a peculiarity which I first pointed out in NATURE, vol. xvi. p. 362. But the radiant of the Orionids has (when the small, unavoidable errors of observation are allowed for) quite failed to exhibit any change of place relatively to the contiguous stars. It appeared to maintain an absolutely persistent position 1° north of the star ξ Orionis. My observation on October 15 placed it at 91° + 16°, and nine nights later, viz. on October 24, I found the meteors were radiating from exactly the same focus. In 1877 and 1879, October 15, I derived the radiant at 92° + 15° and 93° + 17°, and in 1878, October 22, I fixed it at 92° + 14°. A comparison of all these values renders it sufficiently obvious that there is no visible displacement in the position of the Orionid radiant during its active display from October 11 to October 24. And there is a high degree of probability that the point is stationary during the whole period of the shower's sustenance from about October 9 to October 29; but I have never secured many paths and been enabled to get a good radiant near the limiting epochs of its display, when it is extremely feeble.

Mr. Booth, at Leeds, has been carefully observing numbers of meteors during the past few months, and a searching comparison of his results with those obtained at Bristol during the progress of the Orionid shower has shown that several of the same meteors were observed at both stations. Three of these are typical members of the October display, whilst three others had their origin in the minor systems which are so plentifully distributed over the sky at this season of the year. The computed heights and paths of these six meteors are:—

Date 1887 Oct.	Hour G.M.T. h. m.	Mag.	Height at appearance.	Height at disappearance.	Length of real path.	Radiant point. °	°	Inclination to horizon. °
13	10 25	1–2	69	50	26	73	+ 61	48
13	11 25	2–4	70	42	37	127	+ 83	49
14	12 5½	4–5	64	40	26	355	+ 36	67½
15	14 48½	1½–3	89	61	39	87	+ 15	46
20	11 45	4–4	106	90	34	87	+ 21	28½
20	12 55	1–1½	92	53	70½	87	+ 13½	33½

The three last in the list were Orionids, and they appear to have been observed at somewhat greater elevations in the atmosphere than is usual. The 4th magnitude meteor of October 20, 11h. 45m., was no less than 106 miles high at its first appearance, over a point near Eversham, Kent, and the two observations are in perfect agreement in indicating these figures. The mean of the three Orionids gives 96 miles for the beginning points and 68 for the ending, and the average radiant comes out at 87° + 16°, which is 3° or 4° west of the usual position. But the average values deduced from so small a number of instances cannot have much weight as indicating accurately either the heights or radiant of the general body of the meteors forming this notable group.

The Arietids, which have developed into an important shower this year, traverse their paths with medium speed, and are rather conspicuous meteors, without trains or streaks except in exceptional cases. As to the Orionids, they move swiftly, and are accompanied in almost every instance with streaks. The latter will sometimes brighten up considerably after the nuclei of the meteors have died away. The more brilliant Orionids are fine flashing meteors, leaving streaks which are occasionally very durable.

The contemporary showers of the October epoch, though extremely abundant, are not marked by special activity, except perhaps in the case of the Arietids, already referred to. This year the following have been the best of the minor streams:—

Date.	Radiant. °	°	Meteors.	Appearance.
October 14–15	25	+ 44	10	Slow, faint.
,, 14–21	54	+ 71	12	Swift.
,, 14–21	105	+ 22	12	Very swift, streaks.
,, 20–21	125	+ 43	7	Very swift, streaks
,, 14–23	135	+ 68	11	Swift.
,, 12–20	312	+ 77	8	Swift.

Of these the most pronounced is at 105° + 22°, near δ Geminorum, which I also observed in 1877 and 1879. It has also been recorded as a prominent stream by Zezioli and others, and is identical with the Gemellids of Mr. Greg's catalogue (1876). It is chiefly a morning shower; its meteors are often brilliant, and regularly display the phosphorescent streaks which form so characteristic a feature of the Perseids, Orionids, and Leonids. The shower in the head of Ursa Major at 135° + 68° is also an active one at this epoch; I saw it in 1877 at 133° + 68°, October 2–19, and these appear to be the only two observations of it obtained hitherto.

W. F. DENNING.

ON SOME OF THE AFFINITIES BETWEEN THE GANOIDEI CHONDROSTEI AND OTHER FISHES.

THE group of Ganoidei Chondrostei has hitherto been regarded as one developed during the latest period of the history of the earth. Its structure is so different from that of other classes of fish that its relationship with them cannot be easily detected. The zootomic and embryological works of the last ten years, and especially the works of Zalensky in Odessa, Parker in London, Davidoff in Munich, and van der Wighe in Holland, have brought together many important facts as to the organization and development of these interesting animals, but the information provided by these writers is either fragmentary or not full enough, and long study and labour will be required before it can be satisfactorily summed up and completed.

During the last two years I have studied the anatomy of *Acipenser ruthenus*, the commonest representative of the Acipenseridæ to be found here; and although my work is far from being completed I may beg the reader's attention to some interesting facts, which must, I think, be taken into consideration by those who try to settle the question as to the relationship of the Ganoidei Chondrostei to this or to that group of fishes.

We may begin with the teeth of these fishes, as an indication of great value, which served to distinguish this group from other Ganoidei. Teeth have been found in *Polyodon folium*, a member of the Ganoidei Chondrostei, inhabiting the rivers of North America; it has been thought that they might also be found in *Psephurus gladius* of the River Yang-tse-kiang, in China; and Prof. Zalensky has found them in *Acipenser ruthenus*, at the age of from three weeks to three months. I have had the good fortune to find teeth in almost all the Ganoidei Chondrostei of the different ages that I have examined, but they were palatine teeth, not mandibular or maxillary teeth. I have discovered and studied the palatine teeth in a two-months-old sterlet; in an *Acipenser stellatus* of from seven to eight months old; in a *Scaphirhynchus kaufmannii* from Amu-Daria of a year old; in a grown *Scaphirhynchus fedschenkoi* from Sir-Daria; and in full-grown *Polyodon folium*.

The relationship of the dimensions of the snout of fish to the age at which teeth can be found is very interesting. The long and flat snouted *Acipenser stellatus* has teeth to a more advanced age than the short and narrow snouted sterlet; the teeth of a wide-snouted Scaphirhynchus not attaining a good development but are preserved until maturity; the spade-snouted Polyodon preserves its teeth during the whole of its life. In all the other representatives of Acipenser and Scaphirhynchus can be found at any stage traces of palatine teeth in the shape of two similar prominences, which, by their structure, can be distinguished from the surrounding parts of the mouth.

This dependence of a long preservation of teeth on the development of the snout of Ganoidei Chondrostei, together with the geographical distribution of these fish, shows the greater antiquity of the tooth-preserving kinds of Scaphirhynchus and Polyodon, than of the Acipenser. Species of one kind, inhabiting such widely separated water-reservoirs as the Aral Sea and Mississippi, or the Yang-tse-kiang and the continental rivers of North America, must be representatives of very old forms, remains of former fauna; their having, at a mature age, organs that do not serve them, but which merely remain as an inheritance from former periods, is a confirmation of their supposed antiquity—a conclusion drawn from zoogeographical observations.

The structure and development of the dorsal shields, which, in the case of Acipenseridæ, spread all along the dorsal surface, from the back edge of the head down to the dorsal fins, may also, I think, help us to discern affinities between Ganoidei Chondrostei and other fish. The first to pay attention to these shields, and to suppose they were an embryonal dorsal fin, was Prof. Zalensky. About the same time Prof. Goethe described a similar fin of a six-weeks-old sterlet, hinting, by the way, that the dorsal shields might be compared with the dorsal rays of a fossil fish, Cœlacanthus. I have succeeded in investigating the dorsal shields of a two-months-old sterlet, and in making a whole series of cross sections, and I have arrived at the conclusion that Zalensky's and Goethe's suppositions are fully established by facts. Indeed, between the shields spreads a membrane, in which can be seen the same horny rays that are generally seen in developing fins of fish; right and left of each dorsal shield there is a muscle, traces of which can also be found under the shields of grown sterlets. At last, having made cross-sections of oxidized dorsal shields of grown sterlets, a canal could be perceived in them. These canals are particularly well seen in Scaphirhynchus, as an older and a better representative of the original type.

Knowing that Dr. Günther in his excellent book on ichthyology places the Acipenseridæ and Cœlacanthi next to the Polypteroidei, I availed myself of the offer of Prof. Bogdanoff, Director of the Moscow Museum, and my teacher, to let me examine the only dry specimen of *Polypterus senegalensis* that was in the Museum. Comparing the numerous small fins spreading all along the back of Polypterus, there being a great and wide front bone-ray, and the others being thin and horny, I became convinced of their complete similarity to the dorsal shields of a young sterlet and to the membranes which connect them.

In the wide bone-ray of Polypterus a ray channel could also be discerned, and the rays of the membrane that spreads behind the wide ray were also horny, like the rays of the membrane of an embryonal fin of a sterlet. This brought me to the conclusion that the ancestors of both the Acipenseridæ and the Polypteroidei had not only a back fin, but also well-developed front dorsal fins, with great bone-rays and smaller horny rays, and were, perhaps, nearer to each other than their present descendants.

A study of other organs, especially those in young Acipenseridæ and Scaphirhynchus, convinces me that there is a closer relationship between the Ganoidei Chondrostei and the Polypteroidei than has hitherto been supposed. It is well known that the conus arteriosus of Acipenser is distinguished from the same organ of the Polypterus and Lepidosteus by a much smaller number of transversal rows of valves. In young sterlets I have found, besides developed valves, undeveloped folds lying between the valves. In place of such undeveloped valves, in the case of grown fish, as for example in a specimen of *Acipenser huso* which I dissected, and which was about 10 feet long, an unevenness and roughness of surface are noticed. The air-bladder, which in Lepidosteus and Polypterus partly resembles the lung of Dipnoi, when attentively studied in the Acipenseridæ does not appear to be so well adapted to its new functions. Its coatings include many ramifications of vessels, the histological structure of which is so similar to the structure of the coatings of the digestive organs that it is much easier to recognize their relative layers than in those of other fishes, where the air-bladder is fully adapted to its functions. The ductus pneumaticus, in young sterlets especially, is very wide; a two-months-old sterlet has it of almost the same width as an œsophagus, and the food of the small fish, consisting mostly of forms of Cladocera and Ostracoda, and also of statoblasts of Polyzoa, especially Alcyonella, fills the cavity of the air-bladder like the cavity of the stomach.

Though the brain of these fishes has been well investigated, yet in its organization one finds much that is interesting. For example, the cerebral hemispheres of the prosencephalon of Scaphirhynchus proved to be more similar to the hemispheres of Dipnoi and Lepidosteus and Protopterus, than to those of Acipenser. The lateral layers are turned upward, so that the upper portion of the hemispheres proved to consist, not of one pallium, as in Acipenser, but also of the coating of the cerebrum. The epiphysis cerebri, being a changeable organ, proved to vary even in the limits of the genus Acipenser. Thus, its front end in *Acipenser sturio* reached as far as the line connecting the two lower nostrils, forming an angle of nearly 28° with the surface of the brain, whereas in *Acipenser ruthenus* the epiphysis forms an angle of almost 80°, and becomes a much shorter organ. In some sterlets the end of the epiphysis cerebri went through the cranium, and was only covered by the bone shields of the exterior coating. Scaphirhynchus had the epiphysis less changeable and more similar to the epiphysis of other Ganoidei and Dipnoi. In other respects the brain of Scaphirhynchus also proved to have a closer resemblance to the other Ganoidei than to the Acipenser. Thus, its valvula cerebelli and lobi inferiores are more developed than those of a sterlet, and even remind one of the brain of Amia and its near relatives Teleostei.

Notwithstanding the scantiness of the facts stated here, I indulge the hope that they may add something to the means at our disposal for the settlement of the relationship between Ganoidei Chondrostei and other Ganoidei.

NICHOLAS ZOGRAFF.

Moscow, 20/8 September 1878.

SCIENTIFIC SERIALS.

American Journal of Mathematics, vol. x. No. 1 (Baltimore: Johns Hopkins University, 1887).—The number opens with the concluding lecture (the 33rd) of Prof. Sylvester's course on the theory of reciprocants, in which is investigated the differential equation of a cubic curve having a given absolute invariant S^3/T^2. A supplemental "lecture" is supplied by the reporter (Mr. Hammond) from the lecturer's surplus material: this "constitutes probably the most difficult problem in elimination which has been effected up to the present time." All admirers of Prof. Sylvester's brilliant genius will be glad to have the fine presentment of his features which accompanies this number.—Algebraic surfaces of which every plane section is unicursal in the light of n-dimensional geometry is devoted to a proof and to illustrations, by Mr. E. H. Moore, Jun., of a theorem due to Picard, viz. "Les seules surfaces algébriques dont toutes les sections planes sont unicursales sont les surfaces réglées unicursales et la surface du quatrième degré de Steiner."—Mr. Morgan Jenkins, in a paper on Prof. Cayley's extension of Arbogast's method of derivations, presents in a simplified form results given by the elder mathematician in a memoir printed in the Phil. Trans. (read December 1860).—Properties of a complete table of symmetric functions, by Capt. P. A. Macmahon, R.A., establishes some remarkable features of a tabulation set forth by Mr. Durfee in vol. v. of the *Journal*.—Oskar Bolza, in his article on binary sextics with linear transformations into themselves, considers those binary sextics which remain unchanged (or are only changed by a constant factor) for certain linear transformations of the variables.—Prof. Cayley follows with the sequel to his memoir on the transformation of elliptic functions (vol. ix.), and Prof. Woolsey Johnson closes the number with the symbolic treatment of exact linear differential equations.

Bulletin de la Société des Naturalistes de Moscou, 1887, iii.—Comparative osteology of the penguins and its bearing upon the classification of birds, by Dr. M. Menzbier (in German; with a plate).—The Hessian fly, by Prof. K. Lindeman (in German).—Chemical composition of the Lipetsk mineral springs, by A. Kislakovsky. A series of chemical analyses has been undertaken in order to ascertain how far the composition of the springs is liable to undergo changes at different times of the year. The admixture of water flowing from sweet springs makes the amount of $FeCO_3$ to vary from 0·016 to 0·032, and from 0·008 to 0·025 in different springs.—On the increase in the number of thunderbolts and its causes, by J. Weinberg (in German).—Enumeration of the vascular plants of Caucasus, by M. Smirnoff (in French). This fourth paper of the introduction which the author has written to precede his enumeration of plants discusses the following important subjects: evaporation, limits of perennial snow in Caucasia and neighbouring highlands, the present and ancient glaciers of Caucasus, and the geology of the country since the later Tertiary. The twelve botanical regions into which the author divides Caucasia are given with short characteristics of their physical features. On the whole the paper is a most valuable contribution to the knowledge of Caucasus.—List of plants growing in the province of Tamboff, by D. Litvinoff (continued).—*Otiorhynchus turca*, Steven, an enemy of the vine-tree, by E. Ballion. It has been found at Novorosiysk, on the east coast of the Black Sea, and must have immigrated from Asia Minor and Syria.

SOCIETIES AND ACADEMIES.

LONDON.

Mathematical Society, November 10.—Sir J. Cockle, F.R.S., President, in the chair.—Prof. Sylvester, F.R.S., being incapacitated by an accident to his leg from attending in person to receive the De Morgan Medal, awarded him by the Council in June last, deputed Mr. J. Hammond to represent him. The President, after a few remarks eulogistic of Prof. Sylvester's numerous discoveries, presented the medal to Mr. Hammond, who made a felicitous reply.—The Treasurer (A. B. Kempe, F.R.S.), after having read his Report, announced to the meeting that the Society's application to the Privy Council for the grant of a charter had failed.—The following were elected to act as the Council for the ensuing session:—President: Sir J. Cockle, F.R.S. Vice-Presidents: Dr. J. W. L. Glaisher, F.R.S., Prof. Hart, and Lord Rayleigh, Sec.R.S. Treasurer: Mr. A. B. Kempe, F.R.S. Hon. Secs.: Messrs. M. Jenkins and R. Tucker. Other Members: Messrs. A. Buchheim, E. B. Elliott, A. G. Greenhill, J. Hammond, J. Larmor, C. Leudesdorf, Captain P. A. Macmahon, R.A., S. Roberts, F.R.S., and J. J. Walker, F.R.S.—The following communications were made:—On pure ternary reciprocants and functions allied to them, by E. B. Elliott.—On the general linear differential equation of the second order, by the President.—On the stability of a liquid ellipsoid which is rotating about a principal axis under the influence of its own attraction, by A. B. Basset.—On modular equations and geometry of the quartic, by R. Russell.—The differential equations satisfied by concomitants of quantics, by A. R. Forsyth, F.R.S.—On the stability or instability of certain fluid motions (ii.), by Lord Rayleigh, Sec.R.S.—Notes on a system of three conics touching at one point, by Dr. Wolstenholme.

Geologists' Association, November 4.—Mr. F. W. Rudler, President, in the chair.—The President delivered the opening address of the session, entitled "Fifty Years' Progress in British Geology." He drew a picture of the state of geology in 1837, and contrasted it with that in 1887. The principal questions discussed were the old controversy between the Catastrophists and Uniformitarians, the development of Palæozoic geology, the origin of the Drift, and the antiquity of man. In recent years the warmest discussions have referred to the Archæan rocks and to the Glacial Drift. Attention was directed to the debt which geology owes to engineering, especially to the development of our railway system and to artesian borings. The sub-Wealden exploration was explained, and a Jubilee boring suggested. Deep-sea exploration was touched upon. Turning to petrology, its low condition in 1837 was pointed out, and its recent development traced to the introduction of microscopic methods of research. The history of palæontology was briefly sketched, special attention being called to the work of the Palæontographical Society. Improvements in the Geological Department of the British Museum were noticed, and reference was made to the history of the Geological Survey and the Museum of Practical Geology. In conclusion, it was pointed out that by a happy accident the meeting of the International Geological Congress in London next year will coincide with the centenary of the foundation of British geology—the original publication of Hutton's "Theory of the Earth" in 1788.

Chemical Society, November 3.—Mr. William Crookes, F.R.S., President, in the chair.—The following papers were read:—Note on the atomic weight of gold, by Prof. T. E. Thorpe, F.R.S., and Mr. A. P. Laurie.—The interaction of zinc and sulphuric acid, by Mr. M. M. Pattison Muir and Mr. R. H. Adie.—Note on safety-taps, by Mr. W. A. Shenstone.—Note on Guthrie's compound of amylene with nitrogen peroxide, by Dr. A. K. Miller.—The dehydration of metallic hydroxides by heat, with special reference to the polymerization of the oxides and to the periodic law, by Prof. Carnelley and Dr. James Walker, University College, Dundee.—The bromination of naphthalene β-sulphonic acid, by Mr. G. Stallard.—The constitution of the three isomeric pyrocresols, by Dr. W. Bott.—Preliminary note on certain products from teak, by Mr. R. Romanis.

PARIS.

Academy of Sciences, November 7.—M. Janssen in the chair.—On a paradox analogous to the St. Petersburg problem, by M. J. Bertrand. The paper deals with the doctrine of probabilities, and shows that, if a gambler plays under conditions involving all but inevitable ruin, equity requires the remotely contingent prize to be infinite.—On the state of the potassa in plants, in the soil and vegetable humus, and on its quantitative analysis, by MM. Berthelot and André. These studies have been undertaken to determine how far the potassa present in plants and arable land is in the condition of salts soluble in water, or of insoluble salts capable or not of resisting the action of attenuated acids. The researches are in continuation of those already described connected with the analysis of the soluble and insoluble carbon present in the soil, and of the nitrous compounds in their various forms of nitrates, free ammonia, &c.—Inquiry into the two fundamental principles of the accepted doctrines regarding cerebral dualism in voluntary motions, by M. Brown-Séquard. In continuation of his recent communication on this subject, the author here advances facts and arguments, some of which go directly to show that each half of the encephalon may independently serve for the production of voluntary movements in both sides of the body, while others

tend to overthrow the fundamental principles of the views generally held regarding the part played by both hemispheres in producing voluntary movements. Several interesting manifestations are described, proving that the motor effects of cerebral irritations are in absolute contradiction to the current theories.—On the *Elasmotherium*, by M. Albert Gaudry. In connection with some remains of this extinct mammal recently found on the River Kinel in the government of Samara (Russia), and presented to the Academy by M. Paul Ossoskoff, some remarks are made by the author, who assigns to the *Elasmotherium* a position intermediate in size between the mammoth and *Rhinoceros tichorhinus*, his contemporaries. In his general structure he appears to have approached more nearly to the latter animal, the radius, tibia, cubitus, calcaneum, and some other bones presenting the closest resemblance to those of a gigantic rhinoceros.—On a geometric form of the effects of radiation in the diurnal motion of the stars, by M. Gruey. A number of propositions are here announced, whose further development and demonstration are reserved for a future number of the *Bulletin Astronomique*, where a full demonstration will be given of the theorem that, in a sidereal day the apparent position of a star describes a conic section round its true position.—On the internal temperature of glaciers, by MM. Ed. Hagenbach and F. A. Forel. The different temperatures determined by careful experiment in the Arolla glacier are explained by the varying pressure to which different parts of the glacier are subjected. The normal temperature below zero is shown to be the effect of pressure, which lowers the melting-point of ice, thus verifying in Nature facts already theoretically demonstrated by Sir W. Thomson and others, but hitherto studied only in the laboratory.—Remarks on the Gulf Stream, by M. J. Thoulet. Comparing his own observations made on board the *Clorinde* in 1886 with those of Mr. Buchanan during the *Challenger* Expedition, the author finds that the Gulf Stream is comparable to a river with a greater fall in its upper than in its lower reaches. A relatively steep valley separates it on the left from the United States current setting southwards from Newfoundland, while its more gently sloping right bank skirting the ocean presents a much broader expanse. Thus is explained the direction of the driftwood carried from America towards the north-west coast of Europe.—Researches on the distribution of temperature and of barometric pressure on the surface of the globe, by M. Alexis de Tillo. The author describes some general charts which he has prepared, based on the labours of M. Léon Teisserenc de Bort, and of Herr J. Hann, of Vienna, showing the mean isobars and isothermal lines for the year, and the months of January and July, for the whole world. For the general conditions of the terrestrial atmosphere he finds that, when the mean temperature $\left.\begin{matrix}\text{falls}\\\text{rises}\end{matrix}\right\}$ within the limits of 1°·6 and 4°·7, the pressure $\left.\begin{matrix}\text{increases}\\\text{diminishes}\end{matrix}\right\}$ to the extent of 1 millimetre.—On the metallic derivatives of acetylacetone, by M. Alphonse Combes. From the researches here described, the author concludes that this substance decomposes all the carbonates, even that of potassa; that it displaces the acetic acid of the acetate of copper, and even the hydrochloric acid; that it consequently acts as a strong acid on the metallic salts. Nothing, so far, distinguishes its action from that of a monobasic acid, although this function is clearly distinguished by certain properties of its salts from the acid function properly so called.—On the part played by the stomata in the inspiration and expiration of gases, by M. L. Mangin. From the experiments here described the author concludes generally that the stomata are indispensable for the circulation of the gases in aërial plants, the occlusion of these orifices bringing about a greater or lesser diminution in the exchanges of the respiratory gases, and a very considerable decrease in the exchanges of chlorophyllian gases.—On the invasion of *Coniothyrium diplodiella* in 1887, by MM. G. Foex and L. Ravaz. This organism, already observed in 1879 by Spegazzini in Italy, and in 1885 by Viala in the department of the Isère, has this year invaded an extensive region in the South of France. Whether it is a true parasite, or a saprophyte, or whether it assumes both of these characters according to circumstances, is a point which has not yet been decided.

BERLIN.

Meteorological Society, November 1.—Prof. von Bezold, President, in the chair.—Dr. von Helmholtz discussed his most recent researches on the formation of mist under the influence of chemical processes, and laid stress at the same time upon the relation of his results to the phenomena of meteorology.—Dr. Sprung gave an account of observations made with thermometers attached to various barometers. During a comparison of the barometers from various stations with a normal barometer, the experiments being conducted in a cellar, he found that the thermometers showed considerable differences in their readings; their differences were still observed when the comparison of the barometers was made in a room at the surface of the earth, and the barometers were placed side by side in the same frame. The speaker was hence led to compare three thermometers, of which one was surrounded by a nickel-plated cylinder; the second was surrounded by a varnished cylinder, and the third had no covering at all. When placed near an open window the instrument with the nickel-plated covering registered the highest temperature, but when placed near a hot stove it recorded the lowest. The differences in reading varied at different times of the year, and amounted to several degrees. In practice these differences of the thermometer-reading can have no influence on the reading of the barometer, since it may be assumed that the mercury in the barometer has always the same temperature as that indicated by the thermometer, and that the reading of the barometer is reduced to zero.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

A Practical Treatise on Bridge Construction: T. Claxton Fidler (Griffin).—The Real History of the Rosicrucians: A. E. Waite (Redway).—Calendar of University College, Nottingham, 1887–88.—Totemism: J. G. Frazer (Black).—Animal Magnetism: Binet and Féré (Kegan Paul).—Living Lights: C. F. Holder (Sampson Low).—L'Homme avant l'Histoire: Ch. Debierre (Baillière).—The Flora of Howth: H. C. Hart (Bruges, Figgis, and Co.).—Lectures on Bacteria: A. De Bary; second improved edition, translated by H. E. F. Garnsey, revised by I. B. Balfour (Clarendon Press).—The Final Results of the Triangulation of the New York State Survey (Albany, N.Y.).—Catalogue of the Moths of India, part 1: Cotes and Swinhoe (Calcutta).—China in America; a Study in the Social Life of the Chinese: S. Culin (Philadelphia).—Catalog der Conchylien-Sammlung, Vierte Lief (Paetel, Berlin).—Fishery Barometer Manual: R. H. Scott (Eyre and Spottiswoode).—Folk-lore Journal, vol. v. part 4 (Stock).—Proceedings of the Royal Society of Edinburgh, No. 123.—Journal of the Royal Agricultural Society, October (Murray).—Archives Italiennes de Biologie, tome viii. fasc. iii. (Loescher, Turin).

CONTENTS.

	PAGE
Politics and the Presidency of the Royal Society	49
The Storage of Electrical Energy. By Prof. John Perry, F.R.S.	50
Fritsch's Crustacean Fauna of the Chalk of Bohemia	51
Our Book Shelf:—	
Dana: "Manual of Mineralogy and Petrography"	53
Letters to the Editor:—	
"A Conspiracy of Silence."—The Duke of Argyll, F.R.S.	53
The Theories of the Origin of Coral Reefs and Islands. T. Mellard Reade	54
Earthquake at the Bahamas.—Robert H. Scott, F.R.S.; G. R. McGregor; Byron N. Jones and Cornelius S. E. Lotman	54
Researches on Meteorites. I. (*Illustrated.*) By J. Norman Lockyer, F.R.S.	55
Fairy-Rings	61
Notes	63
Our Astronomical Column:—	
The Astronomical Society of France	66
The Lick Observatory	66
Astronomical Phenomena for the Week 1887 November 20–26	66
Geographical Notes	66
Meteorological Notes	67
Gems and Ornamental Stones of the United States. By Dr. A. E. Foote	68
The October Meteor-Shower of 1887. By W. F. Denning	69
On some of the Affinities between the Ganoidei Chondrostei and other Fishes. By Dr. Nicholas Zograff	70
Scientific Serials	71
Societies and Academies	71
Books, Pamphlets, and Serials Received	72

THURSDAY, FEBRUARY 2, 1888.

THE COMPOSITION OF WATER.

DUMAS, in his well-known memoir on the gravimetric composition of water, which every student is taught, and rightly so, to regard as one of the classics of chemistry, states that of all analyses presented to a chemist that of water is the one which offers the greatest uncertainty. Critics of a certain type may possibly take exception to the literal accuracy of this remark. No one, however, will gainsay the statement that, in view of the momentous issues which depend upon our knowledge of the composition of water, this knowledge is not by any means so exact as the state of contemporary science demands. It is, of course, not merely the question of the quantitative composition of water, but the far more important matter of the relative values of the atomic weights of hydrogen and oxygen. Of all stoïchiometrical constants required by chemists nowadays, those of hydrogen and oxygen are infinitely the most important. Every chemist knows what is dependent on these ratios, and he knows too that the difficulties which their direct determination involves are well-nigh insuperable.

All the discussions within recent years on the validity of Prout's law have tended to show that so far as experimental work is concerned, the question may now be said to hang upon these particular values. It is not too much to say that, if any chemist could succeed in showing by irrefragable experimental evidence that the atomic weight of oxygen was exactly sixteen times that of hydrogen, he would in the present state of scientific opinion at once succeed in inducing his brethren to accept Prout's law and all its far-reaching consequences as articles of their chemical creed.

It may be worth while to examine very briefly the nature of the ground upon which the present accepted values for the relative atomic weights of hydrogen and oxygen are based. It will be generally conceded that the evidence upon which chemists have almost exclusively relied is that afforded by Dumas' gravimetric analysis of water, and by Regnault's determination of the specific gravities of oxygen and hydrogen.

Dumas' work was published in 1843. His method was identical in principle with that employed by Dulong and Berzelius for the same purpose, and consisted, as is well known, in heating copper oxide with an unknown weight of hydrogen, and determining (1) the loss of weight suffered by the oxide, and (2) the weight of the water formed. The decrease in weight of the copper oxide was assumed to represent the weight of the oxygen evolved, and the difference between this weight and that of the water formed was held to be the amount of hydrogen which had combined with that of the oxygen.

In all, nineteen experiments were completed, in which quantities of water varying from about fifteen to eighty-six grammes were formed. Treating the results in the manner adopted by Meyer and Seubert—that is, in accordance with the equation

$$x = \frac{b_1 + b_2 + b_3 + \ldots + b_n}{a_1 + a_2 + a_3 + \ldots + a_n} = \frac{[b]}{[a]},$$

in which a = weight of oxygen used, and b = weight of water formed—it follows that $[a] = 840{\cdot}16$ grammes, and $[b] = 945{\cdot}44$ grammes, whence the ratios $H : O = 1 : 15{\cdot}96$.

When, however, we come to examine more nearly the details of the method of determination, we find that these ratios are certainly affected by errors of which the magnitude cannot be even approximately known. In the first place, the sulphuric acid solution employed to generate the hydrogen must have contained dissolved air, the effect of which would be to lower the ratio of the oxygen. This fact was not indeed unnoticed by Dumas, but its effect could not be estimated with any certainty. Moreover, it seems almost impossible to prepare hydrogen from zinc and sulphuric acid without the formation of more or less sulphur dioxide, the last traces of which can only be removed by prolonged exposure to potash solution. Copper is one of the few metals that have the power of forming a hydride, and although this hydride, like the palladium hydride, is more or less readily decomposed by heat, the affinity of hydrogen for copper may be still traceable even at moderately high temperatures. Melsens, working in Dumas' laboratory, found that the reduced copper did actually retain hydrogen, and in amount varying with the temperature to which it had been heated. The weight of the condensed water must have been increased, as Berzelius pointed out, by the air which it eventually dissolved. Now the effect of all these errors would be to lower the value for the atomic weight of oxygen. Of course there may have been errors working in the opposite direction of which we know nothing, but it is reasonably certain that the net result of the constant errors, so far as these can be ascertained, is to give too small a value for oxygen. Above all, there are the fortuitous errors, such as those caused by differences in the power of surface-condensation of the vessels employed; errors of weighing and of reduction to a standard atmosphere, &c.; which, although theoretically allowed for and eliminated by a sufficiently frequent repetition of the experiments, may, on the whole, tend to operate in a given direction. Lastly, there is a source of error of the same order in a circumstance which, as there is a certain touch of pathos in them, may be stated in Dumas' own words:—

"Il faut même ajouter que la durée nécessaire de ces opérations, en m'obligeant à prolonger le travail fort avant dans la nuit, en plaçant les pesées vers deux ou trois heures du matin dans la plupart des cas, constitue une cause d'erreur réelle. Je n'oserais pas assurer que de telles pesées méritent autant de confiance que si elles avaient été exécutées dans des circonstances plus favorables et par une observateur moins accablé de la fatigue inévitable après quinze ou vingt heures d'attention soutenue."

There is, above and beyond all, a fundamental flaw in the principle of the method, of which Dumas himself was fully conscious. After having declared that of all analyses presented to a chemist water is the one which offers the greatest uncertainty, he goes on to state to what this uncertainty is due:—

"En effet, 1 partie [d'hydrogène se combine avec 8 parties d'oxygène pour former de l'eau, et rien ne serait

plus exact que l'analyse de l'eau, si l'on pouvait peser l'hydrogène et peser l'eau qui proviendrait de sa combustion. Mais l'expérience n'est pas possible sous cette forme. Nous sommes obligés de peser l'eau formée, et l'oxygène qui a servi à la produire, pour en déduire, par différence, le poids de l'hydrogène qui en fait partie. Ainsi, une erreur de 1/900 sur le poids de l'eau, ou de 1/800 sur le poids de l'oxygène, affecte d'une quantité égale à 1/90 ou à 1/80 le poids de l'hydrogène. Que ces erreurs étant dans le même sens viennent à s'ajouter, et l'on aura des erreurs qui iront à 1/40."

Let us now turn our attention to the evidence afforded by Regnault's determinations of the densities of oxygen and hydrogen. Prof. Le Conte has detected some slight numerical errors in Regnault's reductions (*Phil. Mag.* [4] 27–29), and when the necessary corrections are made it follows that the density of oxygen is 1·105612, and that of hydrogen 0·069269; whence, on the assumption of Avogadro's law, we have the ratio O : H = 15·9611 : 1. This result is in such striking agreement with Dumas' value that it is generally held to afford the strongest corroboration of it. The number given for oxygen is probably among the most accurate of Regnault's determinations of gaseous densities; the subsequent results of Von Jolly, which are alone comparable in character with those of Regnault, when reduced to the geographical position of Regnault's laboratory have not materially altered the value. The number given for hydrogen is certainly not entitled to the same degree of confidence. Indeed, it has been stated that Regnault was himself of this opinion, on account of the great difficulty of procuring hydrogen free from air. It is hardly necessary to point out that even an extremely minute admixture of air would tend to lower the relative value of the atomic weight of oxygen. Moreover, the hydrogen in the course of its preparation must have been saturated with moisture; and although, of course, all precautions at that time known were taken to dry the gas, it is quite certain that it could not have been absolutely free from traces of water. The experiments of Dixon have shown how extremely difficult it is to dry a gas perfectly, and it is now recognized that the ordinary methods of desiccation still leave appreciable traces of moisture in it. The effect of this moisture in the case of hydrogen would be to increase its density, whereas in the case of the oxygen it would tend to decrease it. On the other hand, oxygen and hydrogen when measured under the standard conditions of temperature and pressure are not, strictly speaking, under exactly comparable conditions, and the assumption of the validity of Avogadro's law is not mathematically correct.

Within recent years the question of the composition of water has been again attacked, and with a fuller knowledge of the various sources of error which the progress of science has shown to be present in the older methods. Julius Thomsen found that 1 litre of dry hydrogen, measured under standard conditions of temperature and pressure, when burnt with oxygen gave, as the mean of eight concordant experiments, 0·8041 grammes of water. Accordingly, 2 litres of hydrogen, on combining with oxygen, would give 1·6082 grammes of water. Assuming the validity of Gay-Lussac's law, and using Regnault's values for the weights of the gases at standard temperature and pressure, the calculated weight becomes—

2 litres hydrogen	= 0·1791	grammes
1 litre oxygen	= 1·4298	,,
water	= 1·6089	,,

The difference is 0·7 milligramme. But the question may be immediately asked, "Is Gay-Lussac's law actually valid?" The work of Regnault and Amagat on the relation of volumes of gases to heat and pressure indicates that, as ordinarily stated, it cannot be absolutely valid. Dr. A. Scott has recently put the question to the test of experiment, and, from a long series of trials in which large volumes of gases were caused to combine, he finds that the most probable ratio is 1·994 : 1 (Proc. Roy. Soc., 1887, 398). Taking Regnault's data as before, we have—

1·994 litres hydrogen	= 0·1876	gramme
1 litre oxygen	= 1·4298	,,
water	= 1·6084	,,

which differs only by 0·2 milligramme from Thomsen's result.

Now, from Regnault's densities of oxygen and hydrogen, as recalculated by Prof. Le Conte, it follows that the weights of equal volumes of the gases are as 1 : 15·9611, which, on the basis of Dr. Scott's ratio for the combining volumes, gives—

$$O = 16{\cdot}009.$$

Prof. J. P. Cooke and Mr. T. W. Richards, of Harvard College, have recently presented us with a further contribution to the subject (Proc. Amer. Acad. of Arts and Sciences, xxiii. 149), which merits very special attention, not only on account of the intrinsic excellence of the experimental work of which it is an account, but also because it is here attempted to obviate certain of the sources of error which have already been pointed out as inherent in Dumas' method. The method adopted by the American chemists was to pass a known weight of hydrogen over heated copper oxide and to weigh the amount of water formed. It will be seen that the essential feature in this method is that the weight of the hydrogen is known whilst that of the oxygen is obtained by difference; in contradistinction to the method of Dumas, where the weight of the oxygen was known and that of the hydrogen found by difference. The preparation of this hydrogen and the determination of its weight were, however, problems which required the highest manipulative skill. Obviously, everything depends upon the purity of the hydrogen. A glass globe of about 5 litres capacity and weighing about 570 grammes was so provided with stopcocks that it could be evacutated by the air-pump. The vacuous globe was weighed against a similar globe, in the manner already adopted by Regnault, filled with hydrogen, and its weight again determined. The weight of hydrogen taken was about 0·42 gramme. The hydrogen was then driven over the heated copper oxide by a current of dry air, and the water formed collected partly in a weighed tube, and partly by means of sulphuric acid and phosphoric oxide. The hydrogen was obtained by three different methods: (1) by the action of sulphuric acid

upon zinc; (2) by means of caustic potash and aluminium; and (3) by electrolysis. In all, sixteen experiments are given. The results are stated in the following table:—

Series.	No. of Experiments.	Sum of the weights of hydrogen. grammes.	Sum of the weights of water. grammes.	Atomic weight of oxygen. max.	min.	Calc. from sums.
I.	5	2·0876	18·7406	15·977	15·937	15·954
II.	5	2·0803	18·6740	15·962	15·942	15·953
III.	6	2·5350	22·7541	15·967	15·937	15·952

The final mean is O = 1 0·0017.

This very bald account does but scanty justice to the beauty and simplicity of the methods adopted by Prof. Cooke and Mr. Richards, and to the manipulative skill and patience with which they carried them out. With respect to the bearing of their result on Prout's hypothesis, the question seems to them to narrow itself to this point: Is the hydrogen they have made use of the typical hydrogen element? They are inclined to believe that the theoretical question in regard to Prout's law has been settled so far as analytical work can solve the problem. On this point, however, we are at issue with them. That statement implies a finality about our present quantitative methods which we have no right to assume. It implies, too, that the methods employed by the authors have yielded as close an approximation to the typical element as we are ever likely to obtain. Their method in principle no doubt removes one fundamental objection to Dumas' plan of work, considered as an experimental process, but it by no means removes all the sources of error, and anybody who will patiently sift out these sources and seek to appreciate their net effect must admit that the ultimate tendency is to apparently lower the relative value of the atomic weight of oxygen.

If we have regard to this fact, and if we consider too what we may call the volumetric evidence, as given above, it seems premature to assume that the ultimate question has actually been narrowed down to the point to which Prof. Cooke and Mr. Richards are disposed to conclude that they have brought it. T. E. THORPE.

PHYSICAL GEOGRAPHY OF THE SEA.

Handbuch der Ozeanographie. Von Dr. Georg von Boguslawski und Prof. Dr. Otto Krümmel. Two Vols. (Stuttgart: Engelhorn, 1884–87.)

THESE volumes belong to a series of geographical hand-books—each written by an acknowledged master of the subject—brought out on a uniform plan, under the editorship of Prof. Dr. F. Ratzel, by the well-known Stuttgart publishing firm of Engelhorn.

Dr. Ratzel's own volume on Anthropogeography, Dr. Hann's on Climatology, and Dr. Heim's on Glaciers have already appeared; and now, under the somewhat novel name of "Oceanography," the physical geography of the sea has been exhaustively treated. The editor has divided the subject into two parts, and given each to a specialist. In Volume I., Prof. Dr. G. von Boguslawski, of the Hydrographical Department of the German Admiralty, treats of the distribution, physical condition, and chemical composition of sea-water; while in Volume II., Dr. Otto Krümmel, Professor of Geography in the University of Kiel, discourses on the motions of the ocean. Each part is complete in itself, but the index to both is given only in the second volume.

It is somewhat difficult to give an idea of the multifarious contents of this exhaustive treatise. In Volume I., Dr. Boguslawski begins with an account of the shape and area of the different oceans, and then naturally describes successively the physical character of their various coast-lines, and the depths of the sea all over the world. But we must remark on the poverty of illustration and the absence of maps which characterize this and so many other first-rate German books. Here we have actually no map, however rough, to show graphically the depth of the oceans. The unfortunate reader who wants to get his information as easily and quickly as possible has first to read through about 100 pages of closely-printed type, and then to try and picture to himself the relief of the floor of the sea.

After discussing ocean depths, the author devotes a short chapter to the chemical composition of salt water; and then a somewhat longer space to the density or specific gravity of the sea. Colour and transparency are next discussed, and the last 200 pages are occupied with what is called maritime meteorology.

Of this space only thirty-four pages are devoted to wind and storms, and very rightly, as these subjects would require a special volume for their proper treatment. The remainder is occupied with a minute account of the temperature of the ocean, both on the surface and at various depths; and with a notice of the distribution of ice in high latitudes. Here too, the value of the admirable text is greatly diminished by the absence of maps.

So far no mathematics have been required, but it is impossible to treat of the motions of the sea without algebraical formulæ. Dr. Krümmel, however, uses great judgment in only giving the formulæ of motion, which only involve simple algebra, and not the investigation of the formulæ, that would require much higher analysis.

In the second volume he begins with the consideration of waves. The theory of wave-motion, both in deep and shallow water, he gives first, mostly following Airy; while experimental illustrations, and observations on the actual length, height, and speed of waves follow next. Breakers and rollers are then discussed, together with their influence on the abrasion of coast-lines. Earthquake and volcanic waves are illustrated by a self-recorded tidal trace from South Georgia, which will be new to English readers; and the section ends with a capital account of stationary waves, *sêches*, and of the curious tidal phenomena in the Straits of Euripus, which so puzzled the ancient Greeks.

The author then turns to tides, dealing mostly with the theories of Laplace, Whewell, Ferrel, Airy, Thomson, &c., but very wisely ending with a chapter on "unsolved problems." The difficult subject of the vertical circulation of the ocean is next discussed, and an admirable account given of the cold *aufreibwasser*—up-rubbed water—of tropical weather coasts. This uneuphonious term is applied to the cold water that is found

close to many tropical shores off which the wind blows steadily. For instance, near Cape Guardafui, when the south-west monsoon blows off shore, cold water is found near the land, but when the north-east monsoon blows on shore nothing but warm water can be discovered. The theory is that an off-shore wind blows, or rubs, the sun-heated surface water to leeward, and that the proper level of the sea is maintained by cold water welling up from below. Mr. J. Murray, of the *Challenger*, has discovered a similar effect in the long, narrow, deep waters of Loch Ness. With a south-west wind the coldest water is at the south-west end of the lake, but when the wind changes to north-east the lowest temperature is found at the north-east extremity.

The remainder of the work is taken up by a description of the currents of the ocean. The theory, of course, is fully given, and we may note that the author uses Ferrel's formula for the deflection of a moving particle to the right, through the influence of the earth's rotation, which has been accepted in every country except England. The long detail of the currents in different oceans of course contains little novelty, but is illustrated by an excellent map in blue and red of the direction and velocity of these well-known cold and hot streams.

Both of these volumes are to a certain extent uncritical compilations, for the results of various experiments and observations are merely recorded, without any comment on the varying quality of the work. We have already commented on the absence of maps; and the instruments used in oceanic research might well have been much more copiously illustrated. Still this work is a most valuable addition to the literature of the subject, and we wish that it could be translated into English.

There is no text-book of the subject in England, beyond School-Board primers, except the work of Maury; and this, in spite of a fascinating style, is too fanciful, and too much out of date, to be of any use.

Though the volumes now under review can never be popular in the ordinary sense of the word, still they would be invaluable to scientific men and others, who though not specialists wish to study in a compact and available form the present state of knowledge of one of the most interesting branches of modern research.

RALPH ABERCROMBY.

BULLETIN OF THE UNITED STATES FISH COMMISSION.

Bulletin of the United States Fish Commission. Vol. VI., for 1886. (Washington: Government Printing Office, 1887.)

THE immense number of short articles in this volume are as usual classified in a topical synopsis of the contents. The largest class is that of articles concerning the fisheries, the next in size contains those concerning aquiculture, the next those concerning natural history, and there are two other classes headed U.S. Fish Commission -General, and Miscellaneous. Of the biological articles Mr. John A. Ryder contributes only three, and the reader regrets there are not more from his hand. One is on the early development of the toad-fish, *Batrachus tau*, whose eggs are described as adherent, being fixed to the under surface of submerged boulders. The young toad-fish have this unique peculiarity, that when the egg-membrane bursts they are not set free but the lower surface of the yolk-sac remains firmly fixed to the adherent portion of the membrane, and this adhesion continues until the yolk-sac has become almost entirely intra-abdominal. The second of Mr. Ryder's papers is on the cleavage of the blasto-disk in the ovum of *Raja erinacea*; and the third on the intra-ovarian gestation of the viviparous *Sebastes marinus*: this last is based upon the examination of a gravid specimen obtained by the *Albatross*.

The few articles on the reproduction and generative organs of eels are of little value, as the information contained in them is not up to date. One, for instance, is a translation of a paper by Prof. Pavesi, published in 1880, and therefore of course treating as probabilities propositions concerning the testes which were proved in 1881 by Otto Hermes.

Among the aquicultural articles there are a great many on the shad-hatching work of the Commission, most of them detailing statistics of the operations of 1886. In one of these Marshall McDonald announces that for the entire period of the Commission's work up to and including 1882, 200,000,000 of young shad were produced, while for 1886 alone the total was 90,000,000, and this last number was fifteen times as great as the number of adult shad captured for market in one season. In another report by the same writer we find that the cost of production of shad-fry was $127·66, or about £25, per million. The exact effect of the artificial production of shad-fry on the supply of the adult fish is not estimated, but in one place we find that the catch in the Potomac was much larger in 1886 than in 1885; and in another that in Connecticut pollutions and sewage are diminishing the number of shad in the rivers.

Evidence is given that shad are now fairly abundant on the whole coast of California, apparently from plantings in the River Sacramento, but no regular run of shad seems to have been produced in that river; and we find statistics of plantings in 1886, in the Columbia and Colorado, from which a better result is expected. But of course the Bulletin is not the place to look for a connected and logical discussion of the operations carried out and their results. The publication contains occasional notes and statistics which are interesting to those who are familiar with the matters to which they belong, and which place on record facts which form materials for a connected study.

Of the very large amount of information comprised under the heading Fisheries, we cannot say more here than that it includes details and statistics not only of American fisheries but of those of all parts of the world.

OUR BOOK SHELF.

Flour Manufacture: a Treatise on Milling Science and Practice. By Friedrich Kick; translated by H. H. P. Powles. (London: Crosby Lockwood and Son, 1888.)

THE art of flour-milling, which of late years has undergone changes in its method of the most marked character, has at no time been productive of anything like a copious technology; and, in the attempt to supply this deficiency, it was natural that Mr. Powles should turn his regard towards Austria, where the manufacture of flour had engaged the attention of scientific experts long before the

necessity for systematic inquiry into its processes became obvious in this country. The publication of Dr. F. Kick's supplement to his treatise "Die Mehlfabrication," which tabulated the improvements in machinery for preparing and grinding cereals introduced up to the year 1883, placed at the disposal of the translator a manual complete in its investigations into the nature of grain from the miller's technical standpoint, and into the best means of reducing it to flour. It is true that the book does not concern itself with the construction of the mill building nor with the motive power to be employed; but, from this point onward, the leading principles which should guide the milling engineer are carefully and accurately related, and their application justified when necessary by mathematical demonstration; the *rationale* at the same time being within the comprehension of the practical miller. Of this method the chapters on "balancing millstones" (p. 113), and on "disintegrators" (supplement, p. 25), afford admirable examples. The various operations of grain preparation, grinding, and of bolting, sifting, and dressing the meal, with descriptions and plates of the best known machines employed, are fully detailed, whilst the controversy between the advocates of "high" milling and "low" milling is discreetly adjusted by the author in the incidental remark that "which of these methods is to be used can only be settled by the local demand, if, as is generally the case, the mill works for the home market."

It is, however, to those portions of the work which relate to roller-mills that the reader at the present time will probably turn in the first instance. He will find here, not only information as to the various kinds in use and as to the manner in which they have been found to perform their work, but an intelligible account of the operations involved in the reduction of cereals by rollers, and good reason shown why the time honoured millstones have become almost entirely discarded in the manufacture of wheaten flour.

The book is very fully illustrated by woodcuts throughout the text, and by some thirty supplementary sheets of diagrams; whilst a preliminary chapter contributed by Dr. August Vogel, of Vienna, on the histology of farinaceous grains, adds completeness to the work.

We congratulate the translator on his introducing to the English reader a volume of the utmost value to millers and engineers, and of great interest to many other persons more or less concerned with this important industry.

Elements of Chemistry: a Text-book for Beginners. By Ira Remsen. (London: Macmillan and Co., 1887.)

OPINIONS no doubt differ much as to what is simple enough for a beginner. A good deal depends on the age of the beginner. We hold, in opposition to the author in his preface, that the present production is not well adapted for very young pupils.

There is a good deal of promise in the book which might be better fulfilled, and there is an attempt to cover far too large a field, with the result—not intended by the author—that it reads more like a book on general chemical information than an elementary introduction to chemistry.

Metals and non-metals are dealt with under "family" groups, and most of their common, and many uncommon, compounds described, generally with formulæ, and this in cases and with equations which cannot be termed simple; for instance, technical processes like soda-making, or bleaching powder, or potassium chlorate, or nitro-benzene, &c. Otherwise the order and arrangement of matter and the questions attached to each section are most excellent, and the book would be most useful even for general reading, exercise, and information on the chemistry of common things to the great mass of partially informed, ordinarily well educated, people of any age. To the senior boys of public schools, who have already had a little instruction in science, this book would be really useful, as taking them in a different manner over ground already partially covered, widening their general knowledge, and cultivating the main thing, "*thinking.*"

A Primary Geometry, with Simple and Practical Examples in Plane and Projection Drawing, and suited to all Beginners. By S. E. Warren, C.E. (New York: Wiley and Sons; London: Trübner, 1887.)

THIS work bears as motto, "Geometry should be begun as early and as simply in behalf of industrial life as arithmetic is in behalf of business life"; and its object is, accordingly, to contribute to a general earlier beginning of the study of geometry. "The truths of *form*, as needed in *drawing*, have been made prominent, while not neglecting elementary ones of *measure*."

The text treats of straight lines, triangles, regular figures, areas, lines and planes in space, the elementary bodies, and projections of elementary solids, the subject being considered in a common-sense fashion without much use of purely geometrical proofs. Having perused a very large portion of his book without detecting any flaw, we consider the author competent for the task he has undertaken, but we do not take kindly to such presentments of geometry. We believe, however, the book to be well adapted to junior pupils as an introduction to the study, and also to artisans and others who are likely to be able to grasp the illustrations given better than they would purely geometrical proofs for which their antecedents have not prepared them.

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 Duke of Argyll's Charges against Men of Science.

I REGRET to find that the Duke of Argyll has once more evaded the point at issue. The question is one not of *formulas* but of *facts*. If the statements upon which his Grace bases the severe strictures of his "Great Lesson" were *true*, I for one should take no exception to any "metaphorical or rhetorical expression" by which he chose to enforce his lesson.

Three months have elapsed since the Duke's attention was directed to the discussions which during the last seven years have taken place upon the subject of Mr. Murray's theory of coral reefs—and especially to that one in which the Director-General of our Geological Survey, and the most eminent of American geologists, Prof. J. D. Dana, bore the leading parts; the Duke has been referred to the scientific journals in which this and the other discussions have been carried on; and the fact has been pointed out to him that all the principal text-books of geology, foreign as well as British, which have been published since the theory was announced, have given it a prominent position in their pages. In the face of these facts, is the Duke of Argyll still prepared to maintain that, with respect to the theory in question, there has been "a grudging silence as far as public discussion is concerned"; that there has been "a silence of any effective criticism"; and that "no serious reply has ever been attempted"? If his Grace admits that he was mistaken in making these assertions, is he prepared to withdraw them and also the comments which he has based upon them?

Instead of doing anything of the kind up to the present, the Duke of Argyll has fathered two stories about the wrong-doings of geologists—both of which stories have as little foundation in fact as his statements in "the Great Lesson."

The first of these stories is related in very circumstantial terms, but without any authority being given for it. It is said that a Fellow of the Geological Society offered a certain paper, which the authorities of the Society refused; and it is asserted that the reason of their refusal was that the paper "was not orthodox," and "they probably smelt heresy." Now the Duke of Argyll is well aware that every Fellow of the Geological Society has the right to present papers for reading, and that the responsibility for accepting or refusing papers rests in the first instance with the President; but he, in the case of exercising his veto, is bound to report the fact, and the reasons for his action, at the next meeting of the Council. The records of the Society show that no such paper was ever offered to it; that the President never exercised his right of veto; and that the Council never discussed the grounds of the supposed refusal. The Duke of Argyll has been informed of these facts, but he has not yet retracted the very serious charge which he has made affecting the honour and good faith of the President and the other twenty-two members of the Council of the Geological Society.

In the case of the second story circulated by the Duke of Argyll, the authority is given. The complaint is made that since 1862 "advanced geologists" have "ignored" views which "tend to dethrone" their own "pet theories." Anyone who chooses to refer to the *Philosophical Magazine* for 1862 will see that the "pet theories" in question are those relating to the antiquity of man; that the "advanced geologists" implicated in the charge must have been the late Sir Charles Lyell, Prof. Prestwich, and those who have followed up their researches and arguments; and that the "views" which they "ignored" were the suggestions which I described in my last letter!

JOHN W. JUDD.

The Total Eclipse of the Moon of 1888 January 28, as observed at Birr Castle Observatory, Parsonstown.

THE total eclipse of the moon on Saturday last was, like its predecessor in 1884 (see NATURE, vol. xxx. p. 589, and Trans. Royal Dublin Society for October 1885), favoured by a very clear sky during the whole time of its progress, so that very extensive observations of the changes of the moon's heat in consequence of the passing over of the earth shadow could be made. The apparatus used was essentially the same as that used before; yet the two old thermopiles had been replaced by two new ones especially made for this occasion by the Earl of Rosse.

The observations began at 7h. 19m. M.T. Greenwich, and were, as much as possible, uninterruptedly continued till 15h. 45m.

During this time 638 distinct readings of the galvanometer were obtained, which, when fully reduced, will enable a very satisfactory heat-curve to be drawn. A few preliminary results, reduced to zenith, I communicate at once.

Galvanometer.		
739·4	...	1h. 10m. before first contact with penumbra.
663·4	...	24m. " " "
624·1	...	First contact with penumbra.
252·1	...	" shadow.
34·9	...	22m. before beginning of total phase.
30·2	...	22m. after " "
231·9	...	Last contact with shadow.
545·6	...	" penumbra.
540·8	...	1h. 34m. after last contact with penumbra.

From these figures it will be seen—

(1) That the heat radiated by the moon begins to decrease a considerable time before the first contact with the penumbra.

(2) That 22m. before the beginning of totality the heat is only 4·7 per cent. of the value obtained 1h. 10m. before the first contact with the penumbra. Unfortunately an unforeseen stoppage of the driving-clock prevented the observations from being carried on closer up to and during the total phase.

(3) That in spite of the rapid fall on approach to totality, the heat, after the last contact with the penumbra, does not at once increase to anything like the value observed at corresponding times before the first contact.

It is worth remarking that points 2 and 3 are confirmatory of the results arrived at in 1884.

OTTO BOEDICKER.

Birr Castle Observatory, Parsonstown, January 30.

"Elementary Chemistry," and "Practical Chemistry."

I CRAVE leave from the Editor for space in which to reply, on my own behalf and on that of my fellow-authors Messrs. Slater and Carnegie, to the charges brought by "H. E. A." in NATURE of January 19 (p. 265) against our method of teaching chemistry. At the outset I thank "H. E. A." for the patience which, as he publicly announces, he has shown in waiting for the publication of these books, and I condole with him in his disappointment. Like him, I too am waiting patiently; I trust my disappointment will be less bitter.

One of the important points in our plan of chemical teaching is the connection of the work in the laboratory with the student's reading and lecture-work. To emphasize this connection, and to make our course run fairly smoothly, we have published two books, one to be used in the laboratory, the other to be used in the lecture-room and in reading in connection with the whole work of the student. "H. E. A." acknowledges the advantages of this division, but throughout his review he ignores the statement distinctly made by us, that one book is complementary to the other and that both must be used together. He confines his remarks almost wholly to one of our books, viz. the "Practical Chemistry"; and yet he condemns our system of teaching. On this ground alone I claim that his review is misleading and unfair. I go further, and assert that "H. E. A." has condemned our system without acquainting himself with its essential features. He says that "in the earlier part of the 'Practical Chemistry' Messrs. Muir and Carnegie do not sufficiently bear in mind their own intention, and that much of the matter would find a more fitting place in the companion volume." No one reading this would suppose that almost every experiment used in Chaps. I. to VIII. of the "Practical Chemistry" is also used in Chaps. I. to IX. of the "Elementary Chemistry." Yet this is the case. In one book the experiments are described, along with others, in such terms as allow attention to be concentrated on their results and on the reasoning on these results; in the other book the experiments are described in detail in order that the student may repeat them in the laboratory. In another part of his review "H. E. A." says that most of the subjects dealt with in the third part of the "Elementary Chemistry" "ought never to have been introduced into an 'Elementary Chemistry.'" He has here made a slip: it is the third part of the "Practical Chemistry" which includes subjects not touched on in the other book. This correction involves a point of some importance. Although the preface to our "Practical Chemistry" states that the book forms part of a course of elementary chemistry, yet the student who uses both books will see that the course of work laid down in the practical book carries him much beyond the limits of treatment adopted in the other volume. There are numerous direct and indirect indications of this in the book itself, which those for whom the work is intended will not fail to notice. One cannot put the whole of one's book into the preface. I admit that it would have been better had we indicated in the preface to the "Practical Chemistry" that many experiments in Parts II. and III. are difficult to perform, and require skill and training; but I assert that the nature of the experiments themselves, the references to the original papers to be read before conducting these experiments, and the suggestions as to other work to be done preparatory to Parts II. and III. respectively, suffice to indicate to the student, although not necessarily to the reviewer, the character of the work described in the later chapters of the "Practical Chemistry."

Chapter I. of Part III. of the "Practical Chemistry" involves a repetition of some of Stas's determinations of the atomic weight of silver. "H. E. A." says that this chapter should have been included in Part I., and he adds, "the remaining chapters ought never to have been introduced into an 'Elementary Chemistry,'" kindly informing his readers that these chapters are included "because of the senior author's well-known tendency to worship physical constants." I venture to remind "H. E. A." that no election has taken place to the office of supreme pontiff of chemistry. Were that official in existence, I feel inclined to think he would admit that accurate determinations of atomic weights—and "H. E. A." allows these in the most elementary part of the course—are determinations of constants which have physical as well as chemical meanings.

"H. E. A." says that in the "Practical Chemistry" there is an "entire absence of anything approaching to a *systematic* arrangement." The boldness and baldness of the assertions made by the reviewer encourage me to meet this statement with

a direct denial. There *is* a systematic arrangement in the whole book, or rather in the whole scheme embodied in both books. Because "H. E. A." fails to discover that plan which finds favour with him, it does not follow that systematic arrangement is absent. To say that the arrangement is not that which one would like to see adopted is fair criticism; but to imply that there is no alternative between one's own system and chaos is to expose one's own ignorance. And what is the feature of that system of practical chemistry in which alone the reviewer thinks the student can find salvation? He says, "in a properly chosen series of experiments everything should be proved; no assumption should be necessary." In another part of the review he tells us that "air and the phenomena of combustion should be first studied: the composition of air should be determined, and oxygen should be *discovered* by the student . . . The composition of water should next be qualitatively ascertained." I should be deeply indebted to "H. E. A." if he would kindly describe experiments on these subjects, suited to beginners in chemistry, in which no assumptions are made, and which convey sound teaching. He must not get over the difficulty by cleverly hiding the assumptions made, and so appearing to make none; everything must be proved, and proved by experiments which the beginner can satisfactorily conduct. I hold, and I am convinced that the history of science bears me out, that all scientific reasoning starts with certain assumptions, and that in every particular train of reasoning assumptions are made. If the beginner can be taught to recognize the assumptions which are involved in his reasoning on experimental data, he will do well. In the "Practical Chemistry" we have tried to emphasize the assumptions which the beginner must make. In our opinion the fatal thing is to cover over and hide away the assumptions; by doing this, the student acquires a habit of confounding hypotheses with facts, and so unconsciously he slides into loose methods of reasoning. I fancy I can detect the effects of such a method in the whole review: has not "H. E. A." tacitly, probably unconsciously, assumed that chemical truth abides with him and with him only?

We thank "H. E. A." for indicating some points in the descriptions of certain experiments which might be improved, and also for reminding us that the drawings of apparatus are not as good as they might be. These things can and will be improved. The mistake in the description of the diffusion-experiment, on p. 30 of the "Elementary Chemistry," to which "H. E. A." alludes, has been already pointed out to us, and a slip has been inserted in all copies except the first few hundred correcting this mistake. We cannot congratulate the reviewer, nor do we think he will be inclined on second thoughts to congratulate himself, on the trifling quibbles in which he has indulged regarding one of our experiments on the electrolysis of water.

Cambridge, January 23. M. M. PATTISON MUIR.

"Physical Science and the Woolwich Examinations."

I AM afraid that the moderation of your article on the regulations for admission to the military colleges may give some readers the impression that science is merely being discouraged more or less seriously in their examinations. The fact is, however, that it is being ousted with absolute certainty, for hardly anyone can afford to take up an optional subject which is at a disadvantage of 1000 marks. Severity of competition has within the last few years quite doubled the number of marks qualifying for admission to Sandhurst, and it will soon be impossible, even if it is not so at present, for a candidate to gain a place if he takes up any subject other than Latin, French, German, or mathematics.

This making all the men fit square holes whether they are round or not can hardly be for the advantage of the service, and one's curiosity is aroused as to the reason for such retrogressive changes—whether it is due, as has been asserted, to the action of head masters who do not desire to accumulate or encourage new-fashioned lore; or whether the military authorities really opine that to an officer who may have to deal with telegraphy, to choose a camping-ground, or perhaps direct a search for water, Latin is half as important again as electricity or physical geology.

Is it really too much to expect that they might insist first on a thorough knowledge of those parts of an ordinary education which are specially necessary or helpful to an officer, and then treat the unessential subjects on an equality as far as possible, and let a boy do in his preparation as he will when a man—adequately fulfil the duties of his position, and then follow his own bent? W. A.

January 30.

"The Art of Computation for the Purposes of Science."

HAVING read with much interest Mr. Sydney Lupton's second article on this subject, I think it right to draw his attention, and that of your readers, to Table III. of my book of five-figure and other logarithms published by Messrs. C. and E. Layton in 1870.

This table was framed by me for the purpose of enabling computers who occasionally require to use logarithms to ten places to get same with as little trouble as possible, and without shifting to any other book. In fact, I believe results can be got from my table almost as quickly as from the voluminous and beautiful volume of George Vega.

For instance, referring to Mr. Lupton's example, I find from my table and the instructions that log 1·0542482375 = log 1·05 + log 1·0040459405—this by simple division; then—

By part A log 1·05	= 0·0211892991
By part B log 1·0040459405	= 0·0017535845
log 1·0542482375	= 0·0229428836

correct by Mr. Lupton's solution from Vega.

My whole table is contained in eight octavo pages, and I believe is in as narrow a compass as is consistent with utility.

I may add that in the preliminary part of my book will be found a method of finding the logarithms of all numbers by nothing more than simple multiplication.

The late Prof. Augustus De Morgan, when I showed him this Table No. III., I well remember, replied: "It is very good indeed, but you will get no one to look at it," showing how rarely logarithms are ever required for any practical use beyond five, or at the most seven, figures. E. ERSKINE SCOTT.

6 Bond Court, Walbrook, London, E.C.,
January 18.

THE articles of Mr. Sydney Lupton on the above subject, which have appeared in recent numbers of your paper, do not profess to be complete; still, as their declared object is to assist those who are not mathematicians to work sums by the aid of tables, it seems to me that the best methods should not be passed over in silence, while others that are practically obsolete are discussed at length.

I beg of you therefore to allow me to call attention to the labours of the late Peter Gray, F.R.A.S., in the direction of supplying facilities for computing logarithms and antilogarithms. He contributed papers on the subject to various magazines; notably a series (with a table for formation of logarithms and antilogarithms to twelve places) to the Journal of the Institute of Actuaries in 1865. His most important work on this subject was, however, published as an independent volume in 1876. It is entitled "Tables for the Formation of Logarithms and Antilogarithms to Twenty-four or any less number of places"; and it contains, besides the tables, an explanatory introduction and an exhaustive historical preface. The published price is only 7*s.* 6*d.*, and it is therefore not beyond the reach of those who require such tools.

Weddle's method, the last mentioned by Mr. Lupton, consists in multiplying the given number down to unity, by means of a series of factors of the form $1 - (\cdot1)^n \times r$, where r may take any integral value from 1 to 9. The logarithms of the factors are then obtained from a previously prepared table, and the complement of the sum of these logarithms is the logarithm of the given number. Weddle also used his method conversely, to calculate antilogarithms.

Hearn, of the Royal Military College, Sandhurst, improved upon Weddle's method, by substituting factors of the form $1 + (\cdot1)^n \times r$ for the computation of antilogarithms, r, as before, ranging in value from 1 to 9; but he retained the factors $1 - (\cdot1)^n \times r$ for computing logarithms.

Gray's improvements on Hearn were twofold. In the first place, he gave r the range from 1 to 999, taking for factors $1 + (\cdot001)^n \times r$, and he thereby brought within narrow compass the arithmetical work involved. In the second place, by a simple arrangement of the calculations, he showed how to use factors of the form $1 + (\cdot001)^n \times r$, instead of $1 - (\cdot001)^n \times r$,

for computing logarithms as well as antilogarithms; and thus, not only made the operations more convenient, but also caused one set of preparatory tables to be sufficient.

The principal table in Gray's book above-named consists of the logarithms to twenty-four places of all the possible factors $1 + (\cdot001)^n \times r$, up to that limit. An auxiliary table contains, also to twenty-four places, the logarithms and their complements of the natural numbers 1 to 9, these being frequently required to "prepare" the given number. A smaller table to twelve figures only appeared, as already mentioned, in the Journal of the Institute of Actuaries, and was subsequently published separately by Messrs. C. and E. Layton; but as the twenty-four-figure table can be worked quite easily to any extent up to that limit, there is no particular advantage in the smaller one.

By means of Gray's tables the work of forming logarithms and antilogarithms is reduced to a minimum, and the process is so simple that any arithmetician can perform it, the more especially as many numerical examples are given in the introduction.

London, January 23. GEORGE KING.

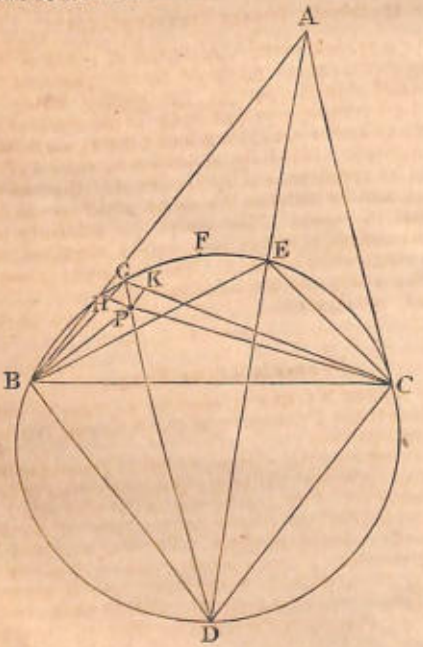
Note on a Problem in Maxima and Minima.

To find a point such that the sum of the straight lines joining it with the angular points of a given triangle shall be a minimum.

This problem was proposed by Fermat to Torricelli, who solved it, and sent it to Vincent Viviani, who also solved it, but called it a problem "quod, ut vera fateor, non nisi iteratis oppugnationibus tunc nobis vincere datum fuit."

The solution is given in Gregory's "Examples of the Differential and Integral Calculus," and in Todhunter's "Differential Calculus," pp. 240–42.

Yet it can be solved in the most elementary manner.



Let ABC be the triangle. Describe an equilateral triangle on BC on the side remote from A. Describe a circle round the triangle BCD. Join AD. Then E is the point required. Join BE, CE.

(1) It follows, from Euc. vi. D, that

$$BE + EC = ED,$$
$$\therefore BE + EC + AE = AD,$$

and evidently $\angle BEC = BEA = AEC = 120^\circ$.

(2) Let F be a point on the circumference BC.

$$BF + FC = FD \quad \text{(Euc. vi. D)},$$
$$\therefore BF + FC + FA = FD + FA > AD.$$

(3) Let P be a point not on the circumference. Join DP, and produce it to the circumference at G. Let fall the perpendiculars PH and PK, on GB and GC respectively.

$$\text{By Euc. i. 26, } GH = GK = \tfrac{1}{2}GP.$$
$$\text{Since } \angle GPH = 30^\circ = GPK,$$
$$\therefore BH + KC = PD,$$
$$\therefore BP + PC > PD,$$
$$\therefore BP + PC + PA > PD + PA > AD.$$

(4) It also follows from the above that if $\angle A = 120^\circ$, then the point required is $A \equiv E$.

If $\angle A > 120^\circ$, the point A will be within the circle, and A itself will be the point required. R. CHARTRES.

Note on the Dimensions and Meaning of J, usually called the Mechanical Equivalent of Heat.

THE title "mechanical equivalent of heat" tends to make one consider that J means the ratio of a quantity of mechanical energy to an equivalent quantity of heat; but since heat *is* mechanical energy (in a molecular form) it follows that J on this supposition is equal to *unity*, and therefore unnecessary.

Another way in which J is sometimes regarded is as the ratio between the ordinary *units* of heat and work; that is to say, in England, it is the ratio of the British thermal unit to a foot-pound, viz. the number 772. This definition makes it a simple number, the number of work units in a heat unit, a number which depends on the units of heat and work employed, and is different in France and England.

Now although J generally has one or other of these significations—that is, must be either unity or some pure number—yet people speak of the *dimensions* of J as being, not zero, but $\frac{\text{Work}}{\text{Mass} \times \text{Temperature}}$.

It is evident that there must be some confusion here, a confusion arising from the fact that most people when *talking* of quantities *mean* only so many times the units of those quantities, and so are not always sufficiently careful about the definitions of the various quantities which they introduce.

Now if we confine our attention to quantities themselves, independently of any systems of measurement, we shall be led to a perfectly consistent mode of regarding J, a way moreover in which it will have the required dimensions $\frac{\text{Work}}{\text{Mass} \times \text{Temperature}}$.

A British thermal unit is the heat required to raise a pound of water at freezing-point through 1° F., and Joule discovered that the mechanical equivalent of that amount of heat was about 772 foot-pounds.

Hence if we wish to consider the work necessary to raise any other mass of water at freezing-point through any small range of temperature, we have only to notice that the quantity $\frac{\text{Work}}{\text{Mass} \times \text{Range of Temperature}}$ is constant, and equal to $\frac{772 \text{ foot-pounds}}{1 \text{ pound} \times 1^\circ \text{ F.}}$.

This quantity is very fitly denoted by J, and might, if thought convenient, be called *a joule*.

But this quantity is the *specific heat of water*, according to the definition that specific heat is the heat required to raise a mass through a small range of temperature *divided by* the mass and the range. So that we have arrived at these conclusions: a quantity of heat is the same thing, whether expressed in British thermal units, or in foot-pounds, or in terms of any other standard; and the specific heat of water at 0° C. is denoted by the letter J.

Indeed it may be said that the result of Joule's experiments is the determination of the specific heat of water in absolute measure. Again, if c is the ratio of the specific heat of any substance to that of water, the full expression of its specific heat is cJ; that is, its specific heat is some multiple or fraction of *a joule*.

The first law of thermo-dynamics will then be expressed as—

$$dQ - p \,.\, dV = cJm \,.\, d\theta + m \,.\, dI,$$

where $dQ - p\,dV$ is the total energy supplied, $cJmd\theta$ is the amount of new energy evidenced by increase of temperature, and mdI is the increment of the latent energy of the body.

Coopers Hill, Staines, January 19. ALFRED LODGE.

The Temporary Thermo-Current in Iron.

In the *Philosophical Magazine* for January, Mr. Herbert Tomlinson has proposed an explanation of the remarkable fact that in an iron wire, heated red hot by a burner, an electric current is produced when the flame is shifted along the wire (see Wiedemann's "Galvanismus," ii. 453).

As his explanation is inadequate, perhaps I may be excused again drawing attention to this subject. Briefly his explanation is as follows:—That, as the portion of the wire in the flame rises in temperature, it, thermo-electrically speaking, becomes in fact like a different metal, and that then, on shifting the flame, the junction with the unaltered wire on the side moved towards becomes hotter than before, while the one on the other side falls in temperature, thus presenting the ordinary case of a thermo-couple with junctions at different temperatures. Now this explanation entirely overlooks the fact that, by the first assumption, just as fast as the temperature on one side rises, the wire there changes into the "second state," and correspondingly changes back on the other side as the temperature falls there; so that, as far as this explanation goes, there ought to be no current whatever, for thus both junctions must always be at the same temperature.

When I first noticed this current, which from considerations to follow I have ventured to call the "temporary thermo-current," it appeared to me to be due to the difference in the temperature-slope (or gradient) along the wire in front from that behind the flame, as it heats more rapidly in front than it cools behind, and to the electromotive force being a function of the slope, *i.e.* of $\frac{d\theta}{dx}$. But this hypothesis did not stand the test of experiment, as I have shown in a paper published in the Proceedings of the Royal Dublin Society, July 1886. So that as there *is* a current, we must suppose the "second state" to be not only a function of the temperature, but also of the time, *i.e.* that the wire changes into (or from) the "second state" more slowly than it is possible for it to change in temperature. So that the electromotive force at any point depends on the rate of change of the temperature slope, or equals $\phi\left(\frac{d}{dt}\frac{d\theta}{dx}\right)$. In support of this it will be found that if the flame be steadily moved along very slowly no current is produced—at all events less than would be otherwise expected; and, secondly, that the maximum current is got by moving the flame the fastest consistent with the condition of keeping the wire red hot.

It is with the view of emphasizing this dependence on the time that the term "temporary thermo-current" seems appropriate.

FRED. T. TROUTON.

Physical Laboratory, Trinity College, Dublin.

Causes influencing the Bathymetrical Range of Deep-Sea Fishes.

You refer (p. 219) to the fact that Dr. Günther has adopted the 100-fathom line as the boundary at which with the extinction of sunlight the bathybial fauna commences. This selection of 100 fathoms as the limiting horizon is of much interest in connection with the theory that the shallow-water marine fauna is greatly influenced by wave-currents. In a letter you published in 1885 (NATURE, vol. xxxii. p. 390) I indicated 100 fathoms as the depth to which wave-action of some sort must extend, as evidenced by the character of the deposits at the mouth of the English Channel. Dr. Günther now shows that the deep-sea fishes do not rise above that horizon. But, although the 100-fathom horizon agrees very well with the apparent limit of wave-action, it does not seem to agree with the most recent experiments on the penetration of sunlight in water.

So recently as November last you recorded the fact that during the past year Prof. Forel found that the greatest "depth-limit of absolute darkness" from March to July in the Lake of Geneva was 100 metres (NATURE, vol. xxxvii. p. 88). If experiments in a fresh-water lake may be taken as a guide to light-penetration in the ocean, 50 fathoms will be nearer the limit than 100. In this case the bathymetrical range of the bathybial fauna cannot be much influenced, if at all, by the presence or absence of sunlight. This view is moreover fortified by the fact that, though the deep-sea forms do not usually ascend above the 100-fathom line, the shallow-water forms go far below it; and there is no reason why they should not do so; for, although a form unfitted to withstand wave-currents cannot face them, there is nothing to prevent a flat-fish, fully equipped in this respect, from passing at will from the disturbed to the tranquil horizon, and *vice versâ*.

A. R. HUNT.

Torquay, January 10.

Wind Force at Sea.

In reference to a letter on the above subject in NATURE (p. 274), I beg to acquaint your readers that Capt. Barker's wish that anemometers should be used more on board ship has been endeavoured to be met by an instrument designed by myself on the sail principle. It has now been in use on some ships at sea for long voyages for five years, and daily observations have been obtained and sent home of the data observed, of pressure, direction, and velocity of the winds met with.

Regarding the further inquiry of ascertaining the rainfall at sea, this has now been carried on for about ten years by means of a rain-gauge designed by myself on the pivot principle, and it has been used by many vessels in all the great seas.

The daily observations have been sent home and are now on hand, and about five years of the returns have already been announced, and a further compilation of the data may be prepared when the materials become sufficient.

It may be added that the late Capt. Symington, of the s.s. *Hankow*, amongst his meteorological observations took the rainfall by rain-gauge on his ship for twenty years or more. The marine anemometer and rain-gauge above mentioned were exhibited at the Liverpool Exhibition in 1886, and at the Meteorological Exhibition of last year.

W. G. BLACK.

Edinburgh, January 21.

Untimely Insect Development.

Some of your readers may be interested in a case of untimely insect development, caused no doubt by the phenomenal mildness of the weather in this part of the country during the last few days. Last evening a perfect imago of the common tortoise-shell butterfly (*Vanessa urtica*) was found inside my house on the wall of my nursery. It is fully developed in every way, and the only thing in its appearance at all abnormal is that the antennæ are bent back and lie between the wings, which are in the erect position usual in repose. The insect has evidently only just emerged from the pupa, and is in a torpid condition, only just flapping its wings when touched. The nursery is a warm room looking to the south, and has a fire in it all day.

St. Albans, January 10.

JOHN MORISON.

Weasels killing Frogs.

Seeing a note in NATURE (December 29, 1887, p. 208), about weasels killing frogs, I thought that the following fact would be a further confirmation.

I was walking near the village of Clifton Hampden in August last, when I saw a weasel, carrying a good-sized frog in its mouth, come cautiously out of the rank grass by the road-side; directly the weasel perceived me, it dropped its prey on the road and retreated to the cover of the grass. The frog was dead. I kept silence, and the weasel left its hiding-place, and advanced a few steps, but again retreated. Soon, after several advances and retreats, it rushed out, seized the frog with its teeth, and running across the road disappeared in the long grass on the other side.

January 20.

M. S. PEMBREY.

"British and Irish Salmonidæ."

The author of "British and Irish Salmonidæ" calls in question the justice of three criticisms in my review of that book. In reply to his first objection, I have to point out that my quotation of the sentence referred to was, as Mr. Day has himself noted, made to draw attention to its grammatical errors, and therefore the omission of a few words which affected the sense but not the construction was of no consequence at all. I omitted the words intentionally, to shorten the quotation, and gave no opinion on the statement contained in the sentence: the statement which is implied rather than expressed is perfectly correct.

With regard to the second point, the statement in the text of the book which I questioned is as follows :—"The main principle is to employ thin layers of well-packed and pressed moss in trays with perforated bottoms, *the eggs being separated from the moss by muslin, mosquito-netting, swans' down, calico, or butter-cloth*, and that each tray contains two or three layers." In all the methods of packing salmonoid eggs in which moss is employed, the descriptions I have read state that the eggs are placed in direct contact with the moss, and Mr. Day does not justify the statement above quoted by referring to another statement in his notes, that for shorter journeys eggs are thrown off the frames on to swans' down. I doubted, and still doubt, if there is any method practised in which layers of moss are used, and are separated from the eggs by muslin or similar material.

With regard to the third point, it is true that on p. 249 of his book, in the chapter on *S. fontinalis*, Mr. Day refers to Brown Goode's "Game Fishes of the United States," and to the statement in that work that *S. namaycush* has, as its nearest relative, *S. fontinalis*. But I think a more direct reference to a speciegraphical description of *S. namaycush* might have been expected in a footnote referring to errors in the descriptions of this fish by certain writers. My remark about the omission of such reference was not made under the impression that S. *namaycush* was not a char, for I am aware that it is described as such in recent American reports on pisciculture, and have no doubt that such description is correct. But reference to speciegraphical determinations are rare in such reports, and I think readers of Mr. Day's book would have been glad of the references which he now supplies in his letter. YOUR REVIEWER.

MODERN VIEWS OF ELECTRICITY.[1]

PART III. (*continued*).

VI.

LET us now pass in review the various facts and experiences which have led us to a dual view of electricity; a kind of two-fluid theory, but in a very modified form.

First, there are the old experiments which vaguely suggest the separate existence of negative electricity, such as :—

(1) The wind from a point whether positive or negative; so that a candle gets blown always away from it, whether the point be on the prime conductor and the candle held in hand, or whether the point be held in the hand and presented to the candle or prime conductor; so, also, that a point whirligig turns the same way, whether supported on the prime conductor, or whether attached to the earth and placed near it.

(2) Phenomena connected with the spark discharge, such as Wheatstone's old experiment on what he called the velocity of electricity, with the three pair of knobs; and the double burr produced in cardboard when pierced with a spark, suggesting that something has pierced it both ways at once.

Then there are the more recently observed facts; as, for instance :—

(3) The fact that an electrostatic strain scarcely affects the volume of a dielectric; thereby at once suggesting something of the nature of a shearing or distorting stress, which alters shape but not size; a displacement of positive outwards and simultaneous negative inwards.

(4) The facts of electrolysis, and the double procession of atoms past each other in opposite directions.

(5) The phenomena of self-induction, and the behaviour of a thick wire to an alternating current. The delay also in magnetizing iron, and especially the possibility of permanent magnetism; combined with

(6) The absence of momentum in an electric current, or moment of momentum in an electro-magnet, as tested by all mechanical means yet tried.

I admit at once that many of these are mere superficial suggestions which may hardly bear examination and criticism. Only (3), (4), (5), and (6) can be at all seriously appealed to; but (5) and (6), in conjunction, seem to me to afford a sort of provisional and hypothetical proof, which (3) greatly strengthens.

At this point we must for the present again leave the question.

Representation of a Magnetic Field.

The disturbance called magnetism, which we have shown to be something of the nature of a spin—a rotation about an axis—is conspicuously not limited to the steel or iron of the magnet: it spreads out through all adjacent space, and constitutes what is called the magnetic field. A map of the field is afforded by the use of iron filings, which cling end to end and point out the direction of the force at every point.

These lines of force so mapped are to be regarded as the axes of molecular whirls. They are continuous with similar lines in the substance of the steel, and every line really forms a closed curve, of which a portion is in the steel and a portion in the air. In a wire helix, such as Figs. 16 or 29, the lines are wholly in the air, but in one part of their course they thread the helix, and in another part they spread out more or less between its faces.

But according to Ampère's theory of molecular currents there is no *essential* difference between such a helix and a steel magnet; directly the currents in the molecules of the magnet are considered, everything resolves itself into chains of molecular currents, threading themselves along a common closed curve or axis. Each atom, whether in the steel or in the air, is the seat of a whirl of electricity, more or less faced round so as on the average to have its plane at right angles to the lines of force. The simplest plan of avoiding having to consider those only partially faced round, is to imagine the whole number divided into a set which face accurately in the right direction, and a set which look any way at perfect random, and to neglect this latter set.

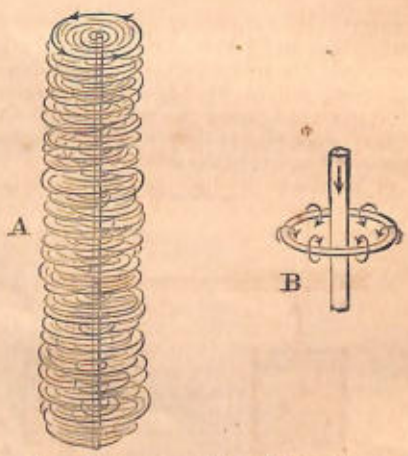


FIG. 30.—A, an element of a magnetic line of force with the electric whirl round it; B, a bit of an electric circuit with one of its magnetic lines of force shown round it, and the electric whirl round this; each magnetic line of force round a current being an electric vortex ring.

Well now try and picture a chain of whirls like beads spinning on a wire threading them all, and think of the effect of a material fluid thus rotating. Obviously it would tend to whirl itself fatter, and to shorten its length. An assemblage of such parallel straight whirls would thus squeeze each other laterally, or cause a lateral pressure, and would tend to drag their free ends together, causing a longitudinal tension.

Such whirls cannot in truth have free ends except at the boundary of a medium—as at the free surface of a liquid. Magnetic whirls are in reality all closed curves;

[1] Continued from p. 110.

but inasmuch as part of them may be in a mobile fluid like air, and part of them in a solid like iron or steel, it is convenient to distinguish between their two portions; and one may think of the air whirls alone as reaching from one piece of iron to another and by their shortening tendency or centrifugal force pulling the two pieces together.

The arrangement shown in Fig. 31 illustrates the kind

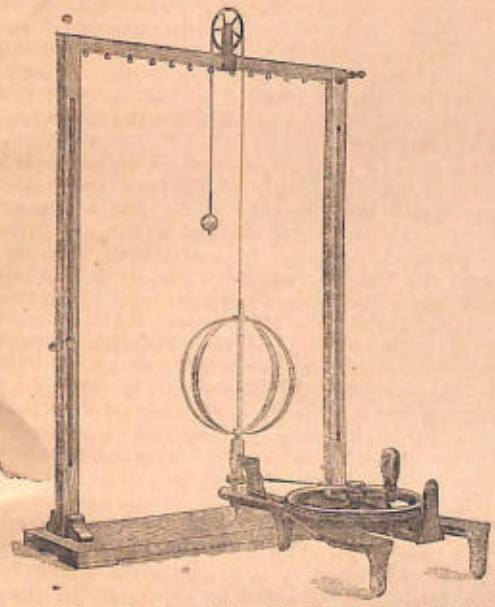


FIG. 31.—A "shape of the earth" model which, when whirled, exerts a tension along its axis, pulling up the weight attached to it, and a pressure at right angles, by reason of its bulging out.

of force exerted by a spinning elastic framework along and perpendicular to its axis of rotation.

One can easily see this effect of a whirl in a tea-cup or inverted bell-jar full of liquid. Stir it vigorously, and leave it. It presses against the walls harder than before, so that if they were elastic they would bulge out with the lateral pressure; and it sucks down the top or free end of its axis of rotation, producing quite a depression or

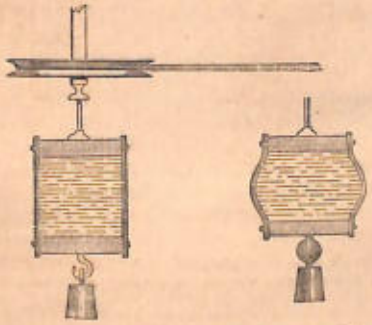


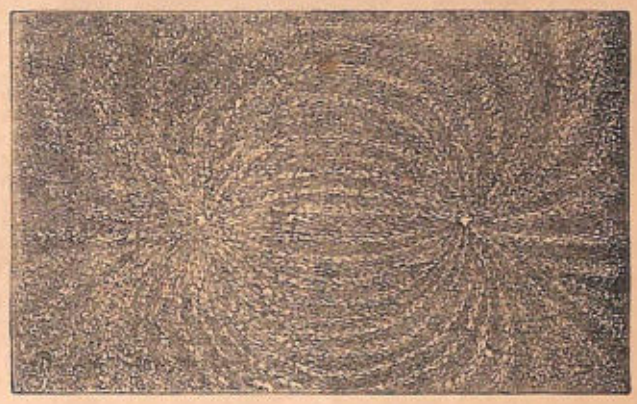
FIG. 32.—An elastic-walled cylindrical vessel full of liquid hanging from a whirling table, and, by reason of centrifugal force, raising a weight and bulging out laterally when spun, thereby illustrating a tension along the axis of rotation and a pressure in every perpendicular direction.

hollow against the force of gravity. Or, as a more striking illustration, make the following apparatus.

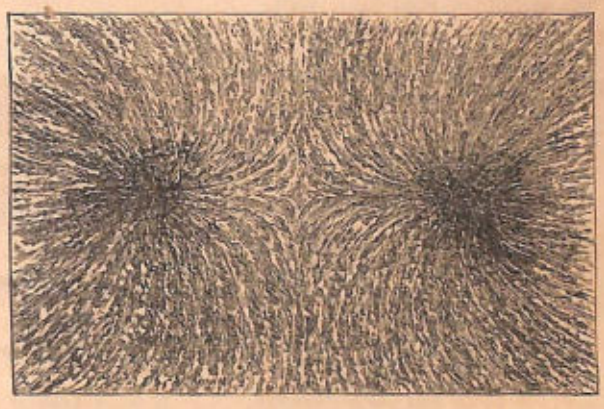
Two circular boards joined by a short wide elastic tube or drum: a weight hung to the lower board, the top board hung from a horizontal whirling table, the drum filled with water, and the whole spun round. The weight is raised by the longitudinal tension; the sides bulge out with the lateral pressure.

There is no need for the whole vessel to rotate. If the liquid inside rotates, the same effect is produced.

Imagine now a medium composed of a multitude of such cells with rotating liquid inside: let the cells be either very long, or else be joined end to end so as to make a chain—a series of chains side by side—and you have a picture of a magnetic medium traversed by a field of force. End-boundaries of the field will be dragged together, thus representing magnetic attraction; while, sideways, the lines of force (axes of whirl) squeeze each



Attraction.



Repulsion.

FIG. 33.—Attraction and repulsion. The tension along the lines of force or axes of rotation drags the one pair of poles together; and the pressure in directions perpendicular to the axis of rotation due to the centrifugal force of the whirls drives the other pair apart.

other apart, thus illustrating repulsion. This is Clerk-Maxwell's view of an electro-magnetic medium, and of the mode in which magnetic stress, and magnetic attractions and repulsions between bodies, arise.

Wherever lines of force reach across from one body to another, those bodies are dragged together as if pulled by so many elastics (Fig. 33); but wherever lines of force from one body present their *sides* to those proceeding from another body, then those bodies are driven apart.

OLIVER J. LODGE.

(To be continued.)

LANGUAGE-REASON.

THE inclosed letter on "Reason-Language" was written to an American friend, and has been published in an American paper in Chicago. I thought it might possibly interest the readers of NATURE.

Oxford, January 22. F. MAX MÜLLER.

"You tell me that my book on the 'Science of Thought' is thoroughly revolutionary, and that I have all recognized authorities in philosophy against me. I doubt it. My book is, if you like, evolutionary, but not revolutionary; I mean it is the natural outcome of that philosophical and historical study of language which began with Leibniz, and which during our century has so widely spread and ramified as to overshadow nearly all sciences, not excepting what I call the science of thought.

"If you mean by revolutionary a violent breaking with the past, I hold on the contrary that a full appreciation of the true nature of language and a recognition of its inseparableness from thought will prove the best means of recovering that unbroken thread which binds our modern schools of thought most closely together with those of the Middle Ages and of ancient Greece. It alone will help us to reconcile systems of philosophy hitherto supposed to be entirely antagonistic. If I am right—and I must confess that with regard to the fundamental principle of the identity of reason and language I share the common weakness of all philosophers, that I cannot doubt its truth—then what we call the history of philosophy will assume a totally new aspect. It will reveal itself before our eyes as the natural growth of language, though at the same time as a constant struggle of old against new language—in fact, as a dialectic process in the true sense of the word.

"The very tenet that language is identical with thought—what is it but a correction of language, a repentance, a return of language upon itself?

"We have two words, and therefore it requires with us a strong effort to perceive that behind these two words there is but one essence. To a Greek this effort would be comparatively easy, because his word *logos* continued to mean the undivided essence of language and thought. In our modern languages we shall find it difficult to coin a word that could take the place of *logos*. Neither *discours* in French, nor *Rede* in German, which meant originally the same as *ratio*, will help us. We shall have to be satisfied with such compounds as thought-word or word-thought. At least, I can think of no better expedient.

"You strongly object to my saying that there is no such thing as reason. But let us see whether we came honestly by that word. Because we reason—that is, because we reckon, because we add and subtract—therefore we say that we have reason; and thus it has happened that reason was raised into something which we have or possess, into a faculty, or power, or something, whatever it may be, that deserves to be written with a capital R. And yet we have only to look into the workshop of language in order to see that there is nothing substantial corresponding to this substantive, and that neither the heart nor the brain, neither the breath nor the spirit, of man discloses its original whereabouts. It may sound violent and revolutionary to you when I say that there is no such thing as reason; and yet no philosopher, not even Kant, has ever in his definition of reason told us what it is really made of. But remember, I am far from saying that reason is a mere word. That expression, 'a mere word,' seems to me the most objectionable expression in the whole of our philosophical dictionary.

"Reason is something—namely, language—not simply as we now hear it and use it, but as it has been slowly elaborated by man through all the ages of his existence on earth. Reason is the growth of centuries, it is the work of man, and at the same time an instrument brought to higher and higher perfection by the leading thinkers and speakers of the world. No reason without language—no language without reason. Try to reckon without numbers, whether spoken, written, or otherwise marked; and if you succeed in that I shall admit that it is possible to reason or reckon without words, and that there is in us such a thing or such a power or faculty as reason, apart from words.

"You say I shall never live to see it admitted that man cannot reason without words. This does not discourage me. Through the whole of my life I have cared for truth, not for success. And truth is not our own. We may seek truth, serve truth, love truth; but truth takes care of herself, and she inspires her true lovers with the same feeling of perfect trust. Those who cannot believe in themselves, unless they are believed in by others, have never known what truth is. Those who have found truth know best how little it is their work, and how small the merit which they can claim for themselves. They were blind before, and now they can see. That is all.

"But even if I thought that truth depended on majorities, I believe I might boldly say that the majority of philosophers of all ages and countries is really on my side (see 'Science of Thought,' pp. 31 *et seq.*), though few only have asserted the identity of reason and language without some timorous reserve, still fewer have seen all the consequences that flow from it.

"Some people seem to resent it almost as a personal insult that what we call our divine reason should be no more than human language, and that the whole of this human language should have been derived from no more than 800 roots, which can be reduced to about 120 concepts. But if I had wished to startle my readers I could easily have shown that out of these 800 roots one-half could really have been dispensed with, and has been dispensed with in modern languages (see 'Science of Thought,' p. 417), while among the 120 concepts not a few are clearly secondary, and owe their place in my list (*ib.* p. 619) merely to the fact that in Sanskrit they cannot be reduced to any more primitive concept. To dance, for, instance, cannot be called a primitive concept: perhaps not even to hunger, to thirst, to cook, to roast, &c. Only it so happens that in Sanskrit, to which my statistical remarks were restricted, we cannot go behind such roots as NṚT, KSHUDH, TṚSH, PAṬ, &c. It is in that limited sense only that such roots and such concepts can be called primitive. The number of really primitive concepts would be so alarmingly small that for the present it seemed wiser to say nothing about it. But so far from being ashamed of our modest beginnings, we ought really to glory rather in having raised our small patrimony to the immense wealth now hoarded in our dictionaries.

"When we once know what our small original patrimony consisted in, the question how we came in possession of it may seem of less importance. Yet it is well to remember that the theory of the origin of roots and concepts, as propounded by Noiré, differs, not in degree, but *toto cœlo* from the old attempts to derive roots from interjections and imitations of natural sounds. That a certain number of words in every language has been derived from interjections and imitations no one has ever denied. But such words are not conceptual words, and they become possible only after language had become possible—that is, after man had realized his power of forming concepts. No one who has not himself grappled with that problem can appreciate the complete change that has come over it by the recognition of the fact that roots are the phonetic expressions of the consciousness of our own acts. Nothing but this, our consciousness of our own repeated acts, could possibly have given us our first concepts. Nothing else answers the necessary requirements of a concept, that it should be the consciousness of something manifold, yet necessarily realized as one. After the genesis of the first concept, everything else becomes intelligible. The results of our acts become the first objects of our conceptual thought; and with

conceptual thought, language, which is nothing if not conceptual, begins. Roots are afterwards localized, and made the signs of our objects by means of local exponents, whether suffixes, prefixes, or infixes. What has been scraped and shaped again and again becomes as it were 'shape-her',' *i.e.* a shaft; what has been dug and hollowed out by repeated blows becomes 'dig-her',' *i.e.* a hole. And from the concept of a hole dug, or of an empty cave, there is an uninterrupted progress to the most abstract concepts, such as empty space, or even nothing. No doubt, when we hear the sound of cuckoo, we may by one jump arrive at the word cuckoo. This may be called a word, but it is not a conceptual word, and we deal with conceptual words only. Before we can get at a single conceptual word, we have to pass through at least five stages :—

"(1) Consciousness of our own repeated acts.
"(2) *Clamor concomitans* of these acts.
"(3) Consciousness of that *clamor* as concomitant of the act.
"(4) Repetition of that *clamor* to recall the act.
"(5) *Clamor* (root) defined by prefixes, suffixes, &c., to recall the act as localized in its results, its instruments, its agents, &c.

"You can see from my preface to the 'Science of Thought' that I was quite prepared for fierce attacks, whether they came from theologians, from philosophers, or from a certain class of scholars. So far from being discouraged, I am really delighted by the opposition which my book has roused, though you would be surprised to hear what strong support also I have received from quarters where I least expected it. I have never felt called upon to write a book to which everybody should say *Amen*. When I write a book, I expect the world to say *tamen*, as I have always said *tamen* to the world in writing my books. I have been called very audacious for daring to interfere with philosophy, as if the study of language, to which I have devoted the whole of my life, could be separated from a study of philosophy. I have listened very patiently for many years to the old story that grammar is one thing and logic another; that the former deals with such laws of thought as are observed, the latter with such as ought to be observed. No, no. True philosophy teaches us another lesson—namely, that in the long run, nothing is except what ought to be, and that in the evolution of the mind, as well as in that of Nature, natural selection is rational selection; or, in reality, the triumph of reason, the triumph of what is reasonable and right; or, as people now say, of what is fittest. We must learn to recognize in language the true evolution of reason. In that evolution nothing is real or remains real except what is right; nay, in it even the apparently irrational and anomalous has its reason and justification. Towards the end of the last century, what used to be called *Grammaire Générale* formed a very favourite subject for academic discussions; it has now been replaced by what may be called *Grammaire Historique*. In the same manner, *Formal Logic*, or the study of the general laws of thought, will have to make room for *Historical Logic*, or a study of the historical growth of thought. Delbrück's essays on comparative syntax show what can be done in this direction. For practical purposes, for teaching the art of reasoning, formal logic will always retain its separate existence; but the best study of the real laws of thought will be hereafter the study of the real laws of language. If it was really so audacious to make the identity of language and reason the foundation of a new system of philosophy, may I make the modest request that some philosopher by profession should give us a definition of what language is without reason, or reason without language? "F. M. M."

FERDINAND VANDEVEER HAYDEN.

WE reprint from the American journal *Science* (January 6) the following article on Dr. Hayden, whose death we lately announced:—Prof. Ferdinand Vandeveer Hayden, M.D., Ph.D., LL.D., who died in Philadelphia on the morning of December 22, was born in Westfield, Mass., September 7, 1829. Early in life he went to Ohio. In 1850 he was graduated from Oberlin College, and soon afterward read medicine at Albany, N.Y., receiving his degree from the Albany Medical College in 1853. He did not begin the practice of medicine, but in the spring of the year of his graduation was sent by Prof. James Hall of Albany, with Mr. F. B. Meek, to visit the Bad Lands of White River, to make collections of the Cretaceous and Tertiary fossils of that region. This was the beginning of his explorations of the West, which continued with little interruption for more than thirty years.

In the spring of 1854, Dr. Hayden returned to the Upper Missouri region, and spent two years in exploring it, mainly at his own expense, although he was aided a portion of the time by gentlemen connected with the American Fur Company. During these two years he traversed the Missouri River to Fort Benton, and the Yellowstone to the mouth of the Big Horn River, and explored considerable portions of the Bad Lands of White River and other districts not immediately bordering upon the Missouri. The large collections of fossils he made were given partly to the Academy of Sciences in St. Louis, and partly to the Academy of Natural Sciences of Philadelphia.

As one of the members of the Geological Survey has recently said, these collections and researches mark the commencement of the epoch of true geologic investigation of the Great West. The collections attracted the attention of the officers of the Smithsonian Institution; and in February 1856, Dr. Hayden was employed by Lieut. G. K. Warren, of the United States Topographical Engineers, to make a report upon the region he had explored; so that the results of his labours during the three previous years were utilized by the Government. This report was made in March of the same year, and in May following he was appointed geologist on the staff of Lieut. Warren, who was then engaged in making a reconnaissance of the North-West. He continued in this position until 1859, when he was appointed naturalist and surgeon to the Expedition for the exploration of the Yellowstone and Missouri Rivers, by Capt. William F. Raynolds of the Corps of Engineers of the United States Army, with whom he remained until 1862. The results of his work while with Liutenant Warren were published in a preliminary report of the War Department, and in several articles in the Proceedings of the Academy of Natural Sciences of Philadelphia for the Years 1857 and 1858, and more fully in a memoir on the geology and natural history of the Upper Missouri, published in the Transactions of the American Philosophical Society, Philadelphia, 1862. This paper also included chapters on the mammals, birds, reptiles, fishes, and recent mollusca of the region in which his geological investigations were carried on. During this period also he found time to make notes upon the languages and customs of the Indian tribes with which he came in contact. These notes were embodied in "Contributions to the Ethnography and Philology of the Indian Tribes of the Missouri River," published in the Transactions of the American Philosophical Society, Philadelphia, 1862; in a "Sketch of the Mandan Indians, with some Observations illustrating the Grammatical Structure of their Language," published in the *American Journal of Science* in 1862; and in "Brief Notes on the Pawnee, Winnebago, and Omaha Languages," published

in the Proceedings of the American Philosophical Society, Philadelphia, 1869.

In May 1862, Dr. Hayden was appointed acting-assistant surgeon of volunteers by the Surgeon-General of the United States Army, and was sent to Satterlee Hospital in Philadelphia. He was confirmed by the United States Senate as assistant-surgeon and full surgeon of volunteers on the same day (February 16, 1863), and sent to Beaufort, S.C., as chief medical officer, where he remained for one year, when he was ordered to Washington as assistant medical inspector of the Department of Washington. On February 19, 1864, he was sent to Winchester, Va., as chief medical officer of the army in the Shenandoah valley. Here he remained until May 1865, when he resigned, and was brevetted lieutenant-colonel for meritorious services during the war. During the remainder of the year 1865 he was employed in work at the Smithsonian Institution. It was during this year that he was elected Professor of Geology and Mineralogy in the University of Pennsylvania,—a position he held until 1872, when the increased executive duties in connection with the Geological Survey of the Territories induced him to resign it.

In the summer of 1866 he undertook another expedition to the Bad Lands of Dakota, under the auspices of the Academy of Natural Sciences of Philadelphia, for the purpose of clearing up some doubtful points in the geology of that region, and returned with large and valuable collections of vertebrate fossils, which were described in a memoir published by the Academy of Natural Sciences of Philadelphia in 1869. From 1867 to 1879 the history of Dr. Hayden is the history of the United States Geological and Geographical Survey of the Territories, of which he was geologist-in-charge, and to the success of which he devoted all his energies during the twelve years of its existence. In this time more than fifty volumes, together with numerous maps, were issued under his supervision. One of the results of his surveys, and the one in which he probably took the greatest interest, was the setting aside by Congress of the Yellowstone National Park. The idea of reserving this region as a park or pleasure-ground for the people originated with Dr. Hayden, and the law setting it apart was prepared under his direction. The work of the Geological Survey of the Territories had its consummation in the Atlas of Colorado, which increased greatly our knowledge of one of the most interesting portions of the Great West. In 1879, after the disbanding of the Survey of the Territories, Dr. Hayden received an appointment as geologist on the newly organized United States Geological Survey. For about three years he was occupied in the completing of the business of the Geological and Geographical Survey of the Territories, and the preparation of the final results of that survey. His health had already begun to fail, but early in 1883 he asked to be relieved from the supervision of the printing of the reports, and during the three following seasons he undertook field work in Montana. By the latter part of the year 1886 his health had become so poor that he was confined most of the time to his bed. He then resigned his position as geologist, closing an honourable connection with the Government that included twenty-eight years of actual service as naturalist, surgeon, and geologist. To the general interest in science excited by the enthusiastic labours of Dr. Hayden in his geologic explorations, is due in a great degree the existence and continuance of the present United States Geological Survey.

In 1876 the degree of LL.D. was conferred upon him by the University of Rochester, and in June 1886 the same degree was conferred upon him by the University of Pennsylvania. Dr. Hayden was a member of the National Academy of Sciences and of many other Societies scattered throughout the country. He was also honorary and corresponding member of a large number of foreign Societies.

As to Dr. Hayden's personal character, those who were personally associated with him know best how genial he was, and how sincere and enthusiastic his desire to forward the cause of science. Although impulsive at times, he was generous to a fault. His subordinates all knew that each one stood upon his own merits, and that due credit would be awarded to his successful efforts. The same spirit actuated him in respect to those not immediately connected with him. His views are expressed as follows in one of his earliest reports, when speaking of those who had preceded him: "Any man who regards the permanency or endurance of his own reputation will not ignore any of these frontier men who made their early explorations under circumstances of great danger and hardship."

His ideas were broad and liberal. He aimed to make a thorough astronomical, topographical, geological, and botanical survey of the Great West, with a view to the development of its mining and agricultural resources. The greater part of his work for the Government and for science was a labour of love.

To the foregoing notice some token of recognition and regret on the part of brother geologists on this side of the Atlantic may perhaps be fittingly appended by one who knew Dr. Hayden personally, was familiar with his writings, and had wandered in his footsteps among the solitudes of the Far West. The first impression which the late geologist made on those who came to know him was one of gentleness, almost of timidity. They could hardly help asking themselves, "Can this be the man who has so successfully won over the blustering Congressmen to grant him year after year such large appropriations for his western surveys; who has organized such wonderful expeditions; who has gone through such hardships, and in an incredibly short space of time has made such excellent reconnaissances and published such voluminous Reports and admirable maps?" It was some time before one could see the real underlying secret of his success. This was undoubtedly a quiet enthusiasm for science, supported by an undemonstrative but indomitable courage, and a determination to gain the proposed end, cost what it might in bodily and mental endurance. No one who has not been in some measure admitted behind the scenes of political wire-pulling in the States, can realize what had to be undertaken by the man of science who would obtain and retain an annual subsidy from Congress for scientific investigation in the days when Hayden carried on his explorations. There were other rival claimants for Parliamentary aid who were doing similar work, under other Government Departments. There was likewise the wide outside circle of scientific men who had no State employment, and some of whom thought themselves at least as deserving of it as those who fortunately had gained it. Then there were the Gallios of Congress, who cared nothing about science of any kind, those who grudged money spent out of their own States, those who required to see on their drawing-room table a well got up Annual Report with pictures and maps before they could be made to believe that the money was well bestowed. And the weeks and months of early summer, so precious for field work, had to be passed in the lobbies of the Capitol, making sure that there would be no failure in the granting of the appropriation. The most wearisome and profitless part of his year was this "lobbying" at Washington. But Hayden had no choice in the matter. He must either go through with that part of his work or abandon his western surveys altogether. This alternative has not always been borne in mind by those who have judged of him.

There can be no doubt that among the names of those who have pioneered into the marvellous geology of Western North America, that of F. V. Hayden will

always hold a high and honoured place. This place will be his due, not only because of his own personal achievements in original exploration. His earlier work exhibits much of that instinctive capacity for grasping geological structure which is the main requisite for a field geologist. He had a keen "eye for a country." But he likewise possessed the art of choosing the best men for his assistants, and the tact of attaching them to himself and his corps. In this way he accomplished much excellent work, keeping himself latterly rather in the background so far as actual personal geological investigations were concerned, and contenting himself with the laborious task of organization and supervision, while he encouraged and pushed forward his coadjutors.

The abolition of his Survey and the appointment of one of his rivals to the post of Director of the reconstituted Geological Survey of the United States, was a blow from which he does not seem ever to have recovered. He was treated, however, with great generosity by the new Director, and had a share of the large annual appropriation to enable him to complete his Reports. He was urged to condense these voluminous works, and to present a concise and readable account of what he and his fellow-workers had done for the geology of the far West. But he had no literary proclivities, and in the end gladly surrendered the task of writing for the more congenial employment of renewing his personal acquaintance with the geology of the Western Territories. Perhaps among those, and there must be many, who personally knew and esteemed him, there may be one competent and willing to compile or complete the summary which he never completed, and thus to erect to his memory a more fitting and lasting monument than one of brass or marble. A. G.

NOTES.

We regret to announce the death of Dr. Asa Gray, the most eminent of American botanists. He died at Cambridge, Massachusetts, on Monday, aged seventy-seven. Next week we shall give some account of his services to science.

Mr. George Godwin, F.R.S., well known as the editor of the *Builder*, died on January 27. He was seventy-three years of age. Among his writings were several works in which, with great earnestness, he pressed upon the attention of the public the evil consequences springing from the neglect of sanitary laws.

Mr. George Robert Waterhouse, late Keeper of the Department of Geology in the British Museum, died at his residence, Curton Lodge, Putney, on January 21, in his seventy-eighth year.

We have also to record the death of the well-known botanist, Dr. J. T. I. Boswell, who was for many years Curator to the Botanical Society in London, and a Lecturer at the Charing Cross and Middlesex Schools of Medicine.

The Medals and Funds to be given at the annual meeting of the Geological Society on February 17 have been awarded as follows:—Wollaston Medal to H. B. Medlicott, F.R.S.; Murchison Medal to Prof. J. S. Newberry, M.D., of New York; Lyell Medal to Prof. H. Alleyne Nicholson, M.D., D.Sc.; Wollaston Fund to John Horne, F.R.S.E.; Murchison Fund to E. Wilson of the Bristol Museum; Lyell Fund to Arthur H. Foord, and T. Roberts, B.A.

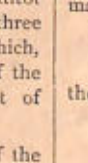
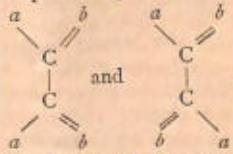
The Academy of Sciences at Turin has awarded the great Bressa prize of 12,000 francs (£480) to M. Pasteur.

The eighteenth International Congress of Orientalists will meet in Stockholm on September 2 next, and be opened by King Oscar in person, attended by the whole of the Royal family. The Congress will sit till September 6, when the members will visit Christiania as guests of the King, in whose name they will be entertained in the Norwegian capital for two days. They will then proceed to Gothenburg, where the Congress will be dissolved. In honour of the Congress a bibliography is to be issued, containing the portraits in heliography of all living Orientalists, and a *résumé* of the works published by each. The work is to be most sumptuously got up. The editor is Count Carlo Landberg.

The following arrangements have been made for the Penny Science Lectures at the Royal Victoria Hall: February 7, by Dr. Percy Frankland, "Germs in the Air, and what they do for us"; February 14, by E. Wethered, "Earthquakes and Volcanoes"; February 21, by F. R. Cheshire, "Insects as Florists and Fruit-makers"; February 21, by E. Hodder, "Incidents in the Life of Lord Shaftesbury."

Information has been received of the arrival of H.M. surveying-ship *Egeria*, Capt. P. Aldrich, at King George's Sound, after a very successful deep-sea sounding cruise across the Indian Ocean. Between latitudes 10° and 35° S.,—a belt 1500 miles wide,—not a single sounding has heretofore been obtained in this ocean, and it is therefore satisfactory to learn that forty-three soundings, all of them accompanied with several sets of temperature observations, have been now obtained. The *Egeria's* track was from Sunda Strait to Mauritius, thence south to latitude 38°·5, and thence to Western Australia.

Memorials are being sent from various public bodies in Hampshire to the Lord President of the Council, requesting that the proposed Forestry School for England may be established in that county. It is pointed out that the extensive Crown lands of Hampshire are peculiarly well fitted for scientific and practical forestry.

A paper of exceptional interest was read by Prof. Victor Meyer at the meeting of the Chemical Society of Göttingen held on January 24 In it were embodied some remarkable speculations upon the shape of the ultimate atoms of carbon. These ideas are the outcome of his recent work upon the oxims of benzil and certain other complicated organic compounds, and may be briefly summed up as follows. Certain compounds of the type  (where a represents a monad and b a dyad radicle) exist in two isomeric modifications which can only be expressed by the following different geometrical arrangement:  This necessitates an expansion of the theory of Van t' Hoff and Wislicenus, according to which, by rotation of one of the carbon atoms in the first case, the latter would be the only stable form; there are cases in which this rotation is free to occur, and cases like the present where it is prevented. From a consideration of the geometrical isomers of the benzyl cyanides, Prof. Meyer further shows that the valencies of carbon may be displaced out of their normal positions at the corners of a regular tetrahedron by the unequal attractions of unlike radicles. Finally, as the only means of accounting for all these varied phenomena, Prof. Meyer expresses his conviction that the atoms of carbon are spheres, each surrounded by an ether-shell, which forms the seat of the four valencies. On account of their probable electrical connection he terms these valencies "electrules," and considers that the electrules of the same atom are in isochronous oscillation, and

therefore, in accordance with the law, repel each other but are attracted by the dissimilar oscillations of other atoms. In consequence of this repulsion they must take up the regular tetrahedral position, from which, however, under the circumstances mentioned above, they may be slightly displaced. In course of the discussion which ensued, Prof. Riecke followed up the subject with a preliminary notice of his own researches upon the shape of odd-valent atoms, and showed that, in case of nitrogen, phosphorus, and other pentad elements, the ether-shell is probably not spherical, but an ellipsoid; three valencies being situate upon the great circle at the angles of an equilateral triangle, whilst the other two are located at the poles. If this indeed be the case, the tri- and penta-valency of the nitrogen group will be completely and very simply explained.

In a paper contributed to the Royal Society of Edinburgh, and to be published in a forthcoming volume of the Society's *Transactions*, Mr. A. Crichton Mitchell gives the results of an experimental inquiry, made at the Edinburgh University Physical Laboratory, into the thermal conductivity of iron, copper, and German silver. The method employed was that of Forbes. The experiments were, for the most part, a repetition of those made by Prof. Tait about ten years ago; the main difference being that the bars used were nickel-plated in order to prevent oxidation, and thus render the estimation of the amount of surface loss of heat a matter of greater certainty than hitherto. The results arrived at confirm, in the main, those of the previous work, on the same bars, already mentioned, and are of some importance in deciding the question as to whether thermal conductivity increases or decreases with increase of temperature. The principal conclusion in the paper is, that in iron, thermal conductivity increases with increased temperature, and that therefore iron can no longer be looked upon as an exception to the rule followed by the other metals, viz. that their conductivity increases with temperature.

The first rack-railway in France was opened lately at Langres (in Haute-Marne), which is perched on a hill 1460 feet high, and is the principal seat of the French manufacture of fine cutlery. The two railway-stations which have hitherto served it are several kilometres from the town, and much inconvenience has been felt. The new line is made on the type of the Righi railway, and rises to the heart of the town. A company has recently been formed to construct a funicular railway at Hong Kong, for ascent of the hill overlooking the town, and much frequented by the inhabitants. This line will rise to a height of 500 metres, and will be 1600 metres in length. The formation of the ground is exclusively granite, affording a fine solid base. There will be three viaducts, the largest 30 metres in length. Two compound engines of forty horse-power each will be installed at the upper station.

One of the most important questions in optics is whether the velocity of propagation, and therewith the wave-length of light, depend on its intensity or not. A determination of wave-length from prismatic decomposition is not capable of great accuracy. Dr. Ebert, of Erlangen, in studying the subject, preferred a more accurate method based on interference phenomena. Using eight variously coloured light sources (lithium, hydrogen, sodium, &c.), varied in intensity between the values 1 and 250, he established the constancy of the wave-length and velocity of propagation to nearly a millionth within those limits of intensity. Considering, with these results, that the great brightness of the sun does not destroy the coincidences of the Fraunhofer lines with lines of our terrestrial light sources, even with the greatest dispersions, Dr. Ebert thinks his affirmation (of independence of intensity) is generally valid within ordinarily occurring limits of brightness.

Mr. M. W. Harrington has contributed to the *American Meteorological Journal* for December the results of an interesting inquiry as to whether the rainfall is increasing on the plains. The subject is one of importance, as an annual increase of the rainfall would increase the agricultural capacity of a large territory. In order to come to a definite conclusion he has used two long series of observations representing the average conditions at the epochs of 1850 and 1880. The author compares the lines of equal rainfall for the two periods, and shows that, if there had been an increase, any one line should have travelled westwards in the interval between the two epochs. The result of the inquiry shows an apparent advance along the zone included between the parallels of 35° and 45°, and a regression above and below these latitudes, in other words that there has been apparently a consistent increase of rainfall toward the plains.

M. A. Lancaster, Meteorologist at the Royal Observatory of Brussels, and Inspector of Meteorological Stations in Belgium, has published a paper on the climate of that country in 1887, based on the observations at Brussels, and three other stations at the west, north, and east limits of the kingdom, the stations selected being typical of all other points of observation. The weather for each month is discussed at considerable length, and the results are compared with the normal values. The observations show that the mean barometer is highest in December and lowest in October; the absolute maximum temperature during the year at Brussels was 91°·0, and the minimum 15°·4. The prevalent winds are from west-south-west to south-south-west. The rainfall during the year amounted to 24·42 inches, being considerably below the average, viz. 28·78 inches. The rainfall diminishes with the distance from the sea, excepting in the neighbourhood of the forests; the amount increases notably in the Ardennes.

Dr. Fines has published, in the fifteenth *Bulletin Météorologique* of the Department of the Pyrénées-Orientales, the results of experiments carried on at the Perpignan Observatory:—(1) To test the theory of M. Kammermann, of Geneva, for the prediction of spring frosts by comparison of the readings of the wet-bulb and minimum thermometers (explained in the *Archives des Sciences physiques et naturelles*, vol. xiv. p. 425); the result being that the data, so far as Perpignan is concerned, do not bear out the character of accuracy attributed to them elsewhere. (2) To compare the results of wind-velocity given at each instant by a Bourdon's anemometer (presented to the Academy of Sciences, July 30, 1882) with the records of Robinson's velocity anemometer. The result of this inquiry shows a mean increase in the maximum velocities of over 21 per cent. by the use of Bourdon's instrument as compared with the means of the greatest velocities obtained by Robinson's anemometer.

We have received from the Imperial Observatory at Tokio the monthly summaries and means of the observations of the meteorological service in Japan for the year 1886, accompanied by charts showing the tracks of central areas of high and low barometer and by synoptic weather charts for each month. The stations are supplied with good instruments (in some cases with self-recording apparatus), and thee obsrvations which have now been regularly taken for about eleven years under the present organization compare favourably with those of European systems. The climate is generally of the oceanic type—the highest mean temperature at Tokio occurring in August. The winds are governed by the monsoon seasons; during the first three months of the year the prevalent directions at Tokio are north and north-west. From April to June the winds shift round to the south, through east. In July and August southerly winds predominate, but in September a sudden change to the north occurs, and continues generally until December. Warn-

ings of wind and weather are issued to various stations; the general percentage of success for both elements during the year 1886 was 80. Rainfall maps for each month and for the year are also given.

We have received the *Annuaire* for the year 1888, published by the Bureau des Longitudes with Messrs. Gauthier-Villars, Paris. It contains, besides the tables usually expected in works of this class, much useful information as to the monetary systems of the various nations of the world, mineralogy, meteorology, and other subjects. We may especially note an excellent account, by Admiral Mouchez, of the International Astronomical Congress which met in Paris in April 1887, to prepare the way for the execution of a photographic chart of the heavens.

The Syndics of the Cambridge University Press have undertaken the publication of a collected edition of the mathematical papers of Prof. Cayley. These papers, originally contributed to the Royal and other Societies and to various mathematical journals, will be arranged for publication by Prof. Cayley himself, who will add notes containing references to the writings of other mathematicians on allied subjects. It is expected that the edition will extend to ten quarto volumes; it is intended to publish two volumes each year until the completion of the work.

"My Telescope; a Simple Introduction to the Glories of the Heavens," is the title of a little half-crown work on astronomy by "A Quekett Club Man," whose kindred volumes on "The Microscope" have been so successful. It will be issued in a few days by Messrs. Roper and Drowley.

A new geological map of the Government of Kutais, by MM. Simonovitch and Sorokin, has been published at Tiflis by the Mining Department.

At a recent meeting of the Asiatic Society of Japan (reported in the *Japan Weekly Mail* of November 19), Mr. Batchelor read a paper on the *Kamui*, or gods of the Ainos of Yezo. He enumerated under thirteen heads these deities as they appear to be arranged in the Aino mind. These are: (1) the chief of all the deities, the possessor of heaven and the maker of worlds and places; (2) the progenitor of the Aino race, and presider over the affairs of men, who is the only human being worshipped by the people; (3) the sun and moon (the stars are not worshipped); (4) the fire-god, worshipped because of its general usefulness in cooking, healing, purifying, &c.,—sometimes spoken of as the "messenger" or mediator between gods and men; (5) the goddesses who preside over springs, lakes, rivers, and waterfalls,—they are worshipped as benefactors of mankind, particularly in alluring fish to ascend and descend the rivers; (6) the sea-gods, two in number, one being good and one evil,—the latter is the originator of all storms, and the direct cause of shipwrecks and death from drowning at sea; (7) bears, the most powerful animals known to the Ainos, as well as the most useful, supplying them at once with food and clothing; (8) the autumn salmon, the largest fish ascending the rivers,—it is not worshipped, but the term *Kamui* or deity is applied to it; (9) many birds, some of good, others of ill, omen, though not worshipped, are called deities. The same term is applied to beautiful localities, to high mountains, to regions full of bears or rivers full of fish, to large trees, to cool breezes on a warm day, to men of official rank, to devils, evil spirits, and reptiles. When applied to anything good, the term *Kamui* expresses the quality of usefulness, beneficence, divinity; when applied to anything evil, it implies dread, hatefulness, and such like. Applied to animals, it represents the greatest, fiercest, or most useful; to men, it is a mere title of respect. Subsequently, in the course of the discussion, Mr. Batchelor said that the facts of the Aino religion were very simply stated. They had one chief god, and all the others were officers or messengers of this supreme being; there was no lightning- or thunder-god. These were the facts, but he could not explain them. The Ainos, he said, regarded the sun as a body in which a deity resides, "distinguishing, so to speak, between a body and a soul."

The fossil head of a mammoth has just been unearthed in the Montmartre cemetery in Paris. The distance between the tusks is nearly 2 feet. Further excavations are being made in the hope that the remainder of the skeleton may be discovered.

About 20 cwt. of bones of prehistoric animals have been found in the bear cave near Rübeland, in the Harz Mountains. Only a part of the cave has yet been explored.

The University of Upsala has recently been presented with the fossil skeleton of a whale, found in a layer of marl at a depth of 10 feet in the province of Halland, in the south of Sweden. The skeleton, which is almost perfect, is that of a whale which has been called *Eubalæna svedenborgii*, from some portions of a whale skeleton found last century in the province of West Gothia, and now also in the Museum at Upsala. The skeleton is the only one complete ever found. It is that of a young whale.

On January 8, about 4 p.m., a magnificent meteor was seen at Porsgrund, in the south-east of Norway. It moved rapidly in an easterly direction towards the constellation Taurus. It was square in appearance, but the corners were rounded, the colours being intense green and violet, increasing in strength as the meteor disappeared behind a hill. The size was about that of the full moon. No sound was heard, nor did it leave a train. The passage occupied about two seconds.

The fog which lately prevailed over our islands and the North Sea extended far into the heart of Norway.

On December 27, about 11.30 p.m., a severe shock of earthquake was felt at Solum, in the province of Bratsberg, in the south-east of Norway. The shock was so severe that beds seemed lifted from the floor, and the occupants fled in terror into the open. The shock lasted several seconds, and was accompanied by several deep detonations. Large cracks were afterwards seen in the earth. The motion was from east to west.

A severe earthquake occurred at Algiers on January 8. It was noticed throughout the whole province. In one village a house fell in, and the church and the school-house were damaged.

From the Consular Report on the Trade of France for 1886–87, it is apparent that the desire for technical education is not at present widespread throughout that country. M. Lockroy, thinking that one of the great causes of the depression in trade, from which France, like England, has been suffering for some years, was the almost total absence of technical education in France, and that the usual remedies were of little or no avail unless aided by a sounder education of tradesmen and merchants, founded a new department in the Ministry of Commerce to supervise the carrying out of his plans. By his help and that of other supporters of his scheme, technical schools have so increased and multiplied, that there are at present ninety of these institutions in Paris and the provinces subsidized by the State. Very few of them are self-supporting, and the number of students in attendance is lamentably small. Students now, it is said, are not anxious to attend, but it is thought that if technical schools received the power of conferring degrees equivalent to the lower degrees in a University, the students would come more readily. It is also urged that these schools should be freed from Governmental control, and be

handed over to the mercantile bodies that in France correspond to our Chambers of Commerce, who understand local needs and local industries better than any department of State. Most of these institutions are behind the age, and the collections at the Conservatoire des Arts et Métiers, Paris, are not so full as those in other countries, and the building itself is in a half-ruinous condition. If the other establishments are inferior to this, as the Report seems to imply, perhaps it is not so difficult to account for the paucity of students and their lack of interest as the Ministry of Commerce seems to thinkit is.

THE additions to the Zoological Society's Gardens during the past week include two Poë Honey-eaters (*Prosthemadera novæ-zealandiæ*) from New Zealand, presented by Capt. Brabazon J. Barlow, s.s. *Tamui*; a Brazilian Hangnest (*Icterus jamaicai*) from Brazil, presented by Mr. Geo. D. Morce; a White-bellied Sea Eagle (*Haliaetus leucocephalus*) from Newfoundland, presented by Mr. Geo. M. Johnson; three Egyptian Cobras (*Naia haje*), three Cerastes Vipers (*Vipera cerastes*), two Hissing Sand Snakes (*Psammophis sibilans*), a Clifford's Snake (*Zamenis cliffordi*), an Egyptian Eryx (*Eryx jaculus*), a Blunt-nosed Snake (*Dipsas obtusa*) from Egypt, presented by Capt. W. G. Burrows; twenty-one Horrid Rattlesnakes (*Crotalus horridus*) born in the Gardens.

OUR ASTRONOMICAL COLUMN.

AMERICAN OBSERVATORIES.—The January number of the *Sidereal Messenger* states that the University of California has allotted $19,000 for the current expenses of the Lick Observatory during the present year. The Observatory has received an accession to its staff in Mr. Charles B. Hill, formerly of Chabot Observatory. The equipment of the Observatory has also been furthered by the arrival of the 36-inch photographic corrector and the micrometer for the great telescope. The micrometer is by Fauth and Co.

A new Observatory has been opened in connection with the Syracuse University, New York. This Observatory, erected in memory of Mr. C. D. Holden, a former graduate of the University, was dedicated on November 18, 1887, Prof. Newcomb pronouncing the inaugural address. The new institution possesses a transit instrument by Troughton and Simms, of 3 inches aperture, a chronometer by Dent and Co., a chronograph by Fauth and Co., and an 8-inch equatorial by the Alvan Clarks. Prof. John R. French is the Director.

At the Washburn Observatory, Prof. Brown, the new Director, who was formerly at the Naval Observatory, Washington, is engaged at Prof. Auwers' request in the determination of the fundamental star-places of the Zusatz-sterne in Auwers' system.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 FEBRUARY 5–11.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on February 5

Sun rises, 7h. 35m.; souths, 12h. 14m. 13·6s.; sets, 16h. 53m.: right asc. on meridian, 21h. 14·6m.; decl. 16° 0′ S. Sidereal Time at Sunset, 1h. 54m.

Moon (between Last Quarter and New) rises, 1h. 20m.; souths, 6h. 24m.; sets, 11h. 19m.: right asc. on meridian, 15h. 23·5m.; decl. 13° 16′ S.

Planet.	Rises.	Souths.	Sets.	Right asc. and declination on meridian.	
	h. m.	h. m.	h. m.	h. m.	° ′
Mercury..	8 10 ...	13 6 ...	18 2 ...	22 6·3 ...	12 58 S.
Venus.....	5 30 ...	9 32 ...	13 34 ...	18 31·8 ...	21 58 S.
Mars	23 17*...	4 41 ...	10 5 ..	13 40·0 ...	7 44 S.
Jupiter....	2 50 ...	7 6 ...	11 22 ...	16 5·6 ...	19 55 S.
Saturn....	15 20 ...	23 15 ...	7 10*...	8 17·5 ...	20 14 N.
Uranus...	22 33*...	4 5 ...	9 37 ...	13 4·2 ...	6 7 S.
Neptune..	11 1 ...	18 40 ..	2 19*...	3 41·6 ...	17 54 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Feb.	h.	
6 ...	1 ...	Jupiter in conjunction with and 4° 2′ south of the Moon.
7 ...	6 ...	Neptune stationary.
8 ..	21 ...	Venus in conjunction with and 1° 24′ south of the Moon

February 11–12.—A partial eclipse of the Sun: not visible in Europe.

Saturn, February 5.—Outer major axis of outer ring = 46″·1; outer minor axis of outer ring = 16″·0; southern surface visible.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·4 ...	81 16 N. ...	Feb.	9, 20	19 *m*
Algol	3 0·9 ...	40 31 N. ...	,,	10, 1	30 *m*
R Canis Majoris...	7 14·5 ...	16 12 S. ...	,,	5, 22	43 *m*
			,,	7, 1	59 *m*
S Hydræ	8 47·7 ...	3 30 N. ...	,,	9,	*M*
T Virginis	12 8·9 ...	5 24 S. ...	,,	7,	*M*
δ Libræ	14 55·0 ...	8 4 S. ...	,,	8, 2	50 *m*
U Coronæ	15 13·6 ...	32 3 N. ...	,,	8, 2	26 *m*
V Coronæ	15 45·5 ...	39 55 N. ...	,,	5,	*m*
U Ophiuchi... ...	17 10·9 ...	1 20 N. ...	,,	9, 1	29 *m*
		and at intervals of		20	8
X Sagittarii... ...	17 40·5 ..	27 47 S. ...	Feb.	9, 5	0 *M*
Z Sagittarii... ...	18 14·8 ...	18 55 S. ...	,,	6, 0	0 *m*
β Lyræ...	18 46·0 ...	33 14 N. ...	,,	7, 21	0 *m*
			,,	11, 2	0 *M*
U Aquilæ	19 23·3 ...	7 16 S. ...	,,	11, 5	0 *m*
η Aquilæ	19 46·8 ...	0 43 N. ...	,,	11, 21	0 *M*
S Sagittæ	19 50·9 ...	16 20 N. ...	,,	6, 4	0 *m*
			,,	9, 4	5 *M*
Y Cygni	20 47·6 ...	34 14 N. ...	,,	6, 20	9 *m*
			,,	9, 20	3 *m*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A.	Decl.	
Near Capella	74 ...	43 N.	
,, λ Draconis	165 ...	73 N.	
,, θ Draconis	240 ...	62 N. ...	February 6.

GEOGRAPHICAL NOTES.

AT Monday's meeting of the Royal Geographical Society, Admiral Mayne gave an account of recent explorations in British North Borneo. The paper of most original interest was, however, that of Mr. Maurice Portman, on the exploration and surveys of the Little Andaman. As an official on the Andaman Islands, Mr. Portman made it his business to conciliate the natives of the Little Andaman, who were regarded as quite intractable, and had been severely punished several times for murdering shipwrecked sailors. After a great deal of trouble and much risk, Mr. Portman succeeded in making friends with the natives, with the result that he and those who accompanied him could visit the island with impunity. He has thus been able to collect much welcome information both concerning the island and its highly interesting inhabitants. He completely surveyed the island, and has thus been able to make important corrections on our maps. At the north end the island consists of mangrove swamp and low belts of sandy soil, on which the aborigines have their huts. On the west and south-west the land rises into low hills of a coarse sandstone, running more or less north and south. The timber appears to be much the same as that of the South Andaman, though Mr. Portman saw no padouk and very few bamboos. The rocks are chiefly lime and sandstone, with a good deal of actual coral rock on the east and south coasts. In one place, south of Daogulé Bay, Mr. Portman noticed an outcrop of igneous rock. He found no minerals of importance. This island is about 27 miles long by 15 miles broad, and is encircled by a fringing coral reef. The products of the sea are the same as at the Great Andaman; but the Tubiporine family of coral, particularly *Tubipora musica*, occur in profusion. Dugong and turtle are very plentiful. On the South Sentinel Island, about 12 miles west of the Little Andaman, the turtle appear to have their breeding-station. This island, which is composed entirely of coral rock, is infested by large iguanas, and the *Birgus latro*, or cocoa-nut-stealing crab

(which certainly does not live on cocoa nuts there, as there are none). In rough weather landing is almost impossible on the coast of the Little Andaman, and even in fine weather there are heavy ground-swells and tide-rips. On the north coast large isolated reefs and ledges exist, which make navigation dangerous. With regard to the aborgines of the island, Mr. Portman is of opinion that the whole of the Little Andaman Island is peopled by one race, calling themselves Ongés. These people are subdivided into tribes, who adhere more or less to their own villages, and who quarrel and fight with each other considerably. They appear healthy; their principal diseases being chest complaints, colds, fever, and itch. In physique they compare favourably with the inhabitants of the Great Andaman. Their manners and customs differ somewhat from those of the Great Andaman people, the principal differences being the following:—Instead of small lean-to's, they build large circular huts, some measuring as much as 35 feet in height, and 60 feet in diameter. In these huts the various families sleep on charpoys of wood and cane matting, raised from 6 to 18 inches off the ground, and about 2 feet 6 inches square. Their habits are more cleanly, particularly as regards their huts, and the manner of preparing their food, which is invariably cooked. They cook, dry, and store in baskets, a small fish like a sprat, and this, with the boiled seed of the mangrove, seems to be their principal food, which they supplement with what they can. Their canoes, utensils, ornaments, and bows, are different from those of other Andamanese, and the women wear a tassel of yellow fibre in place of the leaf. They do not smear their bodies over with red ochre, or tattoo themselves, nor do the women keep their heads clean shaved. They are by no means expert in the use of a canoe in rough water, and do not harpoon turtle or dugong, though very fond of the former. They have no religion of any kind, and Mr. Portman learnt nothing of their traditions or superstitions, from which they seem even more free than their neighbours.

Mr. C. M. Woodford has recently returned from a two years' visit to the Solomon Islands, with extensive collections of mammals, birds, reptiles, Lepidoptera, &c. Nearly six months were spent on Guadalcanar, an island the interior of which has never been previously explored. Ascents were made of several rivers, the furthest point reached being about fifteen miles from the coast; but the hostility of the bushmen prevented the ascent of Mount Lammas.

OUR ELECTRICAL COLUMN.

Mr. Willard Case, of Auburn, N.Y., U.S.A., whose extremely interesting paper on a thermic voltaic cell was read before the Royal Society on May 6, 1886, is systematically pursuing his studies to obtain electric energy direct from carbon without passing through the intermediate stage of heat. A paper read on January 10, 1888, in New York, narrates his latest experiments. Jablochkoff tried to do it by immersing plates of carbon and iron in fused nitre. Mr. Case has been using chlorate of potassium and chlorine peroxide (perchloric acid), and with the latter has obtained an E.M.F. with certain forms of carbon varying from 0·3 to 1·24 volt.

In 1869, Dr. Gore proposed a thermo-magnetic generator of electricity (Proc. R.S. 1868-69, p. 261), in which an increasing or decreasing magnetic field was produced by heating and cooling an iron wire placed as a core to a coil of wire. Mr. Edison has recently endeavoured to make this principle practical, but M. Menges, of the Hague, has been more successful. The difficulties to overcome are waste of heat, energy, and consumption of time, in heating and cooling. The results obtained at present are, however, poor, though encouraging.

M. Tereschin, following Quincke's examples and directions, has found with water, methyl and ethylic alcohol, bisulphide of carbon, ether, oil of turpentine, and rape oil, a considerable transport of mass in capillary tubes in the direction of the positive current (*Beiblätter* No. 10, 1887); and Prof. Horace Lamb, in the *Phil. Mag.* for January, prints the admirable paper on the subject which was read before Section A of the British Association at Manchester last September, in which he criticizes the work of Wiedemann and Helmholtz, and explains the phenomenon on the assumption of Quincke, that there is a contact potential difference between the fluid and its solid boundaries, and his own conclusion that there is a sliding coefficient for a fluid in contact with a solid. This transport of mass, due to currents, and the electromotive forces produced by the passage of liquids through capillary tubes, and porous diaphragms are facts undeveloped and unapplied at present.

Considerable attention is being devoted to the heating and fusing of wires by currents. Short lengths of fine wire are used in nearly all electric light equipments as safety valves or cut outs; but the law determining the behaviour of these fuses was little known. Mr. Preece has written two papers for the Royal Society. Profs. Ayrton and Perry introduced the subject in a recent paper read at the Society of Electrical Engineers, and Mr. Cockburn has brought the whole subject before that Society, where it has been well threshed out. For fine wires, viz. those under ·010 in., the fusing current varies with the diameter; but for wires over ·010 in., is given by the equation—

$$C = ad^{3/2}.$$

The constant a has been determined for all metals. The behaviour of tin, which is very commonly used, is peculiar. When it approaches the temperature of fusion, its surface oxidizes and coats the wire with a thin skin, which acquires a higher temperature and allows a greater current to flow before fusing. Mr. Cockburn breaks through this skin with a weight—a pellet of lead; while Mr. Preece prevents the skin forming by covering the wire with shell-lac, which acts as a flux and prevents oxidation.

Major King, U.S.A., has recently made a mammoth electro-magnet out of two Rodman guns, weighing about 60 tons. It was excited by a powerful dynamo, and the armature resisted a pull of nearly 10 tons. The field was felt and watches were stopped at very great distances.

Von Bernardo's system of welding by directing an arc itself along the crack, fissure, or edge of the metal to be welded is attracting great attention on the Continent. Prof. Rühlmann, of Chemnitz, has read a very interesting paper before the Electrotechnical Society of Berlin. A carbon rod is the positive and the metal to be fused the negative pole of the arc. The arc acts like a blow-pipe flame. It is eminently adapted to repair cracks and leaks in boilers, heaters, and condensers, to repair tools and generally to cover the ground of soldering and welding.

A novel mode of forming electrolytically deposited copper tubes is attracting considerable attention. The copper is slowly deposited in a thin coating on an iron mandril kept constantly rotating in the bath. As the copper forms it is pressed by an agate burnisher, which compresses the molecular structure into a hard and solid mass of great tensile strength. Such copper has reached a breaking strain of 40 tons on the square inch. The process is due to Mr. W. Elmore.

A curious experiment is mentioned by the *Electrician* (January 27). A disk of soft iron has a spindle put through it so that it can be spun like a top. When at rest or moving slowly the disk is attracted by the poles of a magnet; but when it turns with sufficient velocity it is repelled by the magnet. The reaction of the induced currents in the mass of the metal is greater than the magnetic attraction.

H. F. Weber has cast doubts on the dull red rays being the first luminous rays to appear. He says that the carbon filaments, platinum, gold, and iron give a "gray glow," which is evident at temperatures much below that of dull red, viz. 525° C. Gold gives this gray effect at 417°, iron at 377°, and platinum at 390°.

THE PROPOSED TEACHING UNIVERSITY FOR LONDON.

The following is the text of the petition which has been drawn up by the Association for Promoting a Teaching University for London:—

To the Queen's Most Excellent Majesty in Council.

The Humble Petition of the Association for Promoting a Teaching University for London

Showeth—

1. That the Association for Promoting a Teaching University for London was formed in 1884, and has enrolled up to the present time about 250 members, each of whom was specially invited to join on the ground of eminence, or of experience in matters affecting University teaching in London, or of being

actively engaged in educational or administrative work in one of the institutions in which such teaching is given.

2. That your petitioners have been engaged for the last three years in examining the state and requirements of University education in London, and in conferring with the persons responsible for the teaching and administration of the institutions in which such education is carried on. They have thus been led to the conclusion that there exists in the metropolis and its suburban districts a general and growing demand for the development of University education. They are convinced that this demand cannot be met while higher education in London remains in its present unorganized state, and while the various institutions giving University instruction are deprived of the means of common discussion and concerted action. For the teaching given in these institutions their respective governing bodies are alone responsible, and each of these bodies for the most part acts in educational matters on the advice of its own teachers; but there is no common centre, such as a University would supply, where these governing bodies and their teachers could meet for purposes of conference, and wherein measures for the better organization of teaching could be discussed and settled. It is matter of experience to those who have taken part in the administration of such institutions that they suffer from the want of public recognition and support—a want due, not to defects in their work, but mainly to the anomaly of their position as institutions performing some of the functions without having the *status* of a University.

3. That the severance from the work of teaching of the work of examination for degrees, and the assignment of such examination to the existing University for London as its sole function, has had an injurious effect upon University education in London. The restraint exercised over efficient institutions through examinations held by a body which is neither responsible for their teaching nor in communication with their teachers acts as a fetter upon education, and gives undue consequence to examinations and their results. Examinations so arranged are less efficient than they might be made as a test of real merit, and tend to encourage dissipation of intellectual energy. In the Faculty of Medicine, although a systematic course of study in a recognized school is now required by the existing University of London, the want of due relations between the examining institution on the one hand and the teaching bodies and professional authorities on the other has led to unsatisfactory results.

4. That the evils above mentioned cannot be fully remedied but by the establishment in London of a Teaching University—that is to say, a University which (1) provides for the student in all the subjects included in its Faculties the best attainable teaching with the necessary aids and appliances; (2) requires a regular course of attendance on such teaching as a preliminary to graduation; and (3) secures to the teacher a direct and adequate representation in its councils, and a due share in its administration.

5. That such a University may be formed without trenching upon the province of the existing University of London, the functions of which are entirely different, and without superseding any institution now giving genuine University instruction in the metropolis. A Teaching University for London would incorporate or associate such institutions without injury to their individual life, as the Victoria University has incorporated Colleges in the North of England.

6. That the metropolis, regarded as the seat of a Teaching University, possesses for students in every Faculty, but especially in the Faculties of Laws and Medicine, advantages which cannot be equalled in any other place in the United Kingdom. Such a University, once established, would supply the motive power for various amendments in the University education of London, which are generally admitted to be needful, such as the greater concentration of the teaching of particular subjects in the earlier scientific stages of medical education; the foundation of additional chairs, attached either to particular institutions or to the University, for the further prosecution of special studies; the promotion of new Faculties; the encouragement of general education as a preliminary to the training for all professions; and, finally, such a presentation to the public of the needs of higher education in London as might secure from the corporate or private munificence of the metropolis the endowments necessary to enable it to keep pace with the growth of population and with the progress of learning.

7. That your petitioners, wishing to promote the foundation of a Teaching University in London, have held conferences with representative London teachers of University rank in the Faculties of Arts, Science, and Medicine, and have submitted to them the following statement of the objects to be aimed at in the foundation of such a University:—

(1) The organization of University teaching in and for London, in the form of a Teaching University, with Faculties of Arts, Science, Laws, and Medicine.

(2) The association of University examinations with University teaching, and the direction of both by the same authorities.

(3) The conferring of a substantive voice in the government of the University upon those engaged in the work of University teaching and examination.

(4) Existing institutions in London of University rank not to be abolished or ignored, but to be taken as the basis or component parts of the University, and either partially or completely incorporated, with the minimum of internal change.

(5) An alliance to be established between the University and the professional societies or corporations, the Council of Legal Education as representing the Inns of Court, the Royal College of Physicians of London, and the Royal College of Surgeons of England.

8. That these conferences have resulted in three reports, each embodying a substantial approval of the objects above stated and of the proposals of your petitioners for the purpose of effecting them.

9. That your petitioners have also held conferences with committees of the Senate and Convocation of the existing University of London, and that, simultaneously with the action taken by your petitioners, and, as they believe, in consequence thereof, the questions at issue have, it is understood, been under the consideration of the Senate and Convocation. Their deliberations have resulted in a report, which has been communicated to your petitioners, and recommends various changes in the constitution of the Senate as the governing body of the University, the establishment of Faculties and Boards of Studies, and the introduction on the governing body of representatives of such Faculties.

10. That such proposals differ from the proposals of your petitioners in the following, among other, respects:—

(1) In not requiring, as a preliminary to graduation in the Faculties of Arts and Science, a regular course of instruction in some recognized teaching institution. Such a course of instruction, however, continues to be required by the existing University as a preliminary to graduation in the Faculty of Medicine.

(2) In admitting Colleges not situated within the London district, the effect of this provision being that the suggested Faculties and Boards of Studies could meet but seldom, and thus would not afford to the teaching institutions of the metropolis adequate means of common discussion and concerted action.

(3) In the absence of any sufficient conditions for securing that the associated Colleges shall be doing effective University work.

(4) In not providing for the direct representation upon the governing body of the associated institutions, or of University teachers.

(5) In granting an unduly large representation to the graduates of the University.

11. That it appears to be difficult, if not impossible, for the University of London, consistently with its relations towards institutions situated elsewhere, and towards private students, to accept modifications which would enable it to fulfil the objects above mentioned.

12. That your petitioners have also held conferences with committees of the Councils of University College, London, and King's College, London, respectively, and have submitted to them the above statement of the objects of the Association. The subject having been subsequently brought by the committees before their respective Councils, these Councils adopted resolutions expressing their approval of the objects above stated, and have since determined to petition Your Majesty to the same general effect as is set forth in this petition.

13. That an alliance between the Teaching University above described and the chief professional societies and corporations of the metropolis, such as the Inns of Court, the Royal College of Physicians of London, and the Royal College of Surgeons of England, would be desirable as securing professional interests in

the arrangements for graduation, and in simplifying and re-arranging examinations in the Faculties of Laws and Medicine. Your petitioners have accordingly opened communications with the above-named bodies regarding this subject. They understand, however, that the Royal College of Physicians and the Royal College of Surgeons are disposed to seek conjointly for independent powers of granting degrees in a Faculty of Medicine. Your petitioners deprecate any severance of the machinery for granting degrees in London from academic influences. Many serious defects of University education in London are due to such a severance.

14. That, with a view to avoid multiplication of bodies conferring a diploma or a licence to practise, it is expedient that the possession of the conjoint diploma of the two Royal Colleges above named should be a preliminary condition for obtaining a medical degree in the University, the conferring of such diploma remaining, as at present, the function of the said Royal Colleges.

15. That the objects above set forth would, in the opinion of your petitioners, be most readily accomplished by the issue of a charter to a body of persons suitably constituted to be the governing body of a new University in and for London; such body to consist of the following persons:—

(1) The Chancellor of the University; the first Chancellor to be appointed by Your Majesty, and named in the charter.

(2) Members to be named by Your Majesty in the charter. Vacancies to be filled by the Lord President.

(3) Members chosen by the governing bodies of University College, London; King's College, London; and such other Colleges as may be associated with the University.

(4) Members chosen by the governing bodies of the professional societies and corporations hereinbefore referred to, if associated with the University.

(5) Members chosen by the professors or teaching staff of associated institutions doing University work, and assembled in the Faculties, whether of Arts, Science, Laws, or Medicine, to which they respectively belong, such members to be in number not less than one-third of the whole governing body.

16. That power should be given to the governing body of the new University to accept the application for association with the University of any teaching institution in the metropolis, the conditions of such association to be—(*a*) that the institution is giving instruction of a University character; (*b*) that it has established a complete curriculum, and possesses a sufficient teaching staff in at least one of the recognized Faculties; (*c*) and that it has furnished proofs of its means and appliances for teaching being established on a satisfactory basis.

Your petitioners therefore humbly pray Your Majesty to be pleased to grant a charter to a body of persons appointed as is described in this petition, or to such other person as Your Majesty may be pleased to select, constituting a University in and for London upon the principles and for the purposes hereinbefore stated, and having power to grant its own degrees in the Faculties of Arts, Science, Laws, and Medicine, and that Your Majesty will be pleased to make such orders in the premises as to Your Majesty, in your Royal wisdom and justice, may seem meet.

And your petitioners will ever pray, &c.

Executive Committee of the Association for Promoting a Teaching University for London:—W. Grylls Adams, M.A., F.R.S., J. W. Cunningham, Sec. King's College, J. Curnow, M.D., F.R.C.P., Sir Dyce Duckworth, M.D., F.R.C.P., G. Carey Foster, B.A., F.R.S., M. Berkeley Hill, M.B., F.R.C.S., W. H. H. Hudson, M.A., LL.M., J. Marshall, LL.D., F.R.S. (Chairman), Norman Moore, M.D., F.R.C.P., H. Morley, LL.D., W. M. Ord, M.D., F.R.C.P., F. Pollock, M.A., R. S. Poole, LL.D., W. J. Russell, Ph.D., F.R.S., T. E. Scrutton, M.A., LL.B., Rev. Henry Wace, D.D., G. C. W. Warr, M.A., A. W. Williamson, LL.D., F.R.S., Gerald F. Yeo, M.D., F.R.C.S., Sir George Young, LL.D.; Secretary, F. C. Montague, M.A., 12 New Court, Carey Street, W.C.

THE TOTAL ECLIPSE OF THE MOON, JANUARY 28.

THE weather on the night of January 28 proved decidedly unfavourable for those astronomers in London and the neighbourhood who had prepared to observe the occultations of small stars by the eclipsed moon. The sky, which had been beautifully clear in the morning, but which had become partially clouded towards evening, had cleared again a little before the commencement of the eclipse, thus raising hopes which were destined to be disappointed, for the clouds returned, and, with the exception of two or three short breaks, the moon was enveloped in cloud more or less dense during the entire duration of the total phase. Very full preparations for the observation of the occultations had been made at the Royal Observatory, Greenwich, but only the observers at the four largest telescopes were able to see even one of the predicted phenomena. At the Cambridge Observatory a similar disheartening experience was recorded, and at Mr. Crossley's Observatory, Halifax, it was quite cloudy, but in the west and south of England, and in Ireland at Dublin and Belfast, the conditions for observing were very favourable. On the Continent, at Vienna no observations could be made, the moon being enveloped in thick haze; at Paris and Berlin the sky had been overcast, and there had been a fall of snow before the eclipse, but the latter half of the eclipse was well observed at the former station, and some good results were obtained at thelatter during a clear interval about a quarter of an hour after the commencement of totality. At Moscow the eclipse was seen in a very clear sky, and at Madrid it was partially clear.

The following table gives the number of observations obtained at those Observatories from which accounts have been received up to the present time:—

Observatory.	Aperture of telescope, inches	No. of Stars observed. Dis.	Reap.
Royal Observatory, Greenwich	24 ...	3 ...	7
,, ,, ,,	12¾ ...	1 ...	4
,, ,, ,,	6 ...	3 ...	1
,, ,, ,,	6½ ...	0 ...	3
Col. Tupman's—Harrow	18 ...	3 ...	2
Mr. Penrose's—Wimbledon	6 ...	3 ...	2
Mr. Brodie's—Fernhill, I. of W.	8½ ...	9 ...	5
Mr. Stothert's—Bath	6 ...	11 ...	13
Cambridge Observatory	12 ...	0 ...	2
Miss Petrie—Bradford	6 ...	1 ...	1
Mr. Backhouse's—Sunderland	— ...	1 ...	0
Glasgow Observatory	9 ...	6 ...	7
Mr. Heath—Edinburgh	3½ ...	3 ...	1
Dunsink Observatory	— ...	17 ...	18

		No. of Stars observed.
Stonyhurst Observatory	8 ...	1
,, ,,	5½ ...	12
,, ,,	4 ...	4

The 8-inch refractor at Stonyhurst was devoted to spectroscopic observations during the greater part of the eclipse. It had been in the programme to make similar observations at Greenwich had the night proved favourable, and also to take a series of photographs showing the progress of the eclipse. Three photographs were secured, but the clouds prevented all spectroscopic work. Dr. Copeland also at Dun Echt had intended to make spectroscopic observations, but was almost completely thwarted by snow-squalls.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, January 19.—"On the Secondary Carpals, Metacarpals, and Digital Rays in the Wings of existing Carinate Birds." By W. K. Parker, F.R.S.

In a paper "On the Morphology of Birds," already sent in to the Royal Society, but not yet published, I have described certain additional parts in the wings of Gallinaceous birds.

One of these lies on the radial side of the first metacarpal; the other two are on the ulnar side of the second and third metacarpals.

These parts, which at first caused me considerable surprise, being wholly unexpected by me, are only part of what I have since found in other families.

During the past year I have worked out the development of the skeleton in the Duck tribe ("Anatidæ"), in the Auk tribe ("Alcidæ"), and in the Gull tribe ("Laridæ"), and to some degree in some other families. The subject appears to me to be of great interest, and I have, through various English and American friends, obtained many scores of embryos and young birds, &c., that I may be able to trace these parts in every main group of the class. Normally, both the existing Carinatæ and Ratitæ,

and such extinct forms as have been worked out—*Archæopteryx*, *Hesperornis*, *Ichthyornis*—show that the primary form of the bird's wing is simply *tri-digitate*. In this I agree with Baur, who has helped me greatly in this matter, both by his valuable papers and also by personal discussion with me.

The normal "manus" of a Carinate bird contains two permanently distinct carpals: three carpals that lose their independence by anchylosis with the metacarpals, and three digital rays extending from the three fused metacarpals.

In some birds, *e.g.* the Passerinæ, the *pollex* of the first digit has only one phalanx attached to its short metacarpal, the second only two, and the third only one, phalanx. In others, Plovers, Gulls, Cormorants, &c., an additional or *ungual* phalanx is found on the first and second digit; and in some birds, *e.g. Numenius*, during their embryonic state, a small semi-distinct nucleus is seen on the end of the aborted phalanx of the third digit.

In my as yet unpublished paper I have mentioned a sub-distinct tract of very solid fibro-cartilage, which evidently corresponds with what has been called "præ-pollex" by Kehrer and others.[1]

I am satisfied, now, that this very notable part is the remnant of the skeleton of the *spur*, so remarkably developed in the Palamedidæ, certain Geese, Plovers, and Jacanas.

This part therefore need not interfere with the consideration of the *true secondary digital* parts.

Among the last communications received by me from Dr. Baur, I find in print what I had already learned from him orally.

In some "General Notes" published in the *American Naturalist*, September 1887, p. 839, I find the following paragraph:—"The oldest Ichthyopterygia had few phalanges and not more than five digits; the radius and ulna were longer than broad, and separated by a space. Later, through the adaptation to the water, more phalanges were developed, more digits appeared, mostly by division of the former, or by new formation on the ulnar side. I have never found a new digit developed on the radial side."

These are most important facts, some of which—namely, the bifurcation of the digital rays—I had received some light upon before, both from Dr. Gadow and from Prof. D'Arcy W. Thompson.[2]

I find that the *carpus*, *metacarpus*, and digital rays are all apt to increase in number beyond what is normal.

Long ago I found, in one of the Palamedidæ, *e.g. Chauna chavaria*, *two ulnar carpals*, apparently an "ulnare" proper, and "centrale." More recently, in the embryo of a more normal Chenomorph—the Falkland Island Goose (*Chloëphaga poliocephala*)—I found the ulnare nearly divided into two segments.

On the other side of the carpus in an embryo Kestrel (*Falco tinnunculus*) and in a young Sparrow-hawk (*Accipiter nisus*), I found a "radiale" in two pieces, the outer of which in the latter was degenerating into the large "*os prominens*" which is found in the tendon of the "tensor patagii" muscle of rapacious birds.

In the embryos of Gulls, Auks, Guillemots, &c., the large "distal carpal" of the index or second digit sends forward a long wedge of cartilage towards an additional metacarpal nucleus. Evidently this is the rudiment of another carpal seeking to be attached to its own intercalary metacarpal.

Further on, on the large second digit, the flat dilated part of the proximal phalanx, on its ulnar side, also, is developed from a distinct tract of true cartilage, but soon loses its independence; it forms the plate on which some of the *primary quills* are fixed.

Still further on, on the ulnar side, near the small well-developed ungual phalanx of the embryo, but later, after hatching, a small oval cartilage appears, and *is ossified independently*.

A similar tract of cartilage is formed on the pollex or first digit, also, but is somewhat smaller than that on the second; it is on the ulnar side and near the ungual phalanx.

In the feeble third digit I only find a rudimentary secondary metacarpal, on the ulnar side; this is very constant throughout the *Carinatæ*; and sometimes, as I have already mentioned, there is a small rudiment of a second phalanx on that digit which, in the Lizard, has *four* phalanges.[3]

[1] "Beiträge zur Kentniss des Carpus und Tarsus der Amphibien, Reptilien, und Säuger," *Berichte der Naturforschenden Gesellschaft zu Freiburg i. B.*, vol. i. 1886 (Heft 4 und Taf. 4).

[2] See his paper on the hind limbs of Ichthyosaurus, &c., *Journ. Anat. Physiol.*, vol. xx., pp. 1–4 (reprint).

[3] The figures of these parts, and also of the rest of the developing skeleton in these birds—Ducks, Auks, Guillemots, &c.—are ready for publication.

In seeking for evidence of the manner in which these high and noble hot-blooded feathered forms arose from among the Archaic Reptilia, I think that something has been gained in what I have stated above.

The skull brings evidence of the same sort during its development, and it is to ancient long-beaked forms, and not to modern short-faced types of Reptilia, that we must look for any near relationship of the Reptiles in the Birds.

In the Guillemot (*Uria troile*) I have satisfied myself that there has been a considerable amount of *secular* shortening of the beak (rostrum and fore-part of mandibles), and if we look at Dr. Marsh's figures of *Hesperornis* and *Ichthyornis* we shall see what long bills these toothed birds possessed.

But there is no part of a developing bird's skeleton that is not rich with suggestive facts of this kind, as I propose to show in due time.

January 26.—"On the Emigration of Amœboid Corpuscles in the Star-fish (*Asterias rubens*)." By Herbert E. Durham, B.A., lately Vintner Exhibitioner, King's College, Cambridge. Communicated by Dr. P. H. Carpenter, F.R.S., F.L.S.

When small particles (*e.g.* Indian ink) are introduced into the body cavity of a specimen of *Asterias rubens*, they are soon ingested by the amœboid corpuscles of the cœlomic fluid; the latter are carried in various directions by the currents set up by the cilia of the cœlomic epithelium. In the dermal branchiæ these granule-laden corpuscles were observed to adhere to the wall of the branchia, and migrate by amœboid movement to the exterior. [Where such migration is proceeding very actively, a perforation filled by a plug of the corpuscles is formed.] Arrived at the exterior, the corpuscles retain an irregular shape for a while, then they become spherical, swell up, and disintegrate.

Besides corpuscles thus laden with foreign granules, corpuscles containing refringent sphærules (sphæruliferous corpuscles, "Plasma-Wanderzellen") were observed in the extruded material: emigration of such corpuscles was also noted to take place in specimens kept in captivity in glass vessels. Hamann's observation that "Plasma-Wanderzellen" occur in the branchiæ of Echinids helps to confirm the view that this a normal process: further observations are necessary to elucidate its significance. [Dr. Hartog's statements as regards the outward current in the water-tube (stone-canal), were confirmed by the presence of corpuscles in the pore-canals of the madreporite.]

With regard to the other point, it seems clear that minute foreign bodies introduced into the system of a star-fish can be, and are, got rid of by scavenging corpuscles.

"Note on the Madreporite of *Cribrella ocellata*." By the same.

The dogma laid down by Ludwig is that the cavity of the water-vascular system is isolated from other cavities. In a series of sections carried through the madreporite, &c., of *Cribrella ocellata*, it was seen that a few pore-canals of the madreporic plate open directly into the "Schlauchförmiger Kanal"; the ampulla into which the water-tube (stone-canal) dilates being also connected by an opening with the "Schlauchförmiger Kanal": this latter space being enterocœlic in origin, it is interesting to compare the arrangement in Crinoids.

Zoological Society, January 17.—Dr. A. Günther, F.R.S., Vice-President, in the chair.—The Secretary read a report on the additions that had been made to the Society's Menagerie during the month of December 1887, and called attention to a small Fox from Afghanistan, presented by Lieut.-Col. Sir O. B. C. St. John, which should probably be referred to the species shortly noticed by Blyth as *Vulpes griffithi*. It was, however, somewhat doubtful whether the species was really distinct from *Vulpes leucopus*, Blyth, the small Desert Fox of Western India.—Mr. Francis Day exhibited and made remarks on some hybrid fishes from Howietoun, and on a British specimen of the Spined Loche.—Mr. Oldfield Thomas read a report on a collection of Mammals obtained by Emin Pasha in Central Africa, and presented by him to the Natural History Museum. The collection contained 115 specimens belonging to 39 species. The great mass of the collection had been obtained in a district called Monbuttu, just within the Congo Basin. A new Flying Squirrel, of small size, was named *Anomalurus pusillus*, and a new Tree-Hyrax, *Dendrohyrax emini*, after its discoverer.—Capt. G. E. Shelley read a paper on a collection of birds made by Emin Pasha in Equatorial Africa. The series had been formed partly in the Upper Nile district and partly in the Monbuttu country in the

Congo Basin, and contained examples of four species new to science, proposed to be called *Indicator emini*, *Spermospiza ruficapilla*, *Ploceus castanops*, and *Glareola emini*.—Dr. A. Günther, F.R.S., read a report on a collection of Reptiles and Batrachians from Monbuttu, sent by Emin Pasha. The author enumerated seventeen specimens, of which nine were almost generally distributed over the African region; of the remainder, seven were known from various parts of West Africa. One Tree-Snake was described as new, and called, after its discoverer, *Ahætulla emini*.—Mr. Edgar A. Smith, read an account of the Shells collected by Dr. Emin Pasha on the Albert Nyanza, Central Africa. Of the five species of which examples were obtained, three were referred to new species. It was stated that fifteen species of shells were now known from Lake Albert, of which seven were peculiar to it.—Mr. Arthur G. Butler gave an account of the Lepidoptera received from Dr. Emin Pasha. The collection contained examples of 155 species, of which thirteen Butterflies, and two Moths were new to science.—A communication was read from Mr. Charles O. Waterhouse, containing an account of the Coleoptera from Eastern Equatorial Africa received from Emin Pasha. One of the species was new to science, and six of them had previously been received at the British Museum from West Africa only.

Geological Society, January 11.—Prof. J. W. Judd, F.R.S., President, in the chair.—The following communications were read:—On the law that governs the action of flowing streams, by R. D. Oldham.—Supplementary notes on the stratigraphy of the Bagshot Beds of the London Basin, by the Rev. A. Irving. This paper contained the results of field-work during the year 1887. Additional notes on the stratigraphy of the Bracknell and Ascot Hills were given, justifying the reading of the country as shown in Figs. 1 and 2 of the author's last paper (Q.J.G.S., August 1887), the examination of this line of country having been extended as far as Englefield Green. Sections of the beds of the Middle Group as they crop out at Cæsar's Camp, Swinley Park, Ascot, and Sunningdale, were described and correlated with the 76 feet of beds which constitute that group in the Well-section at Wellington College. The stratigraphy of the hills known as Finchampstead Ridges has been worked out from numerous sections on their flanks; and the strata of the Bearwood Hills were correlated directly with them. All along the northern margin a general attenuation of (*a*) the Lower (fluviatile) Sands, and of (*b*) the Middle (green earthy) Sands was shown to occur, and in some places on the northern margin they are found to have entirely thinned away, admitting of distinct overlap at more than one horizon. The second part of the paper dealt with the Highclere district, where the author believes he has established the full succession of the three stages of the Bagshot formation, a section being given across the valley south of Highclere Station, showing the succession of the whole Eocene series (with the *Ostrea bellovacina* bed for its base) as it is developed there. Some important conclusions were drawn as to the Tertiary physiography of the South of England; and the revised tabulation of the Tertiaries put forward by Prof. Prestwich at the Society's last meeting was referred to as supporting some of the main points for which the author has contended. The reading of this paper was followed by a discussion in which the President, Mr. Monckton, Mr. Herries, and Mr. Drew took part.—The red-rock series of the Devon coast section, by the Rev. A. Irving.

Chemical Society, January 19.—Mr. W. Crookes, F.R.S., in the chair.—The following papers were read:—Morindon, by T. E. Thorpe, F.R.S., and W. J. Smith. Morindin, the active colouring-matter of A'l, the root-bark of *Morinda citrifolia*, yields 48·4 per cent. of morindon on hydrolysis. This latter substance is a methylanthracene-derivative of the composition $C_{15}H_{10}O_5$, and differs from all of the eight known compounds of the same formula.—Manganese trioxide, by T. E. Thorpe, F.R.S., and F. J. Hambly. The authors have repeated Franke's experiments (*Journ. für prakt. Chem.*, 1887) on the so-called volatile oxides of manganese, and have been unable to obtain any evidence of the existence of the blue gaseous manganese tetroxide. They find, however, that manganese trioxide exists, and can be formed by the action of a solution of potassium permanganate in sulphuric acid on dry sodium carbonate.—Note on Chatard's process for the estimation of small quantities of manganese, by the same.—Contributions to the theory of the vitriol-chamber process, by G. Lunge. The theory has been recently advanced by Raschig (*Liebig's Annalen*, 241, 161) that the vitriol-chamber process consists in the formation in the first instance from nitrous acid and sulphurous acid of dihydroxylaminesulphonic acid, which, being acted on by nitrous acid, yields sulphuric acid and nitric oxide, the latter being reconverted into nitrous acid. This theory is regarded by the author as untenable on all points, since it completely ignores the existence of nitrosyl sulphate (chamber-crystals), whilst nitric oxide, oxygen in excess, and water do not yield nitrous acid, but nitric acid. In the author's view it is not NO, but N_2O_3 which acts as a carrier of the oxygen in the vitriol-chamber, and the principal reactions are: $2SO_2 + N_2O_3 + O_2 + H_2O = 2SO_2(OH)(ONO)$; $2SO_2(OH)(ONO) + H_2O = 2SO_2(OH)_2 + N_2O_3$. Much NO is present in the front chambers along with N_2O_3; it is formed by a secondary reaction from nitrosyl sulphate, $2SO_2(OH)(ONO) + SO_2 + 2H_2O = 3SOH_2SO_4 + 2NO$, and is principally absorbed by the direct reaction, $4SO_2 + 4NO + 3O_2 + 2H_2O = 4SO_2(OH)(ONO)$; none of it can pass into NO_2 (which does not occur at all in normally working chambers), but some of it may pass into HNO_3, which is at once acted on by SO_2;—$SO + HNO_3 = SO_2(OH)(ONO)$. Thus the normal vitriol-chamber process is not as hitherto understood an alteration of reductions and oxidations, but it is a *condensation* of nitrous acid, or of NO with SO_2 and O_2 to nitrosyl sulphate, and a splitting up of the latter into N_2O_3 and sulphuric acid.

EDINBURGH.

Royal Society, January 6.—Sir W. Thomson, President, in the chair.—Mr. J. T. Bottomley described and exhibited a practical constant-volume air thermometer. This instrument has been designed by Mr. Bottomley so as to be more sensitive, more accurate, and, at the same time, of much greater range than air thermometers hitherto in use. Mr. Bottomley also exhibited some glass globes with internal cavities produced by cooling.—Prof. Tait communicated a paper by Dr. G. Plarr on the roots of $\epsilon^2 = -1$; and a paper by Prof. Burnside on a simplified proof of Maxwell's theorem.—Prof. Tait also read a paper on the Thomson effect in iron.—Dr. Thomas Muir read a paper on vanishing aggregates of determinants. He has obtained the general theorem of which a particular case was discovered lately by Kronecker, and attracted much attention in Germany.—Prof. Crum Brown communicated a paper by Dr. Griffiths on the Malpighian tubes and the "hepatic cells" of the *araneina* and the diverticula of the *asteridea*.

PARIS.

Academy of Sciences, January 23.—M. Janssen, President, in the chair.—Remarks in reference to M. J. Bertrand's recent note on the law of probability of error, by M. F. Tisserand. A solution is given of the problem, "To determine the function $\psi(x_1 - x_2, x_1 - x_3, \ldots x_1 - x_n)$, where $x_1, x_2, \ldots x_n$ indicate n arbitrary quantities independent one of the other, in such a way that this function is symmetrical in relation to $x_1, x_2, \ldots x_n$."—The paper is followed by a communication from M. F. Tisserand on the law of probability as applied to target-firing.—On some notions, principles, and formulas, which come into play in several questions connected with algebraic curves and surfaces, by M. de Jonquières. A rapid summary is given of these principles, &c., some of which have been established by the author himself, some by other mathematicians.—Note on the second volume of the "Annales de l'Observatoire de Bordeaux," by M. M. Lœwy. This volume is largely occupied with the important observations undertaken for the purpose of revising the positions of the stars in Argelander-Oeltzen's catalogue. It contains the precise coordinates of about 3500 stars belonging to the southernmost region of the northern hemisphere.—Contributions to the history of the problematical organisms of old marine basins, by M. Stanislas Meunier. The paper deals with the so-called Bilobites, regarded by some palæontologists as mere physical tracings, by others as real organic remains. Several arguments are advanced in favour of the latter opinion, which is regarded as fairly well established, although not yet rigorously demonstrated.—On the rapidity with which the report of fire-arms is propagated, by M. Journée. All the facts here described tend to show that a projectile possessing a greater velocity than that of the report produces, during its passage through the air, a continuous sound analogous to the explosion of gunpowder.—On the mean distances of the planets from the sun, by M. Roger. It is shown that, apart from certain deviations within a defined

limit, these distances form a geometric progression modified by a periodic irregularity. In a future communication the connection will be pointed out between this law and the theoretic views advanced by the author on the formation of the planetary system.—Summary of the solar observations made at Rome during the last quarter of the year 1887, by M. P. Tacchini. The diminution of spots already noted in September was continued during the two ensuing months, so that the mean was even less than in the previous quarter. The protuberances also were less frequent.—On the phases of Jupiter, by Dom E. Siffert. Most of these observations, which were taken at the Observatory of Grignon, are tabulated for the period from March to December, 1885.—On the application of the Cremonian quadratic substitutions to the integration of the differential equation of the first order, by M. Léon Autonne. In this paper the author develops, for the integration of the differential equation of the first order, the method based on the employment of the Cremonian quadratic substitutions, and applies this method to some special cases of a simple and comprehensive character.—Electric solution of algebraic equations, by M. Félix Lucas. It is shown how, by means of electricity, the solution of equations of any degree p, whose real or imaginary coefficients are given numerically, may be reduced to that of equations of degrees lower than p.—Action of vanadic acid on the alkaline fluorides, by M. A. Ditte. The present paper deals with the fluorides of sodium and ammonium, whose composition is shown to be analogous to that of the fluoride of potassium.—Action of hydrochloric acid on cupric chloride, by M. Engel. The results are tabulated of a series of experiments with the hydrochlorate of cupric chloride. In this substance the chloride of copper appears to be in the anhydrous state, all the water being ultimately combined with the hydrochloric acid.—On the alcoholic fermentation of galactose, by M. Em. Bourquelot. From these experiments, undertaken to determine the true character of the action of the yeast of beer on galactose, the author concludes that pure galactose does not ferment in the presence of the yeast at 15° to 16° C., but that it undergoes alcoholic fermentation when glucose, lævulose, or maltose are added.—On two new genera of Epicarides, by MM. A. Giard and J. Bonnier. The specimens here described live in the fresh waters of the Dutch East Indies, and are regarded as the types of two new genera, Probopyrus and Palegyge, whence are respectively derived the genera Bopyrus and Gyge. They are here named *Probopyrus ascendens*, Semper, and *Palegyge horrei*, G. and B.

BERLIN.

Physiological Society, January 13.—Prof. du Bois Reymond, President, in the chair.—Prof. Fritsch described the detent-joint of a Sheat-fish (Siluridæ). In this fish, as found in the Nile, the adjusting and fixing of the dorsal and pectoral fins is provided for by the various shape and arrangement of the surfaces of the joints, which take the form of hooks and detents. The speaker explained the above arrangements by means of drawings and preparations, by means of which it was easily seen that when once the dorsal spine is fixed it will withstand a very considerable force. These spines constitute a protective mechanism against other predatory fishes, and accounts for the numerical development of these fishes in the Nile. The speaker stated his inability to accept Sörensen's view that the detent-joints of these fishes are organs for the production of sound.—Dr. Joseph had studied the minute structure of the axis-cylinder in the nerves of the electric organ of *Torpedo marmorata* treating them with osmic acid and various staining reagents. By making a careful series of transverse sections he has become convinced that not only the medullary sheath, but also the axis cylinder, possesses a fan-like structure, and that the longitudinal fibrils run in the meshes of the radiating fibres, and are the true conducting tissue of the nerve. The diameter of the axis is six or seven times as great as that of the sheath.—Dr. Weyl had subjected silk to a thoroughly chemical examination, and obtained values for its percentage composition, after purification by treatment with caustic soda, which corresponded with those given twenty-five years ago by Cramer; according to these, silk may be taken as belonging to the proteid class of bodies. Raw silk, and to a greater degree that which has been purified by soda, is soluble in fuming hydrochloric acid; if this solution is poured into alcohol, a white cloudiness is produced, which speedily increases in intensity, and on cooling gives rise to a solid white gelatinous mass. The percentage composition of this new substance obtained from silk, and called by the speaker seroin, is, as regards its carbon and hydrogen, the same as that of silk, but it contains less nitrogen. It possessed in all cases the same composition, so that it is undoubtedly a distinctly characterized chemical substance, and is neither pure silk nor some closely related proteid formed by a splitting-off of ammonia. When treated with dilute acids, seroin yields the same products of decomposition as does fibroin—namely, large quantities of leucin and tyrosin, by which it is characterized as being a proteid. Dr. Weyl hoped shortly to resume this investigation in the direction of a general consideration of the proteid group.

BOOKS, PAMPHLETS, and SERIALS RECEIVED

Voltaic Electricity: T. P. Treglohan (Longmans).—Practical Physics for Schools and the Junior Students of Colleges, vol. i. Electricity and Magnetism: Stewart and Gee (Macmillan).—Behind the Tides: C. B. Radcliff (Macmillan).—Pflanzen-Teratologie: M. T. Masters; German by U. Dammer (Leipzig).—Practical Amateur Photography: C. C. Vevers.

CONTENTS.

	PAGE
The Composition of Water. By **Prof. T. E. Thorpe, F.R.S.**	31
Physical Geography of the Sea.—Hon. Ralph Abercromby	31
Bulletin of the United States Fish Commission	31
Our Book Shelf:—	
Kick: "Flour Manufacture"	31
Remsen: "Elements of Chemistry"	31
Warren: "A Primary Geometry"	31
Letters to the Editor:—	
The Duke of Argyll's Charges against Men of Science.—**Prof. John W. Judd, F.R.S.**	31
The Total Eclipse of the Moon of 1888 January 28, observed at Birr Castle Observatory, Parsonstown.—**Otto Boedicker**	31
"Elementary Chemistry" and "Practical Chemistry."—**M. M. Pattison Muir**	31
"Physical Science and the Woolwich Examinations."—**W. A.**	31
"The Art of Computation for the Purposes of Science."—**E. Erskine Scott; George King**	31
Note on a Problem in Maxima and Minima. (*With Diagram.*)—**R. Chartres**	32
Note on the Dimensions and Meaning of J, usually called the Mechanical Equivalent of Heat.—**Alfred Lodge**	32
The Temporary Thermo-Current in Iron.—**Fred. T. Trouton**	32
Causes influencing the Bathymetrical Range of Deep-Sea Fishes.—**A. R. Hunt**	32
Wind Force at Sea.—**W. G. Black**	32
Untimely Insect Development.—**John Morison**	32
Weasels killing Frogs.—**M. S. Pembrey**	32
"British and Irish Salmonidæ."—**Your Reviewer**	32
Modern Views of Electricity. Part III. VI. (*Illustrated*). By **Dr. Oliver J. Lodge, F.R.S.**	32
Language-Reason. By **Prof. F. Max Müller**	3-
Ferdinand Vandeveer Hayden	32
Notes	327
Our Astronomical Column:—	
American Observatories	330
Astronomical Phenomena for the Week 1888 February 5–11	330
Geographical Notes	330
Our Electrical Column	331
The Proposed Teaching University for London	331
The Total Eclipse of the Moon, January 28	333
Societies and Academies	333
Books, Pamphlets, and Serials Received	335

THURSDAY, FEBRUARY 9, 1888.

MESSRS. GOSCHEN AND HUXLEY ON ENGLISH CULTURE.

WITHIN the last few days two noteworthy utterances on the subject of our national prospects have been made by men whose opinions deserve and command attention. Prof. Huxley has told us, in the *Nineteenth Century*, that though the restraints imposed by civilization have altered the methods by which the struggle for existence is carried on, they have not made it less real or less bitter.

"In a real, though incomplete, degree we have attained the condition of peace which is the main object of social organization; and it may, for argument's sake, be assumed that we desire nothing but that which is in itself innocent and praiseworthy—namely, the enjoyment of the fruits of honest industry. And lo! in spite of ourselves, we are in reality engaged in an internecine struggle for existence with our presumably no less peaceful and well-meaning neighbours. We seek peace, and we do not ensue it."

This application of Darwin's great theory to commercial competition is more than a parable. It is the scientific explanation of causes which have wrecked civilizations in the past and may wreck them in the future. The struggle must go on while men are impelled by the desire for a greater profusion of what sustains life or makes it happier. It often has been, and often is, carried on by the sword, but important victories may be won, and disastrous defeats sustained, by more peaceful means. The discovery of the passage round the Cape transferred the trade of the East from the Mediterranean to London and Amsterdam, and most merchants in the City affirm that the cutting of the Suez Canal has once more deprived England of the advantage of situation. The commercial success of Switzerland, however, proves that national characteristics are at least as important as geographical position, and it is well from time to time to ask if we are doing all that in us lies to train those who shall follow us to maintain what our predecessors have won.

It is from this point of view that the second of the two utterances we have referred to is specially interesting. Mr. Goschen is at one with Prof. Huxley as to the severity of the struggle in which we are engaged. "Our position in the race of civilized nations," he told the undergraduates at Aberdeen, "is no longer what it was. We had a great start in industries and commerce, and by virtue of that start we attained to a station of unprecedented and long unchallenged supremacy. That supremacy is no longer unchallenged. Others are pressing on our heels. We require greater efforts than formerly to hold our own." Theory and experience agree. The biologist tells us that a state of struggle is the normal condition of man as of all other living beings, and that it must become keener as our numbers augment. The Chancellor of the Exchequer, with his hand on the pulse of English trade, is witness that the strife is growing in severity.

And this is not all. Mr. Goschen is not satisfied that we have as a nation all the qualifications for success. In a powerful address, which evidently expressed a matured conviction, he insisted that Englishmen lack "intellectual interest" in their work. They regard their business as a necessary evil, from which they delight to sever themselves as often and as completely as possible. They are ignorant of the general principles which underlie the conduct of trade, or at least are careless in noting their application to particular instances. It is quite in accord with this that they regard education not so much as an essential to fit a man for the battle of life as an ornament for his leisure hours. And here again Professor and politician are at one. The highest intellectual ideal of our University men, says Mr. Goschen in effect, is, or at all events until very lately was, perfection of literary form. Our public schools have aimed chiefly at turning out scholars who could write Latin verse. Our educational systems, echoes Prof. Huxley, were fashioned "to meet the wants of a bygone condition of society. There is a widespread and, I think, well justified, complaint that [our system of elementary education] has too much to do with books and too little to do with things."

To discuss the whole question thus opened—an indictment of University and Board-school alike—would be impossible in the limits of space at our disposal, but regarding it from the point of view in which our readers, like ourselves, are specially interested, we cannot but note a sad corroboration of Mr. Goschen's words. In no trades could a genuine intellectual interest be more easily excited than in those which involve a knowledge of science, and in none have Englishmen more conspicuously failed. It is needless to recapitulate stories like that of the discovery of the aniline dyes in an English laboratory, and the wholesale appropriation of the trade to which that discovery gave rise by German manufacturers. The fact is patent and obvious to all who have studied the question. Science can only be successfully cultivated by men who take an "intellectual interest" in their work; and in trades which depend upon a knowledge of science, it is the foreigner who achieves success. Where does the fault lie? For the masters and foremen, the colleges which are springing up all over the country may do much. They are, we believe, slowly creating a class of men who have a sound foundation of scientific knowledge, and a genuine interest in scientific progress. But for the rank and file, for the clerk and artisan, it is upon evening classes that Prof. Huxley thinks we must chiefly rely, and here the main difficulty seems to be to secure good teachers for classes in science and technology. They are, says Prof. Huxley, "not to be made by the processes in vogue at ordinary training colleges." "As regards evening science teaching"—we quote from the Report of the Royal Commissioners on Technical Instruction—"there seems to be nowhere in Europe any organization for systematic evening instruction comparable, as regards the number of subjects taught, and the facilities afforded for the establishment of classes, and for the examination of the students' work, with that undertaken by the Science and Art Department in this country, and recently supplemented, in the application of science to special industries, by the City and Guilds of London Institute.

"At the same time it must be borne in mind that in many towns visited by the Commissioners the evening science teaching was conducted by Professors of higher standing than, and of superior scientific attainments to, the ordinary science teachers who conduct courses in some of the largest and most important of the manufacturing centres of this country."

Here, then, appears to be at all events one weak point.

The Science and Art Department has a flexible system, capable of application to the wants of town and country. Students are examined by the thousand every May, but though the standard of attainment is rising, there is a general opinion—which is supported by the statements of the Royal Commissioners—that the instruction given by the teachers falls far short of an ideal which might be and ought to be reached. And yet this matter of good or bad teaching is vital. "It is absolutely essential," says Prof. Huxley, "that the mind [of the teachers of scientific subjects] should be full of knowledge and not of mere learning, and that what he knows should have been learned in the laboratory rather than in the library." "This," according to Mr. Goschen, "is the first test of the value of an educational system, whatever its curriculum may be. Is it intelligent? Is it thorough? Above all, is it rousing? Does it excite intellectual interest in those who come under its influence? Does it develop in them the temper which always asks for a reason and struggles to arrive at a principle?"

Teachers competent to work an educational system which satisfies these requirements must be themselves highly-finished educational products. They must have risen above the vulgar pocket-filling ambition of passing so many students per annum. Risen above it, not in the sense of ignoring it, for in this prosaic world a livelihood must be earned, but in the sense that the mere drudgery of bread-winning is for them lit up with a glow of the enthusiasm of the student who has knowledge to impart which he himself values for its own sake.

We want as science teachers not men who have crammed just enough to enable them to cram their pupils in turn, but men—and we believe there are many, though far too few of them—who have learnt to regard themselves as members of the great scientific army the advance of which is the most remarkable movement of the age.

How are they to be got? They cannot be obtained in the requisite numbers without a systematic search and preparation. It may be, as Prof. Huxley hints, that additional pecuniary inducements must be held out to secure them. This is a question on which the Chancellor of the Exchequer may have an opportunity of giving practical aid to English science and education. Or, if this is Utopian, let us suggest to Mr. Goschen that it would be well if his great influence were used to urge the Government to make the most of the machinery it already possesses.

Prof. Huxley has been for years the Dean of the Science Schools which are the centre of the system of evening teaching which the Royal Commission on Technical Instruction has pronounced to be in many respects the best in Europe. Among the highest rewards given to the successful candidates in the May Examinations are free passes for more or less prolonged courses of study at South Kensington.

Teachers in training attend the classes, and year by year batches of science teachers are brought together to receive special instruction in the subjects they are engaged in teaching. One of the great difficulties to be encountered by a provincial College is the fact that the calls upon the Professors are too multifarious. Students of all classes—would-be engineers, doctors, electricians, and a dozen similar groups—all desire courses of instruction designed to meet their particular wants. It has been rightly decided that this obstacle shall not impede the progress of the State-aided system of evening instruction. A special institution is provided to meet the special requirements of those who are engaged in it. The union of the Normal School of Science with the Royal School of Mines has not interfered with the attainment of this end, while it has secured the advantages which result from the mingling of students who are studying the same subjects with different aims.

The State, then, has recognized the need for trained science teachers, just as it feels the necessity for providing properly-educated officers for the Navy. It is admitted that both classes can best receive the instruction they need at special institutions. The Royal Naval College at Greenwich has been provided for the one, the Normal School of Science for the other.

The school gives evidence of vitality and success. Within the last five years the number of students has doubled. A very considerable amount of original research is done in its laboratories. Now, however, its very efficiency is a danger. It has outgrown the buildings which have been assigned to it. By permission of the Commissioners of the 1851 Exhibition, classes are carried on in what was the Colonial Exhibition. But duty to the interests with which they are primarily charged will, before long, compel them to withdraw this hospitality. Driven from the holes and corners in which it has been compelled to seek refuge, the Central School for the training of teachers of evening science classes may be compelled to reduce its numbers, and to limit its usefulness at the very moment when Mr. Goschen, Prof. Huxley, and all competent educationalists are agreed that one of our most pressing national wants is the elevation of our teachers, and of their type of teaching.

We have chosen this as a single example which serves to illustrate the wide generalization which we have been discussing. Is the interest of the average Member of Parliament in the dangers which threaten our trade sufficiently intellectual to lead him to sanction the cost of necessary precautions? In these democratic days the fate of the English people is in their own hands. If they choose that the education of their bread-winners shall be conducted on the principles on which the "accomplishments" were taught in an old-fashioned ladies' school—if they choose to send competent Commissioners all over Europe, and, when they tell them that one of the chief defects of their educational system is the comparative inefficiency of their teachers, they nevertheless deliberately half-close the doors of the school specially provided to remedy this defect—there is no help for it, and but little hope for them.

Wars may be caused by race hatreds which have taken centuries to gather, but success or failure often depends on the placing or misplacing of a few thousand men. Commercial competition may be, as Prof. Huxley tells us, due to causes which affect all living things. The progress or decadence of England will depend upon how it adjusts itself to the altering character of the strife; and we confess that we shall watch with interest to see what amount of practical support the Chancellor of the Exchequer is prepared to give to the views of the Lord Rector of Aberdeen. The test will be applied when the Technical Education Bill is again brought forward, and when the particular need which we have chosen as an illustration has to be met.

THE PROPOSED TEACHING UNIVERSITY FOR LONDON.

WE printed last week the petition which has been presented to the Privy Council by the Association for Promoting a Teaching University in London. We have now before us the petition of University College and King's College, to which is appended a proposed draft charter for the University, under the name of the Albert University of London. If, as seems probable, the promoters have been well advised in claiming no less a surname than that of the Metropolitan district for which the University is to serve, the prefixed name of the late Prince Consort, to whom England is undoubtedly indebted for the encouragement his influence gave to educational and scientific work, is perhaps as good a way as could have been hit upon for avoiding confusion with the existing University. For the rest, the charter appears to be an adaptation to the circumstances of that granted to the Victoria University; the principal differences being—the place reserved for the Royal College of Physicians and Royal College of Surgeons in the University, which is one of complete equality with the governing bodies of the University Colleges themselves; the power conferred upon the legally recognized medical schools of London, as such, to claim, as of right, admittance to the University, on equal terms with the Medical Faculties of University College and King's College; and the greater simplicity of the governing body. In the case of the Victoria University a complicated division of authority was resorted to, with the view of obviating mutual jealousies between the various cities and towns in which, in that case, the several Colleges were to be situated. The Senate proposed for the Albert University consists of three members chosen by the governing body of each College associated with the University; the College of Physicians and College of Surgeons being reckoned among associated Colleges, if willing to accept the position, but the twelve medical schools not being so reckoned; of four members representing the assembly of each Faculty, such assemblies being composed of the teaching staffs of all Colleges or medical schools admitted in respect of the Faculty; and of six representatives nominated by the Crown in the first instance, of whom three are eventually to be replaced by representatives of the graduates in Convocation.

Compared with this body, the composition of the Senate of Physicians and Surgeons proposed in the petition of the two Royal Colleges presents an even greater degree of simplicity. No provision is made for the representation of any other interest than that of the petitioners themselves; and the two Colleges divide the representation equally between their respective governing bodies. It is understood, however, that in the case of the College of Surgeons this proposal has not given satisfaction, even within the limits of the College; and that some representation will be claimed for Fellows of the College other than those who constitute the Council which governs it.

From the point of view which is especially our own, the quarrel about degrees, and the interests of rival institutions, occupy a place secondary in importance to considerations affecting the promotion of knowledge and science, and only important in so far as they are concerned. If the proposal of the Royal Colleges is carried into effect, and a committee of eminent physicians and surgeons is intrusted with the power of examining for and giving medical degrees, there can be little doubt that the great building on the Thames Embankment, and the space behind it shortly to be covered with building, will speedily develop into a teaching institution, with provision for research; and thus knowledge will be increased, and science promoted, by the addition of one more to the number of efficient schools for special purposes which are now open in London. The promoters of the Albert University do not offer us any immediate addition of this nature to the resources which are now available. We have examined the draft charter with care, in order to detect, if possible, the traces of a design to check the foundation or perfecting of new institutions, in the interest of those already existing. But the promoters, we are bound to say, appear to have guarded against all objection, by following, in this respect, the charter of the Victoria University. The appeal which is given to the Privy Council, in case of the refusal of the University to admit a new College, is a satisfactory provision against the spirit of monopoly. In the absence of danger from this point of view, the Teaching University promises more than is offered by the Senate of Physicians and Surgeons, in the interests of science. The prospect of establishing, as a qualification for admission to the University, a general standard of efficiency for Colleges professing to do the work of scientific teaching, has greater attractions for us than that of the opening of a single new school of medical and surgical research. Moreover, by the institution of the Teaching University, we shall secure the first, without rendering it less probable that in time the second also may follow.

We notice that the right of admission offered to all the London medical schools, though absolute so far as the Medical Faculty is concerned, is, in regard to the Faculty of Science, made conditional on efficiency. This is as it should be. Probably some of the smaller hospitals will regard with equaminity the extinction of their pretensions to be recognized as efficient schools of science. Others will be incited to render themselves efficient. In both cases the result to science will be a pure gain. One matter of importance appears omitted in the programme of the Albert University: the position to be assigned within the University, if its admission is contemplated, to the Royal School of Mines and Normal College of Science. It would appear proper that this point should be further considered, if the project ever reaches a more definite stage.

Upon the matters in dispute between the University of London and the University Colleges we desire to maintain an attitude of impartiality. On the one hand, it is urged that the credit of a degree will not stand the strain consequent on the creation of a second degree-giving body in London; and that all the reform desirable, in the interests of education, is the introduction of a larger number of teachers on the governing body of the existing University. On the other, stress is laid upon the importance, for educational purposes, of the independence of teachers from irresponsible external control, and upon the necessity of an organization of teaching for London more thorough than can be afforded by any constitution or reconstitution of an examining body. But whether

the desired reforms are carried out by changing the constitution of the University of London, or by instituting a new University, two things appear in any case to be incontestable: that the open examinations conducted by the existing University shall continue to be conducted by an impartial authority; and that the Colleges shall be allowed to organize their work in the manner best suited to promote their own efficiency.

Sir Philip Magnus, in a letter which appeared in the *Times* on Thursday, appears to consider the dispute as one between the efficiency of "lectures" on the one hand, and of "reading" on the other; and he cites the now well-known dictum of Darwin, in favour of reading, and against lectures. But it would be to mis-read, in a strange manner, the lesson of Darwin's life, if from it were to be drawn a conclusion against the existence of Universities for teaching purposes, and in favour of examinations. If Darwin carried from Edinburgh a profound dislike to unintelligent lecturing, of the epideictic sort, he was at Cambridge known as "the man who walks with Henslow." In Sir W. Hamilton's famous analysis of the work of Universities, examination holds the first place only among no less than seven "exercises" by which study, in a teaching institution, can be promoted; the others being "disputation, repetition, written composition, the practice of teaching, conversation with and interrogation of the learned, and social study." To these must be added, by the student of science, the practice of experiment under competent supervision. Some of these appear to us of more value than examinations, some of less; but it is obvious that an institution which is solely concerned with examinations does not cover the whole ground of institutional aid to study; and it is of no avail, as between one institution and another, to exalt the benefits of "reading," which is not peculiar to either. In conclusion, we trust all parties to the controversy will bear constantly in mind that degrees and examinations, lectures and colleges, are, after all, but means to an end. The end is the spread and advancement of knowledge, through educational methods and research.

MANUAL OF BRITISH DISCOMYCETES.

A Manual of the British Discomycetes. By William Phillips, F.L.S. 8vo, 446 pages, 12 plates. International Scientific Series. (London: Kegan Paul, Trench, and Co., 1887.)

IT is by no means an uncommon misfortune to find that text-books are not written by persons the most competent, or with the widest experience; hence the results are very far from satisfactory, and no one expresses much gratification. Now and then notable exceptions to this rather general rule may be discovered, to the delight of all who are interested in that special branch of science to which the book is devoted, and the great edification of the student. It is beyond our province to inquire why the most suitable men are so seldom engaged in the production of "manuals," or why the most skilful manipulator, with a few months' study and much "coaching," cannot compete successfully with the practical hand well steadied with a twenty years' experience. It will be enough to intimate that no one acquainted with British Cryptogamic botany would for a moment hesitate to pronounce that the most suitable person to undertake a manual of the Discomycetes would be Mr. W. Phillips, of Shrewsbury, if practical knowledge, and persistent investigation, extending over at least two decades of years, are to be accepted as qualifications.

With these preliminary observations it will be at once evident that, in general terms, and as a whole, we feel bound to give this little volume our heartiest commendation; and if, in the course of our remarks, we indicate any weak places, it will be with the desire to act with the tenderness of a friend, and to point out how, in our conception, an admirable manual may be rendered more perfect or more useful.

No apology is needed for restricting a book like the present to a small controllable group of some 600 species, especially when the limits are so well defined that a student may devote himself exclusively to it, with advantage to himself, without any special acquaintance with outside groups. It is generally admitted that the entire British Fungi, with its thousands of described species, is become too unwieldy and extensive for any ordinary individual, not content to become a slave to his subject and a martyr to science. The Discomycetes present an admirable group, capable of isolated study; and for this purpose a careful and trustworthy manual, at a moderate price, is now ready at the student's hands.

"The subject of classification," the author says in his preface, "will not fail to awaken some controversy." "To adhere as closely as possible to the long-accepted Friesian system has been the practice of English authors, but this has been carried a little too far, owing to our 'insular prejudices,' and the time has come when a new departure must be made." We are prepared to accept this paragraph—exclusive of "insular prejudices," which we cannot admit—and with it the "new departure." To our mind this is a most moderate concession, and we doubt not that, if controversy there should be, its direction will be in favour of far greater innovation than Mr. Phillips or ourselves would approve. The details of the new arrangement must be subjected to closer examination and the test of experience, but at present we see no reason to take exception to them. We have long been of opinion that some such modification of the old classification was desirable.

There is, nevertheless, one point on which we have always uttered a protest, and repeat it again, since in two or three instances in the present volume the error has been committed. We allude to the addition to, or alteration of, a generic description, and the appending of the original author's name, with the word "amended" after it. We protest against *amended genera*, because they are nobody's genera; they are not the genera of the original author, but a "thing of shreds and patches." A genus should not be altered or amended, in order to fit any subsequent species which a later author may desire to incorporate. He should keep the new species outside, and accommodate it in other ways, rather than modify or "tinker" the work of a predecessor, and assume the change to be an "amendment," whereas it *may* be something very different, and probably *would* be to the old author himself, if he could be resuscitated to gaze on the freaks of his successors.

As for the number of species described in this volume,

we may remark that it is more than double that which the "Hand-book of British Fungi" included in 1871. One of the many valuable features of the book is, that, wherever possible, measurements are given of the sporidia of the various species, in micromillimetres, in addition to the dimensions of the fungus in its entirety. We specially allude to this feature in order to have an opportunity of adding that in our experience we have never met with a more careful or expert hand at microscopical measurement than the author of the present work, an opinion based on hundreds of observations made in concert during a series of years. Yet we must urge that, however useful the micromillimetre undoubtedly is in spore-measurement, it is not so well to use it for larger bodies, such as the cup of a *Peziza*, when the millimetre or its decimal part would appeal more directly to the eye and experience. 500 μ may be equal to half a millimetre, but the mind more quickly and readily conceives the half millimetre than the 500 μ. We observe a lack of uniformity in dimensions appreciable by the naked eye, which is avoided in measurements under the microscope. For instance "¼ to ½ line broad" (p. 249), "cups 200 to 500 μ" (p. 257), "cups 500 to 800 μ" (p. 321). What relation does the "line" bear to the micromillimetre? If half a line is about 500 μ, why use the two units of measurement? Would it not have been better to follow Stevenson in his "British Fungi," and to reduce all measurements to the centimetre, millimetre, and micromillimetre, which would have been much more consistent, and far better than the mysterious "line," and had the merit of being more intelligible to the foreigner than a unit of which he has no knowledge or experience.

It would be useless to assume that the work is absolutely free from errors, but these are mostly of a trivial character, although more numerous than we could have wished. We doubt whether "conidia" would not have been a better term than "spermatia" in such a connection as *Calloria fusarioides;* and we also doubt whether our author accurately appreciates the value of the terminations in such words as *violascens, virescens, fuscescens, nigrescens*, &c.

As for the general scope of the work, we may say that each species begins with the diagnosis, then follows its synonymy, especially in British works, references to figures, and published specimens. If these are in the main accurate, as we have no reason to doubt, they will be exceedingly valuable, but manifestly only experience can prove this, and figures are very liable to become displaced or transposed. The habitat succeeds the synonymy, which is followed by special notes or comments; then the derivation of the specific name, now and then hardly successfully interpreted, as for instance on pp. 291, 325, and 369, where *ater* would have been better rendered "dark" instead of "black"; and finally a list of localities.

At page 358, *Ephelis* is inserted as a genus of Fries's. The same genus is claimed by Saccardo ("Sylloge," iii. p. 691) for a genus of Sphæropsideæ, and we fear that Phillips will have to give way to Saccardo, as both cannot stand, and there is no evidence that Fries regarded his genus as ascigerous.

Forty pages at the end are most useful appendages to the work, consisting of a glossary of terms, full titles of the various works quoted, and an exhaustive index. To the last page Mr. Phillips has spared no trouble to make his work as complete and useful as practicable, and we trust that he may be rewarded for his labour of love (for such it undoubtedly has been) by being called upon speedily to correct the verbal errors in preparation for a new edition. M. C. C.

OUR BOOK SHELF.

Physiography: an Elementary Text-book. By W. Mawer, F.G.S. (London: Marshall and Co., 1888.)

THIS is another addition to the steadily increasing number of text-books adapted to the elementary stage of physiography. The usual plan of dividing a book into chapters is not adhered to, but probably the author is of opinion that he is working according to the true spirit of physiography in drawing no hard and fast lines.

In the majority of cases the author has succeeded in his endeavours to explain everything in the simplest way, but in a few cases his anxiety to do so has led him astray. The following may be quoted as examples, and the obvious shortcomings need no further comment:—

"Work is the moving of matter" (p. 8); "Energy, when active—when actually doing work—is in the condition called *kinetic*; when it is passive and only ready to do work, it is *potential*" (p. 9).

With a few exceptions of this kind, the book is admirably adapted to the syllabus which it is intended to cover. That it is not a mere cram-book is evidenced by the mass of useful information which is given. A good general outline of the nebular hypothesis is presented, in so far as it concerns the history of our globe, and there is also an outline of the classification of animals and plants. The astronomical portion of the syllabus also receives a fair share of attention. One omission, however, has been made, and that is the use and meaning of the term "stress": the word apparently does not occur even once in the whole book; this is rather unfortunate now that modern physicists are beginning to regard gravitation, magnetism, &c., as stresses.

Apart from its use as a class-book, it can be recommended to the general reader as an outline of science.

A. F.

Early Christian Art in Ireland. By Margaret Stokes. (Published for the Committee of Council on Education, by Chapman and Hall, 1887.)

THIS is one of the South Kensington Museum Art Handbooks, and it deserves to rank among the best of the series. The Christian antiquities of Ireland are in their own way as remarkable as any group of antiquities in the world, and a satisfactory account of them, such as ordinary readers might understand and appreciate, was greatly needed. In undertaking to supply what was wanted, Miss Stokes devoted herself to a task for which she was well equipped by previous study, and she has produced a little book which can hardly fail to excite interest in her subject, and which will be welcomed even by antiquaries to whom the facts of Irish archæology are already well known. A chapter on illumination is followed by one on Irish scribes on the Continent; and then come chapters on metal-work, sculpture, building and architecture, with a chronological table of examples of Irish art the date of which can be approximately fixed. The work is illustrated by upwards of a hundred good woodcuts. In her treatment of all questions relating to early Christian art in Ireland, Miss Stokes displays a thoroughly scientific spirit, and her style has the merit of being always clear, fresh, and unpretending. She rightly claims for her subject that it has a practical as well as an intellectual

interest. If the higher class of workers in Ireland took the trouble to study systematically the objects here so carefully described, an epoch might be marked in the development of Irish technical skill.

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 Duke of Argyll's Charges against Men of Science.

THE Duke of Argyll's singular appetite for besmirching the characters of men of science appears to grow by what it feeds on; and, as fast as old misrepresentations are refuted, new ones are evolved out of the inexhaustible inaccuracy of his Grace's imagination.

In the last two letters which the Duke of Argyll has addressed to you, he accuses me of having charged the members of the French Institute with having entered into a "conspiracy of silence" in respect of Mr. Darwin's views. I desire to say that the assertion that I have done anything of the kind is untrue and devoid of foundation.

My words, in the passage of which the Duke has cited as much as suited his purpose, stand as follows: "In France, the influence of Elie de Beaumont and of Flourens—the former of whom is said to have 'damned himself to everlasting fame' by inventing the nickname of 'la science moussante' for evolutionism—to say nothing of the ill-will of other powerful members of the Institute, *produced, for a long time, the effect of a conspiracy of silence.*"[1] I used the words I have italicized advisedly, for the purpose of indicating that, though the members of the Institute did *not* enter into a conspiracy of silence, the notorious antagonism of some of them to evolution produced much the same result as if they had done so.

If the Duke of Argyll were properly informed upon the topics about which he ventures to speak so rashly, he would know that M. Flourens wrote a book in vehement denunciation of evolutionism. As I reviewed that book not very long after its appearance, I could not well be ignorant of its existence. And being aware of its existence, I could not possibly have charged M. Flourens with taking any part in a "conspiracy of silence."

The "effect" of the known repugnance to Mr. Darwin's views of some of the most prominent members of the Institute, to which I refer, is the effect upon the younger generation of French naturalists. Considering the influence of the Institute upon scientific appointments, the chances of a candidate known to be an evolutionist would have been small indeed; and prudence dictated silence.

Mr. Carlyle has celebrated the courage, if not the discretion, of a certain "Rex Sigismundus," who, his Latin being called in question, declared that he was, as a Royal personage, "supra grammaticam." The Duke of Argyll appears to be of King Sigismund's opinion in respect of the obligations which are felt by humbler persons, who have, wittingly or unwittingly, accused their fellows wrongfully; and I do not suppose that he will descend, on my account, from a position which may be sublime or may be ridiculous, according to one's point of view. The readers of NATURE will choose their own.

T. H. HUXLEY.

Bournemouth, February 4.

[1] "Life and Letters of Charles Darwin," vol. ii. pp. 185-86.

An Explanation.

SINCE the Duke of Argyll's references to myself have been interpreted in a manner likely to convey an erroneous impression to the readers of NATURE, it seems to me to be now necessary to give some explanation of the facts in which I am concerned. I intend, however, to go no further than to establish the position his Grace has taken up as regards myself. Such a step, savouring somewhat of presumption on my part, would not have been taken if Prof. Judd had admitted that, although no paper of mine was ever before the Council of the Geological Society, an offer to present such a paper was, doubtless for sufficient reasons, at once declined.

In the spring of 1885, by the advice of Mr. Murray, who had been for some time engaged in examining my recent geological collections from the Solomon Islands, I offered to Prof. Judd, then Secretary of the Geological Society, to present my observations on the upraised coral-reef formations in the form of a paper, in which, as I stated, Mr. Darwin's theory of coral reefs would be brought under consideration. This offer being declined, my observations were taken up by Mr. Murray and were published in the Transactions of the Royal Society of Edinburgh for 1885. As I saw too plainly that the new view of the origin of coral reefs was very far from being generally accepted, I deemed it advisable in preparing my paper to draw no inferences and to allow the facts to speak for themselves. However, six months after the reading of the paper, whilst going over the proofs, having been assured that the theory of Mr. Darwin was rapidly losing ground, I appended some remarks in which I gave the general bearing of my discoveries.

Had I harboured a desire in my mind to record any disappointment in connection with the appreciation of my work, I might have done so in the preface of my small geological volume recently published. The reflection that I had succeeded, and that Mr. Murray's views, as I was told, were being generally received, gave me ample grounds for satisfaction; and there was therefore no reason why I should refer to any difficulties of a personal character. I must confess, however, I was afterwards deeply disappointed on finding that, although the nature of my discoveries was first announced in the columns of this journal in January 1884, whilst the observations themselves had been nearly two years before the world, my name and work were *studiously ignored* in the recent controversy by those who spoke on behalf of English men of science, and particularly on behalf of the Geological Society. Naturally it was there that I looked most for approval. I soon perceived, however, that it could not be in the want of publicity that the reason lay, nor even in the insufficient lapse of time since the publication of my papers. Long abstracts were given in the columns of this journal of the principal paper (Trans. Ed. Roy. Soc., 1885), and of a paper also read before the Royal Society of Edinburgh (Proc., 1886). At the beginning of 1885 (or perhaps earlier) I sent to Prof. Judd a blue pamphlet published in New Zealand, in which I briefly described the discoveries I had made up to the end of 1883. At the beginning of 1886 I sent to him my principal Edinburgh paper of the previous year.

It then occurred to me that since Prof. Dana's last paper, of September 1885, was the chief rallying point of the opponents of Mr. Murray's views, the cue in estimating the value of my work might have been thence derived. I found, however, that Prof. Dana had only before him, when referring to my discoveries, an extract from a private letter of mine to Mr. Murray written in the midst of my work, and published in NATURE in January 1884. Rightly enough, he did not consider such a brief account as at all conclusive. My published observations had yet to come before him. It was not, therefore, from the other side of the Atlantic that in estimating the value of my observations Mr. Murray's opponents had taken their cue.

I was forced, therefore, to the conclusion that the reason lay rather in the competency than in the bearing of my observations. I could find no other explanation of the fact that in the succession of replies to the Duke of Argyll's article, entitled "A Great Lesson," no reference whatever was made to the recent important evidence I had adduced—evidence of which at least one of the writers had been previously aware during a period of two if not three years. Under these circumstances, I accepted the decision which the lapse of nearly three years had not affected; and, having naturally some degree of sensitiveness, I withdrew from the Geological Society.[1]

[1] Mr. Guppy was induced afterwards to withdraw his resignation.—ED. NATURE.

In conclusion, I may say that these circumstances do not alter my conviction of fighting on the winning side. The reasons of my faith I hope to give in the next journal of the Royal Scottish Geographical Society. H. B. GUPPY.

17 Woodlane, Falmouth.

Snow Crystals.

YESTERDAY was very favourable for observing the beautiful appearance of sunlight reflected from snow crystals. As one walked across a field, stars appeared to start forth by thousands from amongst the fresh-fallen snow. They were particularly bright and numerous when one walked in the direction of the sun. They appeared almost at all distances, and almost of all sizes, those near at hand being never very large but of great brilliancy and most exquisite colour. The phenomenon was sufficiently striking to induce me to stop and observe it more closely, and the first thing I noticed on stopping was the permanence of each little star of light, although the snow was dropping melted from the hedges and trees by the heat of the sun. A slight movement of the head was sufficient to change the colour of a red star to green or *vice versâ*. It seemed as if the most brilliant colours were seen when looking in a direction nearly but not quite towards the sun. The level surface of the snow appeared as if strewn with gems—and not only near at hand, for even twenty and thirty yards away a large star would shine forth with a subdued but fine colour. I then noticed a peculiar uniformity of shape in these reflections from snow crystals. The shape never varied from that of a blunt arrow-head. This was very striking in the large stars which appeared at a distance; but once noticed, it was obvious enough that even the minute specks at one's feet were all of this form. Nor did the position of this snow vary to any appreciable extent. The inclination seemed always a little to the right, and this occurred no matter in what direction I looked, whether towards the sun, or away from it, or in any other direction. Wishing to know the absolute size of the larger snow crystals, or combination of crystals, I looked for a fine appearance, and estimated as well as I could its magnitude by covering it with a small object held at arm's length. The distance of the spot where the crystal appeared proved to be forty-three paces from where I stood, and its magnitude could not have been much less than three inches in this particular case. Now if, as I suspect, the form of the star which appears so persistently is due to the upper or lower stem only of a complete arrangement of crystals in an hexagonal shape, these combinations must occasionally be six inches or more in diameter. I did not succeed in recognizing any larger than very minute arrangements of crystals in the snow itself, but it is obvious that the sun's rays reflected from a long distance must single out those faces of crystals which happen to be parallel to one another over a certain limited area. Observation of these reflections, therefore, calls in to our aid a power of analysis in the sun's rays to detect symmetrical arrangements of snow crystals quite unrecognizable by mere inspection. Might I ask for some explanation of the phenomena?

Hull, January 30. A. N. S.

"The Mammoth and the Flood."

MR. HOWORTH's letter does little more than travel again over old ground, and two only of the points raised require any notice on my part; the third—the value to be attributed to the opinion of any particular geologist—being immaterial to the main question. As another President of the Geological Society has said: "Science needs no infallible Church, and admits of no Pope."

In regard to the localities in which mammoth remains have been found, I have not "resuscitated" any theory, but have taken my facts from Mr. Howorth's book. His second letter appears to me to ignore a distinction which I was careful to draw in my reply to his former one. That mammoth *bones* should be found at considerable distances from, and elevations above, the existing rivers, offers no difficulty. Indeed, they could not occur, except accidentally, in deltas which are now in course of formation. But, so far as I can ascertain, there is no reason why these "beds of clay and gravel" should not be deposits of rivers which drained the same regions under different climatal conditions, in the distant ages when the mammoth lived in Siberia. The case is precisely similar in England. We should not expect to find mammoth bones in the mud-flats about the mouths of our southern rivers, but in the old valley gravels which occur sometimes even 90 or 100 feet above the present level of the rivers. But the facts most difficult to explain are the occurrences of the *carcasses* of mammoths. It was of these, and of these only, that I was speaking in my letter, as I think would be clear to most readers. No geologist, so far as I know, would deny that the Siberian climate has considerably changed since the mammoth wandered over its tundras, and very likely not seldom got bogged; but the question is, Has it changed suddenly or gradually? The occurrence of the frozen carcass is undoubtedly most simply explained by postulating a sudden change; but when we begin to consider what this means, the remedy, though apparently so simple, seems as heroic as that of the father "who cut off his little boy's head to cure him of squinting." It is then for the best preserved of these frozen carcasses that I suggest the possibility of a drifting and a gradual entombment by the deposits of the ancient rivers. I have again consulted Mr. Howorth's book, and find, between pp. 82 and 89, notices of the discovery of at least ten mammoth carcasses, mostly occurring very far north in Siberia, and nearly all mentioned in connection with rivers: of one it is even said, "like most of the others, it is found on the bank of the river, which had been undermined by floods."

Mr. Howorth further asserts that I cannot have read his book because I charge him with invoking a series of catastrophes when he argues "in favour of one catastrophe only." But, notwithstanding his disclaimer, I would like to know how we are to bring about a deluge to drown the mammoth and a sudden permanent fall in temperature to freeze his carcass (query, one catastrophe, or two?) without "a series of catastrophes." I presume that, as this is a scientific question, we must not invoke a miracle. If continents gambolled like whales—which would be needed for Mr. Howorth's far-reaching flood—would this, unless there were a very special arrangement of continents, so materially alter the climate? and, if they did so disport themselves, what set them dancing? If a number of insular volcanoes exploded with twenty-Krakatáo force, this would be a series of catastrophes, but it would probably leave the climate unchanged. If the earth's axis of rotation were suddenly altered materially in position—perhaps the simplest mode of bringing about the two results—would no catastrophic changes be needed to effect this alteration? Mr. Howorth's retort, in fact, indicates better than anything which I can write how completely he has failed to realize the conditions of the problem which he attempts to solve.

But enough. It is impossible for me to continue this correspondence. The reviewer's task is often not a very pleasant one, but a new terror would be added to the work if it involved an interminable controversy with authors on matters of opinion. Dreading this, I deliberately abstained from signing the review, because I knew from past experience that this was my only chance of escape from the flood of Mr. Howorth's controversial eloquence, which, like the proverbial river, *Labitur et labetur in omne volubilis ævum.* YOUR REVIEWER.

An Incorrect Footnote and its Consequences.

IN following up Baltzer's erroneous reference concerning the "Demonstratio eliminationis Cramerianæ," Mr. Muir, as described in his letter on p. 246, seems at first to have been singularly unlucky. For, on referring to the catalogue of Lord Crawford's mathematical library under "Mollweide," although the work itself was not immediately forthcoming, there was a cross-reference to "Prasse, M. von," under whose name the essay was duly catalogued. The Dun Echt copies, for there are two of them bound up in volumes of mathematical pamphlets, are copies of the original "Demonstratio," in 8 folios, with the pages 4 to 15 numbered, and the last blank. In a gap on the title-page of one copy has been written "auct. Mauricius de Prasse," apparently long ago, and in a German hand. But apart from this the last sentence of the first paragraph identifies the author as the writer of "Usus logarithmorum," which bears the same Latin form of the name in print.

The cross-reference is due to the presence in the library of a little book the title of which is worth giving in full, as it contains the names between which Baltzer's mistake arose, and it also gives the German form of von Prasse's name: it is "Logarithmische Tafeln für die Zahlen, Sinus und Tangenten, neu geordnet von Moritz von Prasse ehemals Prof. der Mathematik in Leipzig, revidirt und vermehrt von Karl Brandan Mollweide ordentl. Prof. der Mathematik in Leipzig. Leipzig,

1821," ix. + 110 pp. 16mo. In the preface Mollweide says that von Prasse was his predecessor in the Chair of Mathematics, as stated by Prof. Virchl. This work is entered under both the title-names in Poggendorff's "Biographisch-literarisches Handwörterbuch," a circumstance that might have given a clue to the authorship of the "Demonstratio." Strangely enough, this is not the only instance in which von Prasse omitted his name in essays written by him for academical celebrations. I can only surmise that this was done with a view to republication in his "Commentationes Mathematicæ," and that the name was written on the copies distributed as invitations to the celebrations. Whatever the reason, it has in this instance obviously added greatly to the trouble ordinarily experienced when dealing with this class of academical essay, the bibliography of which is so complicated, and at the same time often so important.

RALPH COPELAND.

Lord Crawford's Observatory, Dun Echt, January 25.

A New Historic Comet?

AT a recent meeting of the Asiatic Society of Japan, a paper was read by Mr. W. G. Aston, H.B.M. Consular Service. This paper will certainly rank high amongst historic papers relating to Japan and Korea. Briefly described, it is a comparison between the ancient records of these two countries and China, and its aim is to establish the relative credibility of these various records. Mr. Aston has so far confined his attention to the period preceding A.D. 500; and his general conclusion is that, as historic writings, the Korean and Chinese chronicles are far superior to the Japanese of the same date.

In the Tongkan, as the ancient records of the Korean kingdoms are called, there is a notice, of which the following is a translation: "Summer, fourth month, Pèkché; comet visible; day-time." The fourth month began on May 14 or 15. At the request of Mr. Aston, I tried to find out if any such comet had been observed elsewhere. The only list of historic comets obtainable in Japan was the list given in Faye's "Astronomy"; and I am not sure if this is meant to be complete. According to Mr. Aston, the Pèkché comet appeared in May or June, A.D. 302. The nearest date in Faye's list is A.D. 295. If this is the same comet, then one *at least* of the dates must be wrong. It is quite possible, however, that both are correct; in which case we shall be indebted to Mr. Aston for having added one more to our list of historic comets. In coming to a conclusion, we must know to what source we owe the knowledge of the 295 comet, and whether this source has greater claims to chronological accuracy than have the Korean records. Not having the references at hand for studying these points, I have written this note to NATURE, in the hope that someone interested in the matter may be able to come to a decision on this question of a possibly new historic comet.

CARGILL G. KNOTT.

Imperial University, Tokio, Japan, December 19, 1887.

"Is Hail so formed?"

UNDER the above heading in NATURE of January 26 (p. 295) there is a short paper by Cecil Carus-Wilson, in which the writer assumes that under certain conditions, drops of water, whilst falling from the upper branches of a tree, become converted into ice before reaching the ground, whilst other drops falling from the same tree, but at 10 feet less altitude, came to the ground in a fluid state. There is, I think, a simpler solution of this question than the one given. Suppose the following conditions—namely, a frost sufficiently severe as to lower the temperature of the leaves and branches of a tree to a few degrees below the freezing-point; after which a very gradual thaw comes on, accompanied by a fine rain or Scotch mist which freezes on the tree.

Where the leaves and smaller branches hang downwards, small beads of ice would form on their points. As the air became warmer the ice would thaw, and fall to the ground either in the liquid form, or the beads at the ends of the leaves and twigs would become detached in their solid state, and reach the ground as ice-pellets.

Sometimes these ice pellets extend in length, and assume the form of small icicles.

J. RAE.

4 Addison Gardens, January 28.

MODERN VIEWS OF ELECTRICITY.[1]

PART III. (*continued*).

VII.

First Representation of the Field due to a Current.

RETURN now to the consideration of a simple circuit, or, say, a linear conductor, and start a current through it; how are we to picture the rise of the lines of force in the medium? how shall we represent the spread of magnetic induction? First think of the current as exciting the field (instead of the field as exciting the current, which may be the truer plan ultimately).

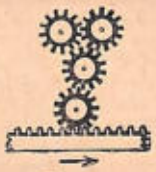


FIG. 34.

If we can think of electricity in the several molecules of the insulating medium connected like so many cog-wheels gearing into one another and also into those of the metal, it is easy to picture a sideways spread of rotation brought about by the current, just as a moving rack will rotate a set of pinions gearing into it and into each other (Fig. 34). But then half the wheels will be rotating one way and half the other way, which is not exactly right.

How is it possible for a set of parallel whirls to be all rotating in the same direction?



FIG. 35.

If there is any sort of connection between them they will stop each other, because they are moving in opposite directions at their nearest points; and yet, if there is no connection, how can the whirl spread through the field?

Well, return to the old models by which we endeavoured to explain electrostatics, and think whether they will help us if we proceed to superpose upon them a magnetic whirl in addition to the properties they already

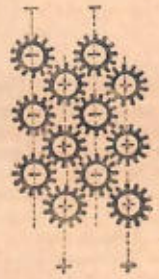


FIG. 36.—Rows of cells alternately positive and negative, geared together; free to turn about fixed axles.

possess. Looking at Figs. 5, 6, and 13, we remember we were led to picture atoms and electricity like beads threaded on a cord. And these cords had to represent, alternately, positive and negative electricity, which always got displaced in different directions.

We are forced to a similar sort of notion in respect of the wheels at present under discussion: in order that

[1] Continued from p. 323.

they may co-operate properly, they must represent positive and negative electricity alternately. If they *then* rotate alternately in opposite directions, all is well, and the electrical circulation or rotation in the field is all in one direction. Each wheel gears into and turns the next, and so the spin gets propagated right away through the medium, at a speed depending on the elasticity and density concerned in such disturbances.

It is not convenient at the present stage to ask the question whether the wheels represent atoms of matter or merely electricity. It may be that each atom is electrostatically charged and itself rotates, in which case it would carry its charge round with it, and thereby constitute the desired molecular current.

The apparent inertia of electricity would thus be explained simply enough, as really the inertia of the spinning atoms themselves; and the absence of any moment of momentum in an electro-magnet as tested mechanically would be equally explained by the simultaneous opposite rotation of adjacent atoms. A question may arise as to why the opposite molecules should have exactly equal opposite inertiæ, as they have, else a fluid magnetized medium would bodily rotate; and there may be other difficulties connected with a bodily rotation of electrostatically charged molecules: it is merely a possibility upon which stress must not be laid till it has been proved able to bear it. For our present purpose a spin of the electricity inside each atom, or even independently



FIG. 37.—Portion of a magnetic field. Another mode of drawing Fig. 36.

of any atoms, is quite sufficient. Besides, since magnetic induction can spread through a vacuum quite easily, the wheel-work has to be largely independent of material atoms.

If any difficulty is felt concerning the void spaces in Fig. 36 it is only necessary to draw it like Fig. 37, which does every bit as well, and reduces the difficulty to any desired minimum.

Representation of an Electric Current.

Now notice that in a medium so constituted and magnetized—that is, with all the wheel-work revolving properly—there is nothing of the nature of an electric current proceeding in any direction whatever. For, at every point of contact of two wheels the positive and negative electricities are going at the same rate in the same direction; and this is no current at all. Only when positive is going one way and negative going the opposite way, or standing still, or at least going at a different rate, can there be any advance of electricity or anything of the nature of a current.

A current is nevertheless easily able to be represented: for it only needs the wheels to gear imperfectly and to work with slip. At any such slipping-place the positive is going faster than the negative, or *vice versâ*, and so there is a current there. A line of slip among the wheels corresponds therefore to a linear current; and, if one thinks of it, it is quite plain that such a line of slip must always have a closed contour. For, if only one wheel slip, then the circuit is limited to its circumference; if a row slip, then the direct and return circuit are on opposite sides of the row. But a large area of any shape with no slip inside it may be inclosed by a line of slip, and this gives us a circuit of any shape, but always closed. Understand: one is not here thinking of a current as analogous to a *locomotion* of the wheels—their axes may be quite stationary,—the slip contemplated is that of one *rim* on another.

Imagine all the wheels inside the empty contour of Fig. 38 to be rotating, the positive clockwise, the negative counter clockwise, and let all those outside the contour be either stationary or rotating at a different rate or in

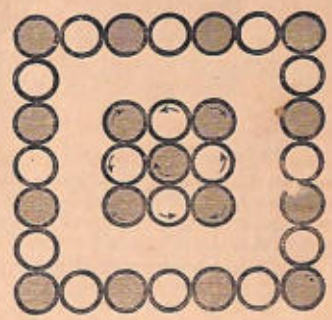


FIG. 38.—Diagram of a peripheral current partitioned off from surrounding medium by a perfect conductor, which transmits no motion, and therefore acts as a perfect magnetic screen.

an opposite direction; then the boundary of the inside region is a line of slip along which the positive rims are all travelling clockwise, and the negative rims the other way, and hence it represents a clockwise positive current.

But it may be said that the spin inside the contour, if maintained, must sooner or later rotate the wheels outside as fast as themselves, and then all slip will cease. Yes, that is so, unless there is a complete breach of connection at the contour, as in Fig. 38 there is. If the outer region has any sort of connection with the inner one the slip at its boundary can only be temporary, lasting during the era of acceleration.

Distinction between a Dielectric and a Metal, as affected by a spreading Magnetic Field.

In a dielectric the connection between the atoms is definite and perfect. If one rotates, the next must rotate too; there is no slip between the geared surfaces; it is a case of cogged wheels. A conduction-current is impossible.

But in a metallic conductor the gearing is imperfect; it is a case of friction-gearing with more or less lubrication and slip, so that turning one wheel only starts the next gradually—it may be very quickly, but not instantaneously—and there is a motion of a positive rim incompletely compensated by an equal similar motion of a negative rim while getting up speed; in other words, there is a momentary electric current, lasting till the wheels have fairly started.

In a perfect conductor the gearing is absent; the lubrication is so perfect that all the atoms are quite free of one another, and accordingly a spin ceases to be transmitted into such a medium at all. The only possible current in a perfect conductor is a skin-deep phenomenon.

A magnetized medium of whatever sort is thus to be regarded as full of spinning wheels, the positive rotating one way and the negative the other way. If the medium is not magnetized, but only magnetic—*i.e.* capable of being magnetized—it may be thought of either as having its wheels stationary, or as having them facing all ways at random; the latter being probably the truer, the former the easier, representation, at least to begin with.

Whether the medium be conducting or insulating makes no difference to the general fact of spinning wheels inside it wherever lines of force penetrate it; but the wheels of a conductor are imperfectly cogged together, and accordingly in the variable stages of a magnetic field, while its spin is either increasing or decreasing, there is a very important distinction to be drawn between insulating and conducting matter. During the accelerating era conducting matter is full of slip, and a certain time elapses before a steady state is reached. A certain time may be necessary for the propagation of spin in a dielectric, but it is excessively short, and the process is unaccompanied by slip, only by slight distortion and recovery. As for a strongly magnetic substance like iron, nickel, or cobalt, one must regard them as constituted in the same sort of way, but with wheels greatly more massive, or very much more numerous, or both.

Phenomena connected with a varying Current. Nature of Self-induction.

Proceed now to think what happens in the region round a conductor in which a current is rising. Without attempting a complete and satisfactory representation of what is going on, we can think of some mechanical arrangements which have some analogy with electrical processes, but do not pretend to imitate them exactly.

Take first a system of wheel-work connected together and moved at some point by a rack. Attend to alternate

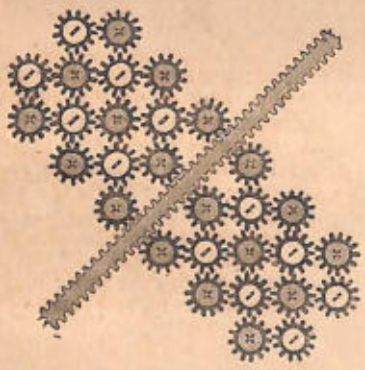


Fig. 39.—A provisional representation of a current surrounded by dielectric medium, either propelling or being propelled.

wheels more especially, as representing positive electricity. The intermediate negative wheels are necessary for the transmission of the motion, and they also serve to neutralize all systematic advance of positive electricity in any one direction, except where slip occurs, but they need not otherwise be specially attended to.

Remember that every wheel is endowed with inertia, like a fly-wheel.

Directly the rack begins to move, the wheels begin to rotate, and in a short time they will all be going full speed. Until they are so moving, the motion of the rack is opposed, not by friction or ordinary resistance, but by the inertia of the wheel-work.

This inertia represents what is called self-induction, and the result of it is what has been called the "extra current at make," or, more satisfactorily, the opposing E.M.F. of electro-magnetic inertia or self-induction.

So long as the rack moves steadily forward, the wheel-work has no further effect upon it; but directly it tries to stop, it finds itself unable to stop dead without great violence: its motion is prolonged for a short time by the inertia of the wheel-work, and we have what is known as the "extra current at break."

If the rack is for a moment taken to represent the advancing electricity in a copper wire, then the diagram may be regarded as a section of the complete field: the complete field being obtained from it by rotating it round the axis of the wire. Imagining this done, we see that the axis of each wheel becomes prolonged into a circular core, and each wheel into a circular vortex ring surrounding the rack and rolling down it as it moves forward, as when a stick is pushed through a tight-fitting umbrella-ring held stationary (see Fig. 30 B).

As one goes further and further from the rack the lengths of the vortex cores increase, but there is only a given amount of rotation to be shared among more and more stuff, hence it is not difficult to imagine the rate of spin diminishing as the distance increases, so that at a reasonable distance from the conductor the medium is scarcely disturbed.

To perceive how much rotation of the medium is associated with a given circuit, one must consider the shape of its contour—the position of the return current. Take first a long narrow loop and send a current up one side and down the other. The rotations belonging to each are superposed, and though they agree in direction for the space inclosed by the loop, they oppose each other outside, and so there is barely any disturbance of the medium outside such a looped conductor; very little dielectric is disturbed at all, and accordingly the inertia or self-induction is very small.

If the loop opens out so as to inclose an area, as the centrifugal force of the wheels will tend to make it do,

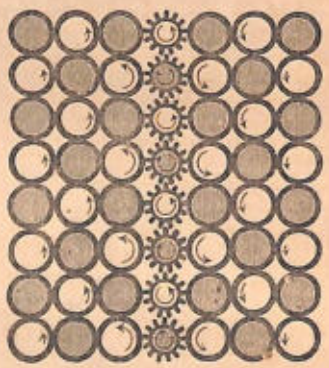


Fig. 40.—Diagram of a direct and return current close together, showing distribution of rotation and of slip in the thickness of the conductor, and in the dielectric between. The dielectric outside is very little disturbed.

then there is a greater amount of rotation, a greater moment of momentum inside it, and accordingly its self-induction is increased. The axis of every wheel is, however, continuous, and must return outside the loop: so the outside region is somewhat affected by rotation, but of a kind opposite to that inside.

Figs. 38 and 41 show the state of things for a closed circuit conveying a current. The free space in Fig. 38 represents a perfect conductor, or perfect breach of connection. Along one side of this space positive electricity is seen streaming in the direction of the arrows, and it may be streaming on the other side also, but nothing happens in its interior—which is therefore not represented.

The corresponding portion in Fig. 41 is intended for an ordinary conductor, full of wheels capable of slip. And slip in this case is a continuous necessity, for the rotation on either side of the conductor is in opposite directions, so the atoms of the conductor have to accommodate themselves as best they can to the conditions; some of them rotating one way, some the other, and some along a certain neutral line of the conductor being stationary. If a conductor is straight and infinitely long, the neutral line of no rotation is in the middle. If it be a loop, the neutral line is nearer the outside than the inside, because the rotation of the medium inside is the strongest. If the

loop be shut up to nothing, the neutral line is its outer boundary or nearly so (Fig. 40). If, again, the circuit is wound round and round a ring, as string might be lapped upon a common curtain-ring to cover it, then the axes of whirl are wholly inclosed by the wire, and there is no rotation outside at all.

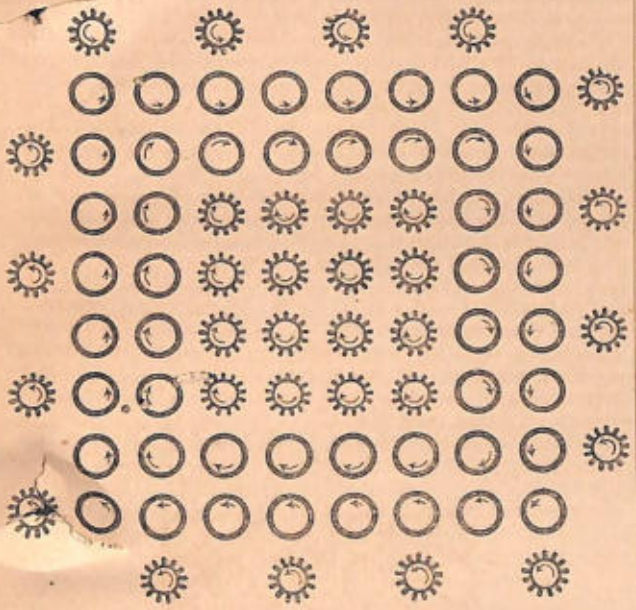


FIG. 41.—Diagram of simple conducting circuit like a galvanometer ring, with the alternate connecting-wheels omitted. The same number of dielectric wheels are drawn outside as inside, to indicate the fact that the total spin is equal inside and out, though the outside is so spread out as to be much less intense.

Fig. 42 shows a section of this last-mentioned condition, and here the wheels of the dielectric outside are not rotating at all. The inside is revolving, it may be furiously, and so between the inner and outer layers of the conductor we have a great amount of slip and dissipation of energy.

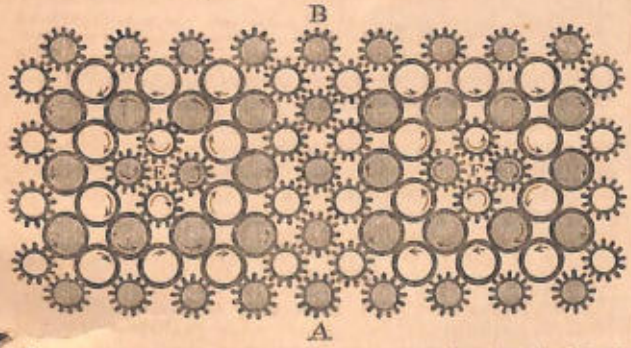


FIG. 42.—Section of a closed magnetic circuit, or electric vortex-ring, or hollow bent solenoid like Fig. 39, inclosing an anchor-ring air space; the axis of the ring being A B, the sections of the core being E and F. The arrows indicate the intensity of the spin, *i.e.* of the magnetic field, which is a maximum at the middle of each section and nothing at all outside. If the core contains iron instead of air, its wheels have to be from 100 to 500 times as massive: slipping wheels if solid iron, cogged wheels if a bundle of fine varnished iron wires.

The process of slip which we have depicted goes on in all conductors conveying a current, whether steady or variable, and in fact *is* the current. The slip is necessarily accompanied by dissipation of energy and production of heat: only in a perfect conductor can it occur without friction. In a steady current the slip is uniformly distributed throughout the section of the conductor; in the variable stages it is unequally distributed, being then more concentrated near the periphery of the wire.

When a current is started in a wire, the outer layers start first, and it gradually though very quickly penetrates to the axis. Hence the lag or self-induction of a wire upon itself is greater as the wire is thicker, and also as it is made of better conducting substance. If it is of iron, the mass or number of the wheels is so great that the lag is much increased, and the spin of its outer layers is great enough to produce the experimental effects discovered by Prof. Hughes.

One must never confuse the slip with the spin. Slip is current, spin is magnetism. There is no spin at the axis of a straight infinite wire conveying a current, and it increases in opposite directions as you recede from the axis either way; arranging itself in circular vortex cores round the axis. But the slip is uniformly distributed all through the wire as soon as the current has reached the steady state. The slip is wholly in the direction of the wire. The axes of spin are all at right angles to that direction.

Rise of Induced Current in a Secondary Circuit.

To study the way in which a magnetic field excited in any manner spreads itself into and through a conducting medium, look at Fig. 43, and suppose the region inside the contour A B C D to be an ordinary conducting region—that is, full of wheels imperfectly geared together, and capable of slip.

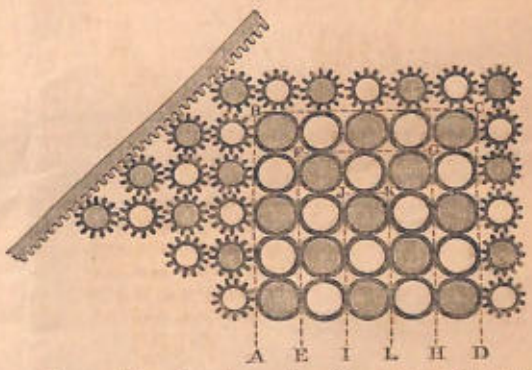


FIG. 43.—Diagram illustrating the way in which an induced current arises in a mass of metal immersed in an increasing magnetic field; also how it decays. The dotted lines A B C D, E F G H, I J K L, are successive lines of slip.

Directly the rack begins to move, all the wheels outside A B C D begin to rotate, and quickly get up full speed. The outer layer of wheels inside the contour likewise begins to rotate, but not at once; there is a slight delay in getting them into full motion. For the next inner layer the delay is rather greater, and so on. But ultimately the motion penetrates everywhere equally, and everything is in a steady state.

But while the process of starting the wheels was going on, a slip took place round the contour A B C D, and round every concentric contour inside it; the periphery of the positive wheels moving in a direction opposite to that of the wheel in contact with the rack, and so suggesting the opposite induced current excited at "make" in the substance of a conductor near a growing current, or generally in an increasing magnetic field.

The penetration of the motion deeper and deeper, and the gradual dying away of all slip, illustrate also the mode in which this induced current arises and gradually dies away, becoming *nil* as soon as the magnetic field (*i.e.* the rotation) has penetrated to the interior of all conductors and become permanently established there as elsewhere.

Suppose the motion of the rack now stopped: all the cogged wheels stop too, though it may be with a jerk and

some violence and oscillation due to their momentum; but those inside the contour A B C D will continue moving for a little longer. The outside layer of this region will slip in such direction as to illustrate the direct induced current at "break," and will begin to stop first; the slip and the stop gradually penetrating inwards, just as happened during the inverse process, until all trace of rotation ceases. This inverse slipping process is the direct induced current at "break."

Through a perfect conductor the disturbance could never pass, for the slip of the dielectric wheels on its outer skin would be perfect, and would never penetrate any deeper. A superficial current lasting for ever, or rather as long as the magnetic field (the rotation of the dielectric wheels) lasts, is all that would be excited, and it would be a perfect magnetic screen to any dielectric beyond and inclosed by it. OLIVER J. LODGE.

(*To be continued.*)

THE BIRDS-NEST OR ELEPHANT ISLANDS OF THE MERGUI ARCHIPELAGO.

OF the geological structure of this group of islands lying off the coast of British Burmah not much is yet known. Our readers will probably be interested in the following account of a visit to one portion of the archipelago, furnished by Commander Carpenter, R.N., to the Hydrographer of the Admiralty, to whose kindness we are indebted for permission to publish it.

The remarkable group of islands called by the Burmans Ye-ei-gnet-thaik (*lit.* sea-birds' nests) is located on the south-east side of Domel Island, one of the largest of the Mergui Archipelago. It is composed of six marble rocks, the highest and largest of which, 1000 feet in altitude, and about one mile in length, is oval-shaped, and rises very abruptly out of a depth of only 5 fathoms. The islands present a very striking appearance, particularly if the weather is hazy, when they are not seen until within five or six miles, for then they gradually loom out through the mist like some huge misshapen monsters that have strayed away from civilization. Their sides are partly clothed with vegetation wherever a break in the limestone has left a cleft in which moisture and dust can lodge. Conspicuous because of its leaning attitudes is a species of tree-fern which grows at any angle, but only above a height of 200 feet from the water. The face of the rocks is reddish, partly from weathering and partly from soil, and where cliffs exist the most beautiful though uncouth stalactites have been formed, showing grotesque and snake-like patterns varying in hue and shape till one feels as if in some enchanted land. But the great feature of the group is the birds'-nest caverns, which as a rule open into the sea, the entrance being below high-water mark; fortunately I visited them at spring tides, and had plenty of leisure to examine each cavern at low water during two days.

At the south end of the largest island stands a "nine-pin" of gray marble 370 feet high, almost separated from the rest. It is hollow, like a huge extinguisher, and the polished light-blue and yellow sides of the interior seem to point to its having been hollowed by the swell of the sea, which on entering the cave would probably expend its force vertically, the mouth of the cave being open to the direction of the strongest seas. This sea-stack forms the western point of a nearly circular cove, 360 yards in diameter, which runs back into the island, and the sides of the cove rise steeply though not perpendicularly from it. At the head of the cove is a perpendicular wall of rock over which can just be seen the 1000-foot summit in the distance.

At half-tide a tunnel, passable for a canoe, opens under the wall of rock at the head of the cove, but a ship's gig can only enter within an hour of low-water spring tides. This tunnel has a roof covered with large stalactitic knobs except at its narrowest part, where it is apparently scoured smooth by the action of the tidal rush. It is about 250 feet long, and 4 feet deep at low water (the rise and fall of the tide being 16 feet), and is covered with dripping marine life, corallines, small corals, Comatulæ, sponges, and sea-horses. Passing through this submarine passage one emerges into another circular crater-shaped basin with perpendicular sides. This basin is only open to the sky; caves here and there enter it, some of which may perhaps lead by long tunnels to other basins. Water was running freely into it from the foot of the cliffs in several places as the tide fell, showing that water spaces existed, and strange gurgling sounds as of air taking the place of water could be heard now and again. There were hardly any signs of the place being frequented by man except here and there the worn ropes of birds'-nest climbers. It was either not the season for the swallows, or they had deserted the islands, for none were seen. A little reddish guano was noticed in some of the caves. There can be but little traffic through the tunnel by which we entered, for the delicate growth on its sides was hardly injured.

On the west side of the northern large island a lofty cavern is connected at half-tide with another nearly circular basin of about the same size as that we have just described, but in this case the basin also opens into the sea on the east side of the island. After contemplating the cliffs that surround these basins, the general circular contour of the ridges of the islands, the undermining action of the sea at the water-line, which causes in some places an overhang of 20 to 25 feet, and the softening of the marble surface of the cavern roofs by moisture, the conviction gradually forces itself on the mind that these circular basins were themselves at one time the floors of huge caverns; that in days gone by the islands rose far higher, with cavern piled on cavern, and that the work of disintegration by solution and wave action is slowly going on, pulling down these marble monuments of a giant age. Indeed, here and there a fall of blocks has occurred lately, and, as there is no shoal off the base of the slip, the destructive action is probably rapid.

A small oyster covers the rocks at the water-line. A handsome kingfisher was secured and sent to the British Museum. A few doves and an eagle or two were the only other birds seen, besides a small bat in the caves. By the position of the nest-seekers' ropes, the swallows appear to build only on the roofs of the caves. The islands appeared to be entirely composed of a blue-tinted marble. A vessel could lie alongside them and lower the cut blocks straight into her hold, but it is probably of too poor a quality to be worth shipment.

ALFRED CARPENTER.

PRIZE FOR RESEARCHES IN NATURAL HISTORY.

IN accordance with the intentions of the founder, the Committee of Schnyder of Wartensee's Foundation, Zürich, have decided to offer for the year 1890 a prize for the following researches in natural history:—

"New investigations are desired regarding the relation which the formation of the bones bears to the statics and mechanics of the vertebrate skeleton. The results of the investigations as a whole are to be demonstrated in detail by way of example on the skeleton of a definite species."

The conditions are as follow:—

Art. 1. Competitors for the prize must send in their work in German, French, or English, by September 30, 1890, at the latest, to the address given below in Art. 6.

Art. 2. The award will be made by a Committee consisting of the following gentlemen:—Prof. Hermann von

Meyer, Zürich; Prof. L. Rütimeyer, Basle; Prof. H. Strasser, Berne; Prof. Otto Mohr, Dresden; and Prof. Albert Heim, of Zürich, representing the Committee offering the prize.

Art. 3. The judges are authorized to award a first prize of two thousand francs; and a further sum of one thousand francs is placed at their disposal for distribution in minor prizes according to their discretion.

Art. 4. The work awarded the first prize becomes the property of the Foundation of Schnyder of Wartensee, which will arrange with the author regarding the publication of the same.

Art. 5. Each competing work must bear on the title-page a distinguishing motto, and must be accompanied by a sealed envelope containing the name of the author, and bearing on the outside the same motto.

Art. 6. Competing works are to be sent in by the date named in Art. 1, to the following address: "An das Präsidium des Conventes der Stadtbibliothek in Zürich (betreffend Preisaufgabe der Stiftung von Schnyder von Wartensee für 1890)."

NOTES.

THE death of Sir Henry Maine, F.R.S., has created a great blank in the serious literature of England. He was the first Englishman who applied to the study of law and early institutions the rigid methods of science, and the results at which he arrived marked an epoch in the investigation of these subjects. His literary style, combining as it did extraordinary vigour, lucidity, and grace, was scarcely less remarkable than his grasp of far-reaching principles. He died suddenly, of apoplexy, at Cannes, on Friday evening last. He was in his sixty-sixth year.

IN a letter received from Mr. John Whitehead, dated Labuan December 13, 1887, that gentleman writes:—"To-day or to morrow I start for Kina Balu, and I hope to make this a famous and last expedition into Borneo, for I really am in wonderful health considering everything, but at the same time I am rather tired of Borneo, with its fevers, heat, and mosquitoes. I hope to be back in England in August and September. I do not like to brag of what I hope to do, as things are so uncertain. Natives may refuse to help me, and may perhaps attack me, for the country round this fine mouutain is by no means settled." On the last occasion of his visiting Kina Balu a year ago, Mr. Whitehead was only able to remain a month upon the mountain, but he discovered nineteen new species of birds in that short time, some of them being really wonderful novelties. He now hopes to remain for at least six months, and this he will doubtless be able to do, if he can secure supplies for his hunters, and keep open his communication with Labuan.

MR. WHITEHEAD'S collections from the island of Palawan have now arrived in this country, and a brief account of them will appear in the April number of the *Ibis*. This island has already been visited by Prof. Steere, Mr. Alfred Everett, and Mr. E. Lemprière, all of whom made collections in the neighbourhood of Puerto Princesa. Mr. Whitehead's labours were also confined to the vicinity of this post, as he was prevented from visiting the interior. He has succeeded, however, in procuring specimens of every species met with by the three travellers above-mentioned, and has besides obtained about sixty additional species, several being new to science.

THE Council of the Royal Meteorological Society have arranged to hold, at 25 Great George Street, Westminster, on March 20–23 next, an Exhibition of Apparatus connected with Atmospheric Electricity, including lightning-conductors, photographs of lightning, and damaged objects. The Committee will also be glad to show any new meteorological instruments or apparatus invented or first constructed since last March; as well as photographs and drawings possessing meteorological interest.

A FRIENDLY meeting of employers and working men, to discuss the best means of obtaining technical education, will be held at the Royal Victoria Hall, Waterloo Bridge Road, on Wednesday, February 15, at eight o'clock. The chair will be taken by Sir Douglas Galton. This meeting has been arranged in consequence of the great interest shown in a similar meeting held at the same place on December 14. The speakers will be limited to ten minutes, and those who wish to speak must send in their names the day before the meeting.

THE new American Folk-Lore Society was definitely organized at a meeting held at Harvard College on January 4. The object of the Society is the study of folk-lore in general, and especially of folk-lore in North America. The first President is Prof. F. J. Child, of Harvard, and the acting Secretary is Mr. W. W. Newell, of Cambridge, Mass. It is expected that the first number of the Society's journal will be published in April.

THE Duchess of Albany has become Patroness of the Parkes Museum, of which the Duke of Albany was President until his death.

DURING the coming spring the construction of the North Sea and Baltic Canal will be begun along the whole line. There will be seven camps of workmen, and 4000 men employed.

THE Education Department of Scotland has issued a circular to the various School Boards in that country, in which are embodied the results of the careful inquiries that have recently been made into the existing sy-tem of elementary scientific teaching in Scottish schools. Technical instruction is discouraged in primary schools till the boys have reached the higher standards, and even then, the Department thinks, no attempt should be made unless skilled teachers and abundance of scientific apparatus are available. In most instances the thorough teaching of elementary science is beyond the reach of the primary schools; but by various School Boards uniting to employ a trained staff of teachers much of the difficulty will be overcome. School Boards are also recommended to seek the aid of local committees consisting of manufacturers who know what technical education is most needed in the district. The Department also recommends the extension of the system of giving evening lectures, which have been so successful in the past, and the charging of fees low enough to be within the reach of all. Nothing would tend to make technical education more popular than a small rate of charge, combined, as it should always be, with trained help and an abundant supply of scientific instruments.

A STRIKING new experiment, exhibiting the terribly explosive nature of chloride of nitrogen, is described by Prof. Victor Meyer in the current number of the *Berichte*. A few drops of the yellow chloride were prepared in the usual manner by inverting an exceptionally thin flask filled with chlorine gas in a leaden dish containing a solution of ammonium chloride. Instead, however of gently agitating the apparatus so as to cause the drops to fall into a smaller leaden capsule placed beneath the mouth of the flask, they were allowed to float freely upon the surface. The whole apparatus was then inclosed in a cover-box fitted with stout plate-glass sides, through the top of which was passed a bent pipette, turning up below just under the mouth of the flask and connected outside with a dropping funnel containing chloride of ammonium solution and a few drops of turpentine. When sufficient chloride of nitrogen had collected, the tap of the funnel was carefully turned so as to allow a little turpentine to slowly rise in the flask. After a moment or two it reached the surface and mingled with the chloride of nitrogen, causing a brilliant

flash of light and a loud explosion, which Prof. Meyer likens to a thunder-clap, so much more powerful is the detonation in a confined space. The flask of course was shattered, not into powder, but into tolerably large fragments; the plate-glass box, however, even after many repetitions of the experiment, remained intact, a small door on the side away from the observers having been left ajar so as to prevent any notable increase of pressure. Curiously, the chloride of nitrogen never entirely exploded; a part remained in the distorted leaden dish and maintained an incessant fusillade for more than a minute.

At the last meeting of the Göttingen Chemical Society, Dr. Gattermann read a preliminary note upon his recent researches as to the nature of chloride of nitrogen. From his analyses it appears pretty clear that the yellow liquid is a mixture of at least two distinct chlorides, which he has hopes of being able to separate. During the course of the experiments the reason of its capricious behaviour, the cause of so many painful accidents in the past, was happily discovered. It is decomposed by the actinic rays of light, being rapidly acted upon by sunlight with periodic spontaneous explosion, and is at once fired by exposure to the rays of burning magnesium. Hence further light upon this difficult and dangerous subject can only emanate from the dark room, a paradox the truth of which Dr. Gattermann is endeavouring to demonstrate.

On the morning of Tuesday, January 31, a distinct shock of earthquake is said to have been felt near Birmingham. In and around Coventry, too, several persons say that they experienced sensible vibrations of their houses and heard rumbling noises. At Hartshill the ceiling of a house was cracked by the shock. On Thursday, February 2, a sharp shock of earthquake was felt over a large part of Scotland. The following details regarding this shock are taken from the *Times* of Friday, February 3:—"The shock was distinctly felt at a quarter past 5 o'clock in Perth. The tremor lasted about one minute, and consisted of five or six slight, wave-like motions from west to east. In the Breadalbane and Grantully districts of Perthshire the shock lasted six seconds. It was also felt very distinctly in Aberfeldy, Acharn, Kenmare, and Strathay. It is twenty years since these districts were similarly affected. In Strathearn two shocks were felt, the first about half past 3 o'clock, and the second about 5 o'clock. Further north, in Inverness-shire and Ross-shire, a shock was felt about 5. It was sharper and seemed to travel from south-west to south-east. The tremor in Dingwall is likened to the vibration caused by a heavy waggon passing along a road, while at Crieff it was like a very heavy body thrown to the ground. In Beauly and Strathglass people were greatly alarmed. Their houses shook, dishes fell, furniture was broken, and numbers of people rushed from their beds and out of houses without dressing. On the west coast the shock was very violent. It was also felt at Mull. From Fort William it is reported that there was a slight shock at 5 a.m., which affected the old Caledonian valley, and extended down to the line of the Moray Firth."

The February Bulletin of Miscellaneous Information, issued from the Royal Gardens, Kew, contains a list of such hardy herbaceous annual and perennial plants as have matured seeds under cultivation in the Kew Gardens during the year 1887. "These seeds," it is explained, "are available for exchange with colonial, Indian, and foreign botanic gardens, as well as with regular correspondents of Kew. But the seeds are for the most part only available in moderate quantity, and are not sold to the general public. In the years 1885 and 1886 the list was printed as an independent publication. It has now been thought more convenient to issue it as a number of the Bulletin. Every effort is made to correctly determine the nomenclature of the plants in the list. As far as it goes, it will serve as a record of the herbaceous species cultivated at Kew. It must, however, be remembered that a considerable proportion of herbaceous plants do not mature seeds in the climate of England, and these are necessarily not included in the list."

Much inconvenience is caused by the fact that lists of recent additions to public libraries are not always readily accessible to persons who would like to make use of them. Readers at the Darwen Free Public Library may congratulate themselves that in their case this difficulty has been overcome. The other day the *Darwen News* printed the first instalment of a list of books which have been lately added to the collection belonging to that institution, and which are not to be found in the catalogue. Two similar instalments will follow, and afterwards lists will be given as books are purchased. If readers will take the trouble to cut out these lists and place them at the end of their copies of the catalogue, they will know exactly what works have been secured for the library. From the instalment just issued, it is obvious that the managers of the Darwen Free Public Library exercise great discretion in their choice of books, and we are glad to see that among the works selected by them science is very fairly represented.

Several correspondents have written to us about Mr. John Morison's letter, printed last week (p. 321), on what he supposed to be a case of untimely insect development. Mr. Edward Buckell, of Romsey, writes:—"Surely Mr. Morison has overlooked the fact that *Vanessa urticæ* hibernates in the imago state, selecting for that purpose houses and such other warm quarters as it can find. I have counted nine in one house. During hibernation the insect is naturally in a semi-torpid condition. As to the 'abnormal appearance' of the antennæ, I think that if Mr. Morison observes other specimens, both during the winter months and after sundown in the summer, he will find the position noted by him to be the usual one."

We referred last week (p. 328) to M. Lancaster's work on the climate of Belgium in 1887; we are also indebted to him for an elaborate discussion of the barometer observations taken at Brussels Observatory during the fifty years 1833–82. That Observatory owes its origin to the efforts of the late L. A. J. Quetelet, President of the International Maritime Conference held at Brussels in 1853, to which Conference our own Meteorological Office owes its origin. The Annals of the Observatory contain one of the most complete series of climatological and phenological observations extant. The barometrical results, which M. Lancaster has carefully resumed, are drawn from the eye observations taken up to June 1841, and since that time from self-recording instruments, one of which is a photographic barograph of the Kew pattern. In addition to the usual monthly and annual means, the tables contain summaries of the days on which the barometer was above or below certain values, the epochs of all remarkable falls and rises, five-day and seasonal means. The mean for the whole period was 29·766 inches (not reduced to sea-level); the greatest height was 30·753 inches on January 17, 1882; and the lowest, 28·367 inches, on December 10, 1872. The mean diurnal range for the year was 0·023 inch. The diurnal and monthly variations are greatest in winter and least in summer; the highest and lowest absolute readings occur generally in the month of January.

Mr. R. H. Scott delivered a lecture on British and Atlantic weather, at the London Institution, on the 2nd instant. After some interesting remarks on the effect of difference of height upon vapour, the dependence of our weather on the upward or downward movement of the atmosphere in cyclonic and anti-cyclonic systems, and on the cause of fogs, he discussed the utility of the present American reports in forestalling storms, based on a consideration of their movements as shown by the Atlantic Weather Charts lately published by the Meteorological

Council. These charts showed that only a small proportion of storms travelled across the Atlantic. The track of the depressions is determined by the distribution of pressure over the ocean, and of this distribution we are ignorant at the time of despatch of telegrams from America. The lecturer stated that in their present incomplete form the telegrams were of no assistance to the Meteorological Office in issuing storm warnings.

Dr. Billwiller reports the establishment of a permanent observatory on the summit of the Santis, in October last. This observatory ranks as the third in height in Europe, being at an elevation of 8200 feet, and 108 feet higher than the temporary station at the Gasthaus, on the Santis, where the observations have been taken for the last five years. The results of these observations are published in a *Neujahrsblatt*, by the scientific Society of Zürich. The lowest temperature during the five years was −9° F. on March 13, 1883, and the highest, 69°, on July 21, 1886. The prevalent winds were westerly and south-westerly, which usually occur on high mountains in these atitudes.

According to the last annual report on the Dutch East Indies, rainfall was measured at 183 stations in these possessions. The military portion of the report, the topographical survey of Java, on a scale of 1 : 200,000, is completed, and the members of the Survey have been sent to the west coast of Borneo, where a preliminary survey to join certain points already astronomically determined has been undertaken. The survey of the west coast of Sumatra will also be continued. A considerable part has already been triangulated, and 344 positions have been determined. The definitive calculation of the triangulation work of Java, on which Prof. Oudemans, of Utrecht, has been at work for five years, is not yet completed.

Recent Java journals give particulars of a remedy for coffee-leaf disease, discovered by Dr. Burck, manager of the Government Botanic Gardens at Buitenzorg, near Batavia. The specific is said not only to cure the disease, but also to prevent its recurrence. For preventive purposes, he makes use of a highly attenuated solution of chloride of iron applied to the under portion of the leaves by means of a pulverisator. The sticky nature of the solution enables it to adhere two months to the coffee-leaves. It is a powerful antidote to the *Hemileia vastatrix*. To stay the progress of the latter when it has once taken hold, a different method is employed. The coffee-leaves in which the *Hemileia* first manifests itself in the form of orange-coloured spots are at once taken in hand. Holes are pricked in the spots with a needle dipped in a strong solution of sulphuric acid, which kills all the germs of the disease in the leaf. Dr. Burck estimates the cost of the preventive specific at 2½ guilders per 133 lbs., and the healing remedy at 4 guilders. He anticipates that the price of coffee will be enhanced in consequence. The second specific in particular is said to have yielded good results and to be easy to administer. The economic value for Java of the discovery of the remedies, should they prove successful, can scarcely be over-estimated. In Ceylon the disease in the coffee-plant produced a revolution in planting; year after year the coffee crops were failures, many planters were ruined, and ultimately tea-growing took the place of coffee with results which are just now astonishing the world. But the period of transition from one staple to the other was one of economic disaster, from which perhaps Dr. Burck has saved Java.

We have received the recent issues of the *Journal of the Asiatic Society of Bengal*. Vol. lv. Part 2, No. 5, is wholly occupied by the second instalment (Rhopalocera) of the Lepidopterous insects collected in Tavoy and Siam during 1884–85, under the superintendence of Mr. Pitman, the Chief Superintendent of Telegraphs. Of this list nearly 100 species are quite new. The list is drawn up by Messrs. Elwes and L. de Nicéville. Vol. lvi. Part 2, has numerous and varied contributions. The first paper is by Mr. Blanford, on the influence of Indian forests on the rainfall. The other papers are: the changes in the density of sea-water, by S. R. Elson; notes on Indian Rhynchota, Heteroptera, Part 1, by E. T. Atkinson; new species of Ficus from New Guinea and Sumatra, by G. King, where eleven new species are spoken about; the mammals and birds collected by Captain Yate with the Afghan Boundary Commission, each species briefly described and commented on by J. Scully; the species of Loranthus indigenous to Perak, by G. King; Étude sur les Arachnides de l'Asie meridionale, by M. Eug. Simon; the differential equation of a trajectory, by H. Mukhopadhyay.

The current number of the *Folk-Lore Journal* (vol. vi. part i.) contains a most interesting collection of Aino tales and legends, made by Prof. Chamberlain, the well-known Japanese scholar. We are glad to see that he was encouraged to publish the collection by certain observations contained in a review, in these columns, of his recent monograph on the Ainos. The collection consists of fifty-four tales (a few being omitted as unfit for general publication), classified under the headings—tales accounting for the origin of phenomena, moral tales, tales of the Panaumbe and Penaumbe cycle, miscellaneous tales, and, finally, scraps of folk-lore. These were all taken down from the mouths of Ainos. The other papers in the number include a continuation of one on Irish folk-lore collected from a "Statistical Account or Parochial Survey" of that country published more than seventy years ago; and a collection of the traditions of a race as curious in its way as the Ainos, the Mentra, or aborigines of Malacca and the adjoining States.

The January number of the *Auk* announces that the affairs of the American Ornithologists' Union have considerably brightened, and that by the public spirit of some of its members the Association begins the new year free from debt. The new part also strikes us as full of vigour, and of more than ordinary interest, and it is evident that the recent exertions of the Americans in getting collections from little-known parts of the Western world, such as Texas and Northern Mexico, are being amply rewarded. The reviews are as good as ever, and some of the original papers are excellent. Mr. Sennett describes a new finch from Arizona, and gives an account of the North American species of *Peucæa*, but not one word is said respecting the "Biologia Centrali-Americana" of Messrs. Salvin and Godman, where all Mr. Sennett's facts were duly set forth a year ago. The new Arizona finch, *Peucæa ruficeps scotti*, of Mr. Sennett, was also described some months ago by Mr. Bowdler Sharpe as *Peucæa homochlamys* in the twelfth volume of the "Catalogue of Birds," but as this work has only just appeared, Mr. Sennett's name secures priority of publication. Mr. Brewster's new species from Mexico seem to rest on somewhat trivial characters, at least in the case of the small owls (*Scops*) and the *Aimophilæ*.

Amongst the subjects of special interest referred to in the report of the Smithsonian Institution for the past year is the exploration for a collection of skeletons and skins of the now almost extinct American bison or buffalo. The exploraton was very successful, a small herd being found in a wild part of Montana, from which the officers of the Institution secured a series of skins, as well as sixteen complete skeletons, and fifty-one dry skulls. The herd appears to have been completely exterminated by the settlers soon afterwards. An expedition was also despatched to the Swan Islands in the Caribbean Sea, which are said to abound in land birds in great variety, and also in large iguanas and other reptiles. The National Museum

collections are extending so rapidly that the provision of additional and adequate space for their exhibition is becoming a matter of pressing importance. In 1884 the number of specimens was estimated at 1,471,000; the number is now increased by more than a million. At the beginning of the present decade there was only one curator with a few assistants; now there are thirty-one regularly organized departments and sections, under the care of twenty-six curators and numerous assistants. The work of the Ethnological Bureau in all its branches—mound explorations, general field studies, and office work—appears to proceed as vigorously as in previous years. The most important forthcoming work of the Bureau appears to be a report by Mr. E. W. Nelson, on the Eskimo of Northern Alaska. During 1886 the vocabularies of twelve Eskimo dialects were arranged in the form of a dictionary, which will form one part of his report; the other will contain chapters on Eskimo life and customs in Alaska, illustrated by photographs taken on the spot.

AN elaborate review of the mineral industries of the United States during the year 1886 has just been issued by the United States Geological Survey. It is the fourth of a series of volumes entitled "Mineral Resources of the United States." The first three volumes contain the statistics from 1882 to December 31, 1885.

THE U.S. Bureau of Education has issued the first of what promises to be a most interesting series of "Circulars of Information." The present Circular is by Dr. H. B. Adams, who has chosen as his subject the College of William and Mary. This College was founded in 1693 by Royal grant, and was long supported by popular legislation in Virginia. The greater part of its property was destroyed during the Civil War, and since that time the institution has been allowed to decline almost to ruin. Dr. Adams' aim, as explained in a prefatory letter, has been to discover the historical beginnings of the higher education in the South; to trace the causes of the early prosperity of William and Mary College; to show its influence upon Virginia statesmen and the Southern States, its relation to the University ideas of Jefferson and Washington, and its significance to the whole country; to point out the causes of the decline of William and Mary College; to explain the rise of the University of Virginia, and the necessity of popular support for the higher education.

POPULAR editions of the late Dr. Parkin's volumes—"Are Epidemics Contagious?" and "The Volcanic Origin of Epidemics" (Sampson Low)—have been published. Dr. Parkin died nearly two years ago at the age of eighty-five, and it is explained in an editorial note that "his long and strenuous life had been devoted to the study of cholera and similar epidemics." His attention was first specially directed to the subject of cholera more than fifty years ago, when he was visiting India and China. A prolonged series of observations and experiments satisfied him that "the cause of the disease was atmospheric, and that carbonic acid gas was its antidote." The editor of these volumes admits that Dr. Parkin's theories have met with comparatively limited acceptance in England. This fact he attributes in part to the comparative mildness of cholera outbreaks in England, in part to "an erroneous notion that the results of Dr. Parkin's teaching were hostile to sanitation."

MESSRS. LONGMANS have just issued the ninth edition of Mr. William Jago's "Inorganic Chemistry, Theoretical and Practical." In this edition the paragraphs are numbered, and have side-headings. The more important statements and definitions are printed in bolder type. A number of students and teachers have pointed out to Mr. Jago that they have been using his book in preparing for the Matriculation Examination of the London University, and have been inconvenienced by its not covering the whole syllabus of that examination. Chapters have now been added to supply this want.

AT the last meeting of the Society of Science of Christiania Prof. Schöyen exhibited and described four species of Lepidoptera new to the Norwegian fauna; viz. *Agrotis præcox*, L., found as larva in the Hval Islands, in the Christiania Fjord; *Asopia glaucinalis*, L., produced from larva at Christiania; *Tortrix inopiana*, Haw., from southern Aurdal; and *Cerestoma nemorella* from Christiansand. At the same meeting Prof. Blytt read a paper on the alterations of the so-called "Strandlinjer," or shore-lines, in Norway, maintaining that the changes in the division of land and sea might have been caused by an alteration in the length of day and night.

A RESIDENT in the isolated little island of Bornholm, in the Baltic, writes to a Danish journal that a curious Christmas custom is observed in that island. When the so-called "Christmas table" has been spread on Christmas Eve, a large long loaf of rye bread is laid at the upper end of it. In this loaf, before it is baked, two transverse grooves are made about 3 inches from each end. On the top of the loaf a large cheese and various articles of food are laid. This is the so-called "*Julegalt*." It remains untouched throughout Christmas, and when the table is not in use, the cloth is gathered from the other end and laid over the "*galt*." This curious custom is believed to have been handed down from Pagan times, the *galt* (pig) having reference to Frey's *galt* or pig "Gyldenbörste" ("Gold bristle"). Frey was the god of rain, sunshine, harvest, and general felicity.

THE acclimatization of the so-called "American" trout in Norwegian waters has been very successful. Attempts are now about to be made to acclimatize black bass obtained from America.

THE Danish Government has decided upon forming an oyster bank in the Limfjord, in Jutland, and has despatched the inspector of the Danish fisheries to Norway to obtain all possible information respecting the artificial banks formed in that country during the last few years.

THE manner in which the spruce and pine forests of Norway are being exterminated, is becoming so serious that the Government is called upon to put a stop, by legislation, to the deforestation of the country. At present there is no law to prevent the purchaser of a forest from felling everything, even down to the tiniest saplings. It is urged by forest officials that trees under a certain diameter should not be permitted to be cut, and that the branches of the trees should not be left in the forest (as is now nearly always done), because they stifle the growth of the young trees. Apart from the wanton exhaustion of this commercial wealth, it is maintained that wholesale felling has the effect of changing the climate in the forest localities.

THE strict preservation of the eider fowl on the south-east coast of Sweden during recent years has had the effect of greatly augmenting the number of these valuable birds. The penalty for killing one is very heavy, and informers receive a considerable reward.

DURING the present winter term there are 26,945 German students at the German Universities. Of this number 5791 study theology, 5769 law, 6650 medicine, and 8735 belong to the Philosophical Faculty. 1644 students are foreign. The Vienna University has 238 theologians, 2569 law students, 1565 medical students, and 634 of the Philosophical Faculty. In Graz there are 1305 students, and in Innsbruck 863. Prague has 3805 Cracow 1234, Lemberg 1112, and Czernowitz 259. At Berne University there are 637 students, 51 theologians, 158 law students, 287 medical students, and 141 physical science students. At Zürich there are 70 female students, 40 being medical.

THE *Bulletin Pharmaceutique* states that a new remedy for Phylloxera has been discovered by M. Laffon, of Capendu, and it has proved successful. It consists of a weak solution of nitrate of mercury.

THE additions to the Zoological Society's Gardens during the past week include a Red-winged Parrakeet (*Aprosmictus erythropterus*); eight Peaceful Doves (*Geopelia tranquilla*) from Australia, presented by the Hon. Stormont Finch-Hatton; a Fulmar Petrel (*Fulmarus glacialis*) from Norfolk, presented by Mr. H. M. Upcher, F.Z.S.; a Jardine's Parrot (*Peocephalus gulielmi*) from West Africa, received in exchange.

OUR ASTRONOMICAL COLUMN.

THE ROYAL ASTRONOMICAL SOCIETY'S MEMOIRS.—The first part of vol. xlix. of the Memoirs of the Royal Astronomical Society has just been published, and contains a new General Catalogue of nebulæ, by Dr. J. L. E. Dreyer. Sir John Herschel's General Catalogue, published in the Philosophical Transactions for 1864, was almost entirely founded upon his own and his father's observations, and hence, since several observers have devoted themselves to the work of searching for nebulæ since that catalogue was prepared, the number known to us has been very largely increased. D'Arrest's great work on nebulæ, which appeared three years later than the General Catalogue, gave the means of correcting many of its positions, and hence Dr. Dreyer had been induced as early as 1876 to compile a supplement to the General Catalogue, which he published in the Transactions of the Royal Irish Academy in 1878 (vol. xxvi.), containing a list of corrections to it, and a catalogue of recently-discovered nebulæ. In 1886, Dr. Dreyer presented a second similar supplement—in which the later discoveries of Messrs. Stephan, Swift, Ormond Stone, and other observers had been incorporated—to the Council of the Royal Astronomical Society; but the Council, considering that the General Catalogue was practically out of print, and that the use of three catalogues and two lists of corrections would be very inconvenient, proposed to Dr. Dreyer that he should prepare from the whole of his materials a single new General Catalogue. This work he has now carried out, and the present catalogue contains 7840 objects, the positions of which have been as thoroughly corrected and revised as the materials available permitted. The epoch of the first General Catalogue, and of D'Arrest's final positions—1860—has been retained, as it is close to the epochs of the great star-charts of Argelander, Schönfeld, Chacornac, and Peters, and nearly all the modern micrometric observations of nebulæ are referred to an epoch but little later. The precessions have been given for 1880, as done by Sir John Herschel, and the descriptions have been carefully revised. The work also contains an index to published figures of nebulæ and clusters, and an appendix giving the places of several new nebulæ discovered by Prof. Safford and Mr. Swift, but published too late to be incorporated in the catalogue itself. These are added that the volume may contain a complete record of all nebulæ of which the places have been published up to December 1887.

PUBLICATIONS OF DUNSINK OBSERVATORY.—The sixth part of the observations of the Observatory of Trinity College, Dublin, at Dunsink, has just been published, and contains the separate results reduced to 1885·0, and the mean places for 1012 southern stars observed with the transit circle by Dr. Dreyer, the late, and Mr. Rambaut, the present, Assistant Astronomers. These stars are nearly all in the Southern Durchmusterung Belt, between S. Decl. 2° and 23°, and were suggested for observation by Prof. Schönfeld on account either of their proper motion or of discordances between their places as given in different catalogues. A few other stars were observed either at the request of Prof. Peters or Dr. Auwers. The work had been commenced by Dr. Dreyer in September 1881, who continued it until his appointment to the Armagh Observatory in May 1882, and Mr. Rambaut took it up, on succeeding to Dr. Dreyer's position, in November of the same year. Mr. Rambaut gives the probable error of a single observation—most of the stars were observed only once—as ± 0·065s. in R.A., ± 0″·864 in Decl.; the faintness of the objects and their low altitude at meridian passage making observation somewhat difficult. A plate at the end of the volume shows a portion of one of the chronograph sheets, and illustrates a convenient method of making notes whilst at the telescope by sending special signals to the chronograph.

ROUSDON OBSERVATORY.—Astronomical observations have been steadily carried on during the past year at Mr. Peek's private observatory, Rousdon, Lyme Regis. The principal work undertaken, besides transit observations for time, has been the observation of twenty long-period variable stars. It is proposed, so soon as any star has been observed over several complete periods, to publish a memoir with plates showing the variations in the light curve. The record of the Observatory shows that there were 165 good observing nights in 1887, as against 146 in 1886.

β DELPHINI.—Mr. J. E. Gore published two years ago elements for this difficult and interesting binary (NATURE, vol. xxxiii. p. 518), in which he gave the period as 30·91 years, a value fairly corresponding to that found by Dubiago a couple of years earlier, viz. 26·07. Sig. Celoria having been placed in possession of Prof. Schiaparelli's observations made in 1875 and 1886-87, and those of Engelmann made in 1885 and 1886, has re-investigated the orbit, and deduced elements differing widely from these two earlier sets, particularly in the period, which he finds to be a little short of seventeen years (*Astr. Nachr.* No. 2824). If this last orbit be correct, the star has already been watched through nearly a complete revolution. There is, however, a considerable divergency between the recent observations of Schiaparelli and Engelmann, and those of the latter would accord better with a longer period. It is, therefore, much to be desired that astronomers who possess sufficient optical power should give early and careful attention to this star. The following are Sig. Celoria's complete elements:—

T = 1868·850	...	ε = 0·09622
Ω = 10°·938	...	a = 0″·46000
λ = 220·952	...	P = 16·955 years
γ = 61·582		

OLBERS' COMET.—The following ephemeris for Berlin midnight is in continuation of that given in NATURE, vol. xxxvii. p. 234:—

1888.	R.A. h. m. s.	Decl. ° ′	Log r.	Log Δ.	Brightness.
Feb. 11...	17 46 35 ...	6 5·7 S. ...	0·3320 ...	0·3974 ...	0·29
13...	48 57 ...	6 17·2			
15...	51 15 ...	6 28·4 ...	0·3394 ...	0·3970 ...	0·28
17...	53 28 ...	6 39·3			
19...	55 36 ...	6 50·0 ...	0·3477 ...	0·3962 ...	0·27
21...	57 39 ...	7 0·4			
23...	59 36 ...	7 10·7 ...	0·3558 ...	0·3951 ...	0·26
25... 18	1 28 ...	7 20·8			
27...	3 15 ...	7 30·7 S. ...	0·3638 ...	0·3936 ...	0·25

The brightness on 1887 August 27 is taken as unity.

NEW MINOR PLANET.—A new minor planet, No. 272, mag. 13, was discovered by M. Charlois, of the Nice Observatory, on February 4.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 *FEBRUARY* 12–18.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24. is here employed.)

At Greenwich on February 12

Sun rises, 7h. 22m.; souths, 12h. 14m. 28·6s.; sets, 17h. 7m.: right asc. on meridian, 21h. 42·4m.; decl. 13° 46′ S. Sidereal Time at Sunset, 2h. 36m.

Moon (New, February 12, 0h.) rises, 7h. 47m.; souths, 12h. 41m.; sets, 17h. 43m.: right asc. on meridian, 22h. 9·2m.; decl. 13° 19′ S.

Planet.	Rises. h. m.	Souths. h. m.	Sets. h. m.	Right asc. and declination on meridian. h. m.	° ′
Mercury..	7 54 ...	13 19 ..	18 44 ...	22 47·5 ...	7 39 S
Venus ...	5 37 ..	9 41 ..	13 45 ...	19 8·2 ...	21 38 S.
Mars ...	22 59*...	4 20 ...	9 41 ...	13 46·6 ...	8 18 S.
Jupiter ...	2 27 ...	6 42 ...	10 57 ..	16 9·1 ...	20 4 S.
Saturn ...	14 51 ...	22 46 ...	6 41*...	8 15·4 ...	20 22 N.
Uranus...	22 4*...	3 37 ...	9 10 ...	13 3·9 ...	6 4 S.
Neptune..	10 33 ...	18 13 ...	1 53*...	3 41·6 ...	17 55 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Feb. h.
13 ... 9 ... Mercury in conjunction with and 3° 8′ north of the Moon.
16 ... 12 ... Mercury at greatest elongation from the Sun 18° east.
17 ... 0 ... Mercury at least distance from the Sun.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·4	81 16 N.	Feb. 14,	19 58	*m*
Algol	3 0·9	40 31 N.	,, 12,	22 19	*m*
R Aurigæ	5 8·3	53 28 N.	,, 18,		*M*
R Canis Majoris...	7 14·5	16 12 S.	,, 13,	21 35	*m*
			,, 15,	0 51	*m*
S Cancri	8 37·5	19 26 N.	,, 16,	21 43	*m*
S Ursæ Majoris ...	12 39·1	61 42 N.	,, 15,		*m*
R Boötis	14 32·3	27 13 N.	,, 17,		*m*
δ Libræ	14 55·0	8 4 S.	,, 15,	2 24	*m*
U Coronæ	15 13·6	32 3 N.	,, 15,	0 9	*m*
W Herculis	16 31·3	37 34 N.	,, 18,		*M*
U Ophiuchi... ...	17 10·9	1 20 N.	,, 14,	2 16	*m*
		and at intervals of		20 8	
W Sagittarii ...	17 57·9	29 35 S.	Feb. 16,	0 0	*m*
U Sagittarii... ...	18 25·3	19 12 S.	,, 16,	4 0	*m*
R Scuti...	18 41·5	5 50 S.	,, 18,		*M*
R Lyræ	18 51·9	43 48 N.	,, 16,		*m*
R Aquilæ	19 1·0	8 4 N.	,, 16,		*M*
S Vulpeculæ ...	19 43·8	27 1 N.	,, 12,		*M*
Y Cygni	20 47·6	34 14 N.	,, 12,	19 56	*m*
			,, 15,	19 50	*m*
δ Cephei	22 25·0	57 51 N.	,, 13,	2 0	*m*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A. °	Decl. °	
Near 49 Camelopardalis	110	62 N.	Slow.
From Monoceros ...	120	5 S.	Slow.
Near *v* Herculis	238	46 N.	February 17.
,, *σ* Ophiuchi... ...	260	3 N.	Swift; streaks.

GEOGRAPHICAL NOTES.

THE French traveller, M. Thouar, who was believed to have perished on his way to the Gran Chaco, has returned to Port Pacheco with his companions. This news was lately sent from Buenos Ayres to Chuquisaca (Sucré).

IN the new number of *Appalachia* Mr. F. H. Chapin describes his ascent of a glacier on Mummy Mountain, Northern Colorado, lying directly north of Long's Peak, and in line with the centre of Estes Park. A single glance at the series of crevasses convinced Mr. Chapin that it was really a glacier, and not a mere accumulation of snow. To the same number Mr. S. H. Scudder contributes a paper on the White Mountains as a home for butterflies.

IN the paper contributed to the Berlin Geographical Society by Dr. H. Meyer on his ascent of Mount Kilimanjaro, he modifies his first statements as to the height which he attained; according to a statement of his companion, Dr. Meyer did not get within 2000 feet of the top.

IN the new Bulletin of the American Geographical Society will be found a useful paper by Mr. A. S. Packard, in which he brings together a *précis* of what was known of Labrador. Accompanying the paper is a good map, in which Mr. Packard has embodied information hitherto unpublished. Dr. Fr. Boas gives the results of his year's sojourn among the Eskimo.

IN the last number of the Proceedings of the Victoria Branch of the Australasian Geographical Society will be found a detailed account of Mr. Cuthbertson's expedition to explore the highlands of British New Guinea. The accompanying map gives a good idea of the nature of the country. Mount Obree was found to be only 8000 feet high, 2000 feet lower than previous estimates.

WE learn from the *Isvestia* of the East Siberian Branch of the Russian Geographical Society (vol. xvii. fasc. 1) that the vertical section of the Angara at its issue from Lake Baikal is 17,920 feet, and that the volume of water discharged from the great Siberian lake reaches 121,353 cubic feet per second. If this outflow were checked, the level of the lake would rise 7 feet in thirteen months.

DR. ROBERT SIEGER contributes to the Geographical Society of Vienna University a paper in which he discusses what information exists as to the changes of level in the African lakes. This shows clearly that for the last ten years at least these have been lowering in level, and, in the case of Tanganyika, to the extent of many feet. The changes which take place are almost entirely dependent on rainfall, and the probability is that there are periods of depression and periods of elevation. It is important that observations should be carried on both in African lakes and African rivers for a period sufficiently long to afford data numerous enough to warrant any conclusion to be drawn.

PROF. EUARD SÜSS, the able author of "Das Antlitz der Erde," recently read a paper to the Vienna Geological Society, on the history of the ocean, which is to some extent supplementary to that work. In this he points out that from the mouths of the Ganges all round the Pacific coasts of Asia and America to Cape Horn, the coasts are outlined by mountain-ranges which close in upon each other in great curves. From Cape Horn, again, all round the Atlantic and the Indian Oceans to the mouths of the Ganges, the coasts are unconnected with mountain-ranges, but are encircled by table-lands or broken mountain patches. We have thus, then, so far as the structure of the ocean basins is concerned, to distinguish between a Pacific and an Atlantic type. As regards the age of the oceans, Prof. Süss concludes from the geological formations that the Pacific is the oldest, next to that the Indian, and last of all the Atlantic. The oceans, he points out, are areas of depression. Each new depression would form a fresh receptacle for water, and so the shore-line of the land would be lowered. Prof. Süss seems to maintain that it is to this, and not to the actual rising of the land, that the elevation of the coast-line in certain regions is due.

MR. J. F. NEEDHAM has been engaged to conduct an expedition from Sadiya to the Hukeng Valley, and thence to Bhamo on the Upper Irrawady. His previous achievements in the Abor Hills, and the country lying between the Brahmaputra and the Zayal Chu, and his success in conciliating the unfriendly tribes on that frontier region, marked him out for selection as the proper officer to conduct the present mission.

THE new part (Nos. 133-34) of the *Zeitschrift* of the Berlin Geographical Society is mainly occupied with Dr. W. Sievers's account of the results of his exploration of the Sierra Nevada of Santa Marta in the north-east of the United States of Columbia, an excellent large-scale map accompanying the number. A considerable section of the paper deals with the geology of the region, after which Dr. Sievers treats of the surface formation, altitudes, climate, vegetation, and agriculture, the land-snails population.

NEWS from Victoria, in the Cameroons, states that the African traveller, Dr. Zintgraff, started for Rio del Rey in the steamer *Nachtigal*, accompanied by thirty porters. He is on his way to the Elephant Lake in order to establish a scientific station. The other half of the Expedition, under the command of Lieut. Zeuner, is to proceed up the Mungo River to Mundame, to reach the Elephant Lake from that part.

OUR ELECTRICAL COLUMN.

IF a platinum plate be immersed in a porcelain or glass vessel containing dilute sulphuric acid, and another similar plate be immersed in another vessel containing caustic potash solution, then if the two vessels be connected by a siphon tube or a cotton wick, a current will be set up, but which rapidly diminishes owing to the polarization of the metal plates by the deposition of oxygen and hydrogen upon them. Becquerel removed the hydrogen by using nitric instead of sulphuric acid, and increased the current considerably. Dr. Alder Wright and Mr. C. Thomson (Royal Society, February 2, 1888) have been examining this form of battery, and have found many other acids which act in the same way, such as potassium permanganate, potassium bichromate, potassium ferricyanide, and bromine dissolved in sulphuric acid, ferric chloride, hydrochloric acid and chlorine. Moreover, they have removed the oxygen by using a concentrated solution of sodium hyposulphite made strongly alkaline with caustic soda, strong caustic soda with pyrogallol, cuprous chloride, ferrous sulphate, and ammonium chloride dissolved in ammonia. They also found the quantity of oxygen

and hydrogen evolved exactly proportional to the current passing. If a silver voltameter were included in the circuit, for every milligramme-equivalent (108 milligrammes) of silver deposited, 1 milligramme-equivalent of hydrogen occupying 11·2 cubic centimetres and 8 milligrammes of oxygen occupying 5·6 cubic centimetres at 0° C. and 760 millimetres, were liberated.

ALTHOUGH Sir William Thomson did not publish any electrical theoretical work in 1887, he perfected during that year his practical electrical measuring instruments. They are in use at the Grosvenor Gallery central station in London. There are no more beautiful or accurate instruments in the world, and they reach over an enormous range both of potential and of current measurement. They were admirably illustrated and described in *Industries* of January 27 by Prof. Fleming.

HERTZ (*Wiedemann Ann.* 1887), has shown that the ultra-violet rays have an influence on the passage of sparks. E. Wiedeman and H. Ebert have been repeating and verifying his experiments. The effect of light falling on the spark region was to lower the potential required to produce it. If a succession of sparks be sent, and a telephone be used, the effect of light falling on the sparks was to change not only the note but the whole character of the sound heard in the telephone. If a Geissler's tube were used, an intermittent and irregular discharge became steady and continuous. The effect was evident only on the negative pole.

IT is known that the magnetic qualities of iron diminish considerably when raised to 525° C. (red heat), but iron remains magnetic up to 650° C. Nickel loses its magnetic properties suddenly at 300° C. Lodeboer recently (January 9) read a paper before the Académie des Sciences, in which he showed that with magnetizing forces of 35, 100, and 200 C.G.S. units the iron retains its magnetic properties up to 680° C. ; that beyond this temperature it rapidly loses them ; that at 750° C. they scarcely exist, and at 770° C. they entirely disappear, to reappear only on cooling. It is known that the specific heat of iron undergoes a change of condition between 660° and 720° C., and the coincidence of these two changes is very interesting.

THE treatment of sewage by electricity is, it seems, likely to receive a practical test at the Metropolitan Board of Works' outfall at Crossness. Mr. Fewson, of Buckingham, made some experiments in this direction at Wimbledon last summer, and now Mr. W. Webster is about to do the same thing at one of the large tanks on the Thames. The electric current is said to have a wonderful disinfecting and purifying influence. The evolution of gas stirs up the liquid, the nascent oxygen is brought into rapid contact with the impurities and reduces them, precipitation is expedited, and the whole cleansed. It is to be hoped that the cost will not swamp this new and useful field for electricity.

THE extraordinary rise in the price of copper has attracted much attention to the use of iron for lightning conductors. Prof. Silvanus Thompson advocates iron in preference to copper under all circumstances. Iron is much used by the War Department to protect magazines. Dr. L. Weber recommends it even in a solid form rather than as a stranded rope, but the latter form is much more portable and workable ; moreover, Prof. Hughes showed it to be less subject to self-induction than a solid rod—an obstruction not to be neglected. Iron conductors are stronger, much cheaper, less easily fused, and less liable to theft than copper. There can be no objection to the use of iron.

THE electro-deposition of aluminium has attracted much attention since the introduction of the Cowles process. Herman Reinbold has proposed the following solution, with which he has obtained good but small results : alum 50 parts, water 300 parts, aluminium chloride 10 parts. This solution is heated to 200° F., and after cooling 39 parts of potassic chloride are added.

THE INSTITUTION OF MECHANICAL ENGINEERS.

THIS Society held its forty-first annual general meeting in the theatre of the Institution of Civil Engineers on Thursday and Friday of last week. After the Annual Report had been presented and accepted, Mr. John Richards' paper "On Irrigating Machinery on the Pacific Coast" was read and discussed. The need of irrigation in this district arises from three causes : the lack of rain, which ceases altogether along the coast in summer-time ; the want of surface-water ; and the free percolation into the sandy soil beneath. The whole of the land in the country, excepting the low-lying sedimentary plains near the mouths of the rivers, and around the Bay of San Francisco, where water reaches the surface by capillary saturation, requires irrigation. Nearly all the land upon which water can be led, either by training small mountain streams, or by leading long canals from the rivers, has been occupied, so that the only remaining resource for getting water will be by lifting it from the rivers or the gravel strata by machinery. The paper is descriptive of the various pumps and hydraulic rams employed, and was illustrated by means of thirty-five figures.

Mr. William Geipel's paper "On the Position and Prospects of Electricity as applied to Engineering" refers to those branches of electric engineering which involve the employment of considerable power, and are in some way or other connected with the use of dynamos. They comprise electric transmission and distribution of power, and electric lighting, locomotion, and metallurgy.

In the author's opinion the transmission and distribution of power by electricity will occupy in the near future most of the attention of the electric engineer. Owing to its simplicity, the ease with which an electric motor can be applied to any purpose requiring power, and its high efficiency, it is certainly an approach to an ideally perfect system of transmission. In the United States great strides have been made in the applications of electric motors, which already rival those for lighting purposes. One of the great advantages of these applications is due to the low efficiency of belts and shafting where high speed is required and the demand for power is variable. By getting rid of shafting the necessity for additional stability in buildings is obviated, and constant lubrication is done away with. The distribution of power by electricity from a central station to small users can be effected from the same mains and generators as are used for electric light purposes ; as to whether gas through the medium of gas engines or electricity by means of electric motors should be used, will become entirely a question of economy and convenience. On the one hand the electric motor can be started and stopped with the greatest ease, it requires little attention, occupies little space, and can be placed anywhere, while against the use of the gas engine, the author brings forward its irregularity of speed owing to the intermittent impulse and the wear and tear in the valves and working parts. Shunt motors, which are now almost exclusively used, possess a practically perfect power of self-control, not only over their rate of speed with varying load, but over the energy absorbed, for they help themselves, as it were, to only such an amount of energy as will enable them to deal with the work imposed upon them. Another advantage in shunt motors, first pointed out by the late Sir William Siemens in 1880, is that they act as generators when themselves driven by any extraneous power, without any complication of the switch gear required with series motors. The author refers to various installations which have already taken place in Europe and America, which are paying their way, whilst at the Falls of Niagara plant is being put down to distribute power obtained from the Falls to neighbouring towns, including Buffalo, which is twenty miles distant ; the amount of power is stated at 15,000 h.p., of which 10,000 h.p. is contracted for at £3 per h.p.

Electricity has been applied with efficiency in collieries for underground hauling, pumping, ventilating, and drilling ; in ship-yards and similar works it has been proved to be a suitable and economical means of transmitting power for riveting, drilling, &c.

In its application to the transmission of power to great distances, electricity is found to be more economical than either hydraulic, pneumatic, or wire-rope transmission, and comparative tables are given showing the first cost of plant per horse-power transmitted, and also the working cost per horse-power transmitted per hour. For a distance of 22,000 yards the cost of installation for the transmission of 100 h.p. is £87, £310, £192, and £162 per h.p. for electric, hydraulic, pneumatic, and wire-rope transmission respectively ; whilst the cost per h.p. transmitted per hour is 4·08, 6·84, 4·50, and 9·73 pence.

Amongst many interesting applications, that made by the Marquis of Salisbury at Hatfield may be specially referred to. The River Lea is utilized to generate electricity by means of turbines, the electricity being transmitted to the house and over the estate for a variety of purposes. The motors at the house

drive pumping and ice-making machinery and an air-propeller fixed in the roof for ventilating; on the farm the motors are used for elevating hay and corn sheaves to the top of the stacks, for thrashing, for cutting rough grass with a chaff-cutting machine for ensilage, in fields extending to a distance of two miles, for grinding corn, &c., to make fodder, and for other purposes. The motors have also been used for pile-driving, for making coffer-dams where necessary in the river, and also for dredging the river and clearing it of weeds, and for pumping the town sewage into a tank at the height of thirty feet for irrigation. The conductors are carried overhead on poles about the farm and underground in wooden troughs to the house. The practical methods employed for electric locomotion—being those of a third insulated rail, an overhead conductor, an underground insulated conductor, and storage batteries—are described, and examples of the application of all are given. To the first belong the Portrush Railway, and Besbrook and Newry Tramway; to the next the electric railway at Moedling, near Vienna, and the Frankfort-Offenbach railway. This plan has been most largely adopted in America, where there are probably not far short of one hundred electric railways at work and projected. Of the underground conductor, the most important example is the electric tramway at Blackpool, while storage batteries are being employed on the North Metropolitan Tramway in London. The ordinary rails have been used as conductors in the short electric railway at Brighton, where the expenses amount to twopence per car-mile.

The plan of transporting material in skips on overhead wire-ropes by means of electricity, introduced under the name of telpherage by Prof. Fleeming Jenkin, has been employed with success for two years past at Glynde, near Lewes, for transporting clay to the railway over a distance of 1600 yards, and is applicable for use in places where material has to be conveyed across hilly districts. In the author's opinion a modification of this plan might be advantageously applied to alleviate the heavy street traffic in our larger cities.

The author considers the question of electric lighting under the three aspects of comfort, convenience, and economy. As regards the first two, electric lighting has the advantage over other systems; whilst as regards cost, although electric lighting, and especially incandescent electric lighting, is still heavy, yet for lighting main streets and railway stations, or other places where concentrated light is required, the arc light is cheaper than gas. As its use extends, the cost of working becomes reduced. Thus in the Waverley Station, Edinburgh, on the North British Railway, thirty-three arc lamps, with 41,884 lamp hours, cost 2·77 pence per lamp hour from July to December 1884; whilst in 1886, thirty-nine arc lamps, having 55,068 lamp hours, cost 1·79 penny per lamp hour.

The cost of incandescent lighting is especially variable, and affected by the local conditions of the installation. The chief of these are the average number of hours of lighting each lamp, and the average distance of the lamps from the generating station. Where conditions are favourable, incandescent lighting can already compete with gas. Messrs. George Jager and Sons' yearly cost of lighting their sugar refinery at Leith is given as an example, it having been £347 with gas and £204 with incandescent lamps. The author draws special attention to the circumstance of the much larger application of electricity to lighting in the United States as compared with this country. In the United States there is hardly a city or town of 20,000 inhabitants which has not a central station for arc or incandescent lamps; and many towns of 3000 to 4000 inhabitants are also supporting them.

The efficiency of dynamo machines being as high as 95 per cent., and there not being much likelihood of material improvement in steam engines, the author draws attention to the importance of improving the lamps by making them with a higher resistance and greater efficiency, the voltage having a great effect on the cost of working distant lamps. Transformers, by means of which high tension currents of electricity, sent from a distant generating station along a small conductor with comparatively small percentage of loss, can then be converted into low tension currents for the supply of ordinary incandescent lamps, are receiving a large amount of attention, the loss by conversion being as low sometimes as 5 per cent. Efforts are also being made to introduce the system of secondary batteries, charged in series by a high tension current, and discharged in parallel circuit, and if it can once be demonstrated to be economical, there would be a large field of application. At Leamington an extensive central station is now at work, the cost of the undertaking being £30,000; while the Bradford Corporation have recently voted a sum of £15,000 for erecting a central station in their town. Both these are instances of direct supply without transformers or secondary batteries. Electric metallurgy is a branch of electric engineering to which attention was first drawn by the late Sir William Siemens, whose death occurred before he had perfected his invention. The electro-chemical separation of ores on a commercial scale by the electric furnace has been recently put to the test, chiefly in obtaining aluminium from conundrum. The furnace designed by Prof. Mabery is built of fire-brick and lined with powdered charcoal; electricity is conducted to the ore by carbon rods, meeting near the centre. The ore mixed with charcoal and granulated copper surrounds and covers the carbons; the furnace is closed with a layer of charcoal and a lid lined with fire-brick. A current of 50 volts electromotive force is supplied and melts the metal around the electrodes, which are moved apart gradually until the whole is melted. The conundrum becomes gradually deoxidized, the aluminium combining with the copper, while the oxygen with the carbon escapes as carbonic oxide, about five hours sufficing to complete the reduction. Aluminium, being only one-third the weight of iron, and possessing great strength, its production at a cheap rate would probably cause a revolution in engineering construction.

The meeting was presided over by Mr. E. H. Carbutt, the President, who was re-elected to the chair, whilst Sir Douglas Galton, K.C.B., was the new member elected on the Council. The meeting was as usual of a very successful character.

THE NATIONAL SMOKE ABATEMENT INSTITUTION.[1]

IN presenting the Report to the members for the year 1887, the Council consider it desirable to reprint from the Memorandum of Association the objects for which the Institution was established. These are the following:—

To promote the abatement of coal smoke and other noxious products of combustion in cities and other places, in order to render the atmosphere as pure and as pervious to sunlight as practicable. To check the present serious waste of coal, and the direct and indirect loss and damage accompanying the over-production of smoke and noxious products of combustion. To continue, organize, and extend the public movement inaugurated and hitherto carried on by the Smoke Abatement Committees (otherwise known as the Joint Committees for Abatement of Smoke, appointed by the National Health Society and Kyrle Society of London, and the Smoke Abatement Committee of Manchester), and to take up and proceed with any work undertaken or commenced by such Committees. To advance the aforesaid objects by promoting and encouraging the better and more economical use of coal and coal products, and the selection of suitable fuel, as well as general improvement in the various modes of obtaining, applying, and using heat and light for domestic and industrial purposes. And in connection with such objects to obtain and provide such buildings, appliances, and assistance as may be deemed expedient. And without prejudice to the advancement of the objects aforesaid by other means to advance the same by the following means more particularly:—

(*a*) By calling public attention to the serious pecuniary loss and injury, to the health and comfort, which arise from coal smoke, and from defective heating, ventilating, and lighting arrangements.

(*b*) By stimulating, assisting, and encouraging inventors, manufacturers, traders, and others to bring forward, develop, and perfect new or improved fuels, substances, methods, and appliances for the generation or application of heat or light, and for consuming or lessening the production of smoke and noxious products of combustion.

(*c*) By conducting practical trials of fuels, apparatus, and systems connected with the generation or application of heat or light, and causing reports to be made thereon for the guidance, assistance, or information of inventors, traders, intending users, and the public generally.

(*d*) By granting awards, certificates, medals, or prizes in connection with approved fuels, methods, or apparatus.

(*e*) By establishing, or assisting in establishing, public exhibitions, either periodical or otherwise, of appliances pertaining to heating, ventilating, or lighting.

[1] Report of the Council for the year 1887.

(*f*) By collecting and recording statistics and information, and making, assisting, or encouraging experiments or researches as to the effects upon the atmosphere, and upon life, health, and property of the use of coal and other fuels and means employed or to be employed in connection with heating or lighting; and by printing, publishing, and circulating any such statistics or information, including the intended report of the Committees aforesaid, or any similar composition or literary work.

(*g*) By imparting information, instruction, and assistance to local authorities, manufacturers, workmen, householders, servants, and the public generally whether by means of lectures, demonstrations, pamphlets, written articles, or otherwise in relation to the subject of smoke prevention or abatement.

(*h*) By joining or concurring with any other institution, society, or persons, in doing or causing, or procuring to be done, any of the things aforesaid.

To promote the abatement of noxious vapours arising from manufactures or manufacturing processes, and to resort to and use for that purpose powers and means analogous to those hereinbefore contemplated with reference to Smoke Abatement and any other reasonable means. For all or any of the purposes aforesaid, either alone or in conjunction with others, to promote legislation and parochial and other regulations, and to assist in the enforcement thereof, and of any existing or future legislative, parochial, or other regulations.

In reporting upon the business transacted by the Institution during the past year, it is essential that the members should be reminded of the urgency for further legislation on the subject of smoke prevention.

The Institution has been in communication with the medical officers of health and chief constables throughout the country, and the most valuable information obtained with reference to the working of existing by-laws is given as supplements Nos. 1, 2, 3, 4, 5, and 6, to a paper on Smoke Abatement, read at the Bolton Congress of the Sanitary Institute. These supplements are published in the Transactions of the Sanitary Institute of Great Britain, and by reference to them it will be seen that the municipal authorities of Liverpool are much more alive to the necessity of prosecuting offenders against the Smoke Abatement Acts than the authorities in any of the other places from which reports have been obtained.

By comparison with the Report issued by the Commissioner of Police for the Metropolis of 1886, it will be seen that the number of cases in which fines were imposed in Liverpool was 545, whereas the number of convictions in the metropolis amounted only to 82. It might further be noted, however, in respect to the penalties imposed, that the average of all the fines in Liverpool was 19*s.* 11¼*d.* The average in London was £1 17*s.* 5*d.* The inadequacy of the fines imposed is a serious obstacle in dealing with police prosecutions, and the fines have little effect, if any, in the prevention of smoke, in consequence of the amount of the penalty being so disproportionate to the financial positions of the persons on whom they are imposed.

During the year attention was prominently called by Lord Stratheden and Campbell to the provisions in the Bill introduced by him to the House of Lords "To amend the Acts for abating the nuisance arising from the smoke of furnaces and fire-places within the Metropolis," and resulted in a Select Committee being appointed to consider the terms of the Bill, and to report to the House of Lords. The minutes of evidence were laid before the House of Lords on the 15th of July, 1887, and the published Report contains much valuable information with respect to the working of the Smoke Abatement Acts:—The nuisance created by steamers on the Thames; the necessity for extension of the metropolitan area to be within the Acts; the necessity for controlling the emission of smoke from club-houses, hotels, private residences, and other buildings not within the scope of the existing Acts; the usual course followed by the police in instituting prosecutions; a return showing the number of police employed in carrying out the Smoke Nuisance Abatement Acts; the effect of the increase of smoke on the health of the people, and the advantages from a sanitary point of view to be derived by the prevention of smoke; also particulars regarding the commercial advantages to be derived by the consumption of smoke; particulars of the methods which might be adopted for the complete combustion of fuel in domestic grates; and generally, a great mass of information dealing with the subject laid before the House of Lords by the following gentlemen: Mr. W. R. E. Coles, the engineer appointed by the Home Secretary to examine furnaces in the metropolis; Mr. James Edward Davis, of the Home Office, legal adviser to the Commissioners; Mr. Charles Cutbush, Superintendent of Police; and Mr. Ernest Hart, Chairman of Council of the National Smoke Abatement Institution.

By reference to the Police Orders and Regulations reprinted at the end of this Report, it will be observed in paragraph 36 that hotel-keepers in the metropolis not using steam-engines can only be proceeded against under Section 19, Sub-Section 3, of 29 and 30 Vict., cap. 90, and be guilty of an offence under that Section. In consequence of this Act of Parliament, Section 19, Sub-Section 3, stipulates that any chimney (not being the chimney of a private dwelling-house) sending forth black smoke in such quantity as to be a nuisance is exempt from the working of the Act and it is left to the justices to dismiss the complaint if they are satisfied such fire-place or furnace is constructed in a manner to consume, *as far as practicable*, all smoke arising therefrom, but it does not state any standard smoke shade or any degree to be fixed upon as the limit, and therefore the justices may or may not convict at their option.

The purpose of Lord Stratheden and Campbell's Bill is to prohibit or regulate the emission of smoke from any building, no immunity being granted to hotels, club-houses, or domestic fire-places now exempted from the existing Acts. The effect of the general evidence brought before the Select Committee of the House of Lords was a resolution to await the results of the further operation of the existing Acts, the purpose and intention of which should, it was held, be more fully carried into effect.

The Council invite the careful consideration of members to the necessity for legislation, and on an early date will arrange for a series of meetings to be held, at which it is expected the sanitary inspectors from the leading provincial towns will assemble, in order to compare and suggest revisions for the existing municipal by-laws, as well as for the purpose of drafting propositions to submit to the authorities on the subject of improved legislation in the metropolis.

The Council having considered the desirability of taking the first opportunity for conducting simultaneous tests of the furnaces of a large number of steam-boilers under equal conditions, thought that such an opportunity might be offered at the forthcoming Exhibition to be held in Glasgow. They accordingly directed the Secretary to write to the Lord Provost of Glasgow, laying the outlines of their scheme before him, and suggesting its adoption by the Exhibition authorities. Briefly stated, the proposal was: That the whole range of boilers to be used for working the machinery of the Exhibition should be erected in such a manner that each boiler should have its setting and chimney independent of the other boilers, so that the several systems of stoking and arrangement of furnaces could be fully tested under identical conditions of fuel, atmosphere, and time; while the results as regards smoke would be evident to the public.

It is to be regretted that the authorities in charge of the Glasgow Exhibition have not been able to see their way to co-operating with the Smoke Abatement Institution as proposed. Simultaneous tests on such a large scale have never previously been made. Many tests of great value have been made on furnaces, but these have been at separate times, and under different atmospheric conditions, and the results, however favourable in themselves, have been incapable of classification for comparison. The proposal of the Council, if adopted, would have supplied what is wanting by making these tests of several boilers at the same time, and under the supervision of an impartial body.

With reference to this subject, a correspondence has taken place with Mr. Fletcher, Chief Inspector under the Alkali Acts, as he has in preparation a report upon the injurious effect of the impurities of the air and water on the Clyde. Mr. Fletcher was asked to furnish a copy of the report, but replied that it was in the hands of the Secretary for Scotland. Application has been made to the Secretary for Scotland, and attention drawn to the importance of the series of tests which the Council proposed. The Secretary for Scotland in his reply stated that Mr. Fletcher's report has not yet been brought before Parliament, and with respect to the testing of the boilers, said that he would inform the Committee of the Glasgow Exhibition that he considers the suggestion of the Institution to be deserving of consideration and adoption.

At a meeting of the Institute of Engineers and Shipbuilders in Scotland, on the 8th of December, a very comprehensive paper

was read by Mr. George C. Thomson on "Smoke," and in the discussions which followed, Mr. W. R. W. Smith, Chairman of the Health Committee of the Glasgow Corporation, urged upon the members present the desirability of doing all in their power to secure that at the forthcoming International Exhibition in Glasgow each of the boilers be supplied with a separate chimney, so that a series of exhaustive trials may be made with mechanical stokers, &c., and other means for the purpose of showing what might be done in the way of smoke prevention.

With reference to the subject of testing, the Committee are of opinion that arrangements should be made as soon as possible for obtaining the use of three testing-rooms for testing stoves, grates, and ranges, the rooms being conveniently accessible for such articles, and having gas connections under command. The tests made in these rooms, under the same conditions of chimney and cubic capacity, would then become of greater comparative value than tests made in independent rooms.

Arrangements will be made as soon as practicable for procuring such accommodation for testing, and also for providing the necessary instruments used for testing; and as the system develops, attention will be given to the establishment of a chemical laboratory, the analysis of gases, and testing-rooms for testing-apparatus incidental to the work of the Institution.

SCIENTIFIC SERIALS.

American Journal of Science, December 1887.—On the destruction of the passivity of iron in nitric acid by magnetization, by Edward L. Nichols and W. S. Franklin. From the experiments described in this paper, which was originally read before the Kansas Academy of Science, November 1885, it appears that the action of the magnet tends to lower the temperature of transition to the active state, and that the intensity of the magnetic field necessary to convert passive into active iron at a given temperature increases rapidly with the concentration of the acid. An account is promised of further researches offering a satisfactory explanation of the manner in which the chemical behaviour of iron is modified, and its passivity destroyed in the magnetic field.—On a method of making the wave-length of sodium light the actual and practical standard of length, by Albert A. Michelson and Edward W. Morley. The preliminary experiments recently carried out according to the method here proposed seem to confirm the anticipation that it would furnish results more accurate than any of those hitherto suggested. The apparatus for observing the interference phenomena is the same as that used in the experiments on the relative motion of the earth and the luminiferous ether.—The work of the International Congress of Geologists, by G. K. Gilbert. This is a reprint of an address delivered before the Section of Geology and Geography of the American Association for the Advancement of Science at the New York meeting, August 10, 1887. It deals largely with a revised system of geological terminology, the substance of which has already been published. The question of geological coloured maps is also considered, and practical suggestions made for their greater efficiency and economy.—On the existence of certain elements together with the discovery of platinum in the sun: contributions from the physical laboratory of Harvard University, by C. C. Hutchins and E. L. Holden. These investigations, carried on with Prof. Rowland's magnificent diffraction grating, deal with cadmium, lead, tin, silver, potassium, and several other elements, including platinum, the presence of which in the solar atmosphere is here for the first time determined. Between 4250 and 4950 were found sixty-four lines of platinum, sixteen of which agree with the solar lines.—The flora of the coast islands of California in relation to recent changes of physical geography, by Joseph Le Conte. A careful study of these insular groups, at present from 20 to 30 miles distant from the coast, shows that they at one time formed part of the mainland, from which they were undoubtedly separated during the Quaternary period. That they still formed part of the continent during later Pliocene times is shown by the remains of the mammoth found on Santa Rosa, one of the largest and furthest off of the whole group.—A new instrument for the measurement of radiation, by C. C. Hutchins. The instrument here described and illustrated presents great advantages over the thermopile as an accurate measurer of radiations. It is much more sensitive and requires no longer time to return to zero than for the galvanometer needle to come to rest. A lighted match at 6 feet drives the needle round to its stop.—Mineralogical notes, by George F. Kunz. Descriptions with analyses are given of a rhodochrosite from Colorado, of crystals of hollow quartz from Arizona, of hydrophane from Colorado, and of a remarkable silver nugget weighing 606 ounces from the Greenwood mines of Michoacan, Mexico.

January.—The speed of propagation of the Charleston earthquake, by Prof. Simon Newcomb and Captain C. E. Dutton. A careful comparative study of the reports from all parts of the disturbed area shows a general average speed of $3{\cdot}214 \pm 0{\cdot}072$ miles, or 5171 ± 116 metres per second.—History of the changes in the Mount Loa craters, Hawaii; Part I, Kilauea, by James D. Dana. The first paper embraces the whole period from 1823 to 1886, during which there appear to have been at least eight discharges from Kilauea. The general dynamical conclusions are that the cycle of movement is simply (1) a rising in level of the liquid lavas, and of the bottom of the crater; (2) a discharge of the accumulated lavas down to some level in the conduit determined by the outbreak; (3) a down-plunge of more or less of the floor of the region undermined by the discharge. It is further shown that Kilauea is a true basalt volcano in its normal state, the rock material being dolerite or basalt, and the heat sufficing for the perfect mobility of the lavas.—The analysis and composition of tourmaline, by R. B. Riggs. The methods of analysis are described, with results for various specimens from different parts of North America and Brazil. The general inference is that there are three types, lithia, iron, and magnesia tourmaline, with an indefinite number of intermediate varieties, iron appearing to be the connecting link between the whole series. The special formulas of the three distinct types are:—

(1) Lithia: $12SiO_2, 3B_2O_3, 4H_2O, 8Al_2O_3, 2(NaLi)_2O$.
(2) Iron: $12SiO_2, 3B_2O_3, 4H_2O, 7Al_2O_3, 4FeO, Na_2O$.
(3) Magnesia: $12SiO_2, 3B_2O_3, 4H_2O, 5Al_2O_3, \frac{1}{2}\frac{5}{2}MgO, \frac{3}{2}Na_2O$.

—On the different types of the Devonian system in North America, by Henry S. Williams. It is shown that in North America the Devonian system offers at least four distinct types in four corresponding areas, blending somewhat at their borders, but in their central parts presenting marked peculiarities. The four areas are: (1) Eastern Border, mainly in Northern New England; (2) Eastern Continental, including New York, thence southwards to West Virginia and north-westwards to Canada West and Michigan; (3) Interior Continental, chiefly Iowa and Missouri, extending northwards probably to the Mackenzie basin; (4) Western Continental, in Nevada and conterminous States.—On the law of double refraction in Iceland spar, by Charles S. Hastings. The general inference from these researches is that Huyghens' law of double refraction in uniaxial crystals is probably true to less than 1 part in 500,000, and consequently that there is no known method by which any error in it can be detected by observation alone.—In the Appendix, Mr. O. C. Marsh describes a new genus of Sauropoda and other new Dinosaurs from the Potomac formation; also a new fossil Sirenian from California.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, January 19.—"Notes on the Spectrum of the Aurora." By J. Norman Lockyer, F.R.S.

I exhibited to the Society on November 17, 1887, a tabular statement showing the bright lines seen in the spectra of various celestial bodies, and I also gave those recorded in the spectrum of the aurora, showing many remarkable coincidences.

I now find that the connection is closest between the auroral spectrum and that of stars III.*a*, and, in anticipation of a subsequent communication of details, I send on the accompanying table, showing the origin of Dunér's bands, so far as I have at present made them out, and their connection with the spectrum in question.

The individual observations which I have used in the table are those collected by Mr. Capron and Mr. Backhouse (NATURE, vol. vii. pp. 182 and 463).

TABLE OF WAVE-LENGTHS OF AURORAL LINES.

Barker	431	470	482	502	517				562	623
Smyth									558	635
Zöllner										628
A. Clerke			485				532			
Herschel							531			
Backhouse	430			501	516·5		532			606
Lord Crawford			more ref. than F			523				
H. R. Proctor			,,							
Vogel		469								
Ellery										635
O. Struve									554	
Ångström		472				521			556	
Lemström	426	469				525				
German North Polar Expedition									557	
Respighi									557	
Peirce	431	464	486			520	531	545	557	
Probable origin	CH	C (hot)	C (cold)	Mg	C (hot)	Mg	*	Zn† (1)‡	Mn (1)	Fe (1)
Wave-lengths of probable origin	431	474	483	500	516·5	520·1		546	558	615
			477 485					550 545		616 627
			9					5		2
Dunér's bands		460 474		495 503	516 521				564 559	
		10		8	7				4	

* Coronal line. † Another probable origin for this in the aurora is 540 Mn. ‡ This means brightest fluting.

Addendum.—The following table shows the above figures in another form and includes the bright lines recorded in γ Cassiopeiæ :—

Aurora.	Dunér's bands.	Bright lines in γ Cassiopeiæ.	Probable origin.	Wave-length of probable origin.
431	...	...	CH	431
474	460–474 (10)	...	C (hot)	474
...	...	462·3	Sr	460·7
483	477–485 (9)	...	C (cool)	483
500	495–503 (8)	499	Mg	500
516·5	516–521 (7)	516·7	C (hot)	516·5
520·1	...	...	Mg	520·1
531	...	531	Coronal line	
...	...	542·2	Mn	540
545	545–550 (5)	...	Zn (1)	546
558	559–564 (4)	555·7	Mn (1)	558
...	585–595 (3)	586	Mn (2)	586
615	616–627 (2)	616	Fe (1)	615
635	...	635·6	*	...

Geological Society, January 25.—Prof. J. W. Judd, F.R.S., President, in the chair.—The following communications were read :—On *Ailurus anglicus*, a new Carnivore from the Red Crag, by Prof. W. Boyd Dawkins, F.R.S. The specimen described is a small fragment of the right lower jaw with the last three molar teeth in position, and belongs to the Crag collection of the Yorkshire Philosophical Society. It differs in a marked degree from all fossil European Carnivores, and presents no important points of difference when compared with a series of jaws of recent *Ailurus*. The author gave a description of the fossil, and comparison of it with *Ailurus fulgens*, and also a table giving the comparative measurements of the teeth and jaws of the fossil and of recent *Ailuri*. The species from the Crag was a more powerful animal than any recent *Ailuri* in the British Museum. The paper concluded with a notice of the range of *Ailurus* in space and time. After the reading of this paper the President remarked that seldom had a fact of greater interest in its bearing upon geographical distribution in past times been brought before the Society. Some comments on the paper were also made by Mr. Lydekker, Prof. Seeley, Mr. Newton, and Mr. Blanford.—A contribution to the geology and physical geography of the Cape Colony, by Prof. A. H. Green, F.R.S.—On two new Lepidotoid Ganoids from the early Mesozoic deposits of Orange Free State, South Africa, by Mr. A. Smith Woodward. The results presented in Prof. Green's and Mr. Woodward's papers were discussed by the President, Prof. Rupert Jones, Mr. Blanford, Dr. Geikie, Mr. Clement Reid Prof. Hughes, and Mr. Irving.

Royal Microscopical Society, January 11.—Rev. Dr. Dallinger, F.R.S., President, in the chair.—The President referred to the death of Dr. Arthur Farre, a former President of the Society, and one of its earliest Fellows.—Prof. C. Stewart exhibited specimens of *Thecalia concamerata*. In this genus the female shell exhibited a peculiarity which was quite unique. As age advanced the mantle became folded back upon itself in a very curious manner, and simultaneously with this there occurred a similar in-folding of the contiguous portions of the shell by which two depressions were produced, forming a fusiform chamber when the two valves came together. In this cavity the embryonic shells were found.—Edmond's automatic mica stage rotating by clockwork was exhibited and described.—Mr. A. W. Bennett gave a *resumé* of his paper on freshwater Algæ of the English Lake District, with a description of a new genus of *Capsulococcus* and five new species, in continuation of his previous communication on the same subject.—Dr. G. Gulliver read a paper on *Pelamyxa palustris*. The large size of this amœboid organism had enabled it to be cut into sections, and the granulated structure of its exoplasm thus revealed was described. As regarded its classification, it was thought that ultimately it would be found to have a nearer relationship to the true Heliozoa than to the more lowly Amœbæ.—Mr. E. M. Nelson handed round for inspection two photographic positives ; one of *Amphipleura pellucida*, and the other of a fungus growth which attacked calcareous sand, as described by Mr. J. G. Waller in the Journal of the Quekett Microscopical Club, i. p. 345. This object presented some photographic difficulty because of its non-actinic colour. With regard to the other he remarked that in resolving diatoms with oblique light it was essential to decide whether they intended to focus upon the real surface or upon the optical image produced in a higher plane, in consequence of the double nature of the structure of the valve. In the latter case they would obtain a result such as he exhibited, which was a photograph of the optical image and not of the real diatom.—Mr. Nelson also called attention to a curious optical effect for which at present he was unable to account. In a flat box he had placed a glass positive of *A. pellucida* which was viewed as a transparency through a piece of tube fitted at right angles to the surface. If this was looked at when held towards a surface of light such as an optical lamp-shade or a sunlight gas-burner, the black lines appeared to be slightly smaller than the white lines ; but if it was turned towards a small light at a distance, then the black lines appeared very

* This line is seen as a pretty bright line in the spectrum of the Limerick meteorite, but its origin has not yet been determined, although comparisons have been made with most of the common elements. So far, it has not been observed in any other meteorite.

large and the white ones were reduced to mere threads. The scale of the photograph showed that the effect was not due to the operation of the first diffraction spectrum, and it was still more curious to note that in the case of another positive taken from the same negative and upon the same scale this optical illusion was not observed.

Anthropological Institute, January 24.—Anniversary Meeting.—Prof. Flower, C.B., F.R.S., Vice-President, in the chair.—The following were elected Officers and Council for the ensuing year:—President: Francis Galton, F.R.S. Vice-Presidents: J. G. Garson, Prof. A. H. Keane, F. G. H. Price. Secretary: F. W. Rudler. Treasurer: A. L. Lewis. Council: G. M. Atkinson, E. W. Brabrook, C. H. E. Carmichael, Hyde Clarke, A. W. Franks, F.R.S., Lt.-Col. H. H. Godwin-Austen, F.R.S., T. V. Holmes, H. H. Howorth, M.P., Prof. A. Macalister, F.R.S., R. Biddulph Martin, M.P., Prof. Meldola, F.R.S., Rt. Hon. the Earl of Northesk, C. Peek, Charles H. Read, Lord Arthur Russell, M.P., Prof. A. H. Sayce, H. Seebohm, Oldfield Thomas, M. J. Walhouse, Lieut.-Gen. Sir C. P. Beauchamp Walker, K.C.B.

PARIS.

Academy of Sciences, January 30.—M. Janssen in the chair.—Note on the first volume of the *Annales de l'Institut Pasteur*, presented to the Academy, by M. L. Pasteur. This volume contains the first twelve numbers of a monthly serial established and directed by Prof. Duclaux, of the Sorbonne, and entirely devoted to the progress of the new branch of pathological physiology to which M. Pasteur gives the name of "Microby" or "Microbiology." His remarks were mainly confined to the important memoir by MM. Roux and Chamberland, entitled "Immunité contre la septicémie, conférée par des substances solubles." In this memoir is contained the rigorous demonstration of the far-reaching fact that the septic vibrion, a living ferment analogous to the butyric vibrion, develops soluble chemical products, which gradually act as an antiseptic on the organism itself. These products, introduced in sufficient quantities into the body of the guinea-pig, confer absolute immunity from the deadly attacks of the virus, to which that animal is specially susceptible.—Note on the total lunar eclipse of January 28, by M. J. Janssen. The observations taken at the Observatory of Meudon were mainly directed towards determining a point of telluric spectroscopy connected with the absorption bands of oxygen. They were necessarily of a somewhat preliminary character, and will be continued during future total eclipses of the moon.—Researches on ruthenium, by MM. H. Debray and A. Joly. The paper deals more especially with hyperruthenic acid, its purification, physical properties, behaviour in the presence of water, and under varying temperatures.—An apparatus adapted for experiments at high temperatures in the presence of gases under high pressure, by M. L. Cailletet. For this apparatus, which the inventor has had in use for some years, it is claimed that it enables experimenters to raise substances to temperatures near the fusion of platinum while keeping them in a gaseous atmosphere, the nature and pressure of which may be varied at pleasure.—On double dielectric refraction; simultaneity of electric and optical phenomena, by M. R. Blondlot. These experiments have been undertaken in order to determine whether the double dielectric refraction of a condenser is produced and ceases simultaneously with the charge, or whether there exists an appreciable interval of time either between the production of the electric phenomenon and that of the luminous phenomenon, or between periods of cessation of both phenomena. The conclusion seems to be that, if there is any difference in point of time between these several manifestations, it cannot exceed 1/40000 of a second.—On the laws of chemical equilibrium, by M. H. La Chatelier. It is shown that the numerical laws of chemical equilibrium, such as they are deduced from the two principles of thermodynamics, may be expressed in a very simple way by means of M. Massieu's characteristic function H′, which may be regarded as the true measure of chemical force.—On cinchonigine, by MM. E. Jungfleisch and E. Léger. The authors describe the process of preparation, the chemical properties, and the salts of this substance, whose composition is expressed by the formula $C_{38}H_{20}N_2O_2$.—Persistence of the virus of rabies in dead bodies, by M. V. Galtier. These researches show that the virus retains all its virulence in the bodies of dogs that have been dead seventeen and buried fifteen days. Inoculation from the bulb produces rabies in ten and kills in fifteen days after trepanation.—On the antiseptic properties of naphthol-α, by M. J. Maximovitch. The experiments here described show that, owing to its feebler toxic and stronger antiseptic properties, this substance is in every way superior as an antiseptic to M. Bouchard's naphthol-β.—On the presence of primordial fauna (Paradoxidian) in the neighbourhood of Ferrals-les-Montagnes (southern slope of the Montagne Noire), Hérault: (1) stratigraphic study by M. Jules Bergeron; (2) palæontological study, by MM. Munier-Chalmas and J. Bergeron. Considerable interest attaches to the recent discovery of these organisms by M. Bergeron, for the first time in any part of France. They belong to the earliest forms of the Silurian group, forms which were not known to exist when that group was first established by Murchison in 1835. These first French Trilobites of the primordial fauna, as it was named by Barrande, include some exceptionally fine specimens of the genera Conocephalites and Paradoxides, the latter closely allied to the *P. rugulosus* of Bohemia, and the *P. Pradoanus* common in the Cambrian of Spain.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

Astronomical Observations and Researches made at Dunsink, sixth part (Hodges, Dublin).—A Student's Manual of Psychology, adapted from Kirchner by E. D. Drought (Sonnenschein).—Cardinal Numbers: M. Hopkins (Low).—Civilization and Progress; new edition: J. B. Crozier (Longmans).—Lessons on Prescriptions and the Art of Prescribing; new edition: W. H. Griffiths (Macmillan).—Lehrbuch der Entwicklungsgeschichte des Menschen und der Wirbelthiere, Zweite Abthg.: Dr. O. Hertwig (Jena).—Practical Forestry: C. E. Curtis.—South African Butterflies; two vols: R. Trimen, assisted by J. H. Bowker (Trübner).—Journal of the Society of Telegraph-Engineers and Electricians, No. 69, vol. xvi. (Spon).—Journal of the Royal Statistical Society, December (Stanford).—Annalen der Physik und Chemie, 1888, No. 2 (Leipzig).—Beiblätter der Physik und Chemie, 1888, No. 1 (Leipzig).—Brain, parts 39 and 40 (Macmillan).

CONTENTS.

	PAGE
Messrs. Goschen and Huxley on English Culture	337
The Proposed Teaching University for London	339
Manual of British Discomycetes	340
Our Book Shelf:—	
Mawer: "Physiography"	341
Stokes: "Early Christian Art in Ireland"	341
Letters to the Editor:—	
The Duke of Argyll's Charges against Men of Science.—**Prof. T. H. Huxley, F.R.S.**	342
An Explanation.—**Dr. H. B. Guppy**	342
Snow Crystals.—**A. N. S.**	343
"The Mammoth and the Flood."—**Your Reviewer**	343
An Incorrect Footnote and its Consequences.—**Ralph Copeland**	343
A New Historic Comet?—**Prof. Cargill G. Knott**	344
"Is Hail so formed?"—**Dr. J. Rae, F.R.S.**	344
Modern Views of Electricity. Part III. VII. (*Illustrated*). By **Dr. Oliver J. Lodge, F.R.S.**	344
The Birds'-Nest or Elephant Islands of the Mergui Archipelago. By **Commander Alfred Carpenter, R.N.**	348
Prize for Researches in Natural History	348
Notes	349
Our Astronomical Column:—	
The Royal Astronomical Society's Memoirs	335
Publications of the Dunsink Observatory	353
Rousdon Observatory	353
β Delphini	353
Olbers' Comet	353
New Minor Planet	353
Astronomical Phenomena for the Week 1888 February 12–18	353
Geographical Notes	354
Our Electrical Column	354
The Institution of Mechanical Engineers	355
The National Smoke Abatement Institution	356
Scientific Serials	358
Societies and Academies	358
Books, Pamphlets, and Serials Received	360

THURSDAY, FEBRUARY 16, 1888.

KINEMATICS AND DYNAMICS.

An Elementary Treatise on Kinematics and Dynamics. By James Gordon MacGregor, M.A., D.Sc., &c., Munro Professor of Physics, Dalhousie College, Halifax, N.S. (London: Macmillan and Co., 1887.)

THE logical order of arrangement has been carefully attended to in this book: Part I., on "Kinematics," building up a new subject on the foundation of Euclid's axioms in conjunction with the idea of the variables, such as velocity and acceleration, due to the flow of time; while Part II., on "Dynamics," requires three new axioms —Newton's Laws of Motion—to make a fresh start and connect mechanical effects with their causes.

But it is doubtful if the strictly logical order is the best order for the student to make his first acquaintance with a new mathematical subject: the ideas must grow in his brain by accretion round simple fundamental problems. A student would master the present treatise more easily by reading Part II. first, and referring back to Part I. as occasion required, for the explanation of the details of the mathematical calculations. There is nothing to prevent this order of study here, although the author has, from logical considerations, placed the kinematical part first.

One defect of the logical system is that it places some of the most difficult parts of the subject in the way of beginners: for instance, the theory of the change of units, a theory of which the importance can only be appreciated by those who have made considerable progress in the subject.

In Part I., "Kinematics," the treatment is simple and concise, but we should like to see more examples of phenomena on a large scale, such as those of physical astronomy, or even of railway-train problems.

In questions involving the size of the earth (pp. 74 and 80) it is the circumference and not the diameter which should be given in metres, the circumference being 40,000,000 metres, a kilometre being a centesimal minute of latitude. Or, if the size of the earth is given in miles, it is the nautical mile which should be used, the circumference of the earth being $360 \times 60 = 21{,}600$ nautical miles, a nautical mile being a sexagesimal minute of latitude.

The expression "knots an hour" (p. 60) is irritating to a sailor, as emanating from the engine-room; the proper nautical expression is "knot" simply, a speed of 10 knots being 10 nautical miles an hour.

The formula $\frac{1}{2}v^2 = \frac{1}{2}v_0^2 + as$ is to be preferred to that on p. 34, $v^2 = v_0^2 + 2as$; in all cases the factor $\frac{1}{2}$ should go with the v^2 in the equation of energy, so that the objectionable expression "*vis viva*" may finally be stamped out from all dynamical treatises.

In dealing with rotation, in Chapter V., the author would do well to study Maxwell's geometrical representation of the direction by means of the screw, right-handed or left-handed; and to discard all attempts by comparison with a clock-wise or counter-clock-wise rotation, requiring as these do a specification of the aspect of the plane of motion.

Pure homogeneous strain is analyzed in Chapter VII. as far as is possible by simple geometrical methods; such a strain may be produced by the superposition of three linear strains in directions at right angles to one another. In a linear strain the increment of distance of two points in the line of the strain is properly their *elongation*; while the ratio of the elongation to the original distance is called the *extension*, not the *elongation*, as on p. 167.

In Part II., "Dynamics," we find in Chapter I. the discussion on the units of measurement of weight, mass, and force customary in mathematical treatises, and of the usual unsatisfactory nature. The author, disregarding the vernacular use of the word "weight," defines the weight of a body as the force with which it is attracted by the earth, but is at variance with his own definition in the statement of the majority of the subsequent examples, relapsing into the language of ordinary life. A collection of 500 different ways of spelling the name of the town of Birmingham has been made, and a similar collection could be made from the present treatise of different ways of expressing the simple ideas of the pound *weight* and the pound *force*, to use the ordinary language of practical men. The attraction of the earth on a pound is, in the vernacular, "the force of a pound," not the "weight of a pound," the latter implying what the mathematician likes to distinguish as the "mass of a pound." Thus a mathematical precisionist, to express the simple idea of a force of 10 pounds, to be consistent should call it "a force equal to the weight of the mass of 10 pound weights," the absurdity of which is evident.

Again, in straining after the equation $F = ma$, when using the gravitation unit of force, the mathematician in the F.P.S. (foot-pound-second) system of units is obliged to use the variable unit of mass of g pounds to measure the invariable quantity, the mass of the body; while what he calls the weight of the body, and denotes by w, measuring it in pounds, is, although variable with g, always measured by the same number.

Next we have the equation $w = mg$, the source of all the confusion in dynamical teaching, and only to avoid writing the dynamical equation with gravitation units in the form

$$F = w\frac{a}{g}.$$

This terminology culminates in the solecisms that on p. 477 we must suppose pressure to be measured in poundals on the square foot in hydrostatical problems; and that if the equation $w = mg$ is supposed to be used with absolute units, that the weight of a body is measured in poundals; as if a mathematician asked in a shop for "half a poundal of tea, or tobacco." Ordinary people measure weight in pounds, so that if mass is also measured in pounds, then $w = m$.

It is time now, as Prof. Minchin has pointed out, that "the astronomical unit of mass," defined in § 315, should disappear, and that in all problems of physical astronomy the gravitation constant k should be retained, while m, the mass, is measured in terms of the ordinary units.

Although the author does not allow himself the use of the methods and notation of the Calculus, still he has managed to discuss a number of interesting problems in the dynamics of a rigid body, usually proved by the methods of Analytical Mechanics.

Working under these restrictions, he has given elegant elementary proofs of the chief properties of the common catenary; but here, again, it is time that the equation

should be presented in the form $y/a = \cosh x/a$, using the notation of the hyperbolic functions; which might also be employed with advantage in the statement of the results of the examples on p. 302. Chains of 5000 feet span, and 400 feet versed sine, are in existence, providing striking numerical examples in this part of the subject.

Most of the examples are carefully chosen, but the author by diligent search could easily add more interest to the collection, particularly to the examples on parabolic trajectories, and problems concerning the motion of railway-trains. Ex. 85, p. 499, certainly requires careful revision. The diagrams of the simple machines are of the usual academic nature; the author should consult Prof. Kennedy's "Mechanics of Machinery" for better illustrations, especially of the differential pulley, and of pulley tackle in general. If the differential screw is given (p. 435), why not also the integral screw, which is to be met with more commonly in real life—for instance, in railway couplings, and in the rigging of ships.

Except for the parts criticized above, on the units of weight, mass, and force, the present treatise shows that the author has read with profit and discrimination the most recent treatises on dynamics; he has produced a very useful work, suitable for instruction in technical colleges, and likely also to prove a necessary corrective to the very abstract treatment of the subject of mechanics too common in the character of University instruction.

A. G. GREENHILL.

ATLAS OF THE DISTRIBUTION OF PLANTS.

Atlas der Pflanzenverbreitung. (Berghaus's "Physikalischer Atlas," Abtheilung V.) Bearbeitet von Dr. Oscar Drude. (Gotha: Justus Perthes, 1887.)

THE history of the science of the distribution of plants begins with Linnæus, who was the first to cite systematically the countries and situations in which the plants he described grew. This we find carefully done in the first edition of the "Species Plantarum," published in 1753. No perceptible advance beyond this was made before the appearance of Humboldt and Bonpland's "Essai sur la Géographie des Plantes" in 1805, which work may be designated the real foundation of the science. It was followed in 1823-24 by the Dane, Schouw's "Grundtræk" and "Plantegeographisk Atlas," the latter containing twenty-two maps illustrating the vegetation of the world, and especially the distribution of plants cultivated for food. There is also a German edition of both the "Outlines" and the "Atlas." From this date onward many of the most eminent botanists investigated distribution in connection with classification of plants, notably R. Brown, A. P. De Candolle, H. C. Watson, C. Darwin, A. De Candolle, J. D. Hooker, Edward Forbes, Von Martius, and Grisebach, to say nothing of the younger botanists. But the results of their labours ars still scattered, or at least only partially elaborated; for Grisebach, in his "Vegetation der Erde," deals with the facts from a peculiarly narrow stand-point.

It is true that both Drude and Engler ("Versuch einer Entwicklungsgeschichte der Florengebiete") have attempted something beyond this, but neither, we suspect, regards his work as more than a preliminary effort. The primary geographical divisions of these two writers are essentially the same, though their nomenclature differs; but, considering the complexity of the subject, probably no two persons would agree exactly on these points; yet it is highly desirable that there should be something approaching uniformity in the names of the divisions. Grisebach designates his primary divisions "Gebiete," and Drude his "Reiche"; whilst Engler's four primary divisions are designated "Reiche," and his secondary ones "Gebiete." Let us now briefly examine the main features of Drude's Atlas. Following the most authoritative English writers on zoological and botanical geography, we will call the primary divisions regions, and the secondary divisions sub-regions.

Drude divides the world into fourteen floral regions, and each of these into a number of sub-regions, indicating by lines and dots the overlapping of the elements of contiguous sub-regions. The regions are: (1) Northern, (2) Central Asia, (3) Mediterranean, (4) East Asia, (5) Middle North America, (6) Tropical Africa, (7) East African Islands, (8) Indian, (9) Tropical America, (10) Cape, (11) Australia, (12) New Zealand, (13) Andes, (14) Antarctic.

While agreeing in the main with the foregoing divisions, we cannot but regard some of them as including too much or too little, according to the number of primary divisions adopted. We recognize the difficulties of the task, and admit that it is practically impossible to divide the vegetation of the world into regions of equal value and importance, even leaving out of consideration the mountain flora within the tropics. Instead, however, of giving Madagascar and the neighbouring islands the rank of an independent region, we should treat it as a sub-region of the tropical African flora. On the other hand, the Indian region seems too comprehensive, as it includes the whole of tropical India, Malaya, Cochin-China, the Malayan Archipelago, New Guinea, North Australia, and Polynesia, even to the Sandwich Islands. The very extensive recent collections of Madagascar plants, made by various English and French travellers, prove that the flora is really a sub-region of the tropical African flora. With regard to the flora of Polynesia, it is true that the littoral element consists almost exclusively of species common to the Malayan Archipelago and North Australia, many having an even wider range; but the Australian and American affinities of the endemic element are certainly too pronounced, in our opinion, to treat this flora as a sub-region of the Indian; and the Sandwich Island flora is as highly specialized, to say the least, as that of New Zealand. Perhaps it would be more convenient to make it an independent region. Again, the purely Australian types surely predominate largely over the Asiatic in North Australia, especially if we eliminate the widely-dispersed coast plants. Dr. Drude's New Zealand region includes the surrounding islands, except the more southern Macquarie; yet, of the eighteen vascular plants recorded from this island, sixteen are common to the New Zealand group. The Auckland and Campbell groups should be reckoned in the Antarctic region rather than New Zealand; and St. Paul and Amsterdam Islands, as well as the Tristan d'Acunha group, do not belong to the same category. Further, the higher mountain flora of Central America and South Mexico has certainly a greater claim to be included in the Andine region than has that of the Galapagos, though Dr. Drude separates them.

We have called attention to these defects or incon-

sistencies in the limitation of the regions, because we believe that the latest and fullest data relating to the regions in question clearly indicate that it is not a matter of opinion.

The Atlas as a whole is a most laborious and careful compilation, and we do not doubt that it will meet with the favour it deserves. The second sheet of maps illustrates the areas of certain important natural orders and genera; the third, the horizontal zones of vegetation of the world; the fourth, the flora of Europe; the fifth, the floras of Europe and Asia; the sixth, the floras of Africa and Australia; the seventh, the floras of America; and the last represents the areas of plants cultivated for their economic products.

It would be easy enough to find fault with some of the details of the limitation of Dr. Drude's sub-regions, those of tropical Africa and Eastern Asia for example, though it would not always be so easy to suggest more satisfactory ones; but we prefer judging the work by its merits rather than by its real or assumed defects.

This Atlas, it should be added, is a cartographical development of Dr. Drude's "Florenreiche der Erde," which appeared in 1884, and formed the Ergänzungsheft 74 to *Petermann's Geographische Mittheilungen;* that is to say, it is a development so far as the maps are concerned, but the explanatory letterpress has been reduced to four pages folio. The maps, sixteen in number, are admirably executed, and exceedingly elaborate; indeed, the only fault we find in them is an excess of detail, with perhaps too little explanatory text for beginners.

As the author very truly observes, the material available for such a work is now almost inexhaustible, and the task of selecting from it for the purposes in view was no easy one. He brings into contrast the position of botanical geography in 1848, when the first edition of Berghaus's "Physical Atlas" was published, and there was nothing approaching a complete flora of any of the larger areas outside of Europe in existence. Even in 1855, when De Candolle gave to the world his now classical work, "Géographie Botanique Raisonnée," he could only deal with fragments of floras. Now, though it may safely be asserted that future discoveries can in no way affect the main theories of distribution based upon what is already known, very much remains to be done in fossil botany before we shall be able to trace in detail the early migrations of plants. Therefore the only thing that can be successfully accomplished yet is to work out more completely the present distribution of plants, which is practically all that Wallace has done for animals. But he deals specially with the quality and probable origin of the zoology of his regions; and it is just this aspect of botanical geography that awaits further development.

OUR BOOK SHELF.

Tenants of an Old Farm. By Henry C. McCook, D.D. (London; Hodder and Stoughton, 1888.)

THE object of this book is to present "a series of exact truths from natural history in a popular form." The author's original intention was to write a number of essays upon insect life, and particularly upon the life of ants and spiders, which he has especially studied. Friends, however, persuaded him to give the essays a colloquial form, so that they might appeal to as wide a circle of readers as possible. We are not sure that the change was in all respects an improvement, for, as Dr. McCook says, "the truths of Nature are attractive enough in themselves, and need not the seasoning of fiction." The book is very popular in the United States, and there can be little doubt that it will also be appreciated on this side of the Atlantic. The author is a keen and accurate observer of Nature, and his enthusiasm for his subject is so steadily maintained that it cannot but exert some influence on the minds of young students. For the present edition a brief introduction has been written by Sir John Lubbock, who bears cordial testimony to the fidelity and skill with which Dr. McCook has carried on his researches. The work is remarkably well illustrated.

Digging, Squatting, and Pioneering Life in the Northern Territory of South Australia. By Mrs. Dominic D. Daly. (London: Sampson Low, 1887).

THIS is an interesting account of a part of South Australia which is sure to become more and more important. The writer spent three years—from 1870 to 1873—in the Northern Territory, and by far the best chapters are those in which she records her own experiences. The history of the district during the last fourteen years has, however, been carefully compiled from the most trustworthy sources. She has, of course, a good deal to say about the natives, her accounts of whom are freshly and brightly written. Mrs. Daly is of opinion that, so far as the treatment of the aborigines is concerned, only one rule holds good—"firmness accompanied by kindness, fair play, and an honest payment for work done." If they make themselves disagreeable, they must be kept "in their proper place," "for," she says, "when a native shows signs of sulkiness and defiance, it is perfectly certain some mischief is brewing."

Photography Simplified. (London: Mawson and Swan, 1887.)

THIS is a third edition, considerably revised and enlarged, of an elementary and practical treatise, intended chiefly for amateurs and those about to become acquainted with the subject. The earlier chapters deal with the purchasing of apparatus, followed by the various processes of taking the negative, developing, printing, &c., and are written in a very plain and intelligent way. The book concludes with an appendix containing additional useful formulæ, together with a set of labels for the photographic laboratory.

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.*]

An Explanation explained.

I AM glad to find that Dr. Guppy has at last enabled us to get to the bottom—I cannot say to the foundation—of the story which was related by the Duke of Argyll on November 17 in last year, to the discredit of Prof. Bonney and the authorities of the Geological Society. It is now admitted that the paper, said to have been "offered to," and "refused by," the Society, never came before the President and Council in any form whatever; and that in fact the paper was not only never presented, but was never even written!

Dr. Guppy's references to myself are capable of the simplest explanation. During the whole time that he was absent in the Solomon Islands, I was in the habit of receiving specimens

and letters from him; and, as he has acknowledged in his book, I carefully studied these specimens and gave him all the advice and assistance in my power in carrying on his geological investigations. Upon his return we had several conversations, always of the most friendly character, concerning the best mode of embodying his observations for communication to different Societies; and until the present time I had not the smallest idea that he was in any way dissatisfied with anything I had ever said or done in connection with the subject.

With respect to a particular conversation referred to by Dr. Guppy as having occurred in the spring of 1885, I have no recollection whatever; but I unreservedly accept his statement as to the *facts* of the case, and only demur to his *interpretation* of them. If Dr. Guppy or anyone else asked my opinion as to the fitness for the Geological Society of "a paper in which Mr. Darwin's theory of coral reefs would be brought under consideration," I should undoubtedly point out to him that the Geological Society had always been averse to publishing papers dealing with such broadly theoretical questions as the origin of coral reefs, and I should advise some other means of publication as more appropriate.

That the Geological Society is not partial in its reluctance to publish papers of a theoretical character will be seen from the fact that although Mr. Darwin in 1837 read a paper to the Society, embodying the chief points of his theory of coral reefs, yet the Society never published the paper in their Transactions. At the time that this occurred Darwin was a member of the Council, and a few months later he became Secretary of the Society.

It is surely unnecessary for me to remark that in any advice which I gave to Dr. Guppy, I was acting simply on my own judgment and individual responsibility. Dr. Guppy was a Fellow of the Society at the time, and had precisely the same right to present papers to it which I had myself. Dr. Guppy chose to ask my advice; I gave it to him as to a friend, and he was perfectly free to act upon it or to reject it as he thought fit. I may add that the Secretary of the Geological Society has nothing to do with the acceptance or rejection of papers, except as a member of the Council, and then only when the question has been brought up by the President for the approval or condemnation of his own action.

How my unofficial act of friendly advice concerning the destination of an unwritten paper came to be represented as the refusal of a paper offered to the Society I am at a loss to conceive! Why the Duke of Argyll, having received the statement which is now before us, should proceed to formulate the very grave accusation against Prof. Bonney personally, and the authorities of the Geological Society, it is for his Grace to explain.

With respect to Dr. Guppy's complaint that his memoirs have been "studiously ignored" during the recent controversy, I cannot help thinking that he has been unduly sensitive. Writing to Prof. Huxley in October last year, and pointing out that the Duke of Argyll was mistaken in supposing there had been no discussion on Mr. Murray's theory, I said "it would be an endless task to attempt to give references to the various scientific journals which have discussed the subject," but in penning these words I had not the smallest idea of speaking slightingly of any of the memoirs which want of space prevented me from citing, and least of all concerning those which contain the facts and observations of Dr. Guppy, of the value and importance of which I had such good opportunities of judging.

JOHN W. JUDD.

Reason and Language.

PROF. MAX MÜLLER has been so kind as to favour the readers of NATURE with his views on language and reason, concisely expressed in a letter to an American friend. As one grateful reader, I much desire both to express my thanks, and also to beg for yet a little further information with respect to matters of such extreme interest.

The Professor says: "Because we reason—that is, because we reckon, because we add and subtract—therefore we say that we have reason." Now, in the first place, I should be glad to be told why "reason" is to be regarded as identical with such "reckoning"? I have been taught to distinguish two forms of intellectual activity: (1) acts of intuition, by which we directly apprehend certain truths, such as *e.g.* our own activity, or that A is A; and (2) Acts of inference, by which we indirectly apprehend others, with the aid of the idea "therefore"—evolving into explicit recognition a truth previously implicit and latent in premisses. The processes of addition and subtraction alone, seem to me to constitute a very incomplete representation of our mental processes.

The Professor also identifies language and reason, denying to either a separate existence. As to "reason" he says: "We have only to look into the workshop of language in order to see that there is nothing substantial corresponding to this substantive, and that neither the heart nor the brain, neither the breath nor the spirit, of man discloses its original whereabouts." The expression "whereabouts" would seem to attribute to those who assert the existence of "reason," the idea that it possesses the attribute of extension! In order to understand clearly the passage quoted, we should learn what Prof. Max Müller really means by the term "spirit," which here figures as one species of a genus also comprising the breath, the brain, and the heart. Reason, however, is not represented as being simply language "as we now hear it and use it," but "as it has been slowly elaborated by man through all the ages of his existence upon earth." Thus understood, the Professor "cannot doubt" "the identity of reason and language." Nevertheless, he immediately proceeds to point out a striking want of identity between them. He says, quite truly, "We have two words, and therefore it requires with us a strong effort to perceive that behind these two words there is but one essence,"—namely, that denoted by the Greek word *logos*—"the undivided essence of language and thought." Now, the intimate connection of language (whether of speech or gesture) with thought is unquestionable; but intimate connection is not "identity." If thought and language are "*identical*," how came two words not to have two meanings, or two thoughts to be expressed by one word? The plain fact that we have different words with one meaning, and different meanings with one word, seems to demonstrate that thought and language cannot be "identical."

"No reason without language—no language without reason" is a statement true in a certain sense, but a statement which cannot be affirmed absolutely. Language (meaning by that term only *intellectual* expression by voice or gesture) cannot manifestly exist without reason; but no person who thinks it even *possible* that an intelligence may exist of which ours is but a feeble copy, can venture dogmatically to affirm that there *is* no reason without language, unless he means by reason, mere "reasoning," which is evidently the makeshift of an inferior order of intellect unable to attain certain truths save by the roundabout process of inference.

But I demur to the assertion that truly intellectual processes cannot take place in us apart from language. In such matters our ultimate appeal must be to our own reflective consciousness. Mine plainly tells me that I have every now and then apprehensions which flash into my mind far too rapidly to clothe themselves even in mental words, which latter require to be sought in order to express such apprehensions. I also find myself sometimes expressing a voluminous perception by a sudden gesture far too rapid even for thought-words, and I believe that other persons do the same. A slight movement of a finger, or the incipient closure of an eyelid, may give expression to a meaning which could only be thought in words by a much slower process.

It is the more remarkable that Prof. Max Müller should deny the existence of reason, since he unequivocally affirms, in rather lofty language, the existence of truth. Yet surely the existence of truth, in and by itself, is inconceivable. What can truth be, save a conformity of thoughts and things? I affirm, indeed, the certain existence of truth, but I also affirm that of reason, as existing anteriorly to language—whether of voice or gesture. What is the teaching of experience? Do men invent new concepts to suit previously coined words, or new words to give expression to freshly thought-out concepts? The often-referred-to jabber of Hottentots is not in point. No sounds or gestures which do not express concepts would be admitted by either Prof. Max Müller or myself to be "language."

The Professor speaks of the "alarmingly small" number of primitive concepts; but who is to be thereby alarmed? Not men who occupy a similar stand-point to mine. I fully agree with Prof. Max Müller in saying, "After the genesis of the first concept, everything else becomes intelligible."

We come now to the supreme question of the origin of languages. As to this, the Professor observes: "No one who has not himself grappled with that problem can appreciate the complete change

that has come over it by the recognition of the fact that roots are the phonetic expressions of the consciousness of our own acts. Nothing but this, our consciousness of our own repeated acts, could possibly have given us our first concepts. Nothing else answers the necessary requirements of a concept, that it should be the consciousness of something manifold, yet necessarily realized as one. . . The results of our acts become the first objects of our conceptual thought." The truth of these statements I venture to question. After noting the dogmatic nature of the assertion "Nothing but this *could*, &c.," I must object to the statement of fact as regards human beings now. I do not believe that the infant's first object of thought is "the results of its own acts." In the first place, no object of our early thoughts is merely the "results of our own acts," but a combined result of our own activity and of the action on us of our environment. Secondly, my observations lead me to believe that the infant's first thoughts relate to things external, and certainly not to the results of its own activity as such, which is a highly complex and developed thought. It may be that the Professor, when he says "The results of our acts *become* the first object of our conceptual thoughts," means that such acts in remote antiquity *became* the object of man's first thought. This is probably the case, since, with respect to the origin of thought and language, Prof. Max Müller has adopted Noiré's crude notion that they sprang from sounds emitted by men at work, conscious of what they were doing, in the presence of others who beheld their actions and heard the sounds; the result being the formation of a conceptual word, to attain which five stages had to be gone through, as follows :—

"(1) Consciousness of our own repeated acts.
"(2) *Clamor concomitans* of these acts.
"(3) Consciousness of our *clamor* as concomitant to the act.
"(4) Repetition of that *clamor* to recall the act.
"(5) *Clamor* (root) defined by prefixes, suffixes, &c., to recall the act as localized in its results, its instruments, its agents, &c."

But, if language and reason are identical, reason could not exist before a single conceptual word existed. Nevertheless, to attain to this first single word, we see, from the above quotation, that man must have had the notion of his own acts as such; the notion of their repetition; the notions of clamor, action, and the simultaneity of clamor and action; the will to recall the act (yet *nihil volitum quia præcognitum*); and finally the notions of consequence, instrumentality, agency, or whatever further notions the Professor may intend by his "&c."

Thus he who first developed language must be admitted to have already had a mind well stored with intellectual notions! But can it for one instant be seriously maintained, close as is the connection of language with reason, that their genesis (miracle apart, of which there is no question) was *absolutely* simultaneous? He must be a bold, not to say a rash, man who would dogmatically affirm this. But if they were not *absolutely* simultaneous, one must have existed, for however brief a space, before the other. That intellectual language could have existed without reason is absurd. Reason, then, must, for however short a period, have preceded language.

In conclusion, I desire to point out a certain misrepresentation with respect to natural selection. The Professor says: "In the evolution of the mind, as well as in that of Nature, natural selection is rational selection; or, in reality, the triumph of reason, the triumph of what is reasonable and right; or, as people now say, of what is fittest." But, we may ask in passing, if reason has no existence, how can it "triumph"? The misrepresentation of natural selection, however, lies in his use of the word "fittest." When biologists say that the "fittest" survives, they do not mean to say that that survives which is the most "reasonable and right," but that that survives which *is able to survive*. What there is less "reasonable and right" in a Rhytina than in a Dugong, or in a Dinornis than an Apteryx, would, I think, puzzle most of our zoologists to determine; nor is it easy to see a triumph of reason, in the extermination of the unique flora of St. Helena by the introduction of goats and rabbits.

ST. GEORGE MIVART.

Mechanical Equivalent of Heat.

I FIND that the mode of regarding J advocated in my letter in last week's NATURE (p. 320) is not quite new, for my brother, Dr. Oliver Lodge, writes to tell me that Clerk-Maxwell, on p. 298 of his "Theory of Heat," has called J the specific heat of water. However, he has not done so throughout the book, and I do not think it is the meaning generally attached to the symbol, though it seems to me that it should be so; that is to say, J should always be considered as denoting the specific heat of water at the temperature 0° C.

ALFRED LODGE.

Coopers Hill, Staines, February 6.

"Is Hail so formed?"

I CANNOT accept Dr. Rae's explanation as a "simpler solution" of the phenomenon described by me in NATURE of January 26 (p. 295), because it is based upon meteorological conditions that were at the time non-existent.

My own observation of the pine-tree convinced me that at or near the summit there was no adherent ice or rime; and had there been beads of ice upon the leaves I should still have failed to see what should have caused them while frozen to become detached and change from beads to pellets.

There was a fine mist during the whole of the day, and I observed the phenomenon at 3.30 p.m.

A letter appeared in NATURE upon the same day as mine, drawing attention to the unusual atmospheric conditions observed about that time, and containing facts which manifestly support my theory.

CECIL CARUS-WILSON.

Bournemouth, February 11.

The New Army Regulations.

THE new regulations for the Woolwich entrance examination have been very unfavourably received by men of science. This hostile criticism is in some respects the consequence of the absence of clear discrimination between them and those already in force for the Sandhurst examination.

It must be remembered that candidates for Woolwich cadetships must be between the ages of 16 and 18; that 6000 marks are awarded for mathematics, with 1500 more for drawing and English composition; and that in both the last June and December competitions less than 4000 marks sufficed to place a student among the successful competitors. Since candidates can pass in these subjects alone, it appears unreasonable to complain that youths of scientific power are excluded from the Royal Military Academy. Classics are sufficiently discouraged by the fact that they have no mark value after the cadet has entered the Academy. The 5000 marks offered in the entrance examination for Latin and Greek merely serve to encourage candidates who have been educated on the classical sides, which are almost always the stronger at our public schools. They really tend to widen rather than to narrow the sources from which candidates are drawn.

After a quarter of a century of continuous experience as a student and teacher of elementary science, I find myself reluctantly forced to the conclusion that chemistry, physics, and geology are not good educational subjects for lads under 16 years of age. I believe that it is in most cases desirable that youths intended for a scientific career should not specialize too early. A sound foundation of mathematics and modern languages is almost necessary to enable them to attack their scientific subjects efficiently. With minds trained to the use of the exact and powerful processes of mathematical reasoning, and able to readily appreciate and avail themselves of the wealth of scientific literature in France and Germany, they will probably become more useful officers than if they had acquired a smattering of science.

On the other hand, your wise censure of the discouragment of science in the Sandhurst regulations must commend itself to all thoughtful men. The case is even stronger than at first sight appears in the studious moderation of your judicious article. The limits of age are higher for Sandhurst, being 20, or in some cases 24. The training of the Line cadets is less complete. As they only spend one year at Sandhurst, they are obliged to confine their attention more strictly to professional subjects. Officers of the Line have often more leisure than those in the scientific corps, and there are many reasons why even a slight acquaintance with science would be helpful to them. It also seems hard that a candidate should be handicapped by not taking up Latin. Sometimes it has been discontinued for a considerable period, and a candidate can ill-afford to take up "a 2000 subject," considering the severity of the competition.

I would wish respectfully to suggest that a memorial should be presented to the War Office by all interested in the teaching of science, praying that, if a candidate for an army examination

wishes to substitute for Latin one of the sciences enumerated in Group II., it should be allowed a maximum of 3000 marks.

2 Powis Square, W. HENRY PALIN GURNEY.

"British and Irish Salmonidæ."

As your reviewer allows that he "intentionally omitted" five words from a sentence of mine which he quoted in order to criticise, I may well leave comments on such a proceeding to your readers. I willingly acquit him of having purposely made me to suggest utter nonsense, as I cannot help thinking that his knowledge of fish-culture was such that he was unaware he was doing so.

As to the second point he says, "I doubted and still doubt if there is any method practised in which layers of moss are used and are separated from the eggs by muslin and similar material." As he rejects the Howietoun account which I gave, I now submit extracts from two standard works, one American, the other English, which will, I believe, be conclusive to those who are ignorant of fish-culture, for every fish-culturist is aware that this plan is commonly adopted. Livingstone-Stone ("Domesticated Trout," ed. 3, 1877) remarked:—"Theodore Lyman recommends placing each layer of eggs in a fold of mosquito netting to keep them from mixing with the moss and so facilitate the unpacking of them. *This is a great improvement. By all means use mosquito netting*" (p. 149). Mr. Andrews, of Guildford, wrote thus in the Badminton Series ("Salmon and Trout," 1885):—"The plan of packing does not vary much with trout breeders. The eggs are placed in alternate layers between moss, and protected by a covering of mosquito netting, muslin, swans' down, calico, or butter cloth, so arranged that the eggs shall not be crushed or escape" (p. 447).

As regards the third point, your reviewer now appears to be convinced that *Salmo namaycush* is a char, as I stated it to be. It must be a matter of regret that he omitted to investigate the foregoing questions prior to authoritatively writing upon them in such a well-known publication as NATURE.

Cheltenham, February 4. FRANCIS DAY.

IN his last letter Mr. Day has certainly proved the correctness of the statement in his book that salmonoid eggs are packed with layers of moss from which they are separated by muslin or other textile fabric. If I had known as much about salmon-culture as he, I certainly should not have questioned the statement; it is to be noted that I only questioned and did not deny. If I had been as completely versed in the knowledge of Salmonidæ as Mr. Day, I should have written a book on the subject instead of reviewing his. But the essential point, which Mr. Day seems incapable of appreciating, is this: that there was nothing in the notes on the subject of packing in his book which confirmed the statement in the text; and although my doubts as to the correctness of that statement are removed by his letter, they were perfectly justifiable in a reader of his book. Mr. Day does not apparently suspect that people interested in the subject, including the reviewer, read his book for the sake of gaining information, and not because they already know as much about the subject as himself. All I had to do was to give my impressions of the book as I found it: the fitness of my criticisms is only the more established by the lengthening appendix to his book which Mr. Day is now publishing in your correspondence columns. YOUR REVIEWER.

MODERN VIEWS OF ELECTRICITY.[1]

PART III. MAGNETISM—(*continued.*)

VIII.

IT will now be perceived that a fly-wheel in rotation is the mechanical analogue of magnetism, or more definitely of a section of a line (or tube) of magnetic force; and that a brake applied to such a fly-wheel, with consequent slip, dissipation of energy, and production of heat, is in some sort a mechanical analogue of an electric current.

The field is regarded as full of geared elastic vortices or whirls, some of which are cogged together, so to speak, while others are merely pressed together by smooth rims.

[1] Continued from p. 348.

It is among these latter that slip is possible, and in the regions occupied by them that currents exist; the energy dissipated here being transmitted through the non-slippery or dielectric regions from the source of power, just as energy is transmitted from a steam-engine through mill-work or shafting to the various places where it is dissipated by friction.

Mechanical Force acting on a Conductor conveying a Current.

In Fig. 41 the conducting portion is shown with opposite rotations on either side of it. Now superpose a uniform rotation all in one direction upon this, so as to increase the spin on one side and diminish it on the other. Immediately the extra centrifugal force on one side will urge any movable part of the conductor from the stronger to the weaker portion of the field.

The field for a direct and return circuit may be similarly drawn by superposition of their separate whirls (see Fig. 40); and so it becomes evident why a circuit tends to expand so as to inclose the largest possible area, even if no other magnetic field than its own be acting on it.

Also if two circuits are arranged near each other in a plane, with their currents in opposite directions, they will more or less neutralize each other's effect on the space between them, causing (if equal) a region of no spin there. Their neighbouring portions will thus get urged together by the unbalanced pressure on the other side: or, currents in the same direction attract.

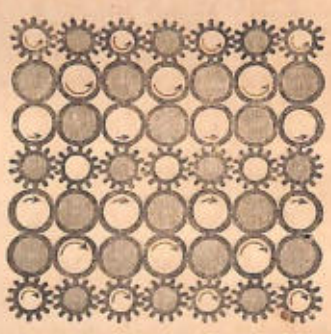


FIG. 44.—Two parallel conductors conveying equal currents in one direction and getting pushed together by the centrifugal force of the outside whirls, no whirl existing between them. The length of the arrows again suggests the distribution of magnetism in the conductors. Fig. 40 showed the correlative repulsion of opposite currents.

As for the effect of iron introduced into a circuit, it brings into the region of space it occupies some two or three hundred times as many lines of whirl as were there before, and these naturally contribute mightily to the effects, both those exhibiting mechanical force and those exhibiting inertia.

When one says, as roughly one may do, that iron brings 300 fresh lines into the field, one means that for every whirl otherwise excited, 300 more are faced round in the iron. And this process goes on while the field is increasing in strength until the total number of whirls in the iron begins to be called upon; when this point is reached the rate of addition is not maintained, and the iron is said to show signs of saturation. Ultimately, if ever all its whirls were faced round, the iron would be quite saturated; but long before this point is reached another cause is likely to make itself felt, viz. the falling off in the strength of the whirls already faced round, by the action of the strong magnetic induction, which is all the time acting so as to weaken the iron currents so far as it is able. And thus at a certain point hitherto unreached by experiment the iron may not only fail to increase the strength of the field any more, but may actually begin to diminish it.

The easiest way to picture the effect of iron is to think of its wheels as some two or three hundred times as massive as those of air, so that their energy and momentum are very great.

That which is commonly called magnetic permeability may in fact be thought of as a kind of inertia, an inertia per unit volume; though how it comes to pass that the ether inside iron is endowed with so great inertia one cannot say. Perhaps it is that the iron atoms themselves revolve with the electricity, perhaps it is something quite different. Whatever the peculiar behaviour of iron, nickel, &c., be due to, it must be something profoundly interesting and important as soon as our knowledge of their molecular structure enables us to perceive its nature.

Induction in Conductors not originally carrying Currents but moving in a Magnetic Field.

To explain the currents induced in a conductor moving through a uniform magnetic field is not quite easy, because none of the diagrams lend themselves naturally and simply to the idea of circuits changing in form or size.

If we take a rigid circuit in a magnetic field, like Fig. 45, and revolve it out of its plane 180°, it is obvious that a current will be excited in it, for the process is essentially the same as if the conductor were kept still and the field reversed.

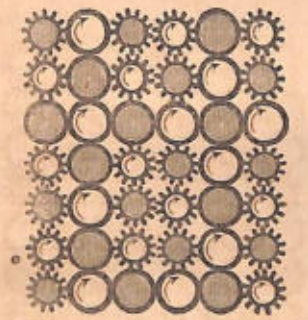


FIG. 45.—Section of a uniform magnetic field with two rails and a slider in it. If the slider be moved to or fro, the wheels inside get initially, compressed or extended, and thereby gain or lose energy respectively thus exciting the state of slip known as induced current.

But to understand the current excited in a closed circuit when a portion of it moves across the lines so as to embrace a greater number of them, one has to take into account the fact that the inside whirls are expanding and doing work in forcing the conductor away, while the outer whirls are resisting the motion, and being thereby compressed and rendered more energetic. Thus the wheels inside revolve slightly slower as the circuit expands, and those outside revolve slightly quicker. Both these processes cause a slipping of the gearing, first all round the inside and then all through the substance of the wire, whereby positive electricity moves forward in one direction round the circuit, the negative moving oppositely; and so a current is accounted for. It is not to be supposed, however, that any finite expansion of the wheels really occurs: the motion is rapidly equalized by diffusion through the wire, and fresh wheels come in round it from outside; hence directly after the conductor has stopped moving the field is again steady, but with many more wheels inside the contour than it possessed at first.

Representation of an Electrostatic Field again, and superposition of it on a perpendicular Magnetic Field.

An electrostatic strain is, we know, caused by a displacement of positive electricity one way along the lines of force, and by an equal displacement of negative the other way. Half the process was indicated crudely in Fig. 6; we may now represent it rather more fully with the help of our elastic cells by Fig. 46.



FIG. 46.—A portion of an electrostatic field between two oppositely charged bodies, with its lines of force going from right to left, and showing a tension along and a pressure at right angles to them, due to the elasticity of the cells (which elasticity may be due to their containing fluid in a state of whirl). Magnetic lines of force perpendicular to the paper are also shown in section. While this magnetic field was being excited and propagated from below upwards, a slight strain would be produced in the elastic cells, like but immensely less than that shown; as contrasted with its normal condition (Fig. 37). Conversely, while this electrostatic strain was being produced, the positive whirls would be infinitesimally quickened and the negative ones retarded during the displacement, thus producing a minute magnetic effect. If the medium is not magnetized, the whirls are not necessarily absent, only faced all ways.

Here the positive cells have been pulled one way, the negative the other way; and when the distorting force is removed, the medium tends to spring back to its normal condition, exerting an obvious tension on bodies attached to it in the direction of its lines of force, its elongated direction, and an obvious pressure in all perpendicular directions, its compressed directions.

Now, if all the cells are full of parallel whirls, as in the preceding magnetic diagrams, it is not improbable that this electrostatic distortion or "shear" of the medium may affect its magnetic properties slightly, and that, if the direction of electrostatic strain were rapidly reversed, a small magnetic oscillation would also ensue; but the exact details of these mutual actions are difficult to specify at present.

Disruptive Discharge.

Disruptive discharge may be thought of as a pulling of the shaded cells violently along past the others; the process being accompanied by a true disruption—a sort of electrolysis—of the medium, and a passage of the two electricities in opposite directions along the line of discharge.

Consider the locomotion of any one horizontal row of shaded cells in Fig. 46 during the occurrence of such a disruption of the medium. The cells slide on towards the right, and, as they slide, the spin of the negative cells above them is retarded while that of those below them is accelerated; consequently a true magnetic effect is produced, just like that accompanying a current, and a disruptive discharge has therefore all the magnetic properties of a current.

Effects of a Moving Charge.

This locomotion of a set of positive cells, or of negative cells the other way, as just considered, is very near akin to the motion of a charge through a dielectric medium.

A charge can only exist at the boundary between a dielectric and a conductor, or at least between one dielectric and another of greater density. So, when a charged body moves along with extreme rapidity, it can be thought of as exciting a rotation in the cells most closely in contact with it greater than that which it excites in the opposite kind of cells, and thus produces the whirl proper to a magnetic field. Thus does a moving charge behave just like a current of a certain strength.

It may be, indeed, that this is the customary way of exciting a voltaic current; for the chemical forces in a cell cause a locomotion of charged atoms, and thus set

up a field, which, spreading out in the way Prof. Poynting has sketched, reaches every part of the metallic circuit and excites the current there.

Electrostatic Effects of a Moving or Varying Magnetic Field.

Just as we have seen that a moving or varying electrostatic field may produce slight magnetic effects, so one can perceive that a moving or varying magnetic field brings about something of the nature of an electrostatic strain.

For a spreading out field is continually propagating the rotation on from one layer of wheels to the next. If there is any slip, we thus get induced currents, and the rate of propagation is comparatively slow, being a kind of diffusion; but even if there is not any slip, yet, unless the wheel-work is absolutely rigid, the rate of propagation will not be infinite. The actual rate of propagation is very great, which shows that the rigidity of the wheels is very high in proportion to their inertia, but it is not infinite; and accordingly the propagation of rotation is accompanied by a temporary strain. One part of the field is in full spin, another more distant part is as yet unreached by the spin; between the two we have the region of strain, the wheel-work being distorted a little while taking up the motion. Thus does a spreading out magnetic field cause a slight and temporary electrostatic strain, at right angles both to the direction of the lines of force and to the direction of their advance.

Generation of a Magnetic Field. Induction in Closed Circuits.

Picture to oneself an unmagnetized piece of iron: its whirls are all existent, but they are shut up into little closed circuits, and so produce no external effect. Magnetize it slightly, and some of the closed circuits open out and expand, with one portion of them in the air. Magnetize it strongly, and we have a whole set of them opened out into vortex cores, still with the whirl round them, and constituting the common magnetic lines of force. There is no need to think of iron and steel in this connection. In air or any substance the whirls are still present, though much fewer or feebler, and their axes ordinarily form little closed circuits—it may be inside the atoms themselves. But wrap a current-conveying wire round them, and at once they open out into the lines of force proper to a circular current.

Again, think of an iron ring, or a hank of wire as bought at an ironmonger's: wrap a copper wire several times



Fig. 47.—Closed magnetic circuit like Fig. 42, with a single-ring secondary circuit, and another open secondary loop; also with a short conducting-rod standing up in it.

round it, as a segment of a Gramme ring is wound (Fig. 47), and pass a current. The closed vortices in the iron at once expand: a portion of each flashes out and across the air-space inclosed by the ring (not by any means confining itself to a plane, of course), and enters the ring on the opposite side; so that directly the current is steady the lines all lie inside the iron again, but now inclosing an area—the area of the ring—instead of being shut up into infinitesimal links. In a sense the iron is still unmagnetized, for its lines of force still form closed contours within it, and none protrude any part of themselves into the air, except for irregularities. But in another sense it is highly and permanently magnetized round and round in itself, the magnetism being not easy to get out of it again, except by judiciously arranged reverse currents.

It is now like one great electric vortex ring instead of like a confused jumble of microscopic ones. Its section was shown in Fig. 42.

During the variable period, while the current is increasing in strength, or while it is being reversed, the region inclosed by the ring and all around it is full of myriads of expanding lines of force flashing across, broadside on, from one side of the iron to the other, and there stopping. It is the presence of these moving lines, changing rapidly from a "simply-connected" into a "multiply-connected" state, or *vice versâ*, which causes the powerful induced currents of "secondary generators."

In every case of varying magnetic field, in fact, we have lines moving broadside on, propagating their whirl, and more or less disturbing the medium through which they move.

Next consider a moving or spinning magnet. Its lines travel with it, and, being closed curves, they also must move broadside through the field, so that in this case we may expect just the same effect as can be obtained from a varying magnetic field.

If a broadside-moving line of force cut across a conductor, its motion is delayed, for its wheels slip and only gradually get up a whirl inside the ill-geared substance; thus, as we know, causing an induced current (see Fig. 43).

If a conducting ring is looped with the iron ring previously mentioned, as a snap-hook is looped with an eye, then every expanding vortex, while the ring is being magnetized, has necessarily to cut through the conducting ring once and no more, no matter what its shape or size. The electromotive force of induction is in this case therefore perfectly definite, and simply proportional to the number of turns made by the secondary round the core of the ring (Fig. 47).

Instead of supposing a closed conducting secondary circuit, imagine an open one: there is still an E.M.F. in it, though rather less than before because a few of the expanding lines flash through the gap and produce no effect, so the electricity must surge to and fro in the conductor as water surges up and down in a tilted trough, and a small condenser attached to the free ends will be alternately charged and discharged. The gap might become so large that nothing is left but a short rod (Fig. 47): in this also similar oscillations would occur.

But now suppose no secondary conductor at all; nothing but dielectric inclosed by the ring. In it there must be an electric displacement excited every time the magnetism of the ring is reversed. It may be an oscillatory displacement, but still on the whole in one direction during rise of magnetism, and in an opposite direction during reversal of magnetism. A charged body delicately suspended within the ring may feel the effect of the minute electrostatic strain so magnetically produced.

To see the mode in which an electrostatic displacement arises in the space embraced by the ring we have only to turn to Fig. 42, and look at the set of wheels along the line A B separating one half the section from the other. They cannot steadily rotate either way, for they are urged in opposite directions by the two halves; in other words, there is no magnetic field near such a ring, as is well known; but, nevertheless, during a change of magnetism, while the whirls inside are changing in speed, the rub on the dielectric necessary for checking the outer wheels of the conductor is either increased or diminished; and if the wheels have any elastic "give" in them, as we know they have, the electrostatic strain in the field is thereby altered during the varying stage of the magnetism.

OLIVER J. LODGE.

END OF PART III.

(To be continued.)

THE MECHANISM OF THE FLIGHT OF BIRDS.

THE following is a translation of an article in *La Nature* (December 3, 1887), on the mechanism of the flight of birds, by Prof. E. H. J. Marey. Through the courtesy of the editor of our French contemporary we are able to reproduce the figures illustrating M. Marey's interesting paper.

In a preceding article [see NATURE, vol. xxvi. p. 84], I showed that photography could represent the successive positions of a bird's wing, at different moments in its



FIG. 1.—Sea-gull. Transverse flight. Ten images per second.

flight; that there might be obtained at the same time the positions of the bird in space at equal and known intervals of time; and I expressed the hope of solving by this method the obscure problem of the mechanism of flight.

Since that time, the photographic method has been perfected, and the number of species of birds to which my researches have extended has been multiplied.

From the comparison of the several species which I have had at my disposal, the results show that, except in certain differences in details, they all execute movements

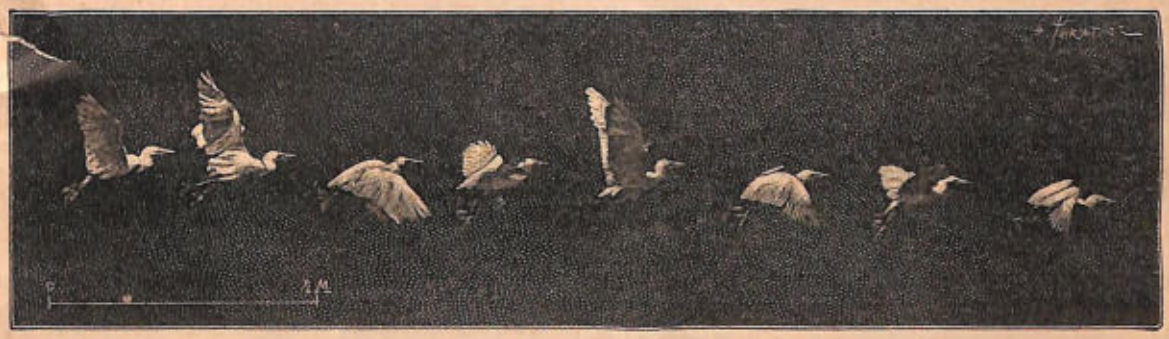


FIG. 2.—Small heron. Transverse flight. Ten images per second.

of the same nature; in all, the wings bend up at the moment of ascension, spread out quickly when at the wished for height, are then lowered, carried in front, and approached to the body; at the close of the descent, the joints anew bend up, and the ascent recommences.

The illustrations 1, 2, 3, 4, and 5 represent the flight of the sea-gull, the heron, the pigeon, and the pelican.

These illustrations reveal curious attitudes which the eye has not time to seize, and with which we are not familiarized in the artistic interpretations of birds. According



FIG. 3.—Pigeon. Transverse flight. Ten images per second. (Fac-simile of instantaneous photographs taken by the author.)

to a just remark of Mr. Muybridge, the European painters almost always represent birds flying with their wings elevated; the Chinese and Japanese, on the contrary, represent them indifferently with wings both raised and lowered. That does not, however, mean that the artists of the extreme East have faithfully reproduced the different attitudes of birds: the comparison of their representations with those of instantaneous photography shows clearly that no more in China than here does the eye perceive actions which last only for a very brief moment.

Seen only under one aspect, representations of a bird

on the wing do not give us correct ideas of the movements of the wings; we must photograph the bird under several aspects in order thoroughly to comprehend this mechanism. We have made several arrangements in order to procure this effect. One of these, placed at a height of 12 metres (nearly 13½ yards), gave representa-



FIG. 4.—Crested heron. Transverse flight. Ten images per second.



FIG. 5.—Pelican. Transverse descending flight. Ten images per second.



FIG. 6.—Sea-gull seen from above. Ten images per second. (Fac-simile of instantaneous photographs taken by the author.)

tions of the bird as seen from above (Fig. 6); others, variously placed, showed it from the side, or flying in the direction of the photographic apparatus (Fig. 7). These representations, taken under different conditions, complement each other. Thus, the birds seen from above show a singular curvature in the flat surface of the

wing, the existence of which one would not suspect from the profile representations. This curvature appears at the end of the depression of the wing, at the moment in which the joints begin to bend upwards in order to prepare for an ascent. Hence results a spiral aspect of the wing, recalling the form which Mr. Pettigrew considers the essential element in a bird's propulsion. But we must observe that this form is only produced at the very close of the act of descent, at the "*point mort*" of the wing's action, as we say in mechanics, and at a moment in which it, having become passive, is about to remount by the resistance of the air. These figures also show a fact wholly unforeseen—namely, that the movements in flying are not symmetrical. It had been previously supposed that the bird, when desirous of turning laterally the direction of its flight, executes movements more extended from the side which is to progress most rapidly; that is to say, that it gives more amplitude to the movements of the right wing if it wishes to turn to the left, and reciprocally. It is scarcely needful to say that photochronography condemns entirely the hypothesis in which it was supposed that one of the wings of the bird could bend more frequently than the other; the movements of the two wings are perfectly synchronous, if not equal, in extent. It is seen, in short, from these representations, that the body of the bird inclines and moves in different ways, so as to carry its centre of gravity to one side or the other, according to the necessities of the equilibrium. The bird whose attitudes are portrayed in Fig. 6 seemed careful to bear the weight of its body to the left on account of the smaller surface of its right wing, from which some feathers were missing.

The representations taken in front and a little obliquely, as in Fig. 7, give also useful information. They show that the extremity of the wing—a part of the organism in full activity, since it strikes the air with greater speed—presents, at the time of lowering, changes of surface which the secondary *rémiges* extending from the carpus to the shoulder do not offer. There exists in the wing feathers of the different orders a species of separation, showing that the carpal articulations are the seat of a light twisting movement favourable to the bending of the surface of the carpal *rémiges*. In these representations may also be readily seen the bending and convergence of the wings at the close of their lowering, the depression which the anterior side of the wing presents at this moment from the effect of a flexion beginning at the elbow. In order to follow in all their details the changes of movement in the

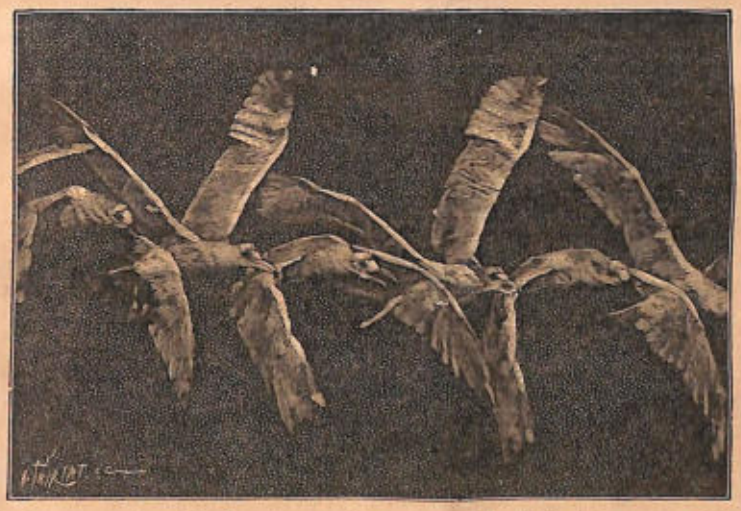


FIG. 7.—Sea-gull flying obliquely in the direction of the photochronographic apparatus. (Fac-simile of instantaneous photograph taken by the author.)

wings, it has been necessary to make many experiments, so as to obtain, during a single stroke of the wing, ten or twelve successive views of the bird seen under each of these different aspects.

These representations having once been obtained, I was in possession of all the elements necessary to understand completely the motions of the wings according to the three dimensions of space. But in order to represent them, figures in relief were necessary; and circumstances were favourable to this. At Naples, where I then was, the almost lost industry of casting bronze in wax has been preserved from the most remote antiquity. I modelled in wax a series of figures representing the successive attitudes in a single revolution of the wing, ten for the sea-gull, eleven for the pigeon: these models, when given to a skilful moulder, were reproduced in bronze with perfect fidelity.

Fig. 8 represents, disposed in a series, and following each other in their order of succession, at intervals of 1/88 of a second, the phases of one stroke of a pigeon's wing.

These bronze figures were made white, in order to render more apparent the effects of light and shade. Thanks to the multiplicity of the attitudes represented in this series, all the phases of the motion of the wings are easily followed: it is seen how they fold, rise, expand, and sink.

In order the better to understand how the movements of the bird's wing follow each other, of which photochronography gives an analysis, I have had recourse to the use of the zootrope, which recomposes them, and gives to the sight the impression of a bird flying.

The zootrope, represented in Fig. 9, offers this speciality, that it is formed by figures in relief. This is a great advantage from the point of view of the impression which it gives; in fact, these small figures of birds, arranged in a circle in the apparatus, present themselves to the observer under various aspects.

At the beginning of the movement the bird's backs are seen; then, in their circular course, they present their sides, pass across in full view, and at last return to the observer. Besides, the movements of the wings, which in nature are extremely rapid, and consequently imperfectly seen, are here much slower, so that the phases may be easily followed, and in an instant, more may be perceived than the most attentive observer of the flight of birds could discover by the most careful observation.

Fig. 9 shows the arrangement of the zootrope; it

cannot unfortunately give an idea of the effect produced by the apparatus in motion.

But it may be said that this rotatory method interprets the movements of the bird without indicating the forces which produces them. While it would be well to know that force, it is better still to measure the mechanical labour expended in order to sustain and transport itself in the air.

Let us see whether our photographic images reveal to us anything in regard to this.

When one knows the mass of a body, and the speed with which it moves, one can calculate the force

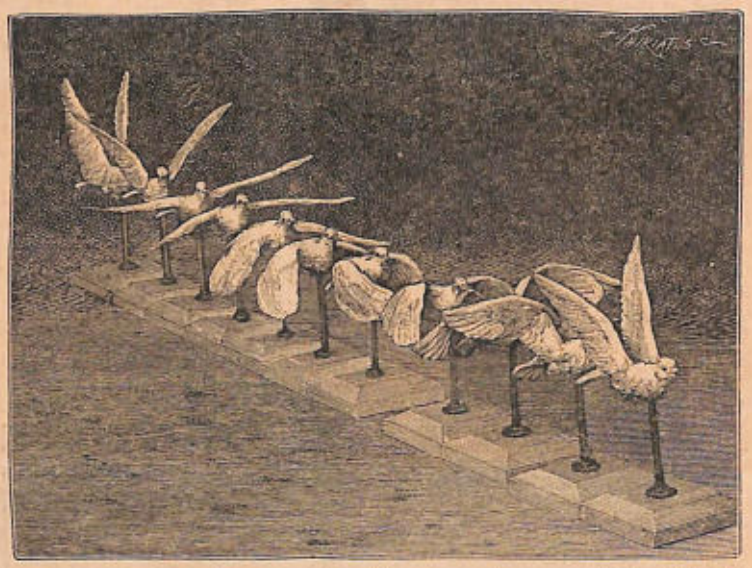


FIG. 8.—Bronze figures representing eleven successive positions at successive moments in the stroke of a pigeon's wing.

which has set this body in motion, and the labour expended by this force. If we take a projectile of a certain weight, and throw it before the photochronographic apparatus, and take a series of images of this projectile at intervals of 1/100 of a second, Fig. 10 shows the trajectory curve followed, and the space which separates the images from each other shows the space traversed by the projectile in each of the hundredth parts of a second during which its movement has lasted. From ten to ten a more brilliant image has been produced by an aperture in the diaphragm larger than the others: these marks are useful in order to facilitate the numbering of the images,

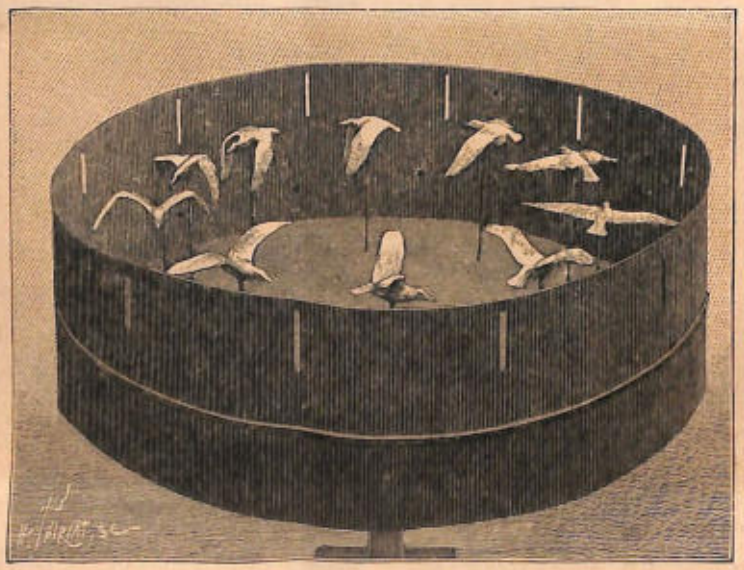


FIG. 9.—Zootrope, in which are placed ten figures, in relief, of a sea-gull in the successive positions of flight.

a fixed metrical scale, photographed at the same time as the object in motion, serves to measure the spaces traversed at each moment; then it is a problem in dynamics, whose solution may be readily obtained by the usual methods of calculation.

The successive images of the flying bird lend themselves to the same dynamical analysis. The balance indicates to us the weight of the bird; we know its size; and in order that photochronography may give us to perfection the trajectory of this mass, it only requires manifold multiplication of the images obtained (a hundred may be taken in a second if need be). But those images

will be partially confused, because the bird, in the hundredth part of a second, only traverses a space equal to the length of its body: the image of the second will therefore partly cover that of the first, the third that of the second, and so on. In this confusion one can scarcely distinguish the moment in which the wing lowers itself, or that in which it is raised. But this is of no importance: we fix on the head of the bird a small but very brilliant metallic point, and the image of this point, clearly seen in the series of figures, reveals the trajectory of the bird, together with its speed, and the accelerations and slackening of speed produced by the movements of the wings. One may then face the dynamic problem of flight. It is granted first that the bird does not oscillate sensibly in the vertical sense, whence one must conclude that the resistance of the air under its wings is precisely equal to its weight. On the other hand, it is to be observed that the motion of the animal presents alternations of speed and slowness, showing that the propelling force and the resistance of the air predominate

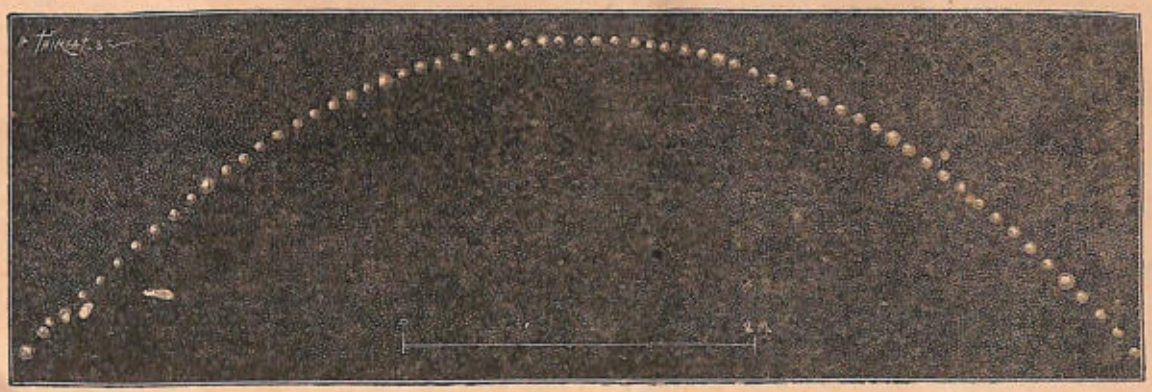


FIG. 10.—Trajectory of a white ball thrown in front of a black screen. The interval between two successive images is measured on the metrical scale. The time taken to travel over this interval is 1/100 of a second.

by turns. From the value of these accelerations there must be deducted the value of the horizontal component of the bird's motion, and that of the resistance of the air.

The calculations based on these experiments have given the following results for the forces which act during the flight of the sea-gull:—

Vertical component	0·623	kilogramme
Horizontal component	0·898	,,
Total ...	1·521	,,

These forces develop themselves during the act of lowering the wings; the ascent is passive, and is due to the pressure of the air upon the lower surface of the wings, which act then for the support of the bird, as in a paper kite.

As the resistance of the air under the wings acts at a point a considerable distance from the articulation of the shoulder, and as the pectoral muscles, by which the wings are lowered, act very near the articulation—that is to say, on the arm of a very unfavourable lever—it results

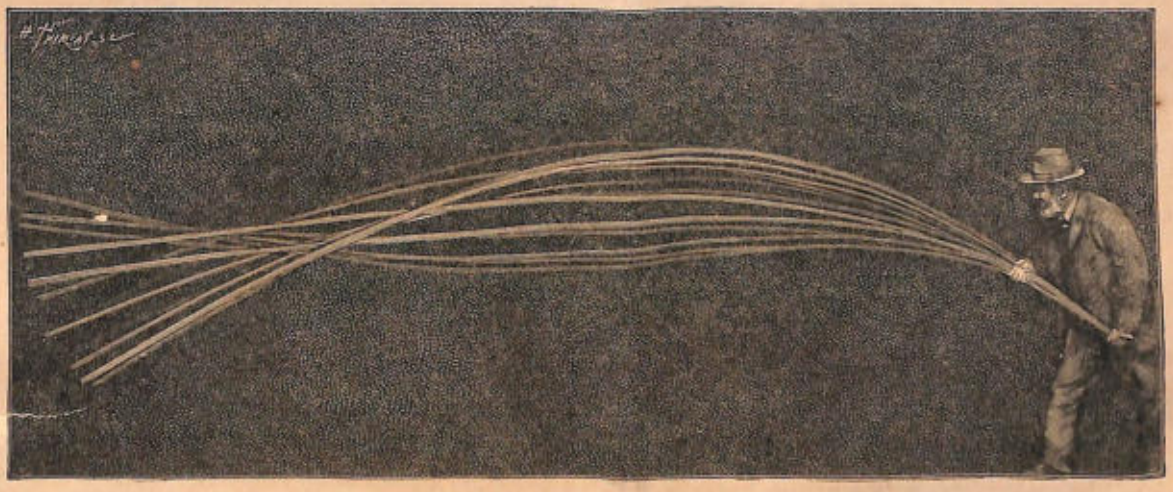


FIG. 11.—Curves and nodes produced by a vibrating stalk, one end of which is fixed. (Fac-simile of instantaneous photographs taken by the author.)

that the effort of the muscles is much greater than the resistance of the air which they surmount. For the pectorals of the sea-gull, the effort developed would be 19 kilogrammes.

It is frequently asked whether the muscles of birds have not a specific strength greater than those of other animals—that is to say, whether two bundles of the same thickness of muscles belonging, one to a bird, the other to a mammal, would have different powers. In the sea-gull which served for my experiments, one transverse section of the pectoral muscles arranged perpendicularly to the direction of their fibres had about 11 centimetres square of surface, or about 1·600 kilogramme per square centimetre. Other birds had formerly given me nearly similar returns for their specific strength; thus, the buzzard developed 1200 grammes per square centimetre, the pigeon 1400 grammes.

Aëronauts hope that they will one day invent a machine

capable of transporting man through the air, but many of them are troubled by a doubt; for they ask themselves whether the force of the bird does not exceed that of the known motors. The experiments on that subject may reassure them, for, if we compare the muscular force of the bird with that of steam, we see that one muscle would be comparable to an engine at very low pressure. In fact, the steam which would develop 1·600 kilogramme per square centimetre would scarcely have more than an atmosphere and a half of pressure. But the true comparison to establish between the animated motors and the engines consists in measuring the work which each of these motors can furnish, with equal weight, in the unity of time.

The measure of the work of a motor is obtained by multiplying the effort put forth, by the path which the point of application of that effort traverses. Photochronography expresses at each moment the spaces traversed by the mass of the bird and the displacement of the centre of pressure of its wings, giving thus the factor path in the measure of the work. In this way it is found that for the five strokes of its wing which the sea-gull gives every second, at the moment when it flies away, the labour done would be 3·668 kilogrammes. This calculation is very high; it corresponds to that which an engine would make in raising its own weight to a height of more than 3 metres in a second.

But that is only a maximum which the bird does not attain to except at the moment of flight, when it has not attained much speed. In fact, according as the passage of the bird is accelerated, the air under its wings presents a more resisting fulcrum. I have previously experimentally demonstrated this fact, announced by the brothers Planavergue, of Marseilles, and of which the following is the theory.

When the bird is not yet in motion, the air which is struck by its wings presents, in the first instance, a resistance due to its inertia, then enters into motion, and flies below the wing without furnishing to it any support. When the bird is at full speed, on the contrary, its wing is supported each moment upon new columns of air, each one of which offers to it the initial resistance due to its inertia. The sum of these resistances presents to the wing a much firmer basis. One might compare a flying bird to a pedestrian who makes great efforts to walk on shifting sand, and who, in proportion as he advances, finds a soil by degrees firmer, so that he progresses more swiftly and with less fatigue. The increase of the resistance of the air diminishes the expenditure of labour; the strokes of the bird's wing become, in fact, less frequent and less extended. In calm air, a sea-gull which has reached its swiftest, expends scarcely the fifth of the labour which it had to put forth at the beginning of its flight. The bird which flies against the wind finds itself in still more favourable conditions, since the masses of air, continually renewing themselves, bring under his wings their resistance of inertia. It is, then, the start which forms the most laborious phase of the flight. It has long been observed that birds employ all kinds of artifices in order to acquire speed prior to flapping their wings: some run on the ground before darting into the air, or dart rapidly in the direction they wish to take in flying; others let themselves fall from a height with extended wings, and glide in the air with accelerated speed before flapping their wings; all turn their bill to the wind at the moment of starting.

My experiments have, up to the present, only been able to apply to the flight of departure. In order to study the full flight there are conditions difficult to realize. With a courtesy for which I thank him, M. Eiffel has offered to me on the gigantic tower which he is erecting (at Paris) a post of observation which will leave nothing to be desired. From that enormous height, birds photographed during a long flight will give photochronographic images much more instructive than those which I have hitherto been able to obtain.

Without entering into the dry details of experiments and calculations made,[1] I have aimed at showing that the movements of birds, if they escape the sight, may be faithfully recorded by a new method which is applicable to the most varied problems of rotation and of mechanics.

Photochronography, in fact, gives experimentally the solution of problems often very difficult to solve by calculation.

Imagine a certain number of forces acting in different ways upon a known mass; the complicated way in which those forces are arranged sometimes renders long calculations needful in order to determine the positions which the moving object will occupy at successive moments; whilst if the body itself, submitted to those different forces, can be placed before the photochronographic apparatus, the path which it will follow expresses itself upon the sensitive plate.

Distinguished physicists disputed lately as to the form the free extremity of a vibrating stalk ought to present in which are produced curves and nodes: the greater number of them supposed that between the last node and its free extremity the stalk would present a bent form. Experiment has shown that it is not so, and that the last elements of the vibrating stalk are perfectly rectilinear (Fig. 11).

How many problems whose solution has formerly cost efforts of genius might be solved by a very simple experiment! Galileo in our day would not have needed to lessen the speed of the falling body in order to observe its motion. He would let fall a brilliant ball before a dark field, and would receive from it photographically successive images. Upon the sensitive plate he would have read, in the simplest way possible, the laws of space, of the speed and the accelerations which he has had the glory to discover.

To return to our subject, the laws of the resistance of the air to the living creatures of different forms which move in it ought to be searched into by photochronography. Already interesting results have been acquired: we have been able to determine the path of motion and the speed of small polished bodies (*petits appareils planeurs*) which move freely in the air, and which the eye has not time to follow in their rapid motions. Studies like these, undertaken and methodically carried out, will certainly lead to a comprehension of the still obscure mechanism of the hovering of birds.

TECHNICAL EDUCATION.

WHEN the time comes for the discussion of the new Technical Instruction Bill, attention will no doubt be given to an important series of resolutions (printed on the next page) which have just been passed by the Executive Committee of the North of England Branch of the National Association for the Promotion of Technical Education. The first six of these resolutions were unanimously adopted by the Committee, and the seventh was, on the motion of Mr. T. Burt, M.P., seconded by Mr. J. H. Girling (President of the Trades Council), adopted with one dissentient. The following are the advantages which the Committee desire to secure:—(1) For primary and secondary education a greater freedom of instruction under the existing code preparatory to technical education in the higher schools. (2) A direct or indirect pecuniary aid for superior education in science and art schools and in Colleges which afford technical education. (3) For all apprenticeship schools or trade classes a supervision by members of the trade, but no Government grant, thus to avoid any objections which might be raised by Trades Unions, or any jealousy arising from an apparent protection of one

[1] See the *Comptes rendus* of the Académie des Sciences, 1886–87.

or more particular trades. (4) For University Colleges a grant similar to that made to training Colleges for education afforded to persons intending to become teachers.

The resolutions are as follow:—

1. That public funds (rates and taxes) should not be employed to meet the current expenses of teaching specific trades.

2. That it is undesirable that instruction in the use of tools should be introduced into primary schools as a grant-earning subject.

3. That with a view to preparing pupils for technical education later on—

(*a*) The grant to day-schools should depend, to a much less extent than at present, on the results of the examination of individual pupils in reading, writing, and arithmetic, and should be largely dependent on the inspector's report of the general character of the teaching and the equipment of the school.

(*b*) There should be greater liberty in the choice of subjects in primary schools, and the same class subject should not necessarily be taken throughout the school.

(*c*) The grant to evening continuation schools should be regulated by the report of the inspector on the character of the teaching, and on the attendance list, and not upon the result of the examination of individual pupils.

4. That when a technical school is combined with a science and art school, the contribution to the building fund, through the Science and Art Department, should exceed £1000, if, in the opinion of the Department, the requirements of the locality demand it.

5. That it is desirable that, when specific trades are taught in technical schools, the practical teaching of each trade should be under the general direction of a committee, consisting mainly of members of that trade; that the teaching should be given in the evening, and be restricted to pupils actually engaged in the respective trades, and that, when specific trades are taught, any deficiency in current expenses should be guaranteed by the trade of the district.

6. That a certain percentage of persons preparing for appointments as teachers in elementary schools should be allowed to attend lectures and laboratory work at Universities and University Colleges, where a curriculum satisfactory to the Education Department is provided, and that the same grant should be made on account of such students as in the case of ordinary training Colleges.

7. That it is desirable that University Colleges in which higher scientific and technological training are combined should be assisted by a Government grant, provided that evening instruction is given in all the subjects taught, at fees which shall bring the advantages of the College within the reach of all classes. The due administration of the grant should be secured by the appointment of certain nominees of the Government on the Executive Council of the College.

THREATENED SCARCITY OF WATER.

THE appendices to the Weekly Weather Reports for the year 1887, recently published by the Meteorological Office, contain some interesting details relative to the rainfall. It is shown that the mean rainfall for the whole of the British Islands during 1887 was only 25·8 inches, whereas the mean for the twenty-two years 1866 to 1887 was 35·3 inches, so that there is a deficiency of nearly 10 inches over the whole area of the British Islands, or 27 per cent. less than usual. In the wheat-producing districts, which comprise the east of England and Scotland, the south of England, and the Midland Counties, the fall during 1887 was 21 inches, and the average value for twenty-two years is 28·5 inches, showing a deficiency in these parts of the Kingdom of 7·5 inches, or 26 per cent. less than usual. In the principal grazing districts, which comprise the west of England and Scotland, as well as Ireland, the fall in 1887 was 30·5 inches, and the value for the twenty-two years is 42·0 inches, showing a deficiency of 11·5 inches, or 27 per cent. less than the average. In the north-west of England the rainfall for 1887 was only 24·9 inches, which is 15·7 inches or 39 per cent. less than the average, and in the south-west of England the fall was 28·3 inches, which is 16·6 inches or 37 per cent. less than usual. Last year was the driest of any year since 1866, and this feature was common to all parts of the United Kingdom; the amount of rain measured was only about one-half of that recorded in 1872, which was the wettest year of the period. If the comparison is confined to the last ten years, the deficiency is nearly as marked, and 1887 is still found to be about 25 per cent. below the average, but the greatest deficiency in this case occurs in the Midland Counties, where it amounts to 36 per cent. of the average. The reports issued by the Meteorological Office for the first five or six weeks of the present year show the deficiency of rainfall still to be augmenting, and even more quickly than in any period last year. In the Midland Counties the rainfall to February 6 was only 0·6 inch instead of 2·9 inches, so that the deficiency from January 3 is as much as 79 per cent. of the average fall; and at Hereford, where the total fall is only 0·29 inch, the deficiency is 90 per cent. of the average. In the east of England the deficiency is 64 per cent., in the south-west of England 61 per cent., and in the north-west of England 58 per cent. There has been a deficiency of rain in all districts of England each week for seven consecutive weeks since December 19, with the exception of a single district (England N.E.) in one week, and since the beginning of October there have been but four weeks in which the excess of rain was at all general. Out of fifty-seven weeks since the commencement of 1887 there have been but ten in the south-west and east of England with an excess of rainfall, and only eleven in the north-west of England. With these facts to hand, there seems reasonable ground for alarm being felt in some localities at the threatened scarcity of water.

CHARLES HARDING.

PROFESSOR ASA GRAY.

WHEN the history of the progress of botany during the nineteenth century shall be written, two names will hold high positions: those of Prof. Augustin Pyrame De Candolle and of Prof. Asa Gray. In many respects the careers of these men were very similar, though they were neither fellow-countrymen nor were they contemporaries, for the one sank to his rest in the Old World as the other rose to eminence in the New. They were great teachers in great schools, prolific writers, and authors of the best elementary works on botany of their day. Each devoted half a century of unremitting labour to the investigation and description of the plants of continental areas, and they founded herbaria and libraries, each in his own country, which have become permanent and quasi-national institutions. Nor were they unlike in personal qualities, for they were social and genial men, as active in aiding others as they were indefatigable in their own researches; and both were admirable correspondents. Lastly, there is much in their lives and works that recalls the career of Linnæus, of whom they were worthy disciples, in the comprehensiveness of their labour, the excellence of their methods, their judicious conception of the limits of genera and species, the terseness and accuracy of their descriptions, and the clearness of their scientific language.

Asa Gray was born in Paris, Massachusetts, on November 18, 1810, and took his M.D. degree when twenty, at the Medical College of Fairfield, Oneida County. His proclivities were all scientific from a very early age, and he is said to have, whilst still a student, delivered lectures on chemistry, geology, and botany, in private establishments of that county. The two former subjects were at first his favourites—indeed, his earliest

contribution to science is a paper, by G. B. Crawe and himself, on the mineralogy of Jefferson and St. Lawrence Counties (N.Y.), in *Silliman's Journal* (1834, 346)—but they soon gave place to botany, owing to his having attracted the attention of Dr. John Torrey, State Botanist for New York, and Professor of Chemistry and Botany, but practically of botany only, in the New York Medical College. In 1833, Dr. Torrey made Gray his laboratory assistant, a post he held for some months, during which he presented what was his first botanical paper to the Annals of the New York Lyceum. This, which was on a very intricate and much misunderstood group of American sedges (*Rhynchospora*) showed Gray's acuteness as an observer, and skill in systematizing, as clearly as anything he has since written. In the following year he was appointed Curator of the New York Lyceum, where he extended his studies to the North American grasses and Cyperaceæ, and prepared his first botanical text-book, which was published in 1836, under the title of "Elements of Botany." The "Elements" is a noteworthy book; it was at once accepted as the best that had appeared in the States, and as second to none in the English language; its only rival was Lindley's "Introduction to the Natural System of Botany," the first edition of which had (in 1831) been reprinted under Dr. Torrey's supervision for the use of the American schools.

Whilst still attached to the New York Lyceum, Gray accepted the appointment of naturalist to Capt. Wilke's South Pacific Exploring Expedition, which was then being fitted out; but after two years' delay, and the curtailment of the opportunities for research that were to have been afforded him on the voyage, he threw up the appointment. This result is much to be deplored, for no young naturalist was ever better fitted by education, and by training as an observer and collector, to have taken advantage of the splendid opportunities which that expedition afforded.

Having relinquished the prospect of Pacific exploration, Dr. Gray was invited by his friend Dr. Torrey to co-operate with him in the preparation of a flora of the North American Continent; and into this work, which became the leading object of his scientific life, he eagerly entered. At the same time he accepted the Chair of Botany in the University of Michigan, subject to the condition of being allowed a year's leave to be passed in Europe for the purpose of verifying the nomenclature of the American flora by a study of the species of which the types existed only in European herbaria. This was in 1838, and his first visit was to Glasgow, where the then Professor of Botany (Sir W. Hooker) was engaged on a flora of British North America, under the auspices of the Secretary of State for the Colonies. After a long sojourn in Glasgow, Dr. Gray visited the principal herbaria in London, France, Switzerland, Italy, Austria, and Prussia, making life-long friendships with scientific men of all pursuits wherever he went; his letters of introduction, coupled with his bright intelligence, genial disposition, and charming personality, giving him the *entrée* to *salons* as well as to the museums of every capital. This was the first of seven visits that Dr. Gray paid to Europe, and of which the last was concluded a very few weeks before his fatal illness

On his return to America in 1839, Dr. Gray resided at New York, when the first volume of the flora of North America was completed, in conjunction with Dr. Torrey, and the second, elaborated wholly by himself, was begun, but not completed till 1843. In the meantime (in 1842) he had been appointed by the Fellows of Harvard College, Cambridge, Fisher Professor of Natural History, the duties of which Chair were restricted to an annual course of lectures on botany, and the charge of the College Botanical Gardens, to which an official residence is attached. This was his home for the rest of his life, and here, with funds partly derived from the College, and partly from private sources, largely supplemented by interchanges of specimens and books, he founded the Harvard Herbarium and Library.

The great desideratum for the conduct of Dr. Gray's new duties was a much fuller class-book of botany than the "Elements" of 1836, and in the same year he completed the first edition of his "Botanical Text-book." In this the subject-matter is divided into two parts, one devoted to structural and physiological botany, the other to the principles of systematic botany, including chapters upon plants useful to man. This was the first of a series of editions of a work that has been for nearly half a century the text-book of schools and colleges throughout the United States, and the latter issues of which have been generally recommended by the botanical professors of the United Kingdom as the best of its class. In 1880 the first volume of the sixth edition appeared, but the advances in botanical science made since the fifth was published, quite a quarter of a century before, had been so many and great that the amount of matter which this sixth will contain is quadruple that of the fifth. It will be when complete a co-operative work in four volumes. The first is by Gray himself, and is devoted to morphology, taxonomy, and phytography; the second, by Prof. Goodale, Gray's able successor in the Fisher Professorship, includes vegetable histology and physiology; the third, by Prof. Farlow, will treat on Cryptogams; and the fourth, which Dr. Gray reserved for himself, was intended to be occupied by the classification of Phænogams, their special morphology, distribution, and products. Gray's other educational works are: "Lessons in Botany and Vegetable Physiology," somewhat on the plan of Lindley's "School Botany," but much fuller; also two smaller works, that for charm of matter and style have no equal in botanical literature—"How Plants Grow," and "How Plants Behave"—they rival chapters in Kirby and Spence's introduction to entomology in instruction and interest; and lastly, "Field, Forest, and Garden Botany."

The great outcomes of Gray's labours in systematic botany are his works on the flora of North America, from the Arctic islands to Mexico, and from the Atlantic to the Pacific Ocean. In one form or another these embrace a great proportion of the 10,000 or 12,000 species which that continent is supposed to contain. More than half are included in the two volumes published in conjunction with Torrey, and in his "Synoptical Flora," of which two parts are published, and in his "Manual of the Botany of the Northern States." The remainder are described or mentioned in more or less detail in multitudes of detached papers, and especially in memoirs upon collections made by naturalists attached to the many Expeditions organized by the Government for the exploration of railway routes across the continent, and by collectors and private individuals in previously unexplored regions. It was the hope of their author that the publication of these collections would have accelerated the completion of the general flora, and such would have been the case had their author lived; but as it is, the stars have in great measure obscured the planet, for one of the greatest obstacles to the study of North American plants is the multitude of these detached memoirs, with complicated titles, in which so many genera and species are to this day buried. If the great work cannot be continued, as it is to be hoped it may be, by Dr. Gray's most competent herbarium keeper, Sereno Watson, it is most desirable that the contents of these memoirs should be reduced to a systematic form with the least possible delay.

Next to the synoptical flora, Dr. Gray's most original work is his "Genera Floræ Boreali-Americana Orientalis," which was intended to contain descriptions, with figures, of every genus of the Eastern States, with discussions upon their affinities, morphology, and distribution. Only two volumes had appeared when want of funds decreed its end. As a fragment it is unique, and had it but been

completed it would have been of great morphological value. To have done this would, however, have required more than the ten volumes that were regarded when the work was commenced as sufficient to complete it, and this independently of the Cryptogams.

Nor was Dr. Gray's all closet work: he diligently collected and observed over a considerable area of his native continent; along the Altantic coast from Canada to Florida; in the prairie and Rocky Mountain regions from Wyoming to the borders of New Mexico; in the great basin of Utah and Nevada; and along the Pacific coast from Oregon to St. Barbara.

With two notable exceptions, Dr. Gray confined his descriptive work to North American botany. These exceptions were: one, the fragmentary botany of Wilkes's South Pacific Exploring Expedition, with the execution of which he was intrusted, but which came to an end before it was half finished, for want of funds it is believed, after the publication of one quarto volume with a superb atlas of plates; the other is a memoir on the flora of Japan, founded chiefly on the collections made in that country by the United States North Pacific Exploring Expedition, which in point of originality and far-reaching results was its author's *opus magnum*. By a comparison of the floras of Japan with those of Eastern and Western America, and of these with one another, and all with the Tertiary floras of the Northern States, he drew in outline the history of the vegetation of the north temperate hemisphere in relation to its geography, from the Cretaceous period to the present time. It is a brilliant generalization, bearing the unmistakable stamp of genius.

It remains to allude to Gray's admirable defence of the doctrine of "the origin of species by natural selection," of which he, as one of a favoured few, had been fully informed by Darwin himself in 1857 ("Life and Letters," ii. 120), before it appeared in the Linnean Journal. His opinion, which was, from the first, cautiously favourable, but with reserve, soon ripened into a conviction of the truth of the principles involved. He alludes to it first in the concluding remarks to his essay on the flora of Japan, cited above, published in 1859, wherein he says that he is "disposed to admit that closely related species may, in many cases, be lineal descendants from a pristine stock." Again, in a letter to Mr. Darwin, dated early in July 1860, speaking in terms of highest praise of the "Origin," the following passages occur:—"The moment I understood your premisses, I felt sure that you had a real foundation to hold on. Well, if one admits your premisses, I do not see how he is to stop short of your conclusions, as a probable hypothesis at least." And, referring to his own review of it in *Silliman's Journal* (March 1860), he says:—"It naturally happens that my review of your book does not exhibit anything like the full force of the impression the book has made upon me. Under the circumstances, I suppose I do your theory more good here by bespeaking for it a fair and favourable consideration, and by standing non-committed as to its full conclusions, than I should if I announced myself a convert; nor could I say the latter with truth." It may be remarked here that just at this time a battle over species was raging in America, of which but faint echoes reached our shores. This was over the question of the single or multiple origin of species by creation. Gray was the champion of single creations, and, believing himself strongly supported by theological considerations, may well have felt that the further leap to evolution was one into the dark. Be this as it may, for the five years following the publication of the "Origin," Gray devoted himself to impressing upon the American public his opinion of its extraordinary merits by reviews in weekly and monthly periodicals, by lectures, and by discourses at scientific Academies. Latterly (in 1876) he collected many of these into a single volume which he entitled "Darwiniana." In it he defines his own position "as one who is scientifically, and in his own fashion, a Darwinian, philosophically a convinced theist, and religiously an acceptor of the 'creed commonly called the Nicene,' as the exponent of the Christian faith." From this position he never moved, and he subsequently delivered two lectures in further exposition of these views, at the Divinity School of Yale College. These were published in 1880, under the title of "Science and Religion." Finally, Mr. Darwin, whilst fully recognizing the different stand-points from which he and Gray took their departures, and their divergence of opinion in some important matters, regarded him as the naturalist who has most thoroughly gauged his work, and as a tower of strength to himself and his cause.

As a reviewer and bibliographer, Gray's labours must have been Herculean, and they were uninterrupted for nearly half a century. Even when on his travels in Europe, he was in the habit of contributing scientific notices to periodicals in the States. In 1836 he commenced writing reviews of botanical works, and notices of botanists, travellers, and collectors for *Silliman's Journal of Science and Arts*; and this function he continued to perform without intermission (latterly as a co-editor of that important periodical) till within a few days of his last illness. The number of these articles is truly astonishing, as is the knowledge they display of all branches of botany, Phænogamic and Cryptogamic. They are without exception just, sober, and discriminating, critical rather than laudatory, and eminently considerate in tone where censure is necessary. A selection from these, many being discussions full of original matter and suggestive observations, would be an instructive and acceptable contribution to the botanical literature of the century, and a meet tribute to their author's merits and memory.

Dr. Gray's figure and features were familiar in the scientific circles of this country; but for the information of others it may be stated that he was of spare, wiry figure, rather below the average height, his expression was keen and vivacious, and his movements, like his intellect, alert. He was a Foreign Fellow of the Royal and Linnean Societies, a correspondent of the Institute of France, and of the other Continental Academies, a Doctor of Laws of Oxford, Cambridge, and Edinburgh, and had served as President of the American Academy of Arts and Science, of the American Association for the Advancement of Science, and as a Regent of the Smithsonian Institution. Accompanied by Mrs. Gray he spent the summer of 1887 in Europe, chiefly in England, returning to Cambridge in September. In October he went to Washington on the affairs of the Smithsonian Institute. Soon after his return, on the 28th of November, he was struck with paralysis, from which he never rallied, and he died at the end of the following January. It is characteristic of him that his last letter, written in pencil immediately before his seizure to the contributor of these lines, was on the subject of a review for *Silliman's Journal* of Planchon's "Review of the Vines." Dr. Gray married in 1848, Jane, daughter of Judge Charles G. Loring, of Boston, who survives him. He left no family. An excellent medallion likeness of him in bronze was, on his seventy-fifth birthday, presented by his friends to Harvard College, Cambridge, U.S.

J. D. H.

NOTES.

On Tuesday evening a question was asked in the House of Commons, by Mr. Howorth, about the new regulations for the entrance examination at Woolwich. Mr. Howorth inquired whether these regulations were final and permanent, or only temporary. Mr. Stanhope, we regret to say, replied that the regulations are intended to be of permanent application. If that be so, it is the more necessary that a vigorous protest against the scheme should be made by those who have any

true appreciation of the place which properly belongs to the study of science in education.

On Tuesday the Committee of the Athenæum Club elected three new members in accordance with the rule which empowers the election of nine persons annually "of distinguished eminence in science, literature, or the arts, or for public services." The names of two of the new members are familiar to students of science—Major-General Donnelly, R.E., C.B., Secretary to the Department of Science and Art; and Prof. G. Carey Foster, F.R.S.

One of the leading native residents of Bombay, Sir Dinshaw Petit, has just given the Bombay Government a property valued at 300,000 rupees for the establishment of the proposed Technical Institute of Bombay.

We regret to announce the death of Dr. Maximilian Schmidt, an eminent geologist and Director of the Zoological Gardens at Berlin. He was born at Frankfurt in 1834, and died at Berlin.

Emile Rousseau, the well-known French chemist, died on the 4th inst., at the age of seventy-three.

The annual general meeting of the Fellows of the Royal Horticultural Society was held on Tuesday, Sir Trevor Lawrence, President of the Society, in the chair. The special Committee appointed to inquire into the working of the Society recommended that premises in No. 111 Victoria Street should be secured, and that for shows and meetings the Society should hire the drill-hall of the London Scottish Rifle Volunteers. The Committee also reported that they had under consideration "the construction of new by-laws intended to facilitate the carrying out of as complete a horticultural policy as possible—one in which all aspects and departments should be considered to the undue preponderance of none; but to the general advantage of all." After some discussion the Committee's recommendations were adopted. Several members of the Council having placed their resignation in the hands of the Fellows, it was resolved on the motion of Mr. Wilks, seconded by Mr. Veitch, to decline to accept their resignation, and they were then all formally re-elected, including Sir Trevor Lawrence, who was re-appointed President.

The Calendar and General Directory of the Science and Art Department for 1888 has been issued.

The curious fact was some time ago brought to light by Nahrwold that solid particles are ejected from a platinum wire glowing under the influence of an electric current, and form a metallic incrustation upon the walls of a glass tube by which the wire is surrounded. The cause of the emission of these solid particles of platinum has, however, until recently, remained a complete mystery. In the number of the *Annalen der Physik und Chemie* just received will be found an interesting paper by Dr. Alfred Berliner, who, in the course of a series of experiments upon the occlusion of gases by platinum and palladium, has discovered the source of this singular phenomenon. Thin strips of platinum, before being charged with the gas under experiment, were inclosed in a narrow glass tube, and freed from all occluded gas by being heated to redness, *in vacuo*, by the passage of a constant electric current for several hours. At the expiration of this time the metallic incrustation was invariably found when occluded gas had been evolved. On charging the strips with various quantities of any particular gas, the amount of incrustation formed after the complete expulsion of the gas in each experiment was found to vary in the same proportion. Hence it appears pretty clear that the evolution of gas is necessary for the emission of solid particles. This result is strongly confirmed by the fact that palladium, which has such a remarkable power of occluding gases, produces a similar incrustation much more readily and at a lower temperature. It appears probable that the action is merely mechanical; that we have, in fact, an immense number of microscopic volcanoes or solfataras, evolving the occluded gases with such energy that portions of the crater walls are detached and carried away by main force, like their brethren on the large scale, the scoriæ and lapilli, distances very considerable in comparison with the size of the vents.

The next meeting of the French Association for the Advancement of Science will be held in Oran from March 29 to April 3. Interesting excursions will be made to Biskra and other places.

At the meeting of the Meteorological Society of France, on December 20, M. E. Lemoine was elected President for the ensuing year. M. Renou made a communication on the relations which exist between the annual number of solar spots and thunder-storms in various places, and concluded that the works hitherto published were far from sufficing to show any direct dependence between the two phenomena. At the meeting of January 11, M. Janssen, the retiring President, expressed his opinion of the necessity of organizing meteorological stations on a uniform plan at a certain number of stations, under the superintendence of professional and paid observers on whom a definite programme could be imposed, instead of having volunteer observers; this view was also supported by M. Renou. The latter gentleman also spoke of the importance of adopting a uniform cloud nomenclature, and announced that he would shortly present to the Society a work upon this subject. The general secretary presented a note from M. Garrigou-Lagrange on a new electrical anemometer giving the direction of the wind, and the horizontal and vertical components of its force (see Nature, November 3, 1887, p. 18), and recommended its adoption at some coast stations.

The Council of the Royal Meteorological Society, 30 Great George Street, Westminster, S.W., have appointed a Committee to collect observations on British hail- and thunder-storms from volunteer observers. The objects which they hope to attain thereby are:—(1) A knowledge of the nature and causes of the different kinds of thunder-storms, their attention having been specially called to the subject by the great loss of life and property during the past summer. (2) A discovery of the localities where hail and thunder are most frequent and destructive. (3) If possible, to obtain an increased power of forecasting hail and thunder, whereby they hope that eventually damage to persons, stock, and property might be lessened. Forms and instructions will be sent to intending observers.

We have received a "Brief Sketch of the Meteorology of the Bombay Presidency in 1886-87." It is by Mr. F. Chambers, Meteorological Reporter for Western India. Mr. Chambers points out that the meteorology of the year 1886-87 in the Bombay Presidency presents several features of special interest. There was a decided reappearance, for some time, of almost exactly the same unfavourable meteorological conditions which characterized the year 1877, when the rainfall in many parts of the Presidency was disastrously deficient. Fortunately, these unfavourable conditions did not last long enough to produce distress, for although a prolonged break in the rains caused considerable anxiety for a time, an excessive fall of rain late in the season brought relief, and on the whole the year was a favourable one.

A severe earthquake occurred in Grenada on January 10. A rumbling noise was immediately followed by a slight shock and gentle lateral oscillations. Then came a very violent shock and vertical undulating oscillations. These were succeeded by gentle oscillations. The shock is supposed to have lasted from twenty to thirty seconds. Several houses in the town of

St. George's were so much damaged that they cannot be safely occupied. The walls of the St. David's Roman Catholic Church are so seriously injured that they will have to be taken down in some places and rebuilt. The sacristy was all but demolished; the basement wall of the presbytery was thrown down, and the building itself had to be propped up with posts. The Court House in the same parish sustained considerable injury. The tower of the Anglican Church in Grenville was cracked. During the succeeding week several mild shocks of earthquake were experienced in the island, the strongest of which occurred between 7 and 8 o'clock on the night of Sunday, January 15.

Dr. Forbes Watson's collection of commercial products, which was lately offered to the University of Aberdeen at the comparatively small price of £250, has been bought by Dr. Carnelley and his father, and presented to University College, Dundee. Prof. D'Arcy W. Thompson, at whose suggestion the purchase was made, writes to the *Dundee Advertiser*: "This excellent gift puts us at once in possession of a museum which is first-class of its kind, and of which town and College should be proud for ever. Dr. Forbes Watson is well known to many of the older men in Dundee for his knowledge of jute and all other commercial fibres. His works are standard on the subject. His collection was amassed with unrivalled opportunities and the highest technical skill. Great part of it was brought together as an official duty for the India Museum, and was presented by the Department to Dr. Forbes Watson when that Museum was broken up. It contains nearly 7500 samples. Between 700 and 800 of these are fibres, including textiles and paper-making materials. There are over 500 dyes and dye-stuffs, 500 oils and oil-seeds, 600 or 700 gums, resins, and guttas, nearly 2000 medicinal substances (may they be useful to us in the future), and more than as many samples of food-stuffs. The bulk of the collection is stored in bottles, filling fourteen cabinets, and there are also stands and cases for the display of specimens. Altogether the cases and bottles in which this great collection is stored represent a cost greater than the price which Dr. Forbes Watson now asks and receives."

The Batavia *Nieuwsblad* announces that the Government has decided upon establishing a bacteriological laboratory in that town. An institution for the pursuit of that special branch of study will be built immediately the funds for the purpose become available. The existing laboratory arrangements will be improved and extended, so as to admit of pathological and bacteriological investigations.

We are glad to learn from the *American Naturalist* that the project of a Marine Biological Laboratory on the New England coast is not languishing. Several thousand dollars have already been subscribed towards the erection of the necessary building and its equipment and maintenance. The Committee on the Laboratory have arranged a course of eight lectures, the proceeds of which are to be added to the fund.

Lieut. Niblack, U.S.N., has returned to Washington from a three years' voyage to Southern Alaska, where he has been engaged on coast-survey duty. He has brought with him many photographs and objects which will be of interest to students of ethnology and anthropology. He devoted special attention to the totem posts of Southern Alaska. He says that in that country winter is the best season for ethnological studies. The natives are then at home, whereas in the summer they are often far inland.

Assistant Charles A. Schott, assistant in charge of the Computing Division of the United States Coast and Geodetic Survey, has addressed a letter to the Superintendent of his Bureau about the recent discovery, by Assistant G. Davidson, of records of the magnetic declination, A.D. 1714. He says that these records greatly increase our knowledge of the secular variation of the declination. *Science* gives the following account of the substance of M. Schott's letter:—"By means of these observations we are able to improve materially the expressions for San Blas and Magdalena Bay, to add the new station Cape San Lucas, and to make their influence felt as far north as San Diego and Santa Barbara. It is the range which is greatly improved; besides, the epoch of maximum declination is shifted in the right direction. Apart from the fact that a region of west declination is here for the first time observationally indicated on the Pacific coast, the power of the newly recovered declinations is due to the circumstance, that, as far as known, they cover a time when the needle was in or near a phase the opposite of the present one. For want of early observations, those previously collected for San Diego and Santa Barbara, Cal., were extremely difficult to handle; and, while it was not an easy matter to establish new expressions for these stations, their correctness, or rather applicability over the whole period of time the observations cover, is quite reassuring. He points out the desirability of new observations (either using funds yet available before July next, or providing funds to be used after that date) at San Diego, Santa Barbara, and Monterey, and states that these stations have received no attention for seven years. These observations are demanded to give greater precision to the computed variations on our charts."

A number of American geographers, all belonging to Washington, have incorporated the American National Geographical Society for a term of 100 years. The principal objects of the Society are to increase and diffuse geographical knowledge, to publish the Transactions of the Society, to publish a periodical magazine and other works relating to the science of geography, to dispose of such publications by sale or otherwise, and to acquire a library under the restrictions and regulations to be established by its by-laws.

The Commissioners of Public Schools of Baltimore, Md., deserve much credit for the efforts they are making to secure for the schools under their care important reforms which have always been advocated by students of sanitary laws. They lately resolved that the Mayor and City Council of Baltimore should be requested to authorize them to appoint an officer, to be known as the Sanitary Superintendent of Public Schools, whose duty would be: (1) to carefully examine all plans submitted for the construction of new school-houses, and suggest such modifications as may be necessary from a sanitary point of view; (2) to advise with the Commissioners with reference to necessary alterations in school-buildings to improve their hygienic condition; (3) to examine all text-books before adoption, in order that type, printing, or paper injurious to the eyesight of pupils may be avoided in selecting such books; (4) to satisfy himself, by personal examination if necessary, that all pupils admitted to the schools have been properly vaccinated, or are otherwise protected against small-pox; (5) to take such other measures, in conjunction with the Health Commissioner of the city, as may be necessary to prevent the spread of contagious diseases in, or through the medium of, the public schools; (6) to examine annually the eyesight of all children attending the public schools, and keep an accurate record of such examinations; (7) to report annually, or as often as may be required by the Commissioners, upon the sanitary condition of the schools, and of the pupils attending them, and to advise the Commissioners upon sanitary questions connected with schools whenever required; (8) to give instruction, by lectures or otherwise, to the teachers in the schools upon the elementary principles of school hygiene.

We have received the second volume of the Transactions of the Meriden (Conn., U.S.A.) Scientific Association. It contains, among other things, a valuable list, drawn up by Mr. Franklin Platt, of the birds of Meriden.

A THIRD edition of Mr. John Venn's "Logic of Chance" (Macmillan) has just been issued. The work has been re-written, but the author explains that the alterations he has made do not imply any appreciable change of view on his part as to the foundations and province of probability. In the preface Mr. Venn mentions that he is engaged in preparing a work on inductive logic.

THE scientific writings of Jean Méry (1645–1722) have been brought together in a work edited by M. L. H. Petit, Assistant Librarian to the Medical Faculty of Paris. The work contains many contributions to biology which have not hitherto been generally known.

TOME III., Cahier 1, of the "Mémoires de la Société des Sciences Physiques et Naturelles de Bordeaux," is devoted to a full bibliography of the Γ function.

WE have received the *Annuaire de l'Observatoire Royal de Bruxelles* for 1888, this being the fifty-fifth year of its publication. It contains the usual astronomical tables and data for the current year, and a mass of meteorological, geographical, financial, and other statistics. There is a complete list of the asteroids and comets discovered in the past year, with the elements of their orbits, and there is also an account of the state of solar activity in 1886. The tables of units and physical constants have been considerably extended, and a detailed account of electrical and magnetic measurements has been added. In addition there are, as usual, several scientific papers by the officials of the Observatory. M. Folie gives an account of his further investigations into the movements of the earth's axis, and in a subsequent article M. Niesten applies the correction for diurnal nutation to the various and widely differing determinations of the annual parallax of γ Draconis; the value obtained is $+0''\cdot086$. The important series of barometric observations, extending over a period of fifty years, are discussed at great length, and illustrated by diagrams.

MR. JOHN HEYWOOD, of Manchester and London, has issued a little book called "Flower-Land," by the Rev. R. Fisher. It is written in a simple style, and will no doubt be useful as an introduction to botany for children.

THE Perthshire Society of Natural Science has begun to issue Transactions and Proceedings. We have received the first part of the first volume. It contains notes on some rare birds lately placed in the Society's Museum, by Colonel H. M. Drummond Hay; mnium riparium, by R. H. Meldrum; some localities for Perthshire plants, by R. H. Meldrum; origin of the interbedded and intrusive volcanic rocks of Kinnoull Hill, by H. Coates; and the flora of the Woody Island, by W. Barclay.

MESSRS. KEGAN PAUL, TRENCH, AND CO. will publish immediately, in the "International Scientific Series," Sir J. William Dawson's new book entitled "The Geological History of Plants."

"A HISTORY OF PHOTOGRAPHY," by Mr. W. J. Harrison, will shortly be published. It is intended to serve as a practical guide to photography, and as an introduction to its latest developments.

A WORK containing a full account of the volcanic eruptions for the last sixty-four years on the island of Hawaii has been printed and will shortly be published in the city of Honolulu. The author is Mr. William Lowthian Green, at present Prime Minister of the kingdom of Hawaii, whose work on "The Vestiges of the Molten Globe" appeared in England some years ago, and has since attracted the attention of M. Lapparent and other Continental geologists. Mr. Green's new work will contain a complete tabular statement of the eruptions, and a map of the island of Hawaii.

MR. EDGAR THURSTON, Superintendent of the Government Central Museum, Madras, has printed a list of fishes obtained during a residence of three weeks at Rámeswaram Island, which is separated on the one hand from the Indian continent, and on the other from the Island of Manaar, by an interrupted ridge of rocks known as Adam's Bridge. The fish-fauna of the coral reefs of this island stands out in striking contrast with that of other places on the Madras coast. "Coral Fishes," *i.e.* brightly coloured fishes—*Chætodon*, *Platyglossus*, *Heniochus*, *Pseudoscarus*, &c.—abound round the reefs, and feed either on the small delicate marine Invertebrata which swarm on the living corals, or, if their teeth are adapted for the purpose, on the hard calcareous substance of the corals. The bright colouring of the fishes is explained by Mr. Thurston on the well-known principle that "the less the predominant colouring of any creature varies from that of its surroundings, the less will it be seen by its foes, the more easily can it steal upon its prey, and the more it is fitted for the struggle for existence." Conspicuous by their abundance are several species belonging to the family Sclerodermi, including *Balistes* (file-fish), whose jaws are armed with teeth well suited for breaking off pieces of hard coral, or boring holes into the shells of the Mollusca, on the soft parts of which they feed. The file-fishes are said to destroy an immense number of mollusks, thus becoming most injurious to the pearl-fisheries. Present, too, in great numbers are several species of the family Gymnodontes: *Tetrodons* (globe fishes), including the beautifully marked little *Tetrodon margaritiferus*, and *Diodons*, which have a very bad reputation among the natives as being very poisonous.

IT is generally supposed that the Ainos of Yezo are amongst the disappearing races of the earth, and that they are "fast dying out," as the phrase usually runs. This appears to be an error, for according to a communication on the subject in the *Japan Weekly Mail*, from Mr. Bachelor, during the past fifteen years there has been little, if any, diminution in their number, which he puts down, so far as the Island of Yezo is concerned, at from 1300 to 1600 souls. Actual detailed statistics respecting the numbers of the Ainos do not appear to be given in the Japanese censuses, but official statistics do exist for certain Aino settlements since 1871, which may be taken as an index. These show an increase of 129 persons in sixteen years, although, by a careful examination of the data, it appears that one village not included in the earlier was given in the later years. In 1871, there were 665 males, 639 females, and 260 huts; in 1886, the numbers were 691, 742, and 318 respectively. These figures, making every reasonable allowance, show at least that there is good ground for doubting whether the Ainos are dying out, in Yezo at least, as rapidly as it is the fashion to assume that they are.

THE seventh annual meeting of the members of the Sanitary Assurance Association was held at their offices, 5 Argyll Place, Regent Street, on Monday. Prof. Roger Smith presided, and expressed his satisfaction at the continued prosperity of the Association.

IN the footnote in NATURE, December 15, 1887, p. 152, second column, line 5 from foot, *for* Careton *read* Cureton.

THE additions to the Zoological Society's Gardens during the past week include a Common Boa (*Boa constrictor*) from South America, a Royal Python (*Python regius*), two West African Pythons (*Python sebæ*) from West Africa, an Indian Python (*Python molurus*) from India, presented by Mr. Leopold Field; a Griffith's Fox (*Canis griffithi*) from Persia, two Cockateels (*Calopsitta novæ-hollandiæ*) from Australia, deposited; a Pluto Monkey (*Cercopithecus pluto*) from West Africa, an Antarctic Skua (*Stercorarius antarcticus*) from the Antarctic Seas, received in exchange.

OUR ASTRONOMICAL COLUMN.

MELBOURNE OBSERVATORY.—The Annual Report of this Observatory, dated August 14, 1887, states that the buildings and equipment of the Observatory were in good condition with the exception of the mirrors of the great Cassegrain reflector, which had become so dull as materially to interfere with the observation of the fainter nebulæ. It was proposed to substitute mirror A, the less tarnished of the two, for mirror B, now in the telescope, and either to have B repolished on the spot or to send it to Dublin to be re-polished under the care of Sir H. Grubb. The new transit circle was in excellent order, and 2487 right ascensions and 1301 polar distances had been observed during the year. Eighty-seven southern nebulæ had been examined with the great reflector, and four searched for, but not found. The use of the photo-heliograph, which had been altered in July 1886, so as to take pictures on a scale of 8 inches to the solar diameter, had been much interfered with by bad weather, and only 121 photographs had been secured. The principal fresh work proposed for the Observatory was the co-operation in the photographic survey of the heavens; the Victorian Government having consented to the Observatory joining in that undertaking, and having placed £1000 on the estimates of the current year towards the necessary expenditure.

THE AMERICAN NAUTICAL ALMANAC OFFICE.—The Report of Prof. Newcomb, Superintendent of the Office, for the year ending 1887 June 30 has recently appeared. From this we learn that the printing of the several Nautical Almanacs published by the Office fell a little into arrear in 1887, the printing of the American Ephemeris for 1890, which should, according to custom, have appeared in June, not being quite ready in October. The computations for the following years were in their usual state of forwardness. The principal part of the Report deals with the new tables of the planets on which Prof. Newcomb and his assistants are engaged. The work is divided into four sections—viz.: (I.) The computation of the general perturbations of the planets, the work now in hand relating to those of the four inner planets; on twelve of the fourteen pairs of planets which come into play in this part of the undertaking, the work has already been completed. The incomplete perturbations are those of Venus and Mars by Jupiter. (II.) The re-reduction of the older observations, and discussion of the later ones, with a view of reducing them all to a uniform system. In this section Maskelyne's Greenwich observations from 1765 to 1811, and Bradley's, 1750 to 1762, have been already reduced, the latter by Dr. Auwers. Airy's Greenwich observations, the Paris observations from 1800 as reduced by Leverrier, and Bessel's Königsberg observations, will need no discussion except that necessary to reduce them to the adopted standard system. The re-reduction of Piazzi's Palermo observations, 1791–1813, is in hand, but it is not yet decided as to whether Taylor's Madras observations should be included. (III.) The computation of tabular places of the planets from Leverrier's tables up to the year 1864—the most laborious and difficult part of the work—is in a fairly advanced state. (IV.) The final discussion of the results. Prof. Newcomb estimates that the equations of condition for correcting the elements of the four inner planets will be ready for solution towards the end of 1889, but they will involve extended discussion and comparison in order to get the results in the final form for publication. Of the work on the four outer planets, Uranus and Neptune are yet untouched; but Mr. Hill's new theory of Jupiter and Saturn is in the hands of the printer, and Mr. Hill is now engaged in the construction of the tables and ephemerides for finally correcting their elements. In connection with the lunar theory, the principal work on hand is the comparison of Hansen's tables with observed occultations since 1750. Another branch of the planetary work is the determination of the mass of Jupiter from the motions of Polyhymnia: the perturbations of the planet have been computed from its discovery in 1850 to October 1888, and the work awaits the observations during the approaching opposition to be brought to a final discussion.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 FEBRUARY 19-25.

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on February 19

Sun rises, 7h. 9m.; souths, 12h. 14m. 6·2s.; sets, 17h. 19m.: right asc. on meridian, 22h. 9·7m.; decl. 11° 22′ S. Sidereal Time at Sunset, 3h. 15m.

Moon (at First Quarter February 20, 2h.) rises, 10h. 25m.; souths, 17h. 46m.; sets, 1h. 18m.*: right asc. on meridian, 3h. 42·8m.; decl. 14° 34′ N.

Planet.	Rises.	Souths.	Sets.	Right asc. and declination on meridian.
	h. m.	h. m.	h. m.	h. m. ° ′
Mercury..	7 29 ...	13 17 ...	19 5 ...	23 13·3 ... 3 7 S.
Venus.....	5 39 ...	9 49 ...	13 59 ...	19 44·5 ... 20 46 S.
Mars	22 38*...	3 57 ...	9 16 ..	13 51·7 ... 8 43 S.
Jupiter....	2 3 ...	6 17 ...	10 31 ...	16 12·1 ... 20 12 S.
Saturn....	14 20 ...	22 16 ...	6 12*...	8 13·4 ... 20 29 N.
Uranus ...	21 36*...	3 9 ...	8 42 ...	13 3·3 ... 6 0 S.
Neptune..	10 5 ...	17 45 ..	1 25*...	3 41·7 ... 17 56 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Occultations of Stars by the Moon (visible at Greenwich).

Feb.	Star.	Mag.	Disap.	Reap.	Corresponding angles from vertex to right for inverted image.
			h. m.	h. m.	° °
20 ...	Aldebaran	... 1 ...	15 56	near approach	346 —
21 ...	119 Tauri	... 5½ ...	19 10	near approach	7 —
24 ...	*d'* Cancri	... 6 ...	21 30	... 22 28 ...	112 226

Feb.	h.	
23 ...	1 ...	Mercury stationary.
24 ...	20 ...	Saturn in conjunction with and 1° 22′ north of the Moon.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·4 ...	81 16 N. ...	Feb. 19,	19 38	*m*
			,, 24,	19 17	*m*
R Arietis	2 9·8 ...	24 32 N. ...	,, 22,		*M*
R Tauri	4 22·2 ...	9 55 N. ...	,, 21,		*M*
R Leporis	4 54·5 ...	14 59 S. ...	,, 23,		*M*
R Canis Majoris...	7 14·5 ...	16 12 S. ...	,, 21,	20 26	*m*
			,, 22,	23 42	*m*
δ Libræ	14 55·0 ...	8 4 S. ...	,, 22,	1 58	*m*
U Coronæ	15 13·6 ...	32 3 N. ...	,, 21,	21 51	*m*
U Ophiuchi ...	17 10·9 ...	1 20 N. ...	,, 19,	3 2	*m*
		and at intervals of		20 8	
X Sagittarii	17 40·5 ..	27 47 S. ...	Feb. 19,	2	0 *M*
W Sagittarii ...	17 57·9 ...	29 35 S. ...	,, 19,	4	0 *M*
Z Sagittarii... ...	18 14·8 ...	18 55 S. ...	,, 25,	1	0 *M*
β Lyræ...	18 46·0 ...	33 14 N. ...	,, 24,	0	0 *M*
S Sagittarii	19 12·9 ...	19 14 S. ..	,, 24,		*M*
δ Cephei	22 25·0 ...	57 51 N. ...	,, 20,	2	0 *M*
			,, 23,	20	0 *m*

M signifies maximum; *m* minimum.

Meteor-Showers.

	R.A. °	Decl. °	
Near β Trianguli... ...	30 ...	35 N. ...	February 24.
From Canes Venatici ...	181 ...	34 N. ...	February 20.
Near δ Serpentis	234 ...	11 N. ...	Swift; streaks.
,, π Herculis	262 ...	36 N. ..	Feb. 20. Swift.

GEOGRAPHICAL NOTES.

AT Monday's meeting of the Royal Geographical Society, the paper read was by Mr. Randle F. Holme, on Labrador, which he visited in July–October of last year. Mr. Holme succeeded in penetrating into the heart of Southern Labrador, as far as Lake Waminikapou, and not far from the Grand Falls, which Mr. Holme believes will turn out to be the greatest falls in the world; but, as General Strachey pointed out in the discussion, Mr. Holme's conception of the height is probably exaggerated. Mr. Holme went from Newfoundland to Bonne Espérance on the south-east coast of Labrador, and sailing northwards touched at several points, proceeding up Hamilton Inlet and the Grand River, to the point mentioned above. Mr. Holme found many difficulties in the way, and much of the country he visited was virtually unexplored. With regard to the height of the Grand Falls, Mr. Holme states that the centre of Labrador, as is generally known, is a vast tableland, the limits of which are clearly defined, though

of course the country intervening between this limit and the coast always consists, more or less, of a slope. Roughly speaking, it may be said that in the south and north there is a more or less gradual slope from the height of land to the coast, while in the south-east the descent is sudden, and almost immediately after leaving the tableland there is reached a level which is but little above that of the sea. In the north-east portion the edge of the tableland approaches nearest to the coast, while it trends considerably to the west in the rear of Hamilton Inlet. The most fertile part of the country is that which lies between the tableland and the sterile belt on the coast, though the height of land itself is by no means a desert. On the height of land there is found a succession of great lakes joined together by broad placid streams. When the streams of water reach the edge of the tableland, they of course commence a wild career down towards the sea. In the case of the Grand River this rapid descent commences with the Grand Falls, and almost the whole of the great drop to the sea-level is effected in the one waterfall. The elevation of the Labrador tableland is given by Prof. Hind as 2240 feet. From this height the Moisie and Cold Water Rivers descend to the sea by means of a considerable number of falls. But in the Grand River below Lake Waminikapou there is only one fall, viz. that which occurs 25 miles from the river-mouth. This fall is 70 feet. It is true that the whole of the river from Lake Waminikapou to the First Falls is rapid, but there is no place where there is any considerable drop, and indeed no place where it is necessary to take the boat out of the water. Now the lake first above the Grand Falls is on the height of land. In the channels joining the various lakes above the falls there are no rapids and there is scarcely any stream. It therefore follows, assuming the elevation of the tableland on the east to be approximate to that on the south, that in the 30 miles beginning with the Grand Falls and ending with Lake Waminikapou, there is a drop of about 2000 feet. Some of this drop is probably effected by the rapids immediately below the falls, but the greater part is no doubt made by the fall itself. The river is said by Maclean to be 500 yards broad above the falls, contracting to 50 yards at the falls themselves. The interior of the country Mr. Holme found was richly wooded, and the climate mild, though the plague of flies and mosquitoes was almost intolerable. The few Indians who inhabit Labrador belong mostly to the Cree nation, and according to Mr. Holme are probably perfectly unmixed with either whites or Eskimo. As an agricultural or pastoral country Mr. Holme thinks Labrador has no future, though something may be made of its iron, of the existence of which strong indications exist. Mr. Holme's observations have enabled us greatly to improve our maps of Labrador, and the photographs he brought home give an excellent idea of the general character of the country.

OUR ELECTRICAL COLUMN.

SOME very interesting and remarkable trials of the transmission of energy were recently made between Kriegstetten and Solothurn in Switzerland, by Prof. H. F. Weber and others, when it was found that 30 horse-power put in at the first place delivered 23 horse-power at the other, 8 kilometres away—showing an efficiency of 75 per cent. The current, 11 amperes, driven under an E.M.F. of 2000 volts, showed absolutely no loss whatever, owing to the use of Johnson and Phillips' "oil" insulators. This mode of insulation proved absolutely perfect.

THE distribution of electricity for lighting purposes by means of secondary generators, is now being discussed at the Society of Electrical Engineers. This mode of working seems to have solved the question of the economical erection of conductors. Alternate currents of high tension in the main conductors allow wires of small diameter to be used, and a special form of induction coil transforms these currents of high tension, 2000 volts, to currents of low tension, 100 volts and under, for use in private houses. The system, due to Messrs. Gaulard and Gibbs, is in use at the Grosvenor Gallery installation, as well as at Eastbourne and Brighton, and is probably going to be largely used. Mr. Kapp's paper "On Alternate Current Transformers, with Special Reference to the Best Proportion between Iron and Copper," will lead to an interesting discussion. All induction coils, when used as transformers, are simply a magnetic circuit or closed iron core interlaced with an electric circuit or a closed copper one, and constructed so that the electric circuit shall embrace as many as possible of the lines of force of the magnetic circuit. Mr. Kapp divides transformers into two classes—one in which the copper coils are spread over the surface of the iron core as in a Gramme armature, and the other in which the iron core is spread over the surface of the copper coil. The former he calls "core transformers," and the latter "shell transformers." He advocates working transformers at low inductions—that is, far below the point of saturation of iron—because it increases the plant-efficiency, reduces the heat or energy lost in the iron core through hysteresis, and prevents the production of sound. The plant-efficiency of transformers sometimes reaches as high as 99 per cent., and they are perfectly self-regulating. There is very little choice between core- and shell-transformers, but the former have the advantage. Economy in construction and facility in manufacture and repair seem to be principal points of advantage to reach. It is amusing to find how, now that the system has proved to be practical, every man is devising his own transformer, and labouring to show that Gaulard and Gibbs were not the inventors of the system, and that their transformers are not the best.

PROF. EWING'S discovery of hysteresis in iron has been shown, both by Kapp and Ferrari, to play a very significant figure in the efficiency of transformers.

GUGLIELMO, of Turin, has shown that no loss of electricity takes place through moist air surrounding an aerial wire unless the E.M.F. exceeds 600 volts, after which the leakage increases with the E.M.F. and the saturation of the air.

IN Boston an electric lamp has recently been used to search for a body drowned in the harbour. The U.S. steamship *Albatross* is furnished with a full complement of lamps for fishing. The glow-lamp is encased in a wire netting, which acts as a trap. The fish, being attracted by the light, swim into the net, which is then closed and pulled in.

THE new number just issued (No. 201) of the Proceedings of the Royal Society contains the following electrical papers: "On the Photometry of the Glow-lamp," by Captain Abney and General Festing; "On the Development of Feeble Currents," by Dr. Alder Wright and Mr. C. Thompson; and "On the Heating-Effects of Electric Currents," by Mr. Preece.

MAKING GLASS SPECULA BY HAND.[1]

THE author of this paper gives a very interesting account of the construction of glass specula, discusses the actual difference in form between a spherical and a parabolic mirror, and gives an account of some experiments to determine the thickness of the silver film. In making the specula Mr. Madsen used glass for the grinding tool in place of metal, as he considered that the coefficient of expansion of iron and glass being different, greater truth would be obtained by the use of the same material for the tool, thus following the practice of Foucault and of the French opticians of the present day. When a true spherical surface was thus obtained the polish was given by rouge on pitch with a tool the same size as the mirror, and the correction of the spherical curve was obtained by a very ingenious plan of graduating the polisher in such a way that the greatest action would be on the required part of the mirror, the arrangement of the squares of pitch being such as to prevent the occurrence of rings of unequal polish. In this Mr. Madsen seems to have been most successful.

In working, the mirror was uppermost, and this is a very important point in many respects. There is no doubt that in working this way the mirror is in the condition of least strain, and if it were possible this plan should always be followed, but it is absolutely impossible to do this with a mirror much larger than the size he worked, which might almost be taken as the limiting size of mirror possible with this method of working. Short, Mudge, Herschel, and all the early workers used this plan in making their comparatively small mirrors; but since, with larger sizes, the mirrors have been worked face upwards as the only possible way, and it is to be regretted that this plan was not followed.

In discussing the actual amount of the glass to be abraded to obtain the correction, the author finds that for telescopes where the focal length exceeds twentytimes their diameter this amount is

[1] "Notes on the Process of polishing and figuring 18-inch Glass Specula by Hand, and Experiments with Flat Surfaces," by H. F. Madsen. (Journal and Proceedings of the Royal Society of New South Wales, vol. xx., 1886, pp. 79-91).

so small that it can be neglected, and that a spherical form is as good if not better than any other; and there is no doubt, for telescopes of about this ratio, Sir John Herschel is quite right when he makes the statement in "The Telescope," p. 81, "that is a good form that gives a good image; and that the geometrical distinctions between the parabola, sphere, and hyperbola, become mere theoretical abstractions in the figuring and polishing of specula." But in the case where the aperture of the mirror is about one-sixth of the focal length the distinction betwen the sphere and the parabola does exist and becomes a large quantity, which only the Foucault method of working allows to be dealt with properly. In enumerating the different plans used by opticians in getting the parabolic curve, the author is in error in stating that Lassell adopted the method of local polishing, as he always used a large tool, and got the figure by alterations of the stroke. Foucault was the inventor of the system of local polishing, and this was afterwards used by Draper, who finally rested on that as the best method of working.

The author considers that when the focal length exceeds 40 feet even with a theoretically perfect mirror the slightest touch or variation in temperature will be sufficient to destroy good definition with high powers, irrespective of the disturbing effects of the atmosphere, and he comes to the remarkable conclusion that "by decreasing the focal length the rays cross at a less acute angle, and small variations in the reflecting surface have not so detrimental an effect"—a statement that is entirely unsupported.

No actual tests of the work that the 18-inch mirrors will do are given. The experiments on the thickness of silver-on-glass films are interesting, as are also those on the effect of pressure or heat in altering the colours or colour-bands seen between two plane surfaces almost in contact. Dr. Draper, by actually weighing the amount of silver deposited on a large surface, came to the conclusion that it was about 1/200,000 of an inch thick; and the author, by comparing its thickness with the length of a wave of light, comes to about the same conclusion, and considers that by ordinary care in polishing no optical change will be produced in the reflecting surface by the film of silver deposited upon it.

The roads to success in making the mirrors of a reflecting telescope are many and various. Almost every maker in this fascinating pursuit had his own that gave to him best results. This was more particularly the case before Foucault published his most admirable memoir on the construction of silver-on-glass telescopes. In this memoir Foucault describes his method of local polishing, and the tests that can be applied to the concave surface, and a method of obtaining the true parabolic surface with absolute certainty, bringing the art of specula-making at once to a system of working by measurements in place of the old empirical process that had up to that time been in use; and everyone now uses Foucault's method of *testing* concave surfaces, and nearly everyone his plan of figuring by local polishing. Mr. Madsen gives a very interesting account of the road he took, an account that would have been much more valuable if the details of the processes used in making both the concave and the flat mirrors had been fully given, as it is now more in the improvements in these details that gain is to be looked for than in any of the main lines already known.

A. AINSLIE COMMON.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, February 2.—"On the Spectrum of the Oxyhydrogen Flame." By Profs. G. D. Liveing and J. Dewar.

In a former communication the authors described simultaneously with Dr. Huggins the strongest portion of the spectrum of water; subsequently they described a second less strong but more refrangible section of the same spectrum. M. Deslandres has noticed a third still more refrangible section. The authors now find that this spectrum extends, with diminishing intensity, into the visible region on the one hand, and far into the ultra-violet on the other. These faint parts of the spectrum they have photographed, using the dispersion of a single calcite prism and a lengthened exposure; and in the present communication they give a map of the whole extent observed, and a list of wave-lengths of upwards of 780 lines.

The spectrum exhibits the appearance of a series of rhythmical groups more or less overlapping one another, and the arrangement of the lines in these groups is shown to follow, in many cases, the law that the distances between the lines, as measured in wave-lengths, are in an arithmetic progression. M. Deslandres had previously announced that the succession of lines in several spectra, as well as in the telluric groups A, B and α of the solar spectrum, follow this law when their distances are measured in reciprocals of wave-lengths, and he has stated that the groups A, B and α have counterparts in the spectrum of water. The authors find a striking resemblance between those groups and certain parts of the water spectrum, but no exact correspondence.

Dr. Grünwald, of Prague, predicted on theoretical grounds that certain lines would appear in the spectrum of water, and the authors have found a considerable number of lines which tally closely with Dr. Grünwald's predictions, some of them, in the extremities of the spectrum, being the strongest lines observed in those regions.

February 9.—"True Teeth in the Young *Ornithorhynchus paradoxus*." By Edward B. Poulton, M.A., F.L.S., of Keble and Jesus Colleges, Oxford. Communicated by W. K. Parker, F.R.S.

This paper was a preliminary account of typical mammalian teeth developing beneath the site of the horny plates, which subserve mastication in the adult animal. In the upper jaw there are three teeth on each side: in the lower jaw two teeth, corresponding to the two posterior teeth of the upper jaw, were proved to exist, but the anterior one may be also present, for the jaws examined were not complete. The animal in which the teeth were found was about 8·3 decimetres long in the curled up attitude in which it had been received, and the larger hairs had alone appeared above the skin.

The anterior tooth of the upper jaw was long, narrow, and simple, as compared with the others; it was very fully developed, containing completely formed dentine and enamel, and its apex was nearly in contact with the lower surface of the oral epithelium. All the other teeth were broad and large, those of the upper jaw possessing two chief cusps on the inner side of the crown, and three or four small cusps on the outer side, while this arrangement was reversed in the lower jaw. Dentine was only formed upon the large cusps, and was not present upon all of these. The histological details and the manner of development appear to be precisely as in the higher Mammalia, a fact which strongly supports the identification of teeth with the placoid scales of Elasmobranchs. If teeth are so extremely ancient, then we should expect them to be unmodified in the ancestral Mammalia, although the other more recently specialized characters in the higher mammals are found in a more primitive condition in the former.

The teeth were found in some sections of the skull prepared for Dr. Parker by his son, Prof. W. Newton Parker. These sections, which had not been examined by Dr. Parker, were lent to the author, and Dr. Parker most generously encouraged the publication of the discovery, and assisted the investigation with other material.

Mathematical Society, February 9.—Sir J. Cockle, F.R.S., President, in the chair.—Messrs. A. E. H. Love and G. G. Morrice were admitted into the Society.—The following communications were made:—Further remarks on the theory of distributions, by Capt. Macmahon, R.A.—The free and forced vibrations of an elastic spherical shell containing a given mass of liquid, by A. E. H. Love.—On the volume generated by a congruency of lines, by R. A. Roberts.—Isoscelians, by R. Tucker.

EDINBURGH.

Royal Society, January 16.—Prof. Chrystal, Vice-President, in the chair.—Obituary notices of some former Vice-Presidents of the Society were read.—Prof. Tait communicated a paper by Prof. A. Macfarlane, on a problem in relationship.—Mr. W. Peddie read a paper on transition-resistance and polarization at platinum surfaces. He showed that transition-resistance increases greatly while polarization is proceeding. The ratio of the final to the initial resistance is in some cases as 2 to 1, when the electromotive force of polarization is equal to that of a Daniell cell. From his results regarding the time-rate of increase of polarization he deduced (10^{-9}) cm. as the value of the distance between the platinum and the layer of gas

condensed upon it.—Mr. Peddie also read a note showing that the phenomenon of "electric-absorption" must be exhibited if a dielectric has a film of gas condensed on its surface.—Prof. Tait communicated a paper by Mr. Albert Campbell on the change in the thermo-electric properties of tin at its melting-point. While the tin is solid its line on the thermo-electric diagram is inclined upwards. Liquefaction occurs before the line reaches that of iron. At this point the direction of the line changes and becomes nearly identical with that of iron. Thus the "specific heat of electricity" in tin changes sign at the melting-point. This shows that the loosening of molecular attraction, which occurs at the melting-point, produces the same effect in tin as is produced in iron, while still solid, at the higher of the two temperatures at which its magnetic and other properties suddenly alter.—Prof. Tait read a paper on the thermo-electric properties of Signor Battelli's iron; and showed from Mr. Omond's Ben Nevis observations that ice-crystals may, in the greater number of cases, have at least a share in the production of the observed phenomena.

PARIS.

Academy of Sciences, February 6.—M. Janssen in the chair.—Second note on the law of probabilities as applied to target-firing, by M. J. Bertrand. The paper deals specially with the objections urged by General Putz in the *Revue d'Artillerie* against the principle admitted by Poisson, and against the law of probability now generally adopted in schools of gunnery. Reference was also made by General Menabrea to the important researches of M. Siacci in this field of inquiry.—Remarks in reply to an objection raised by M. Khandrikoff to the theory of solar spots and protuberances, by M. H. Faye. During his observation of the recent lunar eclipse Prof. Khandrikoff noted some protuberances, the presence of which in the absence of spots for some days before the eclipse seemed to militate against M. Faye's well-known theory. To this objection M. Faye replied at some length, pointing out that it is partly based on a misunderstanding of the true character and bearing of his views.—On perfect numbers, by Prof. Sylvester. Recently M. Servais stated that a perfect number (if such exist) containing only three distinct prime factors is necessarily divisible by 3 and 5. It is here shown that no such number exists, the line of argument employed at the same time demonstrating the theorem that there exists no perfect number containing less than six distinct prime factors.—Observations made at the Observatory of Algiers during the total lunar eclipse of January 28, by M. Ch. Trépied. These observations comprise, among other matters, a study of the colours assumed by the lunar disk; a spectroscopic examination of the eclipsed portion of the disk; and the occultations of the stars contained in the list prepared by the Observatory of Pulkowa for the purpose of obtaining an exact determination of the apparent diameter of the moon. Communications were also received from the Observatories of Bordeaux and Nice on various phases of the same occurrence.—Ephemeris of the planet 252 for the opposition of the year 1888, by M. Charlois. The true positions, right ascension and declination, are given for the period from March 5 to March 19. At opposition (March 12) the magnitude will be 13·4.—Note on permanent deformations and thermodynamics, by M. Marcel Brillouin. Two propositions are established: (1) that for most elastics solids there exists no finite relation between the temperature t, the mechanic variable X, and the geometric variable x; (2) that for most solid bodies there exists a linear equation with total differentials between t, X, and x; or, more correctly, there exist as many equations of this class as there are independent geometric variables. In a future communication the theoretic results of this study will be announced.—Influence of diet in determining the fixation and elimination of carbon in man, by MM. Hanriot and Ch. Richet. The results are tabulated of mixed nitrogenous, fat, and feculent diets, including beef, bread, potatoes, butter, cheese, sugar, wine, and coffee, continued for a period of fifteen days.—On the presence of striated muscles in mollusks, by M. Raphael Blanchard. M. Hermann Fol's recent statement that true transversal striation of the muscular fibre is found in no mollusk, is shown to be erroneous and based on defective observation of these organisms, in some of which true transversal striation certainly occurs.—On the endomorphic modifications of the granulitic systems in Morbihan, Brittany, by M. Charles Barrois. This paper is devoted to a careful study of the remarkable endomorphic modifications and mechanical transformations of the Guéméné, Saint-Jean Brevelay, and Grandchamp granulitic formations, which traverse the Department of Morbihan in its entire length, and the typical constituents of which are: (1) zircon, apatite, black mica, oligoclase, orthose, and quartz; (2) orthose, microcline, quartz, tournaline, and white mica.—On the Senonian and Danian systems of South-East Spain, by M. René Nicklès. Without attempting accurately to determine the respective limits of these formations, the author indicates the presence of extensive marine deposits in the Devonian containing fossiliferous limestones with several species of Hemipneustes associated with large banks of Hippurites and Pironea.—General Menabrea presented to the Academy the prospectus of a new edition of the works of Galileo, in about twenty-five volumes, which is about to be issued at the expense of the Italian Government, and copies presented to all the more important public libraries.—The Administrative Commission of the Academy announces that it has decided to supply Corresponding Members with the *Comptes rendus* free of charge from January 1, 1888. Correspondents are requested to acknowledge receipt of the first number, and notify their change of address to Messrs. Gauthier-Villars et Fils, publishers, Paris.

BOOKS, PAMPHLETS, and SERIALS RECEIVED.

A Treatise on Photography, 5th edition: Capt. Abney (Longmans).—The Story of Creation: E. Clodd (Longmans).—British Dogs, parts 15 and 16: H. Dalziel (U. Gill).—Beobachtungen der Russischen Polarstation an der Lenamündung, ii. Thiel, Meteorologische Beobachtungen: A. Eigner; II. Liefg. Beobachtungen vom Jahre 1883-84: R. Lenz.—Meteorological Observations at Stations of the Second Order for the Year 1883 (Eyre and Spottiswoode).—The Geographical Distribution of the Family Charadriidæ, H. Seebohm (Sotheran).—Annuario publicado pelo Imperial Observatorio do Rio de Janeiro, 1885-86-87 (Rio de Janeiro).—Memoirs and Proceedings of the Manchester Literary and Philosophical Society, 4th Series, vol. i. No. 2.—(Manchester).—Proceedings of the Manchester Literary and Philosophical Society, vol. xxvi. (Manchester).—Zeitschrift der Gesellschaft für Erdkunde zu Berlin, Nos. 133 und 134 (Reimer, Berlin).

CONTENTS.

	PAGE
Kinematics and Dynamics. By Prof. A. G. Greenhill	361
Atlas of the Distribution of Plants	362
Our Book Shelf:—	
McCook: "Tenants of an Old Farm"	363
Daly: "Digging, Squatting, and Pioneering Life in the Northern Territory of South Australia"	363
"Photography Simplified"	363
Letters to the Editor:—	
An Explanation explained.—Prof. John W. Judd, F.R.S.	363
Reason and Language.—Dr. St. George Mivart, F.R.S.	364
Mechanical Equivalent of Heat.—Prof. Alfred Lodge	365
"Is Hail so formed?"—Cecil Carus-Wilson	365
The New Army Regulations.—Henry Palin Gurney	365
"British and Irish Salmonidæ."—Dr. Francis Day; Your Reviewer	366
Modern Views of Electricity. Part III.—Magnetism. VIII. (*Illustrated.*) By Dr. Oliver J. Lodge, F.R.S.	366
The Mechanism of the Flight of Birds. (*Illustrated.*) By Prof. E. H. J. Marey	369
Technical Education	374
Threatened Scarcity of Water. By Charles Harding	375
Professor Asa Gray	375
Notes	377
Our Astronomical Column:—	
Melbourne Observatory	381
The American Nautical Almanac Office	381
Astronomical Phenomena for the Week 1888 February 19-25	381
Geographical Notes	381
Our Electrical Column	382
Making Glass Specula by Hand. By A. Ainslie Common, F.R.S.	382
Societies and Academies	383
Books, Pamphlets, and Serials Received	384

THURSDAY, FEBRUARY 23, 1888.

PHYSICAL SCIENCE AND THE WOOLWICH EXAMINATIONS.

SINCE the appearance of our article of January 26 considerable interest has been manifested in this question, and during the past week important communications on the subject have come from the Secretary of State for War, and from the head master of Clifton College. We learn from Mr. Wilson's letter to the *Times* that the new regulations are not only calculated to do harm by the discouragement of science, but that they are also retrograde in another very important particular. By increasing the range of the obligatory examination in mathematics, though they will not very greatly affect the selection of candidates, yet, in the case of very many of them, by compelling wider and less thorough study, they will damage the training in that subject.

In answer to a question put by Mr. Howorth, in the House of Commons on February 15, the Secretary of State for War is reported to have said that the new regulations are intended to encourage those subjects which it is believed can be least easily crammed; to give preponderance to those subjects which are to the majority of officers of greatest practical importance; and that the new regulations are to be of permanent application.

Are the regulations calculated to achieve these purposes? We think it can be shown very clearly that they are not. It is therefore with renewed hope that discussion will lead to their amendment that we enter upon the following examination of them.

(1) The new regulations are intended to encourage subjects which it is believed can be least easily crammed. Mr. Wilson, as we have already pointed out, has indicated that in the case of mathematics they will distinctly tend to encourage quantity at the expense of quality. With regard to science subjects, the examination statistics which we published in 1884 clearly proved that experimental science was not then chosen by candidates on account of susceptibility to cram, for it was at that time less frequently selected than any other subject by successful candidates. During the three or four years that preceded 1884, a branch of experimental science was offered by only 22 per cent. of the successful candidates; since that date the numbers have risen, notwithstanding the increased thoroughness of some parts of the examination; and in 1887 about 38 per cent. of the successful candidates offered a branch of experimental science. This development is noteworthy, and may be considered to indicate an increased appreciation of the value of such work by teachers and students, since it has taken place in spite of the subject being rather a bad than a good one from the mark-winning point of view, and also during a period notable for improvement in some parts of the examination. Geography and geology, which may be, as some hold, more susceptible of cramming than chemistry and physics, show no corresponding tendency. In 1887 this subject was taken up by a distinctly smaller proportion of successful candidates than in the years that preceded 1884.

There does not appear, then, to be any justification for treating science as a subject more easy to cram than others that are more favourably treated. Had it been true that it is so, it would have been selected by a greater proportion of candidates formerly, and it would certainly have been discouraged by the nature of the examination during the last few years.

(2) Are the subjects selected those most calculated to be of practical importance to a majority of officers in the Engineers and Artillery? A flood of light is thrown upon this point by the course of instruction given to the cadets in the Royal Military Academy.

During the first year of training at Woolwich, cadets will study in the compulsory courses the following subjects:—

Mathematics,	for which	3000	marks are given.
Field Fortification,	,,	2000	,, ,,
Military Topography,	,,	2000	,, ,,
French or German,	,,	1000	,, ,,
Chemistry and Physics,	,,	1000	,, ,,
Model Drawing,	,,	300	,, ,,

During the second year of training, the cadets are divided into two classes. Those who are selected for the Engineers will then study, in addition to purely professional subjects—

Mathematics,	for which	2000	marks are given.
Chemistry and Physics,	,,	1000	,, ,,
Freehand Drawing,	,,	1000	,, ,,

In the case of the Artillery cadets during their second year, chemistry and physics alone of the ten or eleven subjects examined in the entrance competition are considered to be of sufficient practical importance to be retained.

Thus it stands admitted by the military authorities, according to their own regulations for the education of cadets, that, of the subjects examined in the competition for Woolwich, experimental science stands next to mathematics as a subject of practical importance in the training of officers for the scientific branches of the army.

That very great weight should be given to mathematics and modern languages in the examinations for Woolwich cadetships is obviously proper; but since it is admitted, by the courses of instruction in the Royal Military Academy, that capacity for and extensive training in experimental science form part of the necessary equipment of an officer of Engineers or Artillery, a system of selection which includes no means whatever of securing youths capable of such studies in the preliminary examination, and which places youths of scientific power at such considerable disadvantages in the competitive part of the examination, plainly needs to be amended, in the interest both of the service and of the candidates. We do not doubt that those who get into the Academy will be excellently taught there, but under these regulations many will be rejected who are eminently fitted to do well, in favour of others who are less gifted with the qualities that are admittedly most valuable.

The Committee who framed these regulations has, we fear, forgotten that the Professors at Woolwich will not create a capacity for science work by the mere teaching of science to the successful cadets, and that the utmost they can do in the case of those whose talents are linguistic rather than scientific,[1] will be to compel them to acquire by hard, uncongenial labour the necessary minimum of knowledge that is required in the subsequent examinations. It is abundantly

[1] The enormous value to be given to modern languages is likely to result in many such winning admission to Woolwich in future.

evident that the War Office Committee has made a serious mistake. The new regulations, so far as experimental science is concerned, are needless as precautions against cramming; they will not give due weight to the subjects which are, by their own showing, of most practical importance to officers; and they will influence most unfairly the selection of candidates by giving no chance for scientific power to tell in the results of the examinations.

There is another side of this question which is of very great public importance, viz. the influence of these and other examinations on school work in general. Regulations such as those now in force at Sandhurst, and those about to come into operation at Woolwich, make it increasingly difficult for science subjects to maintain their proper place in schools where they are already adopted, and hinder their adoption elsewhere. Many of the ablest youths in our public schools enter as competitors in these and other examinations, and as they must offer the subjects that pay best, such regulations as those under discussion lower the general standard of school work by constantly withdrawing from the science classes a large proportion of the best students. At present good work in science pays less well very often than mediocrity in other subjects. This, as was pointed out by Sir Lyon Playfair in his Presidential Address to the British Association in 1885, helps to arrest progress in science teaching. We do not, of course, claim that the interests of science in schools should be allowed to outweigh the manifest needs of the public services. But the fact that public examinations exercise a potent influence, not only on the education of the candidates, but also on the general tendency of school work, throws great responsibility on those who control them, and makes it our duty to urge that this influence shall not be lost sight of, especially when, as in the case of Woolwich cadets, an aptitude for experimental science is admittedly a quality that will be of great practical value in the professional work of the successful competitors.

THE MOTHS OF INDIA.

A Catalogue of the Moths of India. Compiled by E. C. Cotes, First Assistant to the Superintendent, Indian Museum, and Colonel C. Swinhoe, F.L.S., F.Z.S., &c. Part I. Sphinges. Part II. Bombyces. (Calcutta: Printed by the Superintendent of Government Printing, 1887.)

IT is not too much to say that the task of writing a catalogue of the moths of India is one which might appal an entomologist of far longer experience than either of the authors of this work. For when we consider that no general catalogue or revision of the Heterocera exists more recent than that of Guénée, that almost the whole of the types of the described species are in England, whilst both the authors of this book are in India, and that the number of Indian moths is so great that in the two first families alone upwards of 1600 species or supposed species are catalogued, it is evident that the difficulty of such a work is enormous; and as the authors are not known as lepidopterists of long standing, and are resident on opposite sides of India, no one would expect too much from the first attempt at what has long been very much wanted—namely, some work which would enable the rapidly-growing circle of working naturalists in India to know what has already been described and where the descriptions have appeared.

I think, therefore, that the cordial thanks of all will be given to Colonel Swinhoe and Mr. Cotes for their bold attempt to fill this blank, and that no one will be too critical as to how their task has been done when the great difficulties under which they labour are remembered. There is not a word of introduction to say to what extent either of the authors is responsible for the work, but I believe that Mr. Cotes is really the compiler, and that Colonel Swinhoe, whose collection is much richer than that of the Indian Museum in the species which occur in Western India, has added such additional species and notes as he possesses.

The plan of the work is nothing more than a bare catalogue of names and references, with localities so far as known to the compilers or to the authors of these names; and, as we see that in some genera almost all the species are unknown to either of the authors except from the descriptions or plates, it is evident that a large proportion of the names are names and nothing more.

In the genus Syntomis, for instance, we find forty-two supposed species catalogued, of which fifteen are described by Moore, nine by Walker, and ten by Butler; of all these only eleven are in Colonel Swinhoe's collection, and thirteen in that of the Indian Museum, and we do not find that a single attempt has been made to discover how many of these forty-two names represent distinct species.

As long as authors continue to do as Messrs. Moore, Butler, and the late Mr. Walker have so freely done—namely, to describe anything they do not personally know, with little regard to what has already been described—it is evident that, when their views as to variation are also extremely narrow, a great many synonyms must result, and we think a little genuine work would tend to show that of the forty-two supposed species of Syntomis not more than perhaps twenty really exist in nature. It is, however, quite as probable that while not more than twenty distinct species are described from India, at least twenty more remain undiscovered, for it is hardly possible for anyone who does not know India personally to understand how infinitesimal our knowledge of the moths is, except in some half-dozen localities like Bombay, Calcutta, and Sikkim; and even in such places as these what we know is but little compared to what we do not know. Surely here is a field for study and amusement which must attract many who will, sooner or later, provide the materials and collect the knowledge necessary for a "Catalogue *raisonné*," but the sooner a good example is set, by the careful and scientific description of the genera and species which are known, with due regard to distribution and variation, the more and better will be the work done.

A book is projected by Mr. F. Moore, whose knowledge of Indian moths is certainly greater than that of all other entomologists combined; but it is sincerely to be hoped that he will not adopt such a plan or style of work as his recently published "Lepidoptera of Ceylon." The bulk and cost of such a work on the Lepidoptera of India would quite prevent its use by those most likely to use it to advantage, and even if it was completed in

his lifetime the earlier parts would be out of date before the last were published.

What is wanted is something like Stainton's "Manual of British Lepidoptera," together with a serial publication which would give such a medium to entomologists for publishing their discoveries as *Stray Feathers* gave to Indian ornithologists. When such a journal has been going on for twenty years or so, it will be time to think of a Catalogue of the moths of India really worthy of the name. At present such an ambitious scheme as that proposed by Mr. Moore seems to me only likely to stand in the way of something better hereafter.

It is a great pity that no references are given in this Catalogue to the descriptions of the very numerous genera, so many of which are the creation of Mr. Moore. How many of them will eventually stand, time alone can show, but certainly many of them will merge in genera known in other parts of the world besides India.

I think also that if the authorities for the localities given for the various species were stated, as has been done in the case of specimens in the Calcutta Museum, this would be a great addition to the Catalogue. It is quite as important to know who collected a particular species as to know in whose collection it exists; and many localities are given without any good authority.

Another improvement in the form of the work would be an abbreviation of the references, in the same way as is done in Standinger's "Catalogue of European Lepidoptera"; a short bibliography of works cited, and their abbreviated citations, will take away any possibility of doubt, and save innumerable repetitions of such references as

"Walker, Cat. Lep. Het. B. M.,"
"Moore, Proc. Zool. Soc. Lond.,"
"Felder, Reise Novara Lep.,"
"Butler, Ill. Typ. Lep. Het. B. M.,"

which might be reduced with advantage to

"Walk., B.M.,"
"Moore, P.Z.S.,"
"Feld., Nov.,"
"Butl., B.M."

But notwithstanding the imperfections of this Catalogue, its publication will be a real blessing to naturalists, if only by saving them an immense deal of the most tedious, troublesome, and unsatisfactory work—the hunting up of descriptions and references. How far these are correct, I have not as yet been able to prove; but the few omissions which I have discovered may easily be forgiven.

H. J. ELWES.

PROLEGOMENA TO THE STATISTICS OF THOUGHT.

Die Welt in ihren Spiegelungen unter dem Wandel des Völkergedankens. Prolegomena zu einer Gedankenstatistik. ("The Universe as reflected in the Movement of Thought among the Races of Mankind. Prolegomena to the Statistics of Thought.") By A. Bastian. One vol. in 8vo, with an Atlas or Ethnographical Picture-book in oblong folio. (Berlin: E.S.Mittler, 1887.)

DR. BASTIAN'S idea is that the new science of ethnology supplies materials from which it is possible to construct a system of psychology on the inductive methods of natural science. The inductive study of the material universe has given us our modern science, and with it modern materialism. But materialism, says Dr. Bastian, is but a one-sided expression of the legitimate tendency of the age towards induction and natural science. It overlooks the fact that the world of ideas offers as legitimate a field for the application of scientific method as the world of material phenomena. Ethnology, which considers man not as an individual, but in his social aspects, teaches us that the universe of thought also obeys laws, and can be studied by the genetic method. And therefore Dr. Bastian desires to see the statistics of thought put together in a way that will exhibit the whole range of ideas about the universe and its contents which have been prevalent among the various races of men. These statistics will form the basis for a psychology constructed on inductive principles.

The description of a science which has still to be created must necessarily be vague and hazy, and in the present case the vagueness is increased by the fact that Dr. Bastian writes in a very involved and enigmatic style, so that his meaning cannot be read, but must be divined. But so far as we have been able to follow him we gather that in his "Prolegomena to the Statistics of Thought" he designs something different from what is given in ordinary Prolegomena, and that the volume should rather be called a provisional collection of materials for the comparative study of the ideas entertained by different races, or in different stages of culture, as to the universe and the leading matters of human interest that it contains. It would seem that Dr. Bastian, whose great range of knowlege in matters ethnological is well known, and who is also a voracious and somewhat undiscriminating reader of books on all possible subjects connected with the history of human thought, has accumulated huge commonplace-books to illustrate his favourite project. The small-type sections which make up a large part of the volume are simply chunks from these note-books, to all appearance entirely undigested. Commonplace-books have always a tendency to become chaotic, especially in the hands of a man who reads so widely and miscellaneously as Dr. Bastian has done; but we have never seen anything quite so formless as these pages. In themselves many of the extracts given are interesting or curious, but the disorder in which they stand is simply bewildering. Moreover, there are no exact references to chapter and verse of the authors quoted, and verbal excerpts stand side by side with brief jottings and condensed indications such as a man may make for his own use, but which are so many enigmas to the reader. There has evidently been no verification and no revision of the notes originally made by the author for himself, and many of them, therefore, are not only obscure, but not quite accurate; while others were not worth printing at all. The last remark is specially applicable to a vast number of quotations from ancient and modern metaphysicians, into whom Dr. Bastian has evidently dipped at hazard, without having any clear conception of the history of philosophy as a whole. On half a page we find Proclus, Anaximander, Philolaus, Aristotle, the Pythagoreans, F. A. Müller, Spencer, Schelling, Samuel ben Gebirol, and Anaxagoras. Who can hope to be instructed by such a

jumble? The large-type sections that connect these masses of confused notes are still more perplexing. Here, also, the commonplace-book predominates, but the extracts are worked up into some semblance of a continuous exposition. It is very seldom, however, that one can read a page on end without losing the thread. The reason soon becomes obvious. What is offered as a book is really nothing more than a transcript of rough jottings, in which Dr. Bastian had from time to time recorded his ideas in a form just sufficient to preserve a record for his own use. The sentences are often not even grammatical, and in brief the volume is only the roughest of rough note-books printed without revision. In spite, therefore, of the enormous labour and learning which it attests, the whole must be pronounced a failure, for the elementary reason that it is not a book. We trust that the publication may be useful to the author in helping him to get his superabundant material better under control, and so to produce hereafter something that is a book and can be read.

The ethnological picture-book is designed for young people, and its illustrations of cosmogonic and cosmographic ideas, of various conceptions of the future life and so forth, are well calculated to excite their curiosity and stimulate their interest in such things.

OUR BOOK SHELF.

Experimental Chemistry for Junior Students. By J. Emerson Reynolds, M.D., F.R.S. Part IV. Organic Chemistry. (London: Longmans, Green, and Co., 1887.)

THIS volume on organic completes the author's course of experimental chemistry. Whatever may be the opinions on the three previous volumes, there is no doubt this is the most rational attempt to treat organic chemistry practically—as a thing for students actually to do—that has as yet appeared. There is scarcely an experiment in the book that a student will be unable to do from the description given, and the order in which they are taken and general arrangement is the natural order of synthesis, proceeding from the less complex and easy to the most complex and least known.

The author begins with destructive distillation, and the production of alcohols, their salts, &c. The fourth chapter deals with metallic compounds or organo-metallic bodies. In the description of the manufacture of zinc ethide the method of making from zinc, C_2H_5I, and iodine might have been given, as the action is much quicker than with the Cu—Zn couple and the yield greater. The current of CO_2 can also be dispensed with advantageously. Two experiments here we must take exception to as being rather dangerous for beginners—sealing up sodium with zinc ethide, and in Experiment 691 making mercuric ethide as a sort of starting-point material. The author cautions against inhaling the vapour of this substance, as it is "supposed to be poisonous." We thought it was quite settled that it is about the most dangerous substance one has to deal with; and we certainly do not agree with the author that the method of employing mercuric ethide for making zinc ethide is the easiest of all methods for making the last-named substance.

In the remainder of the little book there is nothing either in arrangement or process to which objection can be taken, and undoubtedly it should be most useful to students attending a course of organic lectures. As a rule English students stop off with organic before they have really made its acquaintance; very few indeed continue its study long enough for it to be of any use to them. Most of the works in England where "organic chemistry" is the rule are obliged to obtain the services of German chemists; the English student's acquaintance with the subject generally stopping at the knowledge that there are such things as hydrocarbons, or "hydrocarbides," as the author of this book calls them.

Perhaps when such practical instruction is given in our schools as the course outlined by this book, we may begin to produce students who can go into "a works" and be trusted not only to follow a process but to originate new ones. W. R. H.

The Farmer's Friends and Foes. By Theodore Wood, F.E.S. (London: Swan Sonnenschein, Lowrey, and Co., 1888.)

THIS is a well-meant and well-put-together little volume, giving an account of the life-history of most of the animals which, for good or for evil, come across the path of the British agriculturist. Throughout, the attempt is made to prove that, when it is necessary for the saving of a crop to destroy any animal, it is far better to trust to Nature, as being more competent, than to man; but then this seems to beg the whole question, as it presumes that man has not already very much interfered with Nature's regulations.

The volume is, in part, the result of personal investigation, but the author quotes freely from all our best-known writers on the subjects of which he treats.

The figures are good. A table of contents would have added to the usefulness of the work, especially as the index is not very detailed. The volume may be safely placed in the hands of all interested in the subject.

The Story of Creation. By Edward Clodd. (London: Longmans, Green, and Co., 1888.)

THE author of this book does not pretend to make his readers acquainted with new facts and ideas. His object is to present a popular exposition of the theory of evolution, using the word evolution in its widest sense. The work is divided into two parts—one descriptive, the other explanatory. In the descriptive part he begins with a chapter on matter and power. He then considers the distribution of matter in space, and gives a general account of the sun and the planets, of the past life-history of the earth, and of present life-forms. In the explanatory part he discusses the questions relating to inorganic evolution and to the origin of life and life-forms, and sets forth in logical order the arguments which are held to establish the truth of Darwin's theory of the origin and development of species. A final chapter is devoted to social evolution, including the evolution of mind, society, language, art and science, morals, and theology. The book is vigorously written, and well illustrated; and readers who have had no special scientific training will find that it enables them to understand and appreciate some of the greatest and most fruitful generalizations of modern science.

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.*]

Botanists and the Micromillimetre.

I NOTICE that in a review of a "Manual of British Discomycetes" which appeared in NATURE on February 9 (p. 340), and apparently also in that work itself, the word *micromillimetre* is used as equivalent to the thousandth of a millimetre.

I have made some inquiry, and am told that it is now commonly employed by biologists, and especially by botanists, with that meaning.

As it would be very unfortunate if the same word were habitually used in different senses by students of different branches of science, may I be allowed to point out that, according to the definitions of the C.G.S. system, a micromillimetre is the millionth of a millimetre.

In the well-known Report of the Committee of the British Association for the "Selection and Nomenclature of Dynamical and Electrical Units" it is laid down that the prefixes *mega* and *micro* are to be employed "for multiplication and division by a million."

This ruling has been generally accepted not only by scientific men, but also by those engaged in commerce. Megohm and microfarad are terms which are used in contracts, and are universally understood to mean a million ohms and a millionth of a farad respectively. It will be hopeless to try to introduce scientific systems of measurement into the affairs of daily life if scientific men themselves disregard the rules on which those systems are framed.

It would also be particularly confusing if the micromillimetre were wrongly used by microscopists. In its proper sense it is the most convenient unit in which to express molecular magnitudes. It has been employed for that purpose by Sir William Thomson and others in England, and also by physicists abroad. If the micromillimetre of the microscopist is 1000 times too large, all sorts of mistakes will be rife as to the relative dimensions of molecules and of the smallest objects visible with the microscope.

The proper name for the thousandth of a millimetre (μ) is the *micrometre*, and though the similarity of this word to *micrometer* is no doubt a drawback, it is not likely that confusion could often arise between them.

If, therefore, I am rightly informed as to the custom of botanists in this matter, I beg respectfully to suggest that they should bring their nomenclature of units of length into conformity with the definitions of the C.G.S. system. Otherwise there will be a permanent confusion between the *micrometre* (μ) and the micromillimetre ($\mu\mu$). ARTHUR W. RÜCKER.

Science Schools, South Kensington, February 17.

"The Teaching of Elementary Chemistry."

ALLOW me to draw the attention of the chemical section of your readers to a few highly misleading passages in the two books reviewed under the above heading in NATURE of January 19 (p. 265).

On p. 65 of the "Elementary Chemistry" we read:—

"Hence when sodium and water interact, a portion of the hydrogen which was combined with oxygen is evolved as hydrogen gas, and another portion enters into combination with the sodium and the oxygen to produce caustic soda."

On pp. 116–17 is to be found the following astounding passage:—

"To prepare Cl_2O, mercuric oxide (HgO) is heated in a stream of dry chlorine. When mercuric oxide is heated it is decomposed into mercury and oxygen; therefore by passing chlorine over heated mercuric oxide we carry out a reaction in which oxygen is produced in presence of chlorine."

In the "Practical Chemistry," under Experiment 1, Chapter II. (p. 6), occurs the following warning to the student:—

"Do not remove the lid at any time for more than a second or so, else some of the magnesia will be volatilized and lost;" while on p. 285 of the "Elementary Chemistry" we read:—

"No compound of Mg has been gasified."

Even this contradiction is excelled by one on pp. 62 and 63 of the "Practical Chemistry," which is not so manifestly a slip. We read (p. 62):—

"The reactions between aqueous solutions of alkalies and the three elements, chlorine, bromine, and iodine, are similar; compounds of similar compositions and similar properties are produced under similar conditions."

Under Experiments 19 and 20, which follow, the student is told to treat cold solutions of potassium hydroxide with chlorine and bromine respectively. The well-known changes are described, and the bleaching properties of the solutions after addition of a little acid, are to be tried. Then follows (p. 63):—

"Exp. 21. Perform an experiment similar to 19 and 20, but use *iodine* in place of chlorine or bromine: the liquid which is produced does not bleach. No compound of iodine analogous to KClO and KBrO has been obtained."

Truly this is "seeing things as they are" with a vengeance.

Z.

Natural Science and the Woolwich Examinations.

MAY I be allowed, as one of the most experienced science masters in the public schools, to say a word in reply to Mr. Gurney's letter in NATURE of this week (February 16, p. 365)?

There seems to me a general fallacy running through that letter arising from "the absence (on the writer's part) of clear discrimination" between the nature and methods of mathematical science (which, as J. S. Mill taught us long ago, are mainly *deductive*) and experimental science (which proceeds by *inductive* methods). It is on this ground mainly, coupled with the extent to which it cultivates the faculty of *observation*, that we claim for it a special educational value.

After an educational experience at least as extensive as Mr. Gurney's, I join issue with him most distinctly on this point. I am afraid there lurks behind Mr. Gurney's depreciation of the educational value of science the disappointment which other mathematicians have experienced in finding that the man who takes to experimental science as a mere excursion-ground for the diversion of the mathematician is not infrequently brought to confusion by Nature. *Science is something more than measurement.* Mr. Gurney's notion that mathematics and a knowledge of French and German are a sufficient groundwork for true scientific knowledge is such a confession on his part of the small value he attaches to experimental demonstration and to laboratory training (or to field-work in the case of geology) as is sufficient to put him out of court as a witness on this question. Nor do I think that he is competent to speak with any authority on the work done in the public schools. If he fancies that the best boys of the public schools go to Powis Square to finish their education, he is labouring under a delusion.

The whole argument of the letter is retrogressive; nor is it strengthened by the writer's condemnation of a "smattering" of science, which is no discovery. But I maintain that a boy can, by the age of eighteen, get a sound groundwork in science laid, though not by the cramming system; and that to this the term does not apply at all. Again, he condemns premature specialization of a boy's studies in favour of science, while he inconsistently advocates a much narrower specialization in favour of mathematical studies.

How far Mr. Gurney is from being convinced by his own arguments is shown by the fact that in the concluding paragraph of his letter he practically surrenders the point on which the whole question turns.

In conclusion I would commend to his careful consideration the letter which appears in the *Times* to-day from the head master of Clifton College, whose competence to form a judgment on the educational aspect of this question I suppose no one doubts. A. IRVING.

Wellington College, Berks, February 17.

WITH your kind permission, and in consideration of the importance of the matter, I crave leave for space for a few remarks in addition to those contained in my reply to Mr. Gurney.

(1) Looking at the history of education in this country, we can account for, though we deplore the existence of, a prevalent notion, a sort of fashionable superstition, which regards scientific studies as outside the range of what is called "culture"; a superstition for which some of those who have spoken in the name of science are not altogether unanswerable, but which derives its chief strength from that profound ignorance of natural science—its nature, its methods, and its object—upon which so large a proportion of educated Englishmen seem rather to pride themselves than otherwise. There can be but little doubt that this has been turned to account as an influence in favour of the contemplated scheme.

(2) It is in no spirit of hostility to literary studies that one writes in these terms; on the contrary, it is as a lover of literature of the better sort that one would gladly see the literary spirit in this country, as in Germany, strengthened and braced by the strong atmosphere of scientific criticism, and a little more first-hand acquaintance with things as they are, which is the true

aim of science. It is impossible to estimate the good that might be done in this direction, if only the Universities had the wisdom and the courage to insist upon a knowledge of some one branch of science for all degrees, as was strenuously advocated years ago by Charles Kingsley.

(3) It seems a great pity that such a change as is contemplated should be adopted just now, since within the last three or four years some of us who are teachers of standing and experience have gladly recognized considerable improvement in the examinations in science as they are conducted by the Civil Service Commissioners. It would appear that the *cramming* of these subjects has been considerably handicapped, if one may judge from the considerable increase in the number of o's affixed to the names of candidates in the published lists *pari passu* with a considerable increase in the number of marks gained by one's own pupils, who have had the same teaching and laboratory training as those of previous years.

(4) It is surely fairly within the province of Parliament to consider the question whether it is expedient or conducive to the common weal, that science shall be placed at such a disadvantage that young men, who are candidates for the more scientific branches of the military service, shall be strongly tempted to eschew all preliminary training in science, as they certainly will be unless the regulations are somewhat modified.

Four years ago the action of a single Member of Parliament (Sir John Lubbock), backed up by the influence of the Councils of the Royal Society and the British Association, was effectual in securing a reconsideration of the provisional examination scheme for admission to Sandhurst; so that, although—as ultimately issued—the regulations contained an absurd anachronism in the proportion of marks assigned to scientific subjects, this was reduced to less startling proportions.

Can it be doubted, then, that if on the present occasion the three Members of Parliament who may be said to be the representatives *par excellence* of science in the Legislature (the President of the Royal Society, the President of the British Association, and the Member for the University of London), were to take united action in Parliament, the position of science (so far as the Army Entrance Examinations are concerned) might be changed from one of semi-strangulation to one of free and fair competition, which is all that its most ardent advocates can desire for it? This could be effected to the advantage of the studies of the cadet, by such a simple modification of the published regulations as would be involved in limiting the choice of optional subjects in Class I. to two, and allowing two subjects of Class II. to be taken up.

A. IRVING.

Wellington College, Berks, February 20.

The Composition of Water.

PROF. THORPE, in his interesting article on the composition of water (p. 313), alludes to Dr. Scott's very valuable determinations of the ratio of the volumes of hydrogen and oxygen which combine to form water.

If we assume with Dr. Scott that the small amount of impurity present in his gases, and which he estimated in each case after the explosion, was evenly distributed between them, a curious relation may be observed between the amount of impurity present and the calculated ratio of the volumes.

This relation is apparent if we subtract the ratios calculated by Dr. Scott from some fixed number, say 2·000, and compare these differences with the relative amounts of impurity. It will be seen from the following table that the greater the amount of impurity present the greater is the difference of the ratio from the constant number, or, in other words, the lower is the ratio. The impurity is given in volumes per 100,000, and the differences are multiplied by 10,000.

Relative impurity.	Difference of ratio from constant.	Relative impurity.	Difference of ratio from constant.
35	83	102	125
36	55	105	133
41	66	106	140
47	44	146	154
52	32	162	106
56	90	167	224
66	72	188	118
72	46	254	187
75	80	495	540
78	120	498	506
98	73		

The relation is better seen, however, by mapping the results, taking the ratios as abscissæ, and the impurity in volumes per 100,000 as ordinates. The dotted straight line (Fig. 1) passes so well through the points that it leaves ten of them on the one side and eleven on the other.

It seems difficult to believe that this apparent relation can be merely a chance coincidence; the direction taken by the points is too definite. Nor can it well be due to any chemical action between oxygen and nitrogen, for Dr. Scott states that the water produced was free from any acid reaction, and that no trace of the oxides of nitrogen could be detected. The relation is even more marked if we assume that the whole of the impurity was in the hydrogen. This is shown in Fig. 2; the points obviously fall about a line which is nearly, if not quite, straight.

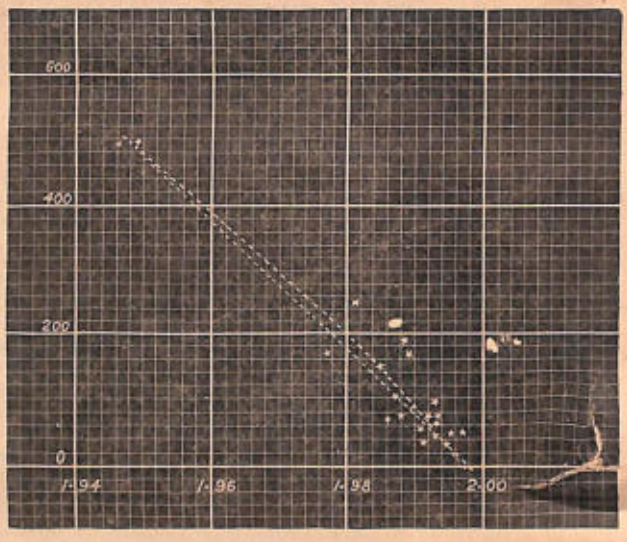


FIG. 1.

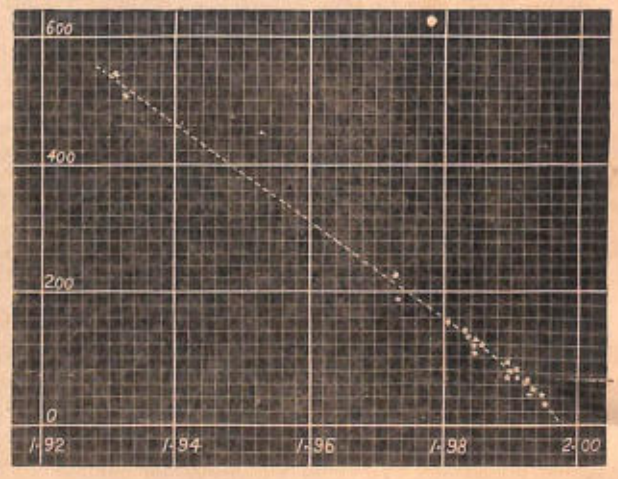


FIG. 2.

If, however, we assume that the whole of the impurity was in the oxygen, and if we neglect the two experiments with the excessive amount of impurity, no such relation is to be observed (see Fig. 3), but the ratios are distributed with fair regularity about a mean value of 1·9965 or 1·9970. The simplest explanation of the facts appears to be that the whole, or at least the greater part, of the impurity was really in the oxygen, and that the apparent relation of the amount of impurity to the ratio

is due to the error introduced into the calculations by referring the impurity to the hydrogen. But, whether this explanation be accepted or not, it is clear that the three lines drawn through the points representing the three series of ratios ought to meet at a point on the horizontal line of zero impurity, for the errors, whether due to chemical action or to calculation, would disappear with the cause that produced them. Hence, if no other source of error is present, the true ratio may be found by taking the most probable point of intersection of the three lines on the horizontal line of zero impurity. It is not easy to determine exactly the position of this point: it probably lies between the values 1·996

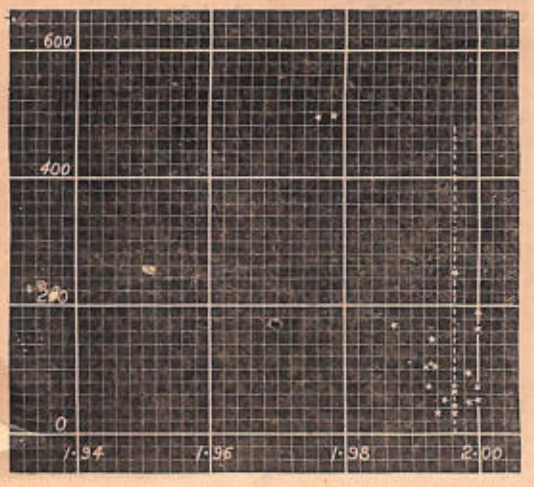


FIG. 3.

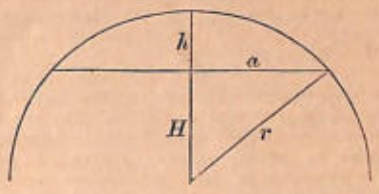
and 1·998, and the true ratio may perhaps be taken as 1·997 Dr. Scott adopts the ratio 1·994, but this appears to me to be certainly a little too low.

Prof. Thorpe shows that the atomic weight of oxygen, calculated from Regnault's densities of oxygen and hydrogen, corrected by Prof. Le Conte, and Dr. Scott's ratio (1·994) for the combining volumes, is 16·029. The ratio 1·997 would make the atomic weight O = 15 985. SYDNEY YOUNG.

University College, Bristol.

The Fog Bow and Ulloa's Ring.

IN the summer of 1875, I made a tour of inspection to our meteorological stations in the surveying-steamer *Hansteen*, Capt. M. Petersen, R.N. During the morning hours of August 7, I was on shore at Gandfjord, on the south side of the Varangerfjord, and measured the height of some terraces there. At 1h. 10m. p.m. we took serial temperatures in the Gandfjord with the deep-sea thermometer. The weather was calm, and a dense fog prevailed. The temperature of the air was 12° C. Leaving the Gandfjord we proceeded northwards. The dense fog continued. At once the fog began to be lighter and the sun to shine through, and a few minutes afterwards we were out of the fog, which was standing as a white wall in the south-west. In the moment the sun appeared, but before we were quite clear of the fog, I saw in the north-east a bow having the shape of a rainbow, but quite white, projected on the fog. With a sextant I measured its amplitude, or the chord along the horizon, and the height of the summit above the horizon—in both cases the middle between the outer and inner edge of the bow. The horizon not being distinctly visible, it is probable that the measures taken do not exactly refer to the true horizon, nor is it certain that the height of the summit was taken from the same horizontal plane in which the amplitude was measured. By the captain's reckoning, the apparent ship's time, at the moment of observation, was 2h. 40m., and the latitude 70° 1′. From these data, and the declination of the sun, I computed the azimuth of the sun at south 46° 5′ west, and its apparent altitude at 31° 12′. Supposing, as the results of the several computations tend to indicate, that the white bow is circular, and has its centre in the anthelic point, we may calculate the angular radius of the bow by three different methods.



Let h represent the height of the summit of the bow above the horizon, a half the amplitude or chord along the horizon, H the dip of the centre of the bow below the horizon, supposed to equal the altitude of the sun, and r the angular radius of the bow. Then we have—

$$r = H + h \quad \ldots\ldots\ldots \quad (1)$$

$$dr = dH + dh \quad \ldots\ldots\ldots \quad (1')$$

$$\cos r = \cos a \cos H \quad \ldots\ldots\ldots \quad (2)$$

$$dr = \frac{\tan a}{\tan r}\,da + \frac{\tan H}{\tan r}\,dH \quad \ldots\ldots \quad (2')$$

$$\cos r = \cos a \cos (r - h)$$

$$\tan r = \frac{1 - \cos a \cos h}{\cos a \sin h}, \text{ or putting}$$

$$\cos a \cos h = \sin^2 M$$

$$\tan r = \cot^2 M \cot h \quad \ldots\ldots\ldots \quad (3)$$

$$dr = \cos (r - h)\frac{\cos r}{\sin h}\tan a\,da - \sin (r - h)\frac{\cos r}{\sin h}\,dh \quad (3')$$

The observations gave $2a = 49°$, $a = 24° 30'$, and $h = 7°$ (or a little more).

From (1) we have $r = 31° 12' + 7° = 38° 12'$.

Putting $dH = \pm 2'$, $dh = \pm 15'$, we have by (1′)

$$dr = \pm \sqrt{2^2 + 15^2} = \pm 15'{\cdot}1 = \pm 0°{\cdot}25.$$

From (2) we have $r = 38° 53'{\cdot}5$,

and by (2′) $dr = 0{\cdot}577\,da + 0{\cdot}770\,dH$, dH being the error in the altitude of the measured chord, or the chord's altitude or depression, reckoned from the horizon.

Putting $da = dH = \pm 15' = \pm 0°{\cdot}25$, we get—

$$dr = \pm 0°{\cdot}24.$$

From (3) we have $r = 41° 8'$,

and by (3′) $dr = 2{\cdot}315\,da - 3{\cdot}074\,dh$.

Putting $da = dh = \pm 0°{\cdot}25$, we get—

$$dr = \pm 0°{\cdot}97.$$

Taking the weights inversely as the squares of the probable errors, we find that the results from (1) and (2) have a weight of 15 times that found by (3), and the mean will be—

$$r = 38° 38' \pm 6'{\cdot}4.$$

From this mean we find that h should have been $38° 38' - 31° 12'$, or $7° 26'$ instead of 7°, or somewhat greater than measured, as supposed in my note-book. Computing from (2) we find that we should have calculated with $H = 30° 51'$ instead of 31° 12′, or the chord has been measured in a level 21′ lower than the horizon, which is highly probable with the fog spreading over the surface of the sea. The measured chord being too great, and the measured height too small, it follows from (3′) that (3) must give the radius by far too large.

The next occasion I had to observe the fog bow was in 1878, on the North Atlantic Expedition, when returning from Spitzbergen. During August 30, our ship, the *Vöringen*, had a rather tedious work in advancing southwards, on account of the foggy weather prevailing the whole day. In the afternoon we had advanced so far south of Bodö as to approach the Sandhorn, a mountain about 3000 feet high, lying to the east of the route.

At 5h. 20m. p.m. I saw an anthelic fog bow, white, with the outer edge reddish, the inner edge bluish. I measured, with the sextant, the amplitude along the horizon at 76°, the sun's altitude at 12°, and the breadth of the bow at 2°. The temperature of the air was about 14° C. The latitude was about 67° 10′.

Assuming the measured chord to lie in the true horizon, we get by (2) from $a = 38°$, $H = 12°$, $r = 39° 35'{\cdot}5$. But it is highly probable, that the measured chord lay deeper than the

fog-veiled horizon, perhaps some degrees deeper, and it may be quite as possible that I have measured the diameter as a chord. This supposition gives $r = \frac{1}{2}\,76°$ or 38°. The mean of these two determinations is 38° 48′, with a probable error surely not less than ± 48′, or half the difference.

At 6h. 1m. p.m., the circumstances were more favourable. I measured $2a = 76°\ 11'$, $H = 8°\ 11'$, the breadth of the bow 2°. The latitude was 67° 7′. From these data we have, by (2) $r = 38°\ 50'$. Assuming the observed amplitude to have been the diameter, which is very probable, we have $r = 38°\ 5'{\cdot}5$. The mean value is 38° 28′ ± 22′.

If the fog bow, like the rainbow, has always the same diameter, we can join the three values thus found for the radius into a mean result. We have thus, giving the single determinations a weight inversely as the squares of their probable errors—

1875 August 7, 2h. 46m.	$r = 38°\ 38' \pm 6'$
1878 August 30, 5h. 20m.	$r = 38°\ 48' \pm 48'$
1878 August 30, 6h. 1m.	$r = 38°\ 28' \pm 22'$
Mean	$r = 38°\ 38' \pm 1'{\cdot}4$

The breadth of the bow being 2°, with a probable error of $\frac{1}{30}$ or ± 6′, we get—

for the outermost red ring $r = 39°\ 38' \pm 6'{\cdot}2$
for the innermost blue ring $r = 37°\ 38' \pm 6'{\cdot}2$.

At 7h. p.m. the bow stood white against the blue sky, the Sandhorn below it. At 6h. 40m. p.m., and sometimes before, I remarked that my own shadow was visible on the fog wall. In order to get a wider view of the phenomenon, I went up upon the roof of the chart-house, where my eye was 27·5 feet (8·4m.) above the surface of the sea. From here I saw how my shadow distinctly imitated all my movements. The shadow of my head appeared dark on a lighter white ground, and from a certain distance surrounded by a concentric coloured glory, in which the colours were arranged in the order of the spectrum, so that the outermost circumference was red, the middle yellow, and the innermost blue. There was no white band in the glory. With the sextant I measured the radius of the yellow ring, which was the most intense, at 1° 31′. The intensity of the other coloured rings was too feeble to allow their radius to be measured with the sextant. From a comparison with the radius of the yellow ring I judged that of the blue at 1° 15′, and that of the red at 1° 45′, with a possible error on both sides of ± 5′. This phenomenon is Ulloa's Ring.

Taking all my results together, we have the following synoptical table :—

Ulloa's Ring.	Radius of inner blue		1° 15′ ± 5.
,, ,,	,, ,,	yellow	1° 31′ ± 2′.
,, ,,	,, ,,	outer red	1° 45′ ± 5′.
Fog Bow	,, ,,	inner blue	37° 38′ ± 6′.
,, ,,	,, ,,	middle	38° 38′ ± 1′.
,, ,,	,, ,,	outer red	39° 38′ ± 6′.

The fog bow cannot be the rainbow with three or four inner reflections, as these rainbows, if visible, would not be anthelic, but have, for the red rays, distances from the sun of 42° and 43°. Moreover, the intensity of the fog bow is too considerable to be the result of so many reflections in drops of rain. The ordinary or first rainbow, with one inner reflection, has a radius or distance from the anthelic point of 42° 30′ for the red, and 40° 30′ for the violet rays, which gives, the sun's radius being 16′, its innermost radius like 40° 14′. The outermost red ring of the fog bow has a radius of 39° 38′. Its distance from the ordinary rainbow is consequently only 36′. This space we see sometimes covered by the supernumerary rainbows, caused, as Sir G. B. Airy's investigations have shown, by the interference of the rays leaving the raindrop.

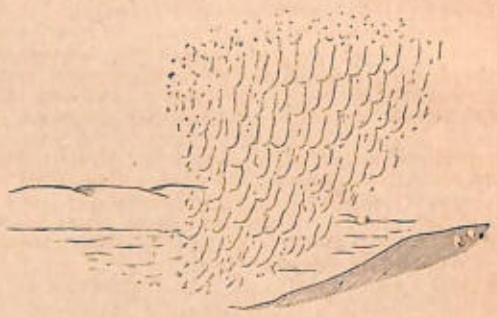
It seems probable that the smallness of the fog globules, as contrasted by the larger size of the raindrops, must enter as an essential part in the explication of the fog bow. In Günther's "Lehrbuch der Geophysik und physikalischen Geographie," ii. p. 151, he speaks of white rainbows, the description of which agrees with the aspect and position of my fog bows, and for which Bravais has given an explanation ("Sur le Phénomène de l'Arc-en-ciel blanc," *Ann. de Chim et Phys.* [3], vol. xxi. p. 348). Not having Bravais' memoir at hand, I may only remark that, as far as I can see from Günther, he assumes the fog drops to be hollow, a supposition which is hardly in accordance with modern investigations.

When I saw the fog bow, I had, I am sorry to say, no polariscope, so that I was unable to investigate the polarization of its light, so characteristic for the rainbow. H. MOHN.

Christiania, January 31.

The Shadow of a Mist.

THE reticulated rippling shadow of the mist described in Mr. Fawcett's letter (NATURE, January 5, p. 224) reminds me of a somewhat parallel phenomenon seen by me a few years ago. I saw a snow-storm some miles away crossing the valley between the Mendips and the Quantocks. It hung like a long, heavy curtain partially obscuring the *bright* western sky. The



light shining through the shower showed a fairly regular pattern. On a reddish-brown ground the darker, because denser, parts of the shower took the form indicated roughly by the accompanying diagram. Was the snow falling in spiral streams, and would a similar explanation apply to the shadow of the mist seen by Mr. Fawcett? HENRY BERNARD.

The English Church, Moscow, January 31.

Instability of Freshly Magnetized Needles.

I MADE no attempt to investigate the fluctuations of the dipping needle. They seemed to me to pass away after a few minutes, and I therefore took that method to get rid of them, supposing that the phenomenon was well known to other observers. The variations that I observed amounted to three or four minutes, I should think. It is true that the dip circle which I used was of an ancient pattern; as Prof. Rücker says, hardly up to modern requirements. I did not send it back to the maker for adjustment, as Mr. Whipple says he would have done, because it was lent to me, and it was the best I was able to get.

Recognizing the fact that we could not expect to get the best results from our outfit, it was deemed best to make only one set of observations at each station, and multiply the number of stations as greatly as possible. This made it necessary to do the work quickly at some stations in order to adapt our time to that of trains, or in order to get the drudgery involved in camp-life done within the twenty-four hours. It is probable that at some stations we overdid the matter, and that the observations would have been better if more time had been taken. The dip observations I always regarded as least satisfactory. But all of the work has been published in such a way that its value can be estimated by anyone interested, and everyone is welcome to place whatever value he pleases upon it. We did the best we could under the circumstances, and the expense was met by private means.

The dip circle was returned to Washington when we were done with it, so that I am not now in a position to throw any light upon the subject under discussion. For most of the stations at which observations were made, I think the magnetic elements were determined with as great precision as a single observation would give them, and they seem to me to be as valuable as they profess to be, and not very much more. The fact that so little magnetic work had been done in the central part of the United States seemed to me to justify the plan of making the number of stations large, rather than of trying to attain the precision of observatory methods in field work at a few stations.

FRANCIS E. NIPHER.

Microsauria and Dendrerpeton.

IN the notice in NATURE of January 12 (p. 244) of Fritsch's new number of his excellent work on the Permian fauna of Bohemia, which has not yet reached me, I observe a reference to *Microsauria*, which would seem to imply that I had included *Dendrerpeton* in that group. Possibly this was not intended by the reviewer, as it certainly could not have been intended by Fritsch, who knows my views quite well; but in case it should be misunderstood I beg to say that I have not held this view, but on the contrary have confined the name Microsauria to species with simple teeth, and have placed *Dendrerpeton* with Labyrinthodonts, though by no means as a typical genus of that group. In my last paper on this subject (Trans. Royal Society, 1882), I expressly exclude the two species of *Dendrerpeton* referred to from Microsauria, and define the latter as having non-plicated teeth (pp. 634-35). I may add, however, that I have always held and now hold that the Microsauria, though in some respects inferior to Labyrinthodonts, embrace in their structures premonitions of the true reptiles not found in the latter.

The study of these creatures was one of those bye-efforts thrust on me by circumstances, and which I have closed up so far as I am concerned in the paper referred to; but I have learned to love the little Microsaurs and to regard them as a hopeful and prophetic group. J. WM. DAWSON.

McGill College, January 26.

A New Historic Comet?

PERMIT me to inform Mr. Knott that the "new historic comet" is not a new comet at all. He will find it as No. 154 in Mr. Chambers's Catalogue No. II. in his well-known "Handbook of Descriptive Astronomy." It is there described, 302 A.D. "in May–June a comet was visible in the morning—(Ma-tuan-lin: Williams 26)." W. H. S. MONCK.

Dublin, February 10.

The Proposed Teaching University for London.

NO one, I am sure, who has carefully read my letters in the *Times* on this subject could agree with the writer of your article that I appear "to consider the dispute as one between the efficiency of 'lectures' on the one hand, and of 'reading' on the other."

The writer of the article has certainly misunderstood my views "upon the matters in dispute," as well as my object in quoting Darwin's dictum on the advantages of "reading" as compared with "lectures." PHILIP MAGNUS.

Exhibition Road, London, S.W., February 10.

Institute of Chemistry.

WITH reference to a circular letter dated the 12th inst., and bearing the signature of Mr. W. Thomson, which has been sent to the Fellows of the Institute of Chemistry, we beg that you will be so good as to allow us to inform the Fellows, through your columns, that we have not been consulted in regard to the action taken by Mr. Thomson, and that we decline to offer ourselves as candidates for election in opposition to the nominations of the Council. BOVERTON REDWOOD.

London, February 20. ALFRED GORDON SALAMON.

CORAL FORMATIONS.

I DESIRE to call attention to a condition of reef that I think has been very little studied, but that may contain a clue to a solution of some of the difficulties that still surround the subject of coral formations generally.

I may as well at once avow myself to be one of those who, on reviewing the later evidence on coral growth, have come to the conclusion that it is sufficient to justify an abandonment of the supposition that subsidence plays a principal part in the production of barrier reefs and atolls, but are at the same time not satisfied with one part of the explanation offered by Mr. J. Murray.

I refer to the great effects attributed by him to the disintegration and solution of dead coral by the chemical action of sea-water, in hollowing out and deepening the large and deep lagoons inside both these classes of reefs.

Mr. Murray's theory on this point, as summarized by himself, is that—

"(1) When coral plantations build up on submarine banks, they assume an atoll form, owing to the more abundant supply of food to the outer margins, *and the removal of dead coral rock from the interior portions by currents and by the action of the carbonic acid dissolved in sea-water.* (2) That barrier reefs have built out from the shore on a foundation of volcanic debris, or a talus of coral blocks, coral sediment, or pelagic shells, and *the lagoon channel is formed in the same way as a lagoon.*"

The italics are mine, and indicate the part of his theory to which from my view, and, I believe, that of others, there are objections, but to which Mr. Murray attaches considerable weight.

Is it necessary thus to invoke the aid of dissolution of the dead coral by chemical action as an important agent in the formation of these deep lagoons and channels? I think not.

An examination of the forms of, and depths on, well-surveyed submerged banks in different regions reveals a considerable number of reefs, which, if their development continues on the same lines as apparently heretofore, must, in the course of time necessary to bring them to the surface, form perfect atolls of large size, inclosing deep lagoons, without any further scooping out by solution.

Many instances occur in the China Sea. The Tizard Bank, in lat. 10° 20′ N., and long. 114° 25′ E., is 32 nautical miles in length, with an extreme breadth of 10 miles, and was well surveyed in 1867. The central portion is very flat and almost void of patches. Its depth is from 30 to 47 fathoms. Its edge is crowned with a coral rim varying from 4 to 10 fathoms in depth, broken here and there by openings, in some cases over 30 fathoms deep. The bank rises steeply from deep water, but, as no sectional soundings were taken, the precise angle of slope is unknown. The rim is composed of coral in luxuriant growth, and it can scarcely be doubted that in time it will reach the surface. In fact, on its periphery of 100 miles, in eight places small patches of reef, three of which bear islets, have already done so.

When the remaining portions of the rim are also awash, the reef will be in all respects an atoll similar to the great Maldive atolls, without any necessity for solvent action enlarging or deepening it.

Eight other banks of similar character, and in various stages, occur not far from this reef.

The great Macclesfield Bank, farther north, over 70 miles in length, and 40 miles in width, is of precisely the same nature, but its development is not so far advanced; the rim being in no spot nearer the surface than 10 fathoms, the water on it varying from that amount to 19 fathoms, while the depth of the inclosed area is from 40 to 60 fathoms. The survey of this bank is not so complete as in the case of some others, but enough has been done to show its character very plainly.

The Prince Consort Shoal (300 miles S.W. of the Tizard Bank) is apparently at a still earlier stage, a few patches of 17 fathoms and a considerable area of 30 fathoms partly inclosing a central area of 40 fathoms depth. The great Seychelles Bank in the Indian Ocean, 200 miles by 100, is very imperfectly known, but in most places the lines of soundings over its edge exhibit this tendency to form a rim. Here, however, the general depth on the bank is not over 30 fathoms. The Amirante Bank is a similar example.

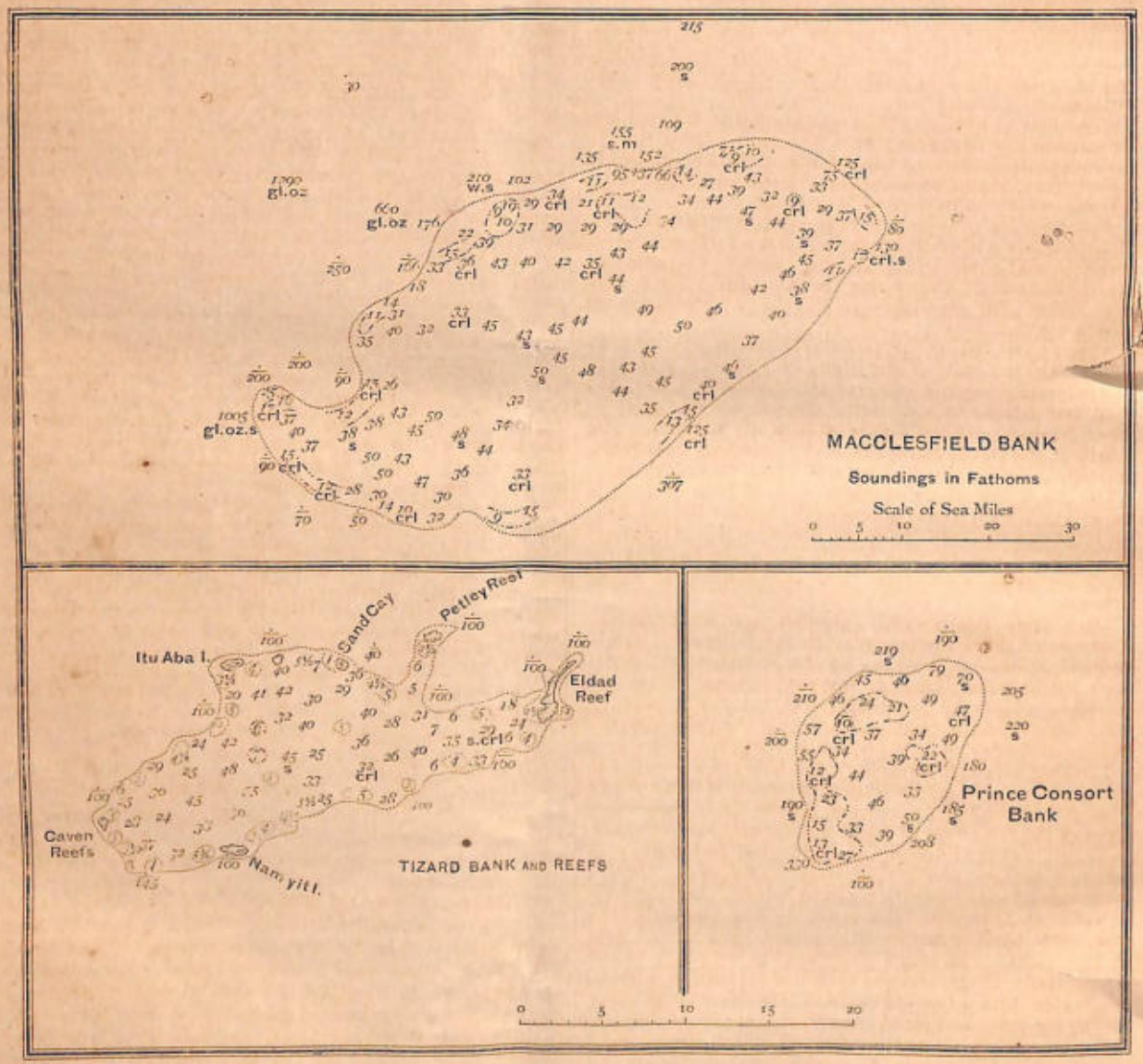
The evidence afforded by these reefs has probably escaped notice from the fact that as published in charts for the purpose of navigation they are mostly shown on a very small scale, in which their character is scarcely apparent. The original manuscript surveys in the records of the Hydro-

graphic Department being not generally available, no opportunity has been afforded for their examination, but on these their form is highly suggestive. It should also be remembered that few submerged banks have yet been surveyed on a sufficiently large scale and in sufficient detail to show their characteristics.

I may, however, add that it is well known to seamen experienced in navigation in coral waters that shallower soundings are frequently found on the edges of banks.

How precisely it comes about that coral is growing on the yet deep rims of these large banks, and that little or none is flourishing in the interior, evidence is yet wanting to show. These, however, are the facts, and the result, so far as the necessity for future scooping out is concerned, seems indisputable. I may nevertheless offer a suggestion.

This condition of reef is apparently only to be accounted for in two ways. Either by subsidence ; or by assuming that the animals, be they corals or other lime-secreting organisms, that settle on the bank, do, when it gets, by their accumulation, within a certain distance of the



surface, and under certain conditions of currents and food supply, intercept so much of the food borne in by the currents, that similar life, suitable to that depth zone, cannot be supported in the central area. Thenceforward, the rim alone will grow, and the organisms fitted to live in the successively shallower zones to the surface will only find on it foothold.

This would be the perfect atoll, but, with less nicely balanced conditions, growth would also take place in patches in the central area, as is so often the case.

It is to be observed that the depths on the more or less level tops of these large banks, as also in many perfect atolls, is frequently between 30 and 50 fathoms.

The smaller the bank, the less would be its general depth when the rim is formed, owing to the different proportions in which an area and its periphery, over which the non-consumed food must pass, relatively increase, and assuming that the animals that live nearer the surface consume more than those at greater depths.

I am fully aware that this view of the growth of an

atoll is not new. Darwin says, "A bank at the proper depth beneath the surface would give rise to a reef which could not be distinguished from an atoll formed during subsidence." Murray says, "Very early in the history of such an atoll, and while yet several fathoms submerged, the corals situated in the central parts would be placed at a disadvantage." It does not, however, appear to have been contemplated that the inclosed lagoon would, under any circumstances, without some further agency than the simple growth of the rim, be so deep as it appears to me, from the cases above cited, that it can be; nor, so far as I can find, have any such instances been before remarked.

Darwin notices the case of the Chagos Bank, but, on the authority of Captain Moresby, he states that the rim is dead, and concludes that life was killed by subsidence, and he apparently also infers that it will not grow to the surface.

I can find no fresh evidence on this particular reef, but for some of the banks in the China Sea I have the independent testimony of two of the officers employed in their survey, Staff-Commanders Tizard and Petley, R.N., that the coral growth is most luxuriant.

Touching for a moment on the point of the formation of banks at a proper distance below the surface; the investigation of banks in the Atlantic, recently described by Mr. J. Buchanan, shows that banks with almost wall-like slopes are growing up by the accumulation of organisms.

Turning to barrier reefs, similar instances can be found. Off the coast of the island of Palawan, a shallow rim is forming on the edge of the bank which stretches from the island to a distance varying from 15 to 30 miles, having a general depth on it of 40 fathoms. The length of the rim is 250 miles, and it consists of streaks and patches of coral with from 4 to 30 fathoms on them.

On the south of the eastern end of New Guinea, a formation, known to navigators as the sunken barrier, lines the edge of a bank similar to the above, and is of precisely the same character. Its length is 140 miles, and the depth between it and the land varies from 30 to 60 fathoms.

Dr. Guppy has recently pointed out some smaller instances of the same tendency in the Solomon Islands, and has made some remarks on the formation of barrier reefs in the same sense as my suggestions. I am not therefore advancing anything novel, but simply pointing out evidence which tends to show that the principle may be carried further than has hitherto been supposed.

Looking now at the fringing class, how comes it that so many wide reefs of this character exist, which, if Mr. Murray's contention be correct, should surely show more signs of the formation of a lagoon channel than they do?

Take the case of Rodriguez, in the Indian Ocean, with which I am personally intimately acquainted. Here a fringing reef surrounds the western sides of the island for a width of 4½ miles. There are narrow channels it is true, but so shallow that in many places boats cannot pass at low water. The island is situated in the heart of the strong trade winds, and the reefs are exposed to a heavy sea, which, with a rise of tide of nearly 6 feet, gives every facility for scour and rapid change of water.

I am not disputing the fact that calcareous dead organisms are dissolved by carbonic acid. I am no chemist, and moreover the *Challenger* observations amply prove it, but I would ask Mr. Murray if there is not a great difference between the position of small shells falling in water which completely surrounds them whilst they are constantly coming in contact with fresh particles of it, and of the more or less solid mass of a coral reef, which can only be attacked on its upper side to advantage, and where the resultant fine mud covers and protects the remaining rock, especially in the case of lagoon channels, when the bottom is partly composed of detritus from the land.

At the bottom of a lagoon of any depth, moreover, the motion of the water must often be comparatively slight, and the action consequently extremely slow.

The rotten state of the surface of every coral reef awash with the water shows that this disintegration is going on, but the fact that for large areas it remains awash, and must have so remained for ages, seems to me to point to the supposition that the removal of matter is too insignificant to account for the formation of deep lagoon channels in this manner, though doubtless it may explain the shallow pools and creeks found in all fringing reefs.

I have addressed myself solely to one point in this many-sided question, but I may add my opinion that, before any explanation which will fully account for the almost infinite variety of coral formations, can be given, much more knowledge of details of the complex conditions under which they may grow is required.

Certain knowledge of the depths at which different corals and other lime-secreting animals live under varying circumstances; of the amount of food carried in the different strata and in different waters; of the effect of the velocity of the currents that bring the food to the banks; and more accurate surveys on large scales, especially of the shapes and contours of coral reefs, and of their composition, are all wanting. These details must greatly affect coral growth, and the results must greatly vary. On the other hand, similar results may be brought about by different causes.

It may surprise some to learn how little in the preliminary matter of surveys alone has been done in the principal coral-reef regions, especially in the Pacific, which is generally quoted; and consequently how very inexact our knowledge is of the depth both inside and outside of the majority of atolls in the world, and also of the state of the bottom of the sea, on which it is very possible that many elevations may be found in the condition of those to which I have called attention.

In the Pacific the vast majority of islands have been merely sketched without a single sounding having been taken, either inside or outside lagoons.

I append a few statistics relating to the larger coral groups to show our position in this respect; merely remarking that the waters of the Fiji and the Society Islands are the only ones which can be said to be in any sense surveyed.

	No. of Islands.	No. surveyed.
Pau uotu Islands	... 74 ...	1 partially.
Ellice ,,	... 10 ...	none.
Gilbert ,,	... 16 ...	none.
Marshall ,,	... 30 ...	none.
Caroline ,,	... 43 ...	3 partially.
Tonga ,,	... 6 groups	... 2 groups partially.

W. J. L. WHARTON.

THE AKKAS, A PYGMY RACE FROM CENTRAL AFRICA.

AT the last meeting of the Anthropological Institute, Prof. Flower gave a description of two skeletons of Akkas, lately obtained in the Monbuttu country, Central Africa, by Emin Pasha, and by him presented to the British Museum. Since this diminutive tribe was discovered by Schweinfurth in 1870, they have received considerable attention from various travellers and anthropologists, and general descriptions and measurements of several living individuals have been published, but no account of their osteological characters has been given, and no specimens have been submitted to careful anatomical examination. The two skeletons are those of fully adult people, a male and a female, but unfortunately neither is quite complete. The evidence they afford entirely corroborates the view, previously derived from external measurements, that the Akkas

are among the smallest, if not actually the smallest, people upon the earth. There is no reason to suppose that these skeletons were selected in any way as exceptional specimens, yet they are both of them smaller than any other normal skeletons known, smaller certainly than the smallest Bushman skeleton in any Museum in this country, and smaller than any out of twenty-nine skeletons of the diminutive inhabitants of the Andaman Islands, of which the dimensions have been recorded by Prof. Flower in a previous paper communicated to the Institute. The most liberal calculation of the height of these two skeletons places that of the male at about an inch below 4 feet, and the female at less than an inch above. We may say 4 feet, or 1·219 metre, as the average height of the two, while a living female of whom Emin Pasha has sent careful measurements is but 1·164 metre, or barely 3 feet 10 inches. The results previously obtained from the measurements of about half-a-dozen living Akkas are not quite so low as these, varying from 1·216 to 1·420, and give a mean for both sexes of 1·356, or 4 feet 5½ inches. Schweinfurth's original measurements were unfortunately lost, and the numbers since obtained are quite insufficient for establishing the true average of the race, especially as it is not certain that they were all pure-bred specimens.

In the list given in the third edition of Topinard's "Anthropologie" (1879) only two races appear which have a mean height below 1·500 metres, viz. the Negritos of the Andaman Islands 1·478, and the Bushmen 1·404. Of the real height of the former we have abundant and exact evidence, both from the living individuals and from skeletons, which clearly proves that they considerably exceed the Akkas in stature. That this is also the case with the Bushmen there is little doubt, although the measurements of this diminutive race are less numerous and carefully made.

The point of comparative size being settled, it remains to consider to what races the Akkas are most nearly allied. That they belong in all their essential characteristics to the black or Negroid branch of the human species there can be no doubt, in fact they exhibit all the essential characteristics of that branch even to exaggeration. With regard to the somewhat more rounded form of head (the cephalic index in these examples being 74·4 and 77·9 respectively), Hamy has long since pointed out that in equatorial Africa, extending from the west coast far into the interior, are scattered tribes of Negroes, distinguished from the majority of the inhabitants of the continent by this special cranial character, as well as by their smaller stature. The Akkas are grouped by Hamy and Quatrefages as members of this race, to which the distinctive name of "Negrillo" has been applied. Their small size has naturally led some anthropologists, including Schweinfurth, to ally them to the diminutive African race inhabiting the southern part of the continent—the Bushmen; but beyond certain characters met with in the whole Negroid branch, including the frizzly hair, there is little in common between them. The Bushmen are a very strongly marked race, and both their external appearance and osteological characters are so exceptional that they can never be confounded with any other. The natives of the Andaman Islands have also very distinctive characters, which they do not share with the Akkas, whose position all recent investigations show to be that assigned to them by Hamy as members of the Negrillo division of the Negroid branch of mankind. It is possible that these people gave origin to the stories of pygmies so common in the writings of the Greek poets and historians, and whose habitations were often placed near the sources of the Nile. The name "Akka," by which, according to Schweinfurth, the tribe now call themselves, has, singularly enough, been read by Mariette Pasha by the side of a portrait of a dwarf on a monument of the ancient Egyptian empire.

REV. JOHN HEWITT JELLETT, D.D., D.C.L.

IT is with extreme regret that we announce the death of the Reverend the Provost of Trinity College, Dublin. He died last Sunday evening after a very short illness.

The Provost was present in his usual health at the Spring Commencements, which were held on the 14th inst. in the Hall of Trinity College; on the 16th he was not quite well; on the 18th some dangerous symptoms appeared; and the end came, painlessly and unexpectedly, on the 19th inst.

John Hewitt Jellett was born at Cashel in the county of Tipperary on December 25, 1817. He entered Trinity College, Dublin, when seventeen years of age. Obtaining a Scholarship in 1836, he graduated as a Senior Moderator and Gold Medallist in Mathematics in 1837, and three years afterwards he obtained a Fellowship.

In 1848, he was appointed to the Professorship of Natural Philosophy; in 1870, on the death of the Rev. Dr. Luby, he was co-opted a Senior Fellow of Trinity College, and on the death of the Rev Dr. Lloyd, in 1881, he was nominated by the Crown to the Provostship.

Twenty years ago he was made one of the Commissioners of National Education in Ireland, and he was President of the Royal Irish Academy from 1869 to 1873.

In 1850, Jellett published his well-known "Treatise on the Calculus of Variations," a subject which had engaged the attention of some of the noblest mathematical intellects of the world. The volume contains improvements of previously existing methods, which, had they been given as so many separate treatises, would in themselves have formed no ordinary title to fame; but the author's aim was rather to compile a memoir which would enable the earnest student to be on a level with the knowledge he had himself attained to, however little that aim might be to his own glory.

For this work the Royal Irish Academy awarded Jellett in 1851 their Cunningham Gold Medal. In 1872 appeared the "Treatise on the Theory of Friction," a work well known and highly appreciated. In addition to these volumes a number of scientific memoirs were from time to time published by him in the Transactions of the Royal Irish Academy, and in Leonville's *Journal de Mathématique*, of which perhaps the more important were on the "Equilibrium and Motion of an Elastic Solid" and "On Researches in Chemical Optics."

Like other well-known mathematicians of the Dublin University, Jellett was as much thought of for his pulpit discourses as for his scientific memoirs. He was of good presence, had a clear articulation and a very persuasive style; and his appearance in the pulpit of the College chapel was always welcomed. When he dwelt on the moral difficulties of the Old Testament, none went their way without being impressed by the straightforward honesty of the man.

Occupying a very conspicuous position in a University city like Dublin, the Provost seemed to command the respect of all. Some perhaps there are who would have preferred that the head of so ancient a seat of learning should have taken no part in modern political strife; but, even amid the feverish excitement of party warfare, the Provost of Trinity College, though he often fearlessly and eloquently put forward his own views, did so without giving offence to any.

Intimately associated, from the year 1834, with the life of the College over which he at the last presided, the Provost knew much of, and was a prime mover in, many of the changes which have marked out a new life in the place. Within its walls he was known and respected, while the tribute to his memory paid by all classes of the Dublin citizens is a striking proof of how he was loved.

This is not the place to dwell on the intense loss his

death brings to those who had experienced the charm of that hidden inner life which was known to his friends as both kind and just, affectionate and sympathetic.

NOTES.

THE retirement of the veteran Prof. Prestwich from the Chair of Geology at Oxford is an event which cannot be chronicled without regret. But it is pleasant to know that he relinquishes the post which he has dignified for so many years to find in the quiet of his country home that leisure and rest to which his long devotion to the cause of science so justly entitles him. He has crowned his professorial career by the publication of the splendid volume which completes his great work on geology. On the very day after the appearance of that volume the electors met at Oxford to select from the numerous candidates a successor to fill his place. His University and the science of geology may both be congratulated on their choice. Prof. Green, whom they have chosen, is one of the most accomplished geologists in the country, one who has been trained in the practical school of the Geological Survey, who has done admirable original work, and who possesses in no common measure the power of luminous exposition. He is gifted, moreover, with a faculty in which geologists are often singularly defective, that of mathematical investigation, and we may hope that one of the results of his transference to Oxford will be to afford him an opportunity of devoting himself to the attack of many geological problems from the mathematical side. He carries with him to his new sphere of labour the best wishes of all to whom the progress of geology and the cultivation of science at the Universities are dear.

ON May 27 next Prof. F. C. Donders, of Utrecht, will be seventy years of age. The law requires that he shall then resign his duties as Professor at the University and as Director of the Physiological Laboratory, and it is thought that men of science in all parts of the world may be glad to take the opportunity of expressing their appreciation of the great services Prof. Donders has rendered to the study of physiology and physiological optics. An influential committee has been formed in Holland for the purpose of giving effect to this idea, and the proposal is that Prof. Donders' name should be connected in a permanent way with the spot where he has lived and worked for more than forty years, by the creation of a fund to be devoted to a scientific purpose, and to be known as the "Donders Memorial Fund." The uses to which the fund will be put, and the rules by which its administration will be governed, will of course be determined in accordance with the wishes of Prof. Donders. The Ophthalmological and Physiological Societies have taken the matter in hand in this country, and we have no doubt that the committees they have appointed will do their work satisfactorily. It ought not to be difficult for them to secure an adequate token of the respect felt in England for an illustrious man of science to whom the medical profession and the public are so deeply indebted. It is proposed that the amounts contributed by the several donors be not specified, but that they be grouped into a common sum for transmission to the Dutch Committee; and that the names of those contributing be inscribed in a suitable form for presentation to Prof. Donders. For this reason smaller as well as larger subscriptions will be acceptable. Subscriptions may be sent to Dr. Gerald F. Yeo, Secretary of the Committee of the Physiological Society (address—King's College, London, W.C.), or to Dr. W. A. Brailey, Secretary of the Committee of the Ophthalmological Society of the United Kingdom (address—11 Old Burlington Street, London, W.). Subscriptions may also be sent to the office of this journal.

AT a meeting of the Royal Society of Edinburgh, held on January 30, Profs. Clausius, Haeckel, and Mendeléeff were elected Honorary Fellows.

SIR JAMES PAGET, F.R.S., has consented to give the annual address to the students of the London Society for the Extension of University Teaching, at the Mansion House, on Saturday, March 3, at 3.30 p.m., under the presidency of the Lord Mayor. The subject of the address will be "Scientific Study."

A MARBLE medallion portrait of the distinguished palæontologist, Dr. Thomas Davidson, F.R.S., first chairman of the Brighton Museum Committee, was unveiled in the geological room of the Free Town Museum, Brighton, by the Mayor, Mr. E. Martin, on Friday, the 17th inst. The medallion, which is much admired, is the work of Mr. Brock, A.R.A. It was presented to the town on behalf of the subscribers by Mr. Edward Crane, chairman of the Museum Committee, who referred in detail to Dr. Davidson's services to science and to the Museum. Sir R. Owen sent a letter regretting that failing health prevented his paying the respect of personal attendance to the memory of his distinguished fellow-worker. Prof. Judd also wrote bearing cordial testimony to the skill and enthusiasm with which Dr. Davidson carried on his researches.

GENERAL PERRIER, the most eminent French authority on geodesy, died at Montpellier on Monday at the age of fifty-five. He had attained the rank of Brigadier-General in the French army, and was at the head of the Geodetic Department at the War Office. He was a member of the Academy of Sciences.

THE relations of science and religion do not form one of those topics which we permit ourselves to discuss in NATURE. At the same time we may call attention to a series of three remarkable articles on "Darwinism and the Christian Faith" recently published in the *Guardian* (January 18, January 25, and February 1, 1888), and now reprinted as a pamphlet. The author is anonymous, but is understood to be an Oxford College tutor, and Honorary Canon of Christ Church. The orthodoxy of the *Guardian* is, we believe, unimpeachable. We notice therefore with gratification that not only is Darwinism thoroughly accepted and lucidly expounded by the writer in the *Guardian*, but that he is an exceptionally well-informed and capable critic, whose scientific knowledge is varied and sound. The publication of these articles in the *Guardian* is a proof that the clergy as a body are not so unwilling to accept new scientific views as might be supposed were we to regard Dean Burgon as a fair sample of his class. The *Guardian's* contributor discusses the difficulty of reconciling the existence of a just, omnipotent, and omniscient God with the existence of pain and the ceaseless "struggle for existence," and *à propos* has a remark tinged with local colour which is worth reproducing. "And yet," he says, "man, who is so wise and good that he is always saying, with King Alphonso of Castile, 'If God had called me to His councils things would have been in better order,' has invented competitive examinations, which mean suffering and pain for all, without even a compensating 'survival of the fittest' or improvement of the race!" We believe that competitive examinations were invented by the Chinese, and introduced into Europe by Jesuit missionaries. The Chinese are celebrated among the nations of the world for the elaborate system of cruel tortures employed in their administration of justice. On the other hand, we owe tea and many other nice things to them.

THE annual winter meeting of the Department of Superintendence of the U.S. National Education Association was held lately at Washington. The most important topics treated were "How and to what extent can Manual Training be ingrafted on our System of Public Schools?" and "How can the Qualifications of Teachers be determined?"

THE Archæological Congress which is to be held at Moscow, in 1890, will have an international character, many German, French, Italian, and other men of science having already been invited to attend. The Congress will consider the following subjects: (1) prehistoric antiquities; (2) historical, geographical, and ethnographical questions; (3) Russian art monuments; (4) ecclesiastical monuments; (5) Slavo-Russian language and writing; (6) Slavo-Byzantine and West European antiquities; (7) Oriental and heathen antiquities; (8) latest progress of archæology.

MRS. ZELIA NUTTALL has been elected a Fellow of the American Association for the Advancement of Science in recognition of her researches in Mexican archæology.

A TRANSLATION of Dr. E. B. Tylor's hand-book of "Anthropology" into Spanish by D. Antonio Machado has just been published in Madrid. The author contributes a special preface drawing attention to the valuable anthropological material still to be found in Spanish America.

THE first number of what will no doubt prove to be an important and valuable periodical has just been issued. It is called *Internationales Archiv für Ethnographie*, and is edited by J. D. E. Schmeltz, of the Ethnographical Museum of Leyden, who has received promises of co-operation from many of the foremost ethnologists and anthropologists in Europe and America. The present number (which contains German, Dutch, and French contributions) opens with a striking article, in German, by Dr. L. Serrurier, Director of the Leyden Ethnographical Museum, on the arrows of New Guinea. Representations of the various types of New Guinea arrows, admirably printed in colours, illustrate this interesting paper.

WE have received the first number of *The American Anthropologist*. This new quarterly periodical is issued under the auspices of the Anthropological Society of Washington, and the editorial committee seek the co-operation of all who are interested in the advancement of anthropological science. In the first number there are papers on the law of Malthus, by Dr. James C. Walling; the development of time-keeping in Greece and Rome, by F. A. Seely; the human hand, by Dr. Frank Baker; and the Chane-abal (four-language) tribe and dialect of Chiapas, by Dr. D. G. Brinton.

THE *American Meteorological Journal* for January contains:—(1) An article by F. Waldo on instruments for measuring atmospheric pressure, showing the differences that exist in the standard barometers of different countries, and that the standards do not always remain constant for a number of years. (2) A paper by Prof. H. A. Hazen on the exposure of thermometers, with a discussion of a new plan proposed by Dr. R. Assmann. The latter paper was presented to the Berlin Academy in November last. (3) On a thirty-day period of the weather, by H. Helm Clayton. The writer considers that the period is strongly substantiated by facts, although at present they remain empirical facts.

THE Deutsche Seewarte has published the seventh volume of the results of meteorological observations for 1° squares of the North Atlantic Ocean. The object is to discuss the observations collected by German and Dutch vessels between latitude 50° and 20° N. in the North Atlantic, adjoining the district of the nine equatorial 10° squares between latitude 20° N. and 10° S., and longitude 10° and 40° W., the observations for which have been discussed by the Meteorological Council. The district now covered by the two institutions embraces 60° of latitude and 30° of longitude, with the exception of one 10° square, which will shortly be published. The German observations are published in a tabular form, showing for each degree the direction of the winds under sixteen points, the number of storms, the mean wind force, pressure, temperature of air and sea, rainfall, and other particulars. The number of observations for each subdivision is sometimes small, but always quoted, and in their present form the observations may be added to subsequently, or amalgamated with those of other countries. A small but important district south of 20° N. (the limit of the German investigation) and west of 40° W. (the limit of the English investigation), embracing the region of the origin of the West India cyclones, has yet to be undertaken by some body, to complete these important contributions to maritime meteorology.

A NEW chloride of gold, Au_2Cl_4, has been prepared by Prof. Julius Thomsen, of Copenhagen (*Journ. für Prakt. Chemie*, 1888, No. 2). The method of preparation is remarkably simple, gaseous chlorine being merely brought into contact with gold in a fine state of subdivision and at a moderately elevated temperature. About 50 grammes of finely divided gold, obtained by precipitating a solution of the trichloride with sulphurous acid, was thoroughly washed, partially dried to the consistency of a thick mud, and placed in a previously weighed glass tube. At its lower end the tube was drawn out and the delivery tube of a chlorine generator sealed on to it. A stopper and exit tube at the upper end completed the arrangement. A rapid stream of chlorine gas (half a litre per minute) was then passed through the apparatus, the lower end of the wider portion containing the gold being gently heated to start the reaction. The whole was then placed in a glass beaker surrounded by cotton-wool in order to prevent too rapid cooling, by which device the heat of the reaction itself was sufficient to complete the combination. At the commencement the absorption of chlorine was perfect, not a bubble escaping, but at the expiration of half an hour the point of saturation was reached. After expelling the uncombined chlorine the tube was again weighed, and the amount of chlorine thus taken up determined. In every experiment the proportion of chlorine to that of gold was found to be very slightly more than two to one, the average ratio being 2·09 to 1·0. The slight excess of chlorine was due to minute spangles of trichloride of gold sparsely disseminated throughout the mass. The simplicity of this mode of preparation and the constancy of the results may perhaps excite wonder that Au_2Cl_4 has not hitherto been as well known as AuCl and $AuCl_3$. As a matter of fact, Prof. Thomsen discovered it several years ago, and published his results, but owing to the adoption of different methods by later workers the conclusions of Prof. Thomsen were not considered confirmed. Now that the work has been repeated and completely verified there is no longer any reason why Au_2Cl_4 should remain in the background. It is interesting theoretically as being the aurous salt of chlor-auric acid, $HAuCl_4$.

FROM the Annual Report of the New York State Department of Public Instruction, it appears that during last year over 31,000 teachers were employed in the State of New York, and that of this number only 5821 were males. The number of children of school age was 1,763,115; the total enrolment, 1,037,812; the average attendance, 625,610. Mr. Draper, the Superintendent, the author of the Report, says that the attendance in the schools does not keep pace with the growth of the population, and that the uneducated class is increasing.

COLONEL LE MESSURIER has just brought out a third edition of his useful pocket hand-book on the "Game, Shore, and Water Birds of India." The utility of this unpretending little work has been vouched for by the call for its re-issue in an octavo form with the addition of many drawings made by the author during his recent furlough in England, and we are glad to see to what practical use he has turned some of Prof. Flower's exhibits at the Natural History Museum. Colonel Le Messurier writes as a field-naturalist for field-naturalists and sportsmen, without any

great pretensions to scientific knowledge, but there is no doubt that all naturalists will gain useful hints from this little volume, which is profusely illustrated with woodcuts, giving the characteristic features of most of the species.

"THE STATESMAN'S YEAR-BOOK" for 1888 has been published. It contains additions and alterations which largely increase the value of the work, and the statistical and other information has been brought up to the latest available date.

WE have received three issues of the "Annuario" published by the Imperial Observatory at Rio de Janeiro—the issues for 1885, 1886, and 1887. The work is well compiled, and the editor evidently takes great pains to secure that each issue shall be decidedly better than its predecessors. Besides the usual collection of astronomical facts, the work contains useful tables relating to meteorology, chemistry, physics, geography, and other sciences.

THE "Annuaire Géologique Universel" of Dr. Dagincourt, which has just been issued for the third time, has been much enlarged and improved. The new volume contains an exhaustive review of recent work in palæontology and geology.

A FOURTH edition of Prof. Nichol's "Tables of European History, Literature, Science, and Art, from A.D. 200 to 1888" (Maclehose) has been issued. The idea of the work is good, but we cannot say that the scientific tables are always quite satisfactory. In his lists of men of science the compiler includes the names of some writers who have a very inadequate claim to the place he accords to them.

THE Trustees of the Australian Museum have issued a descriptive catalogue, by Dr. R. von Lendenfeld, of the Medusæ of the Australian seas. Speaking of the Scyphomedusæ, Dr. von Lendenfeld says that he has observed three species in New Zealand, three species on the coast of Victoria, and five species in Port Jackson. Two of the latter are identical with the Victorian species. Of the nine species, six have been described by Dr. von Lendenfeld; his specimens of the remaining three were not sufficiently well preserved for description. The difficulty connected with the preservation of these beautiful animals has, he points out, been a great obstacle in the way of a thorough knowledge of them.

THE fifth volume of the collected works of Paul Broca has just been issued by M. Reinwald, in Paris. This volume, which, like the others, is sold separately, is particularly interesting to zoologists. It contains Broca's numerous and important memoirs on the brain of man and primates. It is well illustrated.

M. ZOGRAFF's new work on the structure of the *Acipenser ruthenus*, which appeared in the *Izvestia* of the Moscow Society of Amateurs of Natural Science (vol. lii. fasc. 3), will be most welcome to zoologists. Following the methods adopted by Günther, Johann Müller, and Pallas, and more especially by the Swedish ichthyologist, F. A. Smitt, who has applied the system of numerous measurements used in anthropology to the study of fishes, M. Zograff has undertaken to give anew a complete description of the Russian species of *Acipenser*, and the Central Asian species of *Scaphirhynchus*. He begins his work by a general description of the body of the Russian Acipenseridæ: the varying shapes of their heads; the indexes of length, width, and thickness of the body; the skin and spines; the teeth; the muscles; and the brain. The whole is accompanied by numerous engravings and coloured plates, great attention being given to the minute anatomy of all parts of the different species.

AMONGST the papers contained in the last issue of the Transactions of the Seismological Society of Japan (vol. xi.) is one on earth tremors in Central Japan, by Prof. Milne. The paper is a continuation of one on the same subject read before the same Society in 1883, which was referred to in these columns at the time. In the present paper the writer discusses recent investigations into earth tremors in Italy, describes tremor recorders, with special reference to an automatic tromometer, gives numerous tables of records of the latter instrument, and finally refers to the subject of earth tremors on mountains. The paper is one of great length, and is accompanied by numerous charts and tables, which make it a respectable volume in itself. The conclusions may, however, be given in a brief space. Prof. Milne says that his chief object has been to show the relationship which earth tremors hold to barometrical fluctuations, barometrical gradients, and the wind. He concludes that they are more frequent with a low than a high barometer, but even with the former they may often not be observed; that with a high gradient they are almost always observed, but with a small gradient only seldom: that the stronger the wind the more likely they are to be observed; when there has been a strong wind and no tremors it has often been a local wind, or one blowing inland from the Pacific Ocean; the recorded earthquakes do not appear to be connected with earth tremors, more than that both are more frequent at the same seasons; and tremors are as severe on the summit of a lofty mountain as on the plains. So far as his observations have hitherto gone in Japan, it appears that the majority of earth tremors are movements produced by the action of the wind upon the surface of the earth, and that these may often be propagated to distant places where wind disturbances have not occurred.

ON January 13, at 11.10 p.m., a faint shock of earthquake was felt in the district of Örebro, in Central Sweden. It was not accompanied by any subterranean noise.

AT five o'clock on Sunday afternoon a cyclone broke over Mount Vernon, a town in Illinois, sixty miles to the south-east of St. Louis. Many persons were killed or injured, and five hundred buildings were demolished in a few minutes. The cyclone is said to have come up from the south-west with a rotary whirling motion, sweeping a path five hundred yards wide and several miles long, within which everything was destroyed.

DURING last month the so-called "red after-glow" was observed at sunset in the vicinity of Stockholm. Varying in intensity the glare extended considerably towards the zenith.

THE Finnish Government is on the point of organizing a number of stations along the coast of Finland for the observation of the nature and peculiarities of the drift-ice during the winter months.

A LARGE block of stone with rude drawings and some Runic inscriptions has just been discovered in the island of Tjörn, on the south-west coast of Sweden. It is of particular interest as being the first of its kind found in the southern part of the province of Bohus.

A MEETING has just been held at Tönsberg, in Norway, of those interested in the Arctic seal fisheries in that country, for the purpose of considering the Scottish Fishery Board's proposals that the close time for seal should end on April 10 instead of as at present April 3, and begin on July 10 instead of July 15. Both proposals were unanimously rejected, the reason advanced being that their adoption would tend to ruin the industry, so far as Norway is concerned. The Scotch proposal that young and old seal should be treated alike during the open season was adopted. Finally, the following resolutions were passed: (1) that it was advisable that the close season should end at 6 a.m. on April 3 instead of at midnight; (2) that the law of preservation of seal should be altered so that the area covered by it should range from 60° to 70° N., and from 10° E. to the coast of Greenland. A report of the meeting will be forwarded to the Scottish Fishery Board for their consideration.

THE Norwegian Fishery Promotion Society of Bergen has petitioned the Government for a grant of £15,000 for the development of the deep-sea fisheries of Norway.

THE Merchant Taylors Company have recently voted ten guineas to the Parkes Museum to aid in its work of practical teaching and demonstrating sanitary science.

THE additions to the Zoological Society's Gardens during the past week include a Bonnet Monkey (*Macacus sinicus* ♀), two Rhesus Monkeys (*Macacus rhesus* ♀ ♀) from India, an Alpine Marmot (*Arctomys marmotta* ♀), European, a Vulpine Phalanger (*Phalangista vulpina* ♂) from Australia, presented by Mr. H. Austin Clow, F.Z.S.; three Esquimaux Dogs (*Canis familiaris*) from Greenland, presented by Mr. W. T. Tournay, F.Z.S.; three Derbian Wallabys (*Halmaturus derbianus*) from Australia, presented by Lieut. C. M. Hepworth, R.N.R.; four Alpine Accentors (*Accentor alpinus*), European, presented by the Lord Lilford, F.Z.S.; a Cardinal Grosbeak (*Cardinalis virginianus*) from North America, presented by Mr. Ayerst; four Lion Marmosets (*Midas rosalia*), an Eyra (*Felis eyra*) from Brazil, four Parrot Finches (*Erythrura psittacea*) from New Caledonia, two Common Gulls (*Larus canus*), a Black-headed Gull (*Larus ridibundus*), British, purchased; four Cereopsis Geese (*Cereopsis novæ-hollandiæ*) bred in the Gardens.

OUR ASTRONOMICAL COLUMN.

MR. TEBBUTT'S OBSERVATORY, WINDSOR, NEW SOUTH WALES.—Mr. John Tebbutt, the well-known and enthusiastic amateur astronomer of New South Wales, has just published a little pamphlet giving a history and description of his private observatory, the work of which, he remarks with justifiable pride, "has proved of sufficient importance to admit of Windsor being placed in the list of observatories in the British and American *Nautical Almanacs*, the *Connaissance des Temps*, and the *Berliner Astronomisches Jahrbuch*." And this distinction has been well earned, for the lists here given of observations made, and of papers contributed to various scientific publications, show the history of the little observatory to have been a most honourable one. Mr. Tebbutt has made all the observations himself, and until 1881 performed all the reductions; latterly he has received occasional assistance in the computations from his son or friends near. His instrumental equipment was for many years of the most modest description: for seven years it consisted principally of a sextant, and a telescope of 1½ inches aperture. In 1861 a refractor of 3¼ inches aperture, and in 1864 a transit instrument of 2 inches, were added. In 1872, Mr. Tebbutt became the possessor of an equatorial of 4½ inches, which was his chief instrument until about a year and a half ago, when he bought a fine 8-inch equatorial by Grubb, once the property of the late Dr. Bone, of Castlemaine. The observations made have been principally of comets, for a number of which Mr. Tebbutt has also computed orbits, but daily meteorological observations have been kept up for twenty-five years, the results of which have been published in five parts, and transit observations have been taken regularly for time. Mr. Tebbutt has also done good service to science by his papers on astronomical subjects in various organs of the colonial press, for hitherto the private pursuit of astronomy has been greatly neglected in the Australian colonies, and he has almost stood alone as an amateur observer. It is to be hoped that this record of his labours and his success may call forth many imitators.

PULKOWA OBSERVATORY.—The Report of this Observatory for the year ending May 31, 1887, refers to the heavy loss the institution sustained in the death of Dr. August Wagner. Owing to his death, the work of publication has been somewhat delayed; he had, indeed, finished a memoir on personal and instrumental errors for the introduction of vol. xii., but the materials he left for the stellar and planetary catalogues were not so readily dealt with. Still, it is expected that this volume, and the introduction to vol. xiv., may soon be ready for publication; vols. xv. and xvi., which will contain meridian observations for the period 1872-80, and the catalogue, are ready to follow vol. xiv. through the press. Of vol. viii., the catalogue, forming the first part—meridian observations 1840-69 of Bradley and other stars down to mag. 6—has already been distributed, and the remainder is in hand; vol. x., Prof. Struve's double-star observations, is still incomplete. The observational work of the Observatory has suffered no great change. The 30-inch refractor has been used by Dr. H. Struve for the measurement of the more difficult of Burnham's stars, the fainter satellites of Saturn and the satellite of Neptune. The old 15-inch has been used by Drs. H. Struve and Hasselberg for photographic experiments, and by Prof. O. Struve for observations of Procyon, which has now been followed through nearly a complete period of its orbital motion.

WOLSINGHAM OBSERVATORY.—The Rev. T. E. Espin reports that during the last year he has continued his sweeping for red stars and stars with remarkable spectra, and that he has published spectra of 126 objects in the *Astronomische Nachrichten*, Nos. 2788 and 2825, of which eighty-six were found in the sweeps. Fifteen of the stars were of Secchi's type IV. Three new variables of long period have been discovered, and the usual observations of variables have been made and forwarded to Prof. E. C. Pickering. The Observatory has been enriched by the present from Canon Slatter of a fine 4·8-inch equatorial by Troughton and Simms. The new edition of Birmingham's Red Star Catalogue will be ready for the printer in a few weeks.

ASTRONOMICAL PHENOMENA FOR THE WEEK 1888 FEBRUARY 26—MARCH 3

(FOR the reckoning of time the civil day, commencing at Greenwich mean midnight, counting the hours on to 24, is here employed.)

At Greenwich on February 26

Sun rises, 6h. 54m.; souths, 12h. 13m. 10·0s.; sets, 17h. 32m.: right asc. on meridian, 22h. 36·3m.; decl. 8° 48′ S. Sidereal Time at Sunset, 3h. 56m.

Moon (Full, February 27, 12h.) rises, 16h. 22m.; souths, 23h. 50m.; sets, 7h. 4m.*: right asc. on meridian, 10h. 15·3m.; decl. 12° 53′ N.

Planet.	Rises.	Souths.	Sets.	Right asc. and declination on meridian.
	h. m.	h. m.	h. m.	h. m. ° ′
Mercury..	6 53 ..	12 50 ..	18 47 ...	23 3·3 ... 1 26 S.
Venus ...	5 39 ..	9 58 ..	14 17 ...	20 20·4 ... 19 26 S.
Mars ...	22 15* ..	3 33 ...	8 51 ...	13 55·0 ... 8 58 S.
Jupiter ...	1 40 ...	5 53 ...	10 6 ..	16 14·6 ... 20 17 S.
Saturn ...	13 50 ...	21 47 ...	5 44*...	8 11·6 ... 20 35 N.
Uranus ...	21 8*...	2 41 ...	8 14 ...	13 2·6 ... 5 56 S.
Neptune.	9 38 ...	17 18 ...	0 58*...	3 42·0 ... 17 57 N.

* Indicates that the rising is that of the preceding evening and the setting that of the following morning.

Variable Stars.

Star.	R.A. h. m.	Decl. ° ′		h. m.	
U Cephei	0 52·4 ...	81 16 N. ...	Feb. 29,	18 56	*m*
Algol	3 0·9 ...	40 31 N. ...	Mar. 1,	3 12	*m*
λ Tauri...	3 54·5 ...	12 10 N. ...	,,	3, 1 28	*m*
ζ Geminorum ...	6 57·5 ...	20 44 N. ...	Feb. 28,	22 0	*m*
R Canis Majoris...	7 14·5 ...	16 12 S. ...	,,	29, 19 18	*m*
			Mar. 1,	22 34	*m*
U Monocerotis ...	7 25·5 ...	9 33 S. ...	Feb. 26,		*m*
δ Libræ	14 55·0 ...	8 4 S. ...	,,	29, 1 32	*m*
R Ophiuchi... ...	17 1·3 ...	15 57 S. ...	,,	28,	*M*
U Ophiuchi... ...	17 10·9 ...	1 20 N. ...	Mar. 1,	0 42	*m*
		and at intervals of		20 8	
X Sagittarii	17 40·5 ..	27 47 S. ...	Feb. 26,	2 0	*M*
W Sagittarii ...	17 57·9 ...	29 35 S. ...	Mar. 2,	5 0	*m*
R Lyræ	18 51·9 ...	43 48 N. ...	,,	2,	*M*
U Aquilæ	19 23·3 ...	7 16 S. ...	,,	3, 5 0	*m*
S Aquilæ	20 6·5 ...	15 17 N. ...	Feb. 26,		*m*
Y Cygni	20 47·6 ...	34 14 N. ...	,,	27, 19 24	*m*
			Mar. 1,	19 18	*m*
S Cephei	21 36·6 ...	78 7 N. ...	,,	3,	*M*
δ Cephei	22 25·0 ...	57 51 N. ...	,,	1, 21 0	*M*

M signifies maximum; *m* minimum.

Occultation of Star by the Moon (visible at Greenwich).

Feb.	Star.	Mag.	Disap.	Reap.	Corresponding angles from vertex to right for inverted image.
			h. m.	h. m.	° °
26	7 Leonis	6½	5 16	6 7	112 291

March. h.
1 ... 23 ... Mars in conjunction with and 2° 37′ south of the Moon.
3 ... 19 ... Mercury in inferior conjunction with the Sun.

Meteor-Showers.

	R.A.	Decl.	
Near δ Virginis	192°	2° N.	March 2 and 3.
From Sagittarius	280	17 S.	Very swift; streaks.

THE RELATIONS BETWEEN GEOLOGY AND THE BIOLOGICAL SCIENCES.[1]

IN the remarks which at our last anniversary I had the honour of offering from this chair, I congratulated the students of geology and mineralogy upon the new and intimate relations which, to their mutual advantage, are now growing up between those departments of science. It has, however, been suggested that, while geologists are thus being brought into closer alliance with mineralogists, the strong bonds of union which have so long united us with the biologists are becoming somewhat relaxed, and, indeed, stand in no small danger of actual dissolution.

Highly as I estimate the value of the *rapprochement* between the geological and mineralogical sciences, I for one should regard such a result as far too dearly purchased, if it necessarily involved any interruption of the close relations which have so long subsisted between geology and biology. But I cannot for one moment believe that such a grievous misfortune seriously threatens the cultivators of the two great departments of natural science.

Notwithstanding certain divergencies of opinion which have made themselves heard within an ancient University, and have awakened a faint echo in the halls of our National Museum, I cannot doubt that the teachers of geology and biology will easily discover a *modus vivendi* upon what is, after all, a subject of very secondary importance—the arrangement of natural-history collections.

No one can read recent declarations of the present Director of our National Museum without being impressed by his manifest desire to make the splendid collections under his care reflect, as completely as possible, the present condition of our knowledge of natural history. And if, on the other hand, we turn to the remarks made by the Keeper of the Zoological Department, at Swansea, in 1880, and to those of the Keeper of the Palæontological Department, at Manchester, last year, we shall find in those utterances ample guarantees that, in the arrangement of their collections, questions of practical convenience will not be lost sight of; we shall be satisfied that there is not the smallest danger of revolutionary ideas leading to the removal of "ancient landmarks," or of unattainable ideals being sought through the wholesale commingling of incongruous elements. The collections of our Universities are happily free from the conditions which must always hamper an institution where the interests of popular amusement have to be reconciled with those of scientific work; and it is for the teachers of natural science in those centres of thought to agree upon an arrangement which may best serve to illustrate their courses of instruction.

But while the discussion on museum-arrangement may be regarded as a purely academical one, which, after scintillating for a while in letters and pamphlets, died out in some not very formidable explosions at the recent meeting of the British Association, it may be wise on our part not to pass by quite unnoticed some indications of the attitude of the younger school of biologists towards palæontological science, this attitude having been very conspicuously manifested during the discussion in question.

If I rightly apprehend the views of some of my biological friends, as gathered not only from their published utterances, but also from private conversations, the position they are inclined to take up may be expressed somewhat as follows:—

"Palæontology has no right whatever to separate existence as a distinct branch of science. Fossils are simply portions of animals and plants, and ought to be dealt with as such; for all scientific purposes it is quite immaterial whether the organism which we are called upon to study expired only an hour since or died millions of years ago. Imperfect fragments can only be properly interpreted in the light afforded by the more complete structures found in recent organisms; and hence the naturalist who is engaged in studying a particular group of living organisms is the only person competent to deal with its fossil representatives. In our laboratories and our museums alike, therefore, fossil remains ought to be studied side by side with the living types which most nearly resemble them, and always by the same investigators. This being the case, it is neither necessary nor expedient that there should be a class of students whose chief concern is with extinct forms of life; and as for the geologists, they have really no farther concern with fossils than just to find them, attach a label indicating the period at which they must have lived, and hand them over to the biologist for study and incorporation in his collections. Any action beyond this can only be regarded, indeed, as an act of usurpation on the part of geologists, and must tend, not to the advancement, but to the injury of true science."

Such, so far as I have been able to gather them, are the extreme opinions which some biologists now entertain. It may, perhaps, seem presumptuous on my part to venture to offer a plea for palæontology, but there are considerations which may induce us to regard such a plea as coming better from one whose place in the ranks of the geological army lies nearer the centre than in the biological wing; from one who regards palæontology as the borderland of the geological and biological sciences—a borderland where the cultivators of both ought ever to meet, not for rivalry and aggression, but for the necessities of intellectual commerce and the advantages of mutual help.

The view of palæontology which I have ascribed, I believe not unjustly, to some biologists is one which has just such an amount of truth in it as to render it plausible, but at the same time, as I cannot but believe, is one of those half-truths which are proverbially more dangerous than downright errors. Palæontology is not, as has often been confidently asserted, simply a branch of biology; it is equally a part of geological science, and there are the strongest grounds, both of reason and expediency, for retaining it in that position. All geological science is based on the principle that the past can only be interpreted by the study of the present; Darwin was the intellectual child of Lyell, and the "Origin of Species" was the logical outcome of the "Principles of Geology." No palæontologist, worthy of the name, has ever dreamed of studying fossils except in the light afforded by the investigation of their recent analogues. Indeed, if we were to carry out the aggressive ideas of some biologists to their legitimate consequences, there would be left to us no science of geology at all; for why, it may be asked, should the study of physical processes in the past be carried on separately from the investigation of the same processes as exhibited at the present time? But then, by a strange Nemesis, I fear the same all-devouring physics, after swallowing up geology, would make very short work indeed with biology itself. And there is still in the background another claimant for universal empire in the realms of thought, for are there not some who dream of all sciences ultimately becoming the victims of that new portent of ambition—"geography"?

In considering the present position and future claims of palæontology, I may be permitted at the outset to offer a protest against a class of objections which has sometimes been very unfairly urged against the votaries of that branch of science. It has often been assumed that the students of fossils are contented with a lower standard of excellence than that which is aspired to by the cultivators of other branches of natural history. Now, setting aside for a moment the very important consideration that, owing to the imperfection of the remains which they are called upon to study, palæontologists are confronted by difficulties which do not beset the investigators of recent forms, I maintain that the charge is an altogether unjust one. Palæontologists are no more responsible for the unwise use made of fossils by incompetent persons than are zoologists for the vagaries of shell- and butterfly-hunters, or botanists for the absurdities of fern- and diatom-collectors.

Doubtless there has been much work done in connection with

[1] Address to the Geological Society by the President, Prof. John W. Judd, F.R.S., at the Anniversary Meeting, on February 17.

fossils, as well as with other natural history objects, of which we can only speak with shame and regret as having been undertaken unadvisedly and performed ignorantly,—work which, prompted by an unwise ambition, has been conceived in error and brought forth in presumption.

It would ill become anyone from this chair to speak lightly of the great, the inestimable services rendered to our science by the collectors of fossils. How many interesting and novel forms have been brought to light by their patient efforts! How often has the structure of obscure types been rendered clear through their constant and persevering endeavours to obtain more perfect specimens! Yet sometimes the very zeal of collectors has led them astray. Despairing of finding systematic zoologists and botanists who could devote the necessary time and attention to the study of objects which they have obtained with so much trouble and pains, they have unwisely undertaken, without the necessary training and knowledge, the naming and description of forms of life which required for their proper interpretation all the skill and experience of the most able comparative anatomist or vegetable morphologist.

I feel sure that, if those who have thus erred, through acting with "a zeal which is not according to knowledge," could realize the injury done to science by such proceedings, they would pause before burdening scientific literature with premature names, imperfect diagnoses, and ill-digested materials. Fossils are, it is true, "the medals of creation," and for the purposes of the historian of past geological times, it may seem that any name, however bad, which can be employed for purposes of reference must be better than none at all. But fossils, it must be remembered, are much more than mere "medals." They are the precious relics of the faunas and floras of bygone times; landmarks—the only ones we can ever hope to discover—which may serve to guide us in tracing the wonderful story of the evolution of the existing forms of life. Reverently—as the mineralogist treats meteorites, those pocket-planets and errant members of the outer universe—should the biologist regard fossils, the fragments of an earlier life, the collateral, if not the direct, ancestors of living types.

So far I am from thinking that the study of fossils ought in all cases to be undertaken by those who are actually engaged in working out their recent representatives, that I believe such a practical abolition of palæontology as a distinct branch of science would tend, not to the advantage, but to the injury, of both biology and geology. And I will venture to set forth my grounds for this conclusion.

It may be remarked at the outset that at a time when all the tendencies of biological science appear to be towards an extreme specialization, it is strange to find that there are advocates for the suppression of what is now so well-developed a department of biological science as palæontology. When the work to be done has become so vast that some biologists feel themselves compelled to restrict their studies and labours to the morphological, or even to the histological department, others to the embryological, the physiological, the taxonomic, or the chorological branches of zoology or botany respectively, why should not some concentrate their efforts upon the elucidation of the ancient forms of life? When the study of a single group, often a very limited group, of animals or plants is sufficient to exhaust the energies of a particular naturalist, it is surely not unreasonable that forms which have become extinct and have left only very imperfect evidence of their structure and affinities, and these requiring peculiar methods for their study, should attract the attention of special investigators.

The study of fossils, we may remark, if it be undertaken by any biologists, must fall to systematic zoologists and botanists, and these have become somewhat rare and out of fashion in modern times; so few in numbers, indeed, do they seem as to be scarcely able to cope with the ever-increasing array of living forms; and it would be a hopeless task if upon them were also cast the overwhelming mass of fossil ones.

Imagine the embarrassment and dismay of a student of living sponges, whose favourite (possibly his only) method of research has consisted in studying with the microscope innumerable thin slices cut from tissues and embryos, if a cartload of chalk-flinks were thrown down at his door, and he were required to interpret the fragments of sponge-skeletons which they contained in every conceivable variety of disguise through peculiar processes of mineralization!

There are, indeed, a variety of special reasons why ordinary systematic zoologists and botanists become, by the very habits acquired in their daily pursuits, singularly ill fitted for dealing with fossil forms.

In studying recent forms the zoologist or botanist is bound to take into consideration, in fixing the systematic position of an organism, not only its skeleton, but all its soft parts, and even the structure and mode of development of its embryo; he may also be called upon to note physiological peculiarities, before he is in a position to arrive at a decision as to its place in the zoological or botanical series. But for the student of fossil forms none of these aids are available, he is compelled to do his best without them. Investigators of the recent Mollusca are, of course, "malacologists," but he who studies the extinct forms of the group must perforce labour under the stigma of being "a mere conchologist." In examining recent vertebrates it is allowable to make every possible use of the aid afforded by a study of the ligamental skeleton, in unravelling their affinities; but he who works on fossil vertebrates is and must remain a pure osteologist. Botanists have been led to the conclusion that for the classification of plants the reproductive organs always afford the safest guides; but palæontologists, alas! are frequently called upon to do their best in deciphering fragmentary remains of the vegetative organs.

It is not, as some biologists would almost seem to imagine, that palæontologists are led by any perversity of mind to reject the light which is afforded to them, or that they are not deeply sensible of the great value and importance of many recent researches in respect to living forms; but simply that they realize—often very sadly realize—the impossibility of availing themselves of the help afforded by such researches, in connection with the very imperfect material with which they are called upon to deal.

If we were to suppose that a surveying ship brought home from a newly-discovered island a heterogeneous mixture of [illegible] bones and teeth, of shells, bits of stick and fallen leaves, zoologists and botanists might be perfectly justified in refusing to waste their time upon such unsatisfactory materials. But if, subsequently, news arrived that after the departure of the ship the whole island had sunk beneath the ocean, then the circumstances would have completely changed, and no pains and care would be felt to be too great if expended in dealing with such a unique collection, however imperfect it might be. Or, to take a case which has actually occurred, the curators of the Ashmolean Museum were fully justified in ordering the destruction of the moth-eaten dodo skin, so long as they had no reason for doubting that other and better specimens were procurable; but now no labour and pains is considered too great in studying the most imperfect fragment of the bird.

And here I may perhaps be permitted to say a word in defence of what has been treated as an absurd practice on the part of palæontologists—that of giving names to small fragments of organisms. It must be admitted that when subsequent investigation proves that distinct generic and specific names have been given to the root, the stem, the outer and the inner bark, the pith, the foliage, and the fruit of the same plant, the absurdity does seem striking. But it is impossible to defer giving a name to a fossil until all doubts about its structure and affinities have been completely settled by the finding of exceptionally perfect specimens. Nevertheless, it ought certainly to be insisted on that names should be given to very fragmentary fossils only by a competent naturalist, and that he must accept the responsibility of his act. A single tooth of a mammal may afford good grounds for the establishment of a genus and species, while it might be utter folly to treat the tooth of a shark in the same manner.

The remains of many extinct forms are in such a peculiarly mineralized condition as to require special skill and training for their proper interpretation. Skeletal elements which were originally siliceous are now represented by pseudomorphs in calcite, and *vice versâ*. Characteristic structures in bones, shells, or wood may be wholly obliterated, and mineral structures of a strangely deceptive kind may be developed in their place. The curious story of *Eozoon canadense* and its supposed allies is surely a sufficient justification for the existence of palæontologists—that is, of specialists trained equally in the interpretation of biological and petrological structures. Dr. Sorby has shown that whole families of Mollusca may disappear from a fauna because of the unstable condition of the calcic carbonate which composes their shells, and his conclusions have been confirmed by Mr. Kendall. Prof. Sollas has similarly shown that the absence of the por-

cellanous types of the Foraminifera from the Palæozoic rocks may be due, not to their non-existence when those rocks were formed, but to the fact of their shells being composed of the unstable aragonite.

Such facts as these must convince any unprejudiced person of the absolute necessity, to the naturalist who attempts to study fossil forms, of an acquaintance with the nature of the mineral changes which organic remains undergo. In his interesting memoir upon those curious and enigmatical fossils, the Receptaculitidæ, Dr. Hinde has admirably shown the advantages of this combination of biological and petrographical study.

In this connection I cannot avoid alluding to a very prevalent and, as I cannot help thinking, very erroneous notion, that an intermingled zoological and palæontological collection, however inconvenient, would certainly be very instructive. To this view I offer the strongest protest, for I believe that the mistakes which would arise from the examination of such a collection would far outweigh any instruction to be derived from it.

I fail to see what useful lesson would be taught by swamping a collection of the lizards, snakes, tortoises, and crocodiles living at the present day with the vast slabs containing the relics of Reptilia which have existed in periods ranging from the Permian to the Pliocene. Nor is it apparent to me why the precious remains of *Archæopteryx* should be hidden away among a wilderness of bird-skins.

Any arrangement which could lead to the idea that even the richest collection of fossils is in any way commensurable with the assemblages of specimens that in our museums represent the existing fauna is very greatly to be deprecated. So numerous are the gaps among fossil faunas, owing to the fact that only animals with hard parts, and, as a rule, only those that lived in the sea, had any chance of preservation, that the finest palæontological collections are, and must always remain, extremely fragmentary. We have, in the past, fallen into so many and such grievous errors, by ignoring the imperfection of the geological record, that we may well hesitate before doing anything that would confirm this mischievous delusion.

On the other hand, it may be pointed out that our acquaintance with extinct forms of life has increased to such an extent in recent years that a biologist may well be pardoned for not realizing the vastness and importance of the problems involved in the study of fossils. It can only be a very inadequate idea of the value of palæontological evidence which leads fossils to be regarded (like the fauna and flora of a newly-discovered territory) as simply supplying a few missing links required to fill up gaps in a natural-history classification, or as the appropriate ballast for a Noah's Ark on a scale of national grandeur. Small as may be the whole bulk of a palæontological collection in the eye of the student of recent forms, its great and transcendent value depends on the fact that the objects composing it belong to the faunas and floras of periods widely separated from the present and from one another. The discovery of a new type of reptiles in the Trias is a very different matter from the detection of an equally remarkable form living in New Zealand. The latter may, it is true, be a singular survival of some old type; but the former is an actual landmark in the course of reptilian development; and by the study of the fossil we are actually brought much nearer to the solution of the problems connected with the history of that development than is possible by the study of any recent form.

In pointing out how vast has been the progress of our knowledge in recent years concerning the ancient life of the globe, I may remind you of the estimates made by Prof. Huxley when speaking from this chair a little more than a quarter of a century ago. He then characterized "the positive change in passing from the recent to the ancient animal world" as "singularly small"; and he regarded the extinct orders of animals as not amounting "on the most liberal estimate" to more than one-tenth of the whole number known. The evidence which has been accumulated during the last twenty-five years, however, has modified this estimate in a remarkable manner, as no one would be more ready to admit than the author of it himself.

There is no little difficulty in making a calculation of the proportion of living to extinct orders, owing to the discrepancies in the opinions of zoologists and comparative anatomists as to what are the characters which ought to be considered as of ordinal rank. For my present purpose I very gladly avail myself of the useful "Synopsis of the Animal Kingdom" prepared by Mr. E. T. Newton, which is "founded on the classification proposed by Prof. Huxley, with such modifications as are rendered necessary by recent discoveries"

We may, I think, take the whole number of living orders of animals generally accepted by zoologists at about 108. But in any comparison of these with fossil forms, it is only fair to exclude from our consideration such as possess no hard parts and stand little or no chance of being preserved in a fossil state. Few would be bold enough to doubt that such soft-bodied forms must have existed in the past, or that they probably bore about the same proportion to the forms with hard skeletons as in the existing fauna; even the boldest sceptic on this subject would, I should think, be convinced by such singular accidents as that of the finding of the impression of *Rhizostomites*, one of the Discophoræ, preserved in the soft calcareous mud of the Solenhofen Slate.

Now among the 108 living orders of animals, at least 36 are totally destitute of any hard parts capable of being preserved in a fossil state, and we have thus left 72 living orders with which our comparison of the extinct orders must be made.

What is the number of orders which must be created to receive extinct forms, is a question that has given rise to wide diversities of opinion in recent years. While few naturalists would consider 18 as an excessive estimate, there are others who would probably double that number.

Taking the lower estimate and comparing the 18 extinct orders with the 72 living ones which contain animals with hard parts, we find the proportion of extinct orders to be 20 per cent. of the whole number known at the present time.

But in comparisons of this kind, it must be remembered that there is an unconscious tendency among the students of recent forms of life to *under-estimate* the differences between extinct and living forms. If we take such groups as the *Graptolitidæ*, the *Monticuliporidæ*, and the *Stromatoporidæ*, of the nature of the polyps of which we can know nothing, we can only place them in existing orders on the ground of some very general analogies in the skeleton. How little this may be worth, recent zoological researches, like those of Prof. Moseley on the *Milleporidæ* and the *Stylasteridæ* have amply shown.

The students of existing forms of life have arranged their pigeon-holes; and into those pigeon-holes our unfortunate fossils are too often *made* to go. If there were no other objection to the wholesale commingling of recent and fossil types in a museum, there would be the valid and insuperable one arising from the fact that there are very considerable and important groups of fossils which cannot, without violence, be made to find any place in our accepted classification of existing animals—and perhaps never will.

If, however, we consider the modifications which have been brought about in our views concerning the relations of extinct to living forms by the important discoveries that have been made since 1862, we shall be impressed by the conviction that no comparison of the numbers of living and extinct orders can give any adequate idea of the important influence of palæontological studies upon biological thought. The discovery of transitorial forms, like the *Archæopteryx*, the toothed birds of America, and the reptiles with avian affinities, together with the working out of the rich faunas of the Rocky Mountains, of Pikermi, Quercy, and the Siwaliks, of the Pampean formations of South America, the Karoo beds of South Africa, and the caves of Australia, have already done much towards revolutionizing the ideas held twenty-five years ago by biologists concerning the significance and value of fossil forms. While the recognition of the less specialized precursors of such types as the horse and the elephant have perhaps produced most effect in removing objections to evolutionary doctrines, the light thrown by the study of fossil forms on the manner in which individual structures have arisen, as has been so well shown by Prof. Alexander Agassiz, in the case of the Echinodermata, opens up to us a wide and perhaps far more hopeful field of inquiry. We are, however, only at the beginning of the great task of utilizing the grand palæontological collections of mammals, of reptiles, of fishes, and of the various groups of the invertebrates, for explaining the significance and tracing the origin of the structures found in living types.

While maintaining that studies of this kind demand and justify the concentration of the labours of a special class of investigators, I feel sure that no one will misinterpret my meaning as to the qualifications required by the students of fossil forms. Far from suggesting that the palæontologist may be one destitute of a proper biological training, or that he may be satisfied with an equipment of knowledge which would be insufficient for a systematic zoologist or botanist, I would maintain that no one

has a right to take up the study and description of any fossil group until he has made a very careful and exhaustive study of its nearest living allies; but, in addition to this, he ought also to have made himself acquainted with the peculiar mineral changes which organic remains are liable to undergo. He will, moreover, be far more likely to interpret aright and to make the best use of the materials that come to his hand, if he have at least a general knowledge of what others working on similar materials belonging to other departments of the animal or vegetable world have been able to accomplish, and of the methods which they have followed. Such palæontologists, I insist, have as much right to recognition as any other class of biological specialists.

Still less should I wish it to be implied that I think systematic biologists can afford to be ignorant of the results of palæontological studies, in their own particular fields of labour. One of the most mischievous weeds that have accompanied the evolutionist in his incursions into various parts of the biological field is the preposterous "genealogical tree." We can scarcely turn over the leaves of a modern systematic work without finding it flourishing in full luxuriance. No sooner has the student of a particular group arranged his families, genera, and species, than he thinks it incumbent upon him to show their genetic relations. Very admirably has Prof. Alexander Agassiz pointed out the utter fatuity of such a proceeding. As Lyell used to say, in speaking of such proceedings, the imagination of the systematist, untrammelled by an acquaintance with the past history of the group, "revels with all the freedom characteristic of motion *in vacuo*." If for no other reason, zoologists and botanists ought to study fossil forms in order that, by encountering a few hard facts in the shape of fossils, they may be saved from these unprofitable flights of the imagination.

(*To be continued.*)

SCIENTIFIC SERIALS.

Rendiconti del Reale Istituto Lombardo, December 1887.—On the Tertiary formations near Cape La Mortola, in Liguria, North Italy, by Prof. T. Taramelli. The paper deals specially with the abrupt interruption which occurs in the prevailing Eocene and Secondary systems about this part of the Ligurian coast. This interruption is brought into connection with the great development in Liguria of the marine Pliocene formation, which in the Varo basin and near Ventimiglia stands at a present altitude of over 550 metres above the sea, but which does not occur at all further east in Istria and Friuli, where it is represented by thick alluvial deposits of vast extent.—On the neutralizers of tubercular virus, by Prof. Giuseppe Sormani. In continuation of his previous studies, the author here deals with twenty-one additional substances, or chemical reagents, making eighty altogether. According to their different action on Koch's Bacillus these are grouped in three categories: those that have no effect; those that only attenuate, and those that entirely destroy, the virus. As many as twenty-two, including camphorated chloral, the bromide of ethyl, and the nitrite of ethyl, are found to be effective.—Meteorological observations made at the Brera Observatory during the month of November 1887.

Rivista Scientifico-Industriale, January 15.—The crepuscular tints in connection with the hygrometric state of the atmosphere, by Prof. Costantino Rovelli. Constant observation shows that red and orange tints prevail in a dry, yellow and green in a moist, state of the atmosphere. This suggests a threefold division of the solar spectrum into (1) the region of warm rays transmitted by the lower atmospheric strata, and corresponding to a dry condition of the air; (2) the region of middle rays, yellow and green, more easily diffused and partly transmitted by the air in moist weather; (3) the region of cold rays diffused by an atmosphere abounding in aëriform vapour. The terrestrial dust suspended in the air, by condensing the aqueous vapour, as is now generally accepted, may also tend to produce those occasional after-glows of intense brightness, which have been so often observed after violent volcanic eruptions. The various character and intensity of the tints may all be thus explained by the theory of the eclectic transmission of the coloured rays by the corresponding states of the atmosphere, and partly also by the particles of dust held in suspense.—On the constitution of fogs and clouds, by Prof. F. Palagi. These phenomena are attributed to the presence of minute drops of water with diameter of 1/10 to 1/20 mm. at a temperature above zero. The recent observations made by the author on Mount Titano show that when the temperature falls below zero these globules are converted into minute hexagonal needles and flakes of the same form, the former about 1/20 mm. thick, and from two to ten times longer, the latter from 1/10 to 1/4 mm. in diameter. In their passage fro[m] the higher regions through the lower and less cold strata even still below zero, these simple crystalline forms appear to be [in]formed by the process of condensation and agglomeration into the stars and flakes of ordinary snow. But when the temperature rises above zero they are again changed to the minute liquid drops of clouds, fog, and rain according to the varying degrees of altitude and temperature.

Bulletin de l'Académie des Sciences de St. Pétersbourg, vol. xxxii. No. 1.—On the effects of the earthquake of February 23, 1887, at the Observatory of Pavlovsk, by Dr. Wild (in German). The effects of the catastrophe having been observed at the Observatories of England, France, Italy, Germany, and Austria, in trepidations of the magnetic instruments, it was interesting to see whether the earthquake was felt as far as St. Petersburg. The results indicate that it was not.—On the genus Hemiculter and a new species of Hemiculterella, by N. Warpachowski (in German).—Russian words used in the Sagai dialect, and their phonetic modifications, by N. Katunoff; and lists of Sagai names of rivers, villages, and tribes, by the same. This little dictionary is highly spoken of by M. Radlof.—Studies, by O. Backlund, about the Pulkowa catalogue of stars, "Positions moyennes de 3542 étoiles," published in 1886 (in German). A detailed comparison of the Pulkowa catalogue with the measurements by Lamont, Romberg at Pulkowa, as also with the catalogues of Becker, Santini, pighi, and Boss.—Hydrological researches, by Dr. Carl Schmidt [illegible] —The temperature-maxima before midday in tropical [illegible] according to the observations of the corvette *Vitiaz*, by M. Rykatcheff (in German). They show the existence of two separate maxima, one of which sets in half an hour before midday and the other half an hour later. More extensive observations are needed.—On the synthesis of albumen in chlorophyll-bearing plants, by Chrapowitzki (in German). The chlorophyll spots must be considered as places where synthesis of both carbohydrates and albumen is going on.—New additions to the Asiatic Museum, by C. Salemann. Summaries of two Persian and three Kagatai manuscripts brought in by M. Pantusoff from the Semiryetchensk province.

THE *Izvestia* of the Russian Geographical Society (1887, iv.), contains most valuable papers and maps. Dr. Junker contributes a report on his seven years' journeys in Equatorial Africa, and his paper is accompanied by a map, 53 miles to the inch, of the region extending for ten degrees on the north of the Equator, between the 22nd and 33rd degrees of longitude. Two papers, by M. Potanin, contain a summary of the information gathered from the natives as to Eastern Tibet (the regions of Amdo and Kam), and the region of Central Mongolia situated between the Nan-shan, the Khangai, Hami, and the Utai-shan. Both papers are accompanied by maps, on a scale of 100 miles to an inch, and the two maps complement one another, so as to give a very accurate idea of the upper Hoang-ho. Of the other papers, one by M. Krasnoff, on the manners of life of the Kirghizes in the Semiryetchensk province, will be welcome to ethnographers. The same number contains also a list of fifteen places in Laponia, the latitudes and longitudes of which have been measured in 1864 by Captain Ernefeld; and, in a separate appendix, tables, by Prof. Sharnhorst, for the calculation of heights from barometrical observations. It is self-evident, although it is too often lost of sight, that the calculation of heights upon observations of the barometer, when it is made by means of logarithms, means a much greater accuracy of results than anything that can be obtained from a few observations of atmospheric pressure during a journey, and that some plainer tables would give the results with an accuracy quite sufficient for the accuracy of the data themselves. M. Sharnhorst's tables are an improvement upon those formerly in use, and ought to be introduced into every manual for travellers, instead of the usual logarithmical tables.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, January 26.—"Report on Hygrometric Methods. First Part, including the Saturation Method and the Chemical Method, and Dew-point Instruments." By W. N

Shaw, M.A. Communicated by R. H. Scott, F.R.S., Secretary to the Meteorological Council.

With the exception of certain "absolute hygrometers," the behaviour of which has not yet been sufficiently tested, the determination of the pressure of water-vapour in the air is indirect, and requires a formula of reduction. The formulæ in use are based upon assumptions which are at present not so completely verified by experiment that any hygrometric method can be relied upon to give measures of the pressure of aqueous vapour trustworthy to within 0·1 mm. of mercury. The authority for these statements is given in detail in an account of the hygrometric work done since 1830, appended to the report as Note A.

In the report, the chemical hygrometric method is provisionally recommended as a standard.

The assumptions upon which the formula of the method is based are (1) that it is possible to absorb the whole of the moisture from air by passing it over desiccating substances; and (2) that a numerical value can be assigned to *d*, the specific density of aqueous vapour referred to air at the same temperature and pressure. The first assumption is sufficiently nearly accurate for hygrometric observations. With regard to the second, Regnault's direct observations upon steam (free from air) and other evidence point to the value 0·622. The assumption can, however, be tested, by applying the chemical method to air saturated at a known temperature, assuming the value 0·622 for *d*, and comparing the results with the table of saturation pressures *in vacuo*. This, however, *assumes Dalton's law to be accurate*, an open question upon which opinion is reserved until further experimental investigation is concluded. Regnault found that the value 0·622 gave results for saturated air which were less than the tabulated pressures, the errors being of the same sign, but so small in amount that he neglected them in his subsequent work.

The ultimate object of the experiments described in the report was to examine the behaviour of dew-point instruments in air of known state, and for this purpose air was saturated at a known temperature and drawn by an aspirator through vessels in which the dew-point instruments could be placed when required, and subsequently through drying tubes of special pattern. The vapour-pressure was thus obtained at the two extremities of the series of apparatus and the results compared.

The following questions are raised and discussed:—(1) Were the drying tubes used as efficient as Regnault's? (2) Does the pressure of vapour in the air become changed by passing through the apparatus designed to contain the dew-point instruments, or by the mere presence of those instruments themselves? (3) Do the results of the chemical method agree with the tabulated vapour-pressures *in vacuo* when the air is more or less heated after being saturated? (4) Can the observed differences between the results be obviated by assuming a value for *d* (other than 0·622), which is compatible with values obtained by other methods? (5) Can any reason be assigned for the differences observed by Regnault in the case of saturated air?

(1) The answer to the first question is given in an account of a series of twelve experiments practically repeating Regnault's observations with saturated air. The tabulated results show divergences in the same direction and of the same order of magnitude as those in Regnault's paper. Some incidental points are also discussed—namely, the comparative efficiency of phosphoric anhydride, sulphuric acid, and calcium chloride, and the effect of india-rubber and glass connections between drying tubes. It is shown that the sulphuric acid and phosphoric anhydride tubes are efficient, that as a rule one tube is all that is strictly necessary, but that two should be used to provide for the case of exhaustion of the first tube or too rapid flow of air, and further, that the glass-and-mercury connections between the tubes employed in the second series of experiments cannot be regarded as producing any effect.

(2 and 3) The answers to the second and third questions are furnished by the results of eighty-two experiments with the chemical method upon air saturated at known temperatures by a specially designed "saturater" in a water-bath. The temperatures of saturation lay between 1° C. and 21° C., and, with one exception, were below the temperature of the surrounding air. Each experiment involves upwards of thirty readings of weight, pressure, and temperature. The temperature readings are corrected by means of a special comparison at Kew. Of the eighty-two observations thirty-two are retained as being free from any known disturbing causes, and from them it appears that, with *d* equal to 0·622, the pressure deduced by the chemical method is on the average greater by 0·03 mm. than that given in Regnault's table of vacuum pressures, as recalculated in Landolt and Börnstein's tables. This difference is very small compared with the discrepancies from Dalton's Law observed by Regnault in the case of water vapour.

(4) With regard to the fourth question; if the observations be employed to determine the value which must be substituted for *d*, the value obtained is 0·6245, which agrees very closely with 0·6240, the mean value for the same range of temperature deduced from Clausius's calculations based on thermo-dynamical reasoning. The value 0·622 is probably correct if the air is not nearly saturated; in that case the measure of the pressure of vapour in the air is 2/622 greater than it would be if the same air were reduced in temperature (at constant pressure) until it was saturated.

(5) The one observation of the second series with saturated air gives a result 0·18 mm. smaller than the tabulated pressure, and thus with the twelve experiments of the first series confirms the results of Regnault's observations. To account for this it is suggested that air which is very nearly or quite saturated would deposit some of its moisture on the glass tubes used to conduct it from one vessel to another. This behaviour of nearly saturated air has been already noticed, and it is confirmed by the observations on dew-point instruments, and moreover, by experiments, directly designed for the purpose, quoted in a note.

Details are given of observations with Regnault's hygrometer and Dines's hygrometer when exposed in glass vessels between the saturater and the drying tubes. The two instruments are separately discussed. With Regnault's instrument, after some practice, two different observers obtained practically identical results. In ordinary observations, the observed temperatures of the dew-point were below the temperature of saturation, but seldom by more than 0°·1 C. A considerable amount of uncertainty was shown to be attached to the readings, and by very close inspection readings of the dew-point were obtained above the temperature of saturation, in one case by as much as 0°·7 C.

From the experiments with Dines's hygrometer, it appears that the instrument is likely to give very easy determinations of the dew-point that are within small limits of error; but that, if it be observed with the closest attention, the result will be considerably too high in consequence of the formation of a dew deposit at a temperature above the dew-point, and it may possibly be erroneous in consequence of variations in temperature of the different parts of the box containing the thermometer.

An account is given of Alluard's modification of Regnault's hygrometer, and of Bogen's hygrometer.

A second note, B, is appended to the report, showing the tables used in various countries for the reduction of wet and dry bulb observations.

Chemical Society, February 2.—Mr. W. Crookes, F.R.S., in the chair.—Profs. Geuther, Ladenburg, Landolt, Nilson, Van't Hoff, Wislicenus and M. Lecoq de Boisbaudran were elected foreign members of the Society.—The following lecture was delivered:—The range of molecular forces, by Prof. A. W. Rücker, F.R.S. In discussing the range of molecular forces it is convenient to adhere to the language of the theory of action at a distance, though with full expectation that it will ultimately be replaced by another, such as the vortex-atom theory of Sir W. Thomson, or the granular theory of Prof. Osborne Reynolds, which involves only action in proximity. If we do this, however, it must be admitted that the law of molecular action may be very complicated. It may be granted that we naturally look for simplicity in our fundamental assumptions, but it is certain that we have *a priori* no more right to expect simplicity in the results of the action of a medium than simplicity in its constitution, and that the two are not necessarily obtained together. The largest values of the magnitude of the radius of molecular action which have been published have been deduced from observations on the condensation of gases and vapours on the surfaces of solids. Estimates on this basis made by Müller-Erzbach (*Exner's Rep.*, 1885, xxi. 409) and Kayser (*Wied. Ann.*, 1881, xiv. 450) have ranged between 1500 and 3000 micromillimetres[1] ($\mu\mu$). Such observations are open to many

[1] The micromillimetre is the millionth of a millimetre.

objections. Bunsen (*Wied.*, 1885, xxiv. 335) has shown that CO_2 will not condense on glass unless a film of water be previously formed. Warburg and Ihmori (*Wied.*, 1886, xxvii. 481, and *Wied.*, 1887, xxxi. 1006) adduce reasons for believing that the water film is largely due to uncombined or loosely combined alkalies on the surface. On clean unvarnished metals, washed glass and quartz, the thickness of the water film which can be removed by dry air without heating does not exceed 12 $\mu\mu$. A striking exception is agate, on which films 1640 $\mu\mu$ thick are stated to have been formed. As this substance, however, is composed of alternate layers of quartz and a porous impure opal, the basis for an accurate calculation does not exist. On the whole, it seems that no definite conclusions as to the magnitude of the radius of molecular action (ρ) can at present be drawn from these experiments. Quincke (*Pogg. Ann.*, 1869, cxxxvii. 402), as is well known, by measuring the capillary elevation of liquids between glass plates coated with thin wedge-shaped films, found $\rho = 50\,\mu\mu$. Plateau ("Statique des Liquides," 1873, i. 210) showed that the surface-tension of a soap-bubble, which thinned until its thickness was 118 $\mu\mu$, was unaltered. He concluded that $\rho < 59\,\mu\mu$. Maxwell ("Ency. Brit.," 9th ed., Art. "Capillary Action"), however, though by a confessedly imperfect theory, shows that the surface-tension may not change until the thickness of the film $= \rho$. Hence Plateau's result may mean only that $\rho < 118\,\mu\mu$. Reinold and Rücker (Phil. Trans., clxxvii. Part ii. 1886, 627) have proved that the surface-tension does not alter by 0·5 per cent, when the film is so thin as to show the black of the first order of Newton's colours. This appears at first sight at variance with Quincke's result, but their observations are really in remarkable accord with his. The black and coloured parts of a film are separated by a sharp line, which proves a discontinuity in the thickness (Proc. Roy. Soc., 1887, No. 182, 340). The colours, which correspond to certain thicknesses, which may be called the unstable range of thickness, are always missing. The black part of the film has been proved by Reinold and Rücker (Phil. Trans., Part ii. 1883, 645) to be of a uniform thickness, which differs but little from 12 $\mu\mu$. Sir William Thomson (Proc. Royal Institution) and these observers independently arrived at the conclusion that these curious phenomena are due to the fact that the surface-tension diminishes to a minimum, and then increases again when the thickness is somewhat $>12\,\mu\mu$. The colours of the film prove that the upper limit of the range of unstable thickness is between 96 and 45 $\mu\mu$. Quincke's result indicates that it lies between 100 $\mu\mu$ and 50 $\mu\mu$, according as we adopt Plateau's or Maxwell's views. These calculations are therefore in complete accord. Quincke's result is not an isolated fact, but is supported by observations on soap films. The statement that 50 $\mu\mu$ and the radius of molecular action are of the same order of magnitude may now perhaps rank as an ascertained fact. Another method of investigating the radius of molecular action is based on the phenomena of electrolytic polarization, by observing the change in the difference of potential between a metal and a liquid in which it is immersed, when a gas or metal is deposited on it by electrolysis. In the former case we do not know the density of the gas, in the latter Oberbeck (*Wied.*, 1887, xxxi. 337) concludes that a plate of platinum is completely polarized by a film of another metal of from 3 to 1 $\mu\mu$ in thickness. The method of experiment is, however, open to objections, which are indicated by Oberbeck himself. Measurements of the thickness of the double electric layer of Helmholtz, which is closely related to the distance between two consecutive layers of molecules, have been made by Lippmann (*Compt. rend.*, 1882, xcv. 687), and by Oberbeck and Falck (*Wied.*, 1884, xxi. 157). The values they give vary between 1 and 0·02 $\mu\mu$. Wiener (*Wied.*, 1887, xxxi. 624) has studied the alteration in the phase of light reflected from very thin silver plates deposited on mica. He finds that the effect begins to alter when the thickness is reduced to 12 $\mu\mu$, and that it was possible to detect a silver film the thickness of which did not exceed 0·2 $\mu\mu$. The diameter of a molecule is a conventional term for the mean distance of the centres of two molecules during an encounter. It may therefore be different in the liquid and gaseous states. Sir William Thomson ("Natural Philosophy," Thomson and Tait, Part ii. 295, 1883), as the result of his celebrated discussion of this point, concludes that the mean distances between the centres of molecules in liquids (supposed arranged uniformly) is between 0·07 and 0·02 $\mu\mu$, and that the latter quantity is an inferior limit to the diameter of a gaseous molecule. The diameters of molecules (d) may be calculated if we know the mean free path (L), and the so-called condensation coefficient (v), which is the volume the molecules contained in a unit volume of the gas. Loschmidt (*Sitzungsber. Wien. Akad. Math. Classe*, lii. abt. 2) and O. Meyer ("Die Kinetische Theorie der Gase," 225, 1887) first calculated d on the assumption that the molecules in a litre practically fill the whole space it occupies. Exner (*Rep. Physik*, xxi. 226, 1885), using a formula given by Clausius, $v = (K - 1)/(K + 2)$, where K is the specific inductive capacity and can be replaced by $v = (n^2 - 1)(n^2 + 2)$, where n is the refractive index, finds values of d about five times smaller. Three independent methods of calculating the diameter of a gaseous hydrogen molecule lead to results between 0·14 and 0·11 $\mu\mu$. The most reliable conclusions which have been reached as to molecular magnitudes may be summed up in the following table, which is reproduced from a diagram exhibited during the lecture.

$\mu\mu$.		
118	Superior limit to ρ	Plateau (Maxwell)
96–45	Range of unstable thickness begins	Reinold and Rücker
59	Superior limit to ρ	Plateau
50	Magnitude of ρ	Quincke
12	Range of unstable thickness ends	Reinold and Rücker
12	Action of silver plate on phase of reflected light alters ...	Wiener
10·5	Thickness of permanent water film on glass at 23° C. ...	Bunsen
4·3	Mean distance between centres of molecules in gases at 760 mm. and 0° C.	O. Meyer, by M.
3·1	Thickness of metal films which polarize platinum	Oberbeck
1–0·02	Thickness of electric double layer	Lippmann and Oberbeck
0·2	Smallest appreciable thickness of silver film	Wiener
0·14–0·11	Diameter of gaseous hydrogen molecule	Exner O. Meyer Van der Waals
0·07–0·02	Mean distance between centres of liquid molecules	W. Thomson
0·02	Inferior limit to diameter of gaseous molecule	W. Thomson

—The following papers were read:—A new method of obtaining monohydrazides of α-diketones, by Prof. F. R. Japp, F.R.S., and Dr. F. Klingemann. The authors have prepared with Pechmann's monohydrazide of diacetyl by the action of azobenzene chloride on sodium methacetate.—The formation of dihydrazides of α-diketones, by the same.—The action of phenylhydrazine on anhydracetophenonebenzil, by Prof. F. R. Japp, F.R.S., and Mr. G. N. Huntly.—The supposed identity of rutin and quercitrin, by Dr. E. Schunck, F.R.S. A comparative examination of rutin obtained from the leaves of *Polygonum fagopyrum* and of quercitrin shows that, though they are extremely similar, yet they differ in composition and in some of their properties. Rutin has the composition $C_{42}H_{30}O_{25}$, and yields, on hydrolysis, one molecule quercetin and three molecules isodulcite, whilst quercitrin $C_{36}H_{28}O_{20}$, as is known, yields, under like conditions, one molecule quercetin and two molecules isodulcite.—The composition of bird-lime, by Dr. E. Divers, F.R.S., and M. Kawakita. Japanese bird-lime prepared from *Ilex integra* contains, in addition to 6 per cent of caoutchouc and minute quantities of oxalates, the ethereal salts of palmitic acid, and, in small quantity, of a semi-solid undetermined fatty acid. On hydrolysis these yield *ilicyl alcohol*, $C_{32}H_{66}O$, differing only slightly from Personne's ceryl alcohol, and *mochylic alcohol* $C_{26}H_{44}O$. A resinoid body $C_{26}H_{44}O$, was also separated. When heated with palmitic acid the two alcohols are converted into compounds just like purified bird-lime. The authors consider bird-lime to be closely allied to the *waxes* in chemical constitution.

ERRATA.—P. 335, line 15 (from top), *for* $3SOH_2SO_4$ *read* $3H_2SO_4$; line 19 (from top), *for* SO *read* SO_3.